f-Rayet Stars

Proceedings of a Symposium held at the Joint Institute for Laboratory Astrophysics

National Bureau of Standards, Boulder, Colorado

June 10-14, 1968

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Special Publication 307

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Wolf-Rayet Stars

Proceedings of a Symposium held at The Joint Institute for Laboratory Astrophysics University of Colorado Boulder, Colorado, June 10–14, 1968

Edited by

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JILA

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ABSTRACT

A symposium on Wolf-Rayet stars was held at the Joint Institute for Laboratory Astrophysics on the campus of the University of Colorado, Boulder, Colorado, 10-14 June 1968. The Wolf-Rayet stars represent the most extreme example studied of an interaction between aerodynamic motions and a radiation field to produce a high temperature, large-scale plasma in a steady but non-equilibrium state. As such these stars provide a perfect example of the kind of gaseous ensemble that JILA was created to study. In order to understand them, we require a knowledge of gases with temperatures between 10^4 and 10^7 °K and differential velocities between 0 and 10^3 km/sec. In particular we need information on radiative and collisional atomic cross sections for a wide range of ions, on collective interactions of ions and photons, on methods of diagnostic spectroscopy, and on velocity fields generated by convective, gravitational, nuclear, radiative, rotational, thermal, and other instabilities. The material of the symposium was divided into four broad topics: the distribution, physical properties and evolutionary status of Wolf-Rayet stars; the detailed features of their spectra; the interpretations of these features and the models on which they are based; and finally a survey of the material and ideas arising out of the symposium itself. This volume contains the introductory summaries of each of these broad topics, together with an edited version of the discussions which followed.

Key words: atomic cross sections, atmospheric aerodynamics, diagnostic spectroscopy, non-equilibrium gases, stellar instability, Wolf-Rayet stars.

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CONTENTS

The Spectrum of V444 Cygni;
Freddo, F. A.; Ap.J., 83, 515, 1936.

There was a remarkable pygmi,
Who arrived from V444 Cygni.
He said, as he grope,
Through his old spectroscope,
"It's them emission lines that intrygmi!"

from "Aerodynamic Phenomena in Stellar Atmospheres - a Bibliography",
Technical Note No. 30 (1959)
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PREFACE

The symposium on Wolf-Rayet stars, sponsored jointly by the American Astronomical Society, the Harvard College Observatory, the Joint Institute for Laboratory Astrophysics (JILA), and the Smithsonian Astrophysical Observatory was held at JILA, 10 to 14 June 1968. It was one of a series of symposia that JILA has undertaken to sponsor in collaboration with other institutions on topics of current interest in overlapping areas of aerodynamics, astrophysics, atomic physics, chemical physics, and the physics of high temperature gases.

The Organizing Committee consisted of C. Payne-Gaposchkin, K. B. Gebbie, L. Goldberg, and R. N. Thomas. There were thirty-eight invited participants, and a number of graduate students and scientists in related fields attended as auditors. The symposium was modeled after the Cosmical Gas Dynamics Symposia, which are sponsored jointly by the International Astronomical Union and the International Union of Theoretical and Applied Mechanics. Each of the four formal sessions consisted of a survey paper followed by general discussion during which participants were encouraged to present their views and results where relevant. There were no contributed papers.

The National Science Foundation made a generous grant toward the publication of these proceedings and other expenses of the symposium. The balance of the costs were covered by JILA (the University of Colorado and the National Bureau of Standards).

In all the work connected with the symposium, we have been wholly dependent on Robert N. Alvis, Executive Officer of JILA, Mrs. Robert J. Low, whose experience in running symposia is incomparable, and Judy Schlepp, our secretary for astrophysics. Assisting them were Anne Cannon, Cordelle Yoder, and Mary LaCasse. All technical facilities were coordinated by Stuart Jordan, who was on loan to us from the NASA Goddard Space Flight Center. He was assisted by three JILA graduate students, Stephen Hill, David Van Blerkom, and Kenneth Ziebarth. We are also indebted to the JILA administrative and technical services and, in particular, to William Kellet, Victor Holliger, and Floyd Howerton. The cooperation of Mr. J. K. Emery of the University of Colorado Publications Services is gratefully acknowledged.

Alice Levine has overseen from start to finish the editing and preparation of the camera copy for these proceedings. To her professionalism and good sense of fun, we owe not only the appearance and timing of this volume but also our own enjoyment in editing it. Although each participant received and corrected a draft of his remarks, the responsibility for the final editing is ours. For instant responses to demands for additional figures and references, we are grateful to many of the participants and also to a number of their colleagues who did not attend the symposium. We were fortunate in being guided to the quick and inexpensive form of these proceedings by the Publications Office of the National Bureau of Standards and, in particular, by W. R. Tilley, J. E. Carpenter, and Betty L. Oberholtzer. The typescript was prepared by Paulina Thure.

Katharine B. Gebbie
Richard N. Thomas

1 September 1968

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OPENING REMARKS

Richard N. Thomas

On behalf of its joint sponsors, the American Astronomical Society, the Harvard College Observatory, the Joint Institute for Laboratory Astrophysics (JILA), and the Smithsonian Astrophysical Observatory, I welcome you to JILA to participate in this working symposium on Wolf-Rayet stars. We bring together a number of people interested in these stars to discuss what these objects are and the problems we must solve in order to construct satisfactory models of them. The last such gathering to deal extensively with this subject was held in Paris at the Collège de France in 1939. At that meeting Wolf-Rayet stars were considered in the context of the more general discussion of phenomena in novae, P Cygni stars, WR stars, and white dwarfs. The theme there was the possible linkage of these stars through the phenomena attending an ejected envelope. Only superficial attention was paid either to the cause of the ejection or to its aerodynamical implications for the atmospheric structure and spectra. In the present symposium, we shall concentrate on the WR stars themselves, considering related objects only where they contribute to our understanding of WR stars. We shall, however, explore in detail any suggestions for a self-consistent kinematic structure of the WR configuration.

In 1949 I suggested that we were entering a new era of stellar atmospheric models: under the impetus of our then new understanding of the outer solar atmosphere, we were possibly beginning to consider models with a significant supply of mechanical energy, instead of models whose properties were fixed wholly by radiative energy transport, hydrostatic equilibrium, and local thermodynamic equilibrium. The question was whether such a mechanical energy supply would affect all, or only parts, of the atmosphere, and whether it would have a significant effect on the momentum balance. The suggestion was that the Sun represented one extreme where the influence of the mechanical energy was only marginally detectable in the visual disk spectrum, and where it did not significantly perturb the momentum balance. The WR stars possibly represented the opposite extreme where the mechanical energy dominated those factors controlling the formation of the visual disk line spectrum and greatly perturbed the momentum balance. Between these extremes were those phenomena in other stellar atmospheres that are anomalous in terms of the classical atmospheric model. No general suggestions were offered on the source or structure of the mechanical energy supply. It was recognized that our biggest problem lay in the development of diagnostic methods by which we could infer the actual situation in these atmospheres. Now, twenty years later, with the impact of new observations and a considerable increase in the sophistication in our understanding of how to analyze a stellar atmosphere without imposing the classical restrictions, it seems worthwhile to look again at the WR stars.

When we narrow our discussion to WR stars, we must begin by establishing what we mean by the WR class. Spectroscopically, the broad classification rests on 4 features which describe what I shall call hereafter a "pure" WR spectrum.

1. The spectrum consists almost wholly of emission lines. When absorption lines occur, they occur as satellites at the violet edges of the emission lines.

2. The emission lines are very broad. Interpreted as Doppler-broadening, the widths correspond to differential motions of some hundreds to thousands of km/sec and are not necessarily the same for all ions.

3. The lines in any one star represent a wide range of excitation and ionization. The excitation level of the line spectrum is generally much higher than that of the continuum as estimated from its spectral energy distribution.

4. The spectrum falls into one of two groups. Either it shows strong lines from carbon and oxygen, or it shows strong lines from nitrogen. Both groups show strong lines of helium plus other, weaker lines.

I suggest we designate as "classical WR stars" those whose spectra show unambiguously the four features of the "pure" WR spectrum and no other features.

The broad category of "classical WR stars" is thus divided into two groups, each of which is further divided into subclasses on the basis of relative line intensities. We then have the choice between two alternative physical pictures:

a. Is a star that exhibits the "pure" WR spectrum a distinct kind of star that exhibits this spectrum as a consequence of its initial mass, chemical composition, association with other astronomical objects, and subsequent evolution that carries it through the WR stage at some point? If so, is each subclass associated with a distinct kind of star in the same sense, or do the several subclasses represent different stages in the evolution of one kind of star? I suggest the model represented by this first alternative be called a "WR object".

b. Does the "pure" WR spectrum simply imply a distinct kind of atmospheric condition, a distinct state of the stellar atmosphere which can be attained by different objects with different chemical compositions, from different causes, and along different evolutionary paths? In this case, the different subclasses would represent variations on this excitation state. I suggest that this model be called the "WR phenomenon".

There are other objects whose spectra closely resemble the pure WR spectra except that they either lack some of the necessary features or show additional features. Still other objects have at some phase of their observed lifetime shown a spectrum containing WR features. I suggest we call all such objects "quasi-WR objects" and refer to their spectra as "quasi-WR spectra". I use the term "objects" rather than stars because the observations do not always refer unambiguously to a single star. Some quasi-WR stars have been identified as binary, and superposed on a "pure" WR spectrum are features <u>thought</u> to come from the companion. Sometimes a quasi-WR spectrum appears to come only from part of a star, such as from an ejected shell or nebula; it is then difficult to identify particular features with given parts of the object. At this stage in our knowledge, we cannot say that if such-and-such a structural feature of a given object were or were not present, a "pure" Wolf-Rayet spectrum would result.

Because of the variety of quasi-WR objects, I lean toward my second alternative - that the pure WR spectrum represents a phenomenon rather than an object. I would suggest that the spectral features result directly from a supply of mechanical as well as radiative energy to the atmosphere and that the range from pure to quasi-WR spectra corresponds to differences in the quality and quantity of this mechanical energy supply. But at the moment this is sheer conjecture: We are here to examine the actual body of knowledge as a basis on which to test such conjectures.

We need two kinds of investigations. On the one hand, we need a taxonomic study of classical WR stars, covering those properties not included in spectral classification. Do they have other features in common, and is there a relation between the spectral subclasses and any other physical parameter? On the other hand, we require a detailed study of the spectrum: What do we see, and what combination of physical circumstances could produce it?

TAXONOMY

An empirical spectral classification scheme does not fullfil its purpose until the variation in spectral features from one class to another can be correlated with a variation in physical parameters. In the classical LTE interpretation of the Herzprung-Russell diagram, these parameters were temperature, density, and chemical composition, which were supposed to depend, in turn, on initial mass, initial composition, and evolution. In the WR stars and related phenomena, which are obviously non-equilibrium configurations, it is not so easy to identify the physical parameters. The taxonomic investigations must be made without preconception as to which parameters are relevant.

In order to decide whether the WR spectral subclasses represent different stages in the evolution of a single type of object or whether they represent alternative stages in the evolution of distinct types of objects, we must know as much as possible about their differential physical characteristics. The same is true for quasi-WR objects. Indeed, if we could simply establish whether such things as mass, luminosity, and the size of the differential velocity fields are constant across the broad WR category, we would have made a start.

Questions of distribution and association with other phenomena are obviously crucial for obtaining absolute luminosities and masses of these objects and may give some indirect clues on chemical composition and evolution. Of equal importance in settling the physical significance of the WR classification are the statistics on quasi-WR objects. The problem of the possible binary character of the classical WR objects - and of quasi-WR objects such as novae - is also pressing. The search for an association with nebulosity, which may bear both on the problem of mass ejection and on a possible association of spectral characteristics with planetary nebulae, is critical. Finally, any kind of observation or statistic that can give information on velocity fields should be pushed to its limit.

THE SPECTRUM

In some senses it is easier to comment on the interpretation of the spectrum than to speculate on what causes the atmosphere to produce the spectrum. We think we know more about the state of the atmosphere that produces the spectrum than we do about what effects that state. The following are brief comments on the four broad spectral characteristics of the WR class.

1. *Emission Lines*

To produce a spectrum that shows only emission lines - or that shows only emission lines with a few absorption edges - is not trivial. Let me remind you of the alternatives.

a. *Intrinsic Emission Lines*

(i) A Schuster-type mechanism: Recent work by Gebbie and Thomas (1968) casts severe doubts on the utility of this mechanism.

(ii) Fluorescent effects: To produce all the emission lines by such effects is, in practice, impossible, especially in view of the low excitation of the continuum relative to the line spectrum.

(iii) Chromosphere-corona mechanism; mechanical heating: This is a tempting possibility, especially in view of the large line breadths which suggest large differential velocity fields, hence a potential supply of mechanical energy.

b. *Geometrical Effects*

Whether the WR spectrum can be produced wholly by the geometry of an extended, expanding, turbulent atmosphere remains to be shown. We must remember that there is little evidence of extensive dilution effects.

2. *Broad Lines*

The most pressing question is: Can a simple, expanding, optically thin atmosphere give the observed line profiles, or do we require a system of random motions coupled with optical-depth and radiative transfer effects?

3. Range In Excitation Conditions

This point is closely linked to (1), (2), and (4): What are the basic physical conditions that can produce such a range in excitation and ionization? Is stratification necessary, or can uniform but inhomogeneous layers produce the spectrum? Clearly, an empirical model of the distribution of excitation would be most welcome.

4. The Two Spectral Groups

The outstanding question is whether the existence of these two groups requires a difference in chemical composition, or whether differential excitation alone can be the explanation. In order to incite further questions, let me remind you that C. Payne-Gaposchkin classified the first solar rocket spectrum as WC6, and that C. Pecker-Wimel reproduced this spectrum by using standard abundances in a rough model of the solar atmosphere. Let me also remind you of the current efforts by Paczynski and his colleagues to produce chemical differentiation in terms of mass exchange in close binaries. Unfortunately, Paczynski, who will be with us at JILA next year, was unable to come in time for this symposium.

We have organized the symposium in such a way that our information on these points will first be summarized and then systematically discussed. Starting with a taxonomic survey, we will go on to summarize the empirical spectral features, and finally examine those conditions responsible for the production of the spectrum. The important aspect of the symposium will be the discussion not only of the summary papers, but also of the other discussions. There is plenty of time, both in formal sessions and in free periods, for a complete airing of all topics.

PROLOGUE

THE WOLF RAYET STARS: INFORMAL PERSPECTIVE

C. S. Beals

Let me begin my remarks by thanking Dick Thomas for inviting me to take part in this symposium. The invitation is the more appreciated since it was accepted with some trepidation due to the considerable length of time that has elapsed since my active participation in research on Wolf-Rayet Stars. I am naturally sensitive to a comparison with Rip van Winkle who slept for 20 years, waking up to experience difficulties in communicating with his friends and relatives who had outgrown him. Perhaps a kinder assessment of my present relation to the Wolf-Rayet problem would be an analogy with the semi-hibernators or denners of which the black bear is the most notable example. Unlike the true hibernator who falls into a deep sleep in the autumn and spends many months in a state of complete unconsciousness, the bear sleeps lightly during the winter, is easily disturbed and on occasion wakes up, emerges from his den and investigates the state of the outside world.

In a similar way, due to the force of circumstances I have spent considerable periods of time in a state of mental somnolence induced by the performance of non-scientific tasks, but have wakened up from time to time and used the nearest library to see what was going on in the scientific world. During these excursions I have become aware that work on the WR stars has been going forward and that the progress has been such that keeping in touch and catching up is a major problem for anyone with limited time on his hands. I have therefore welcomed this symposium as an aid in the catching-up process and can only hope that my talk, necessarily concerned mainly with the past, will not only have some value as history but may also have some relevance to the problems of the present day.

In talking about Wolf-Rayet stars I shall refer to the work prior to 1945 as Phase I and the subsequent observational work including that now going on as Phase II. My talk will be mainly about Phase I with a few tentative excursions into Phase II. I will also make a few references to a Phase III which is looming on the horizon and about which I have great curiosity but little knowledge.

Phase I. General Discussion of Wolf-Rayet Spectra

The first of these stars were discovered just over 100 years ago on objective prism plates. On such plates the emission-line WR stars stand out as conspicuously as a Marxist at a Republican convention or a giraffe in a herd of shorthorn cattle. While the discoverers were not unaware of the scientific promise of these bizarre looking objects, they were completely outside the mainstream of astronomical research of that day, which was concerned with the absorption-line stars. In addition to these considerations, the statistically insignificant numbers of Wolf-Rayet stars resulted in a long period of neglect during which these stars received the treatment normally accorded to a group of non-voters in an election year.

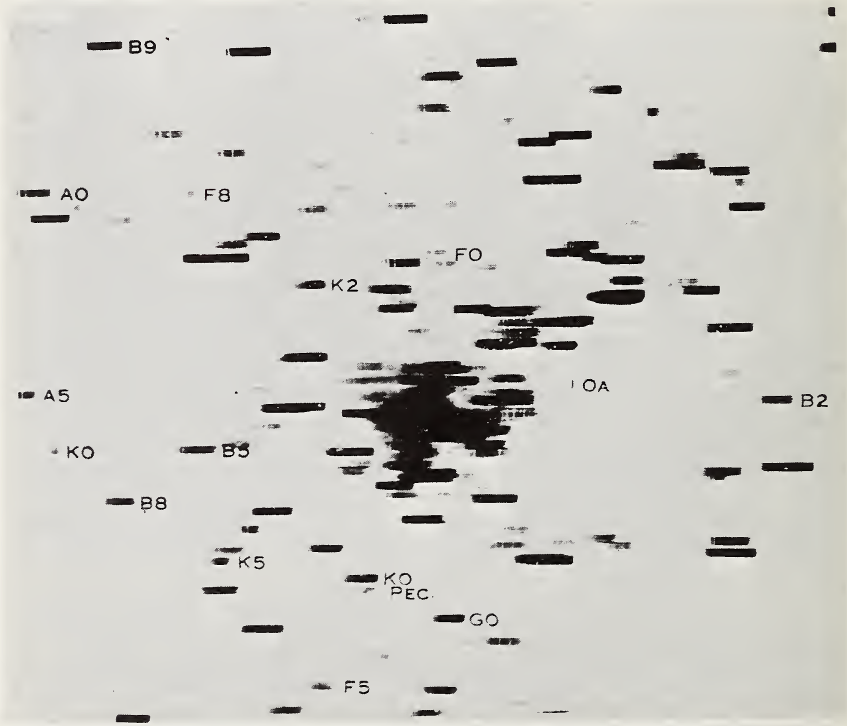

Figure 1. Objective prism spectrum of WC star in η Carinae region.

Thirty years after discovery this period of neglect was broken by a brilliant series of visual observations of Wolf-Rayet bands by Campbell using a slit spectroscope at the Lick Observatory in 1894. The next major break came in the early 1920's when J. S. Plaskett at Victoria brought the spectra of Wolf-Rayet stars into the open, and it is fair to say that ever since they have been objects of very great interest to astronomical science.

Subsequent to and including the work of Plaskett, slit spectra have demonstrated the following charac-

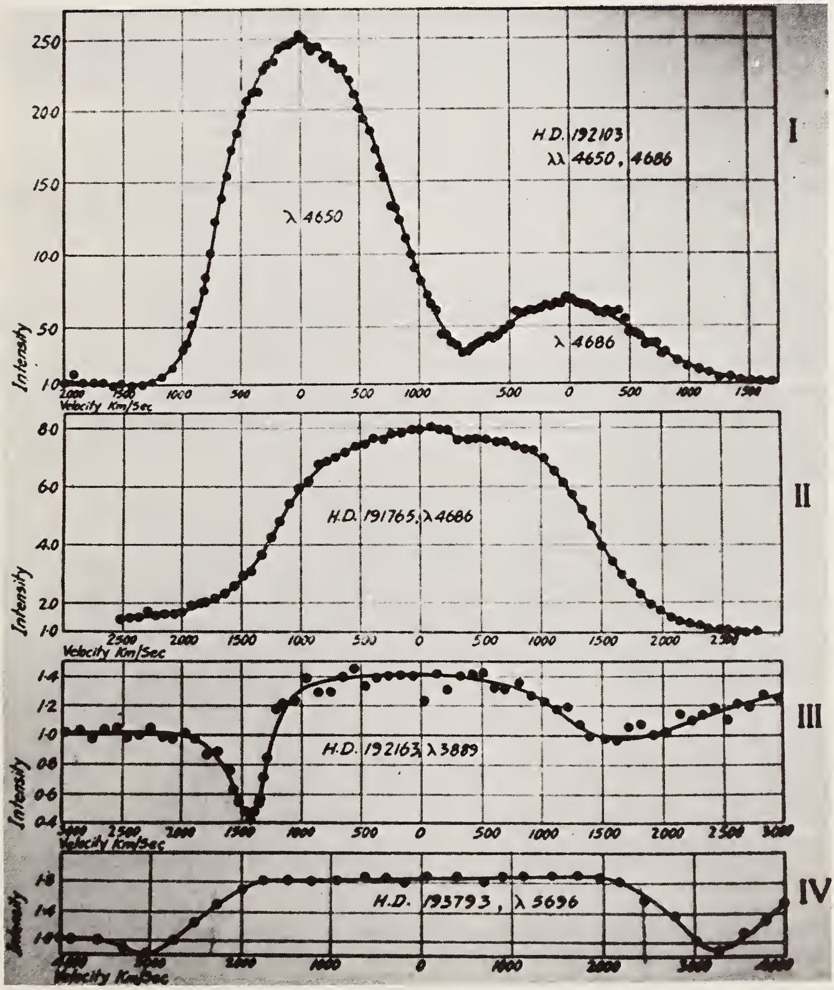

Figure 2. Spectrum line profiles from several WR stars.

teristics of Wolf-Rayet spectra.

(a) The emission lines are very wide, of the order of 50 Å. Both high and low pressure explanations have been offered for the width, but it now appears that low pressure explanations based on ejection velocities hold the field.

(b) The range of central intensities is very great (from 25 × the continuum to close to zero).

(c) The profiles of lines are usually rounded but are sometimes flat-topped, and the flat-topped lines offer interesting possibilities for interpretation of stratification, the scientific possibilities of which have not yet been fully exploited. In this connection it should be noted that a flat-topped line finds its normal interpretation in a restricted distribution of ejection velocities, none of which approaches zero. It seems likely that any given ion would exist only in one particular region or stratum of the envelope, where the physical conditions for its production are favorable. Thus given sufficient resolution in the observations and interpretation, we should be able to relate the distribution of expansion velocities to the distribution of ionization in the envelope. By contrast, the explanation of a rounded profile appears to require a velocity distribution that ranges from zero to the maximum velocity displacement from the line center. So here we do not have such a clearcut method for studying the relation between velocity and excitation.

(d) Displaced violet absorption lines have been observed on some but not all or even most lines. Strong displaced lines are observed for He II and C III lines, and some of the associated emission lines have flat tops. No alternative to the ejection interpretation of violet absorption lines has yet been found.

(e) The level of ionization indicated by emission lines is high including He II, C III, C IV, N III, N IV, N V, O III, O IV, O V and O VI leading to the conclusion that the stars are of high temperature. No discussion of quantitative temperatures is given here since it will no doubt be dealt with by later speakers.

(f) The existence of parallel sequences became obvious early in the game and its cause is still under discussion. In my own observations a carbon-oxygen sequence appeared pretty well exclusive of nitrogen and vice versa. Now this is not so certain, but the division is still very well marked. Suggestions to explain it include abundance

Figure 3. Two WR spectra of comparable excitation but of parallel sequences: HD192163 - nitrogen sequence (above); HD192103 - carbon-oxygen sequence (below).

and excitation. I have an intuitive preference for the former but am sure that these points will be discussed in more detail later in the symposium.

(g) Although the continuous spectra of Wolf-Rayet stars sometimes appear weak or absent in underexposed spectra, they are in fact always present and of course very important. It appears that even yet our knowledge of the distribution of energy in Wolf-Rayet continuous spectra is insufficiently known. This is a good open field for young astronomers, and it should be mentioned that observations of the far-ultraviolet taken outside the Earth's atmosphere are especially important. It is essential to have observations over the whole field from the far-UV to the far-infrared in order to make suitable correlations between the character of the continuous spectra and the observed level of excitation in the Wolf-Rayet envelope.

Phase II. Relation of Wolf-Rayet Stars to Other Objects.

(a) Absorption O's. These are very dissimilar in appearance. The O's show incipient emission but of much lesser intensity and smaller width. The absolute magnitude of absorption O's appears to be somewhat greater. The big question is: Can we regard temperature as the most important distinction between Wolf-Rayet and O-type stars or are there other more fundamental differences? Certainly later speakers will be discussing this point. I believe

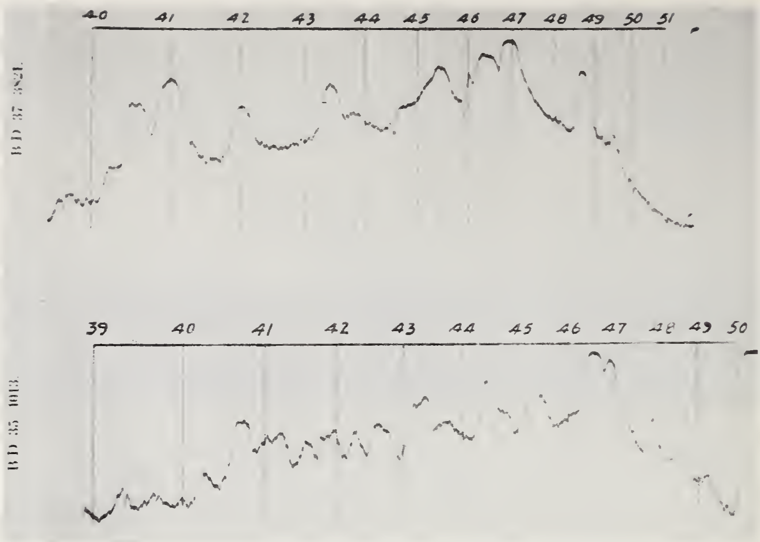

Figure 4. Tracings of continuous spectra of WR stars: BD 37°3821 (above) and BD 35°4013 (below).

that a careful comparison of the distribution of energy in the continuous spectrum for the two types of objects over the entire observable region is of prime importance in solving these problems. Those involved in space science who have the facilities to observe outside the Earth's atmosphere can make a decisive contribution here.

(b) Novae. Some photographs of the spectra of novae are very similar to those of Wolf-Rayet stars, so much so that a second look is required to say which is which. Actually there are easily observed differences. The excitation of Wolf-Rayet stars is normally greater, the strong nova lines being mostly hydrogen. The displaced absorption borders of novae lines are usually more clearcut and are often complex, suggestive of a series of absorbing shells. Also the nova spectrum is normally variable whereas the WR spectrum is normally not. Each spectrum in general seems explicable only in terms of ejection either of successive shells as in novae or of more or less steady ejection as for the Wolf-Rayet stars.

Some old novae show Wolf-Rayet characteristics, and it would be in order to study them in more detail, particularly the distribution of energy in the continuous spectrum.

(c) The P Cygni Stars. These stars show emission lines with absorption on their violet edges.

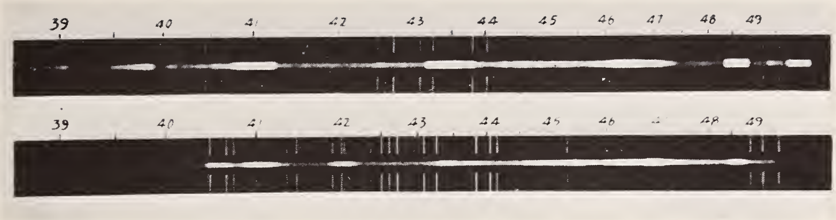

Figure 5. Comparison of WR and Nova spectra: Nova Aquilae (above) and HD192163 (below).

In general the lines are narrower and the excitation lower than in Wolf-Rayet spectra. There exist some Wolf-Rayet stars, e.g., HD151932, which would certainly be classified as P Cygni objects if there were no other class to put them in. In general (not always), the P Cygni stars are of rather high absolute magnitude. The α Cygni stars often show Hα in emission with a violet absorption border, and when this occurs we include them with the P Cygni stars. I believe that it is important to consider the WR and the P Cygni stars together insofar as their ejection characteristics are concerned.

The P Cygni star HD190073 has a very interesting calcium line which appears in emission with a double absorption border on the violet edge. In a nova this line would be considered as an indication of successive shells, but such an explanation is difficult for this star since this peculiar line has remained relatively unaltered for a period of many years. I regard it vital to solve the problem of this line if we are to understand clearly the process of the ejection of atoms from the surface of any star.

(d) Nuclei of Planetary Nebulae. The nuclei of planetary nebulae are sometimes Wolf-Rayet stars and, at least at first glance, it would appear normal for a star ejecting atoms to give rise to a spherical nebula. This is especially true if, as now seems probable, not all atoms are ejected with very high velocities. It would appear legitimate to speculate whether all planetary nuclei were once Wolf-Rayet stars.

Planetary nebulae with O-type nuclei are in general of higher excitation than those with central Wolf-Rayet stars. It is somewhat difficult to reconcile this with the generally accepted idea that the Wolf-Rayet stars are of higher temperature. This difficulty seems to remain even if we add the Wolf-Rayet bands to the total nebular emission.

The spectra of Wolf-Rayet planetary nuclei are

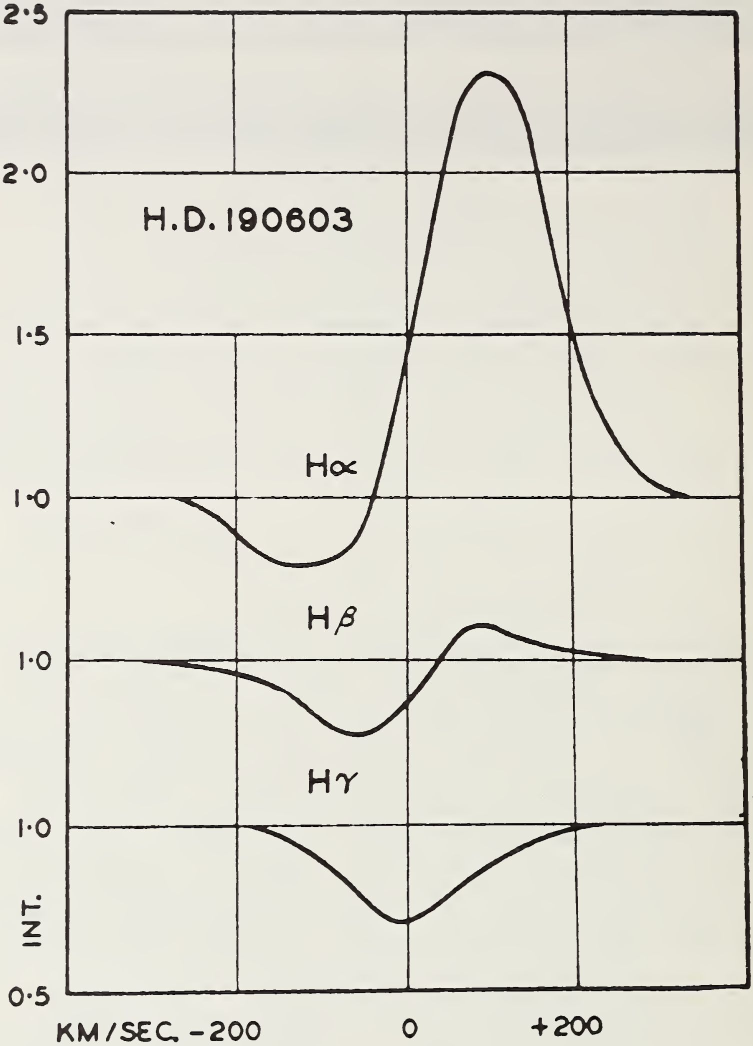

Figure 6. P Cygni star HD190603.

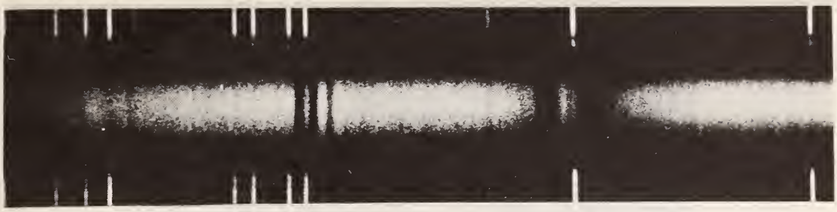

Figure 7. Ca II H and K lines, HD190073.

not always well matched with those of their surrounding nebulae, as strong nitrogen lines are observed in nebulae with carbon-oxygen nuclei.

It seems reasonable to regard the relation of Wolf-Rayet stars to planetary nebulae as part of the general Wolf-Rayet problem, each part of which requires a solution before we can feel confident of our knowledge of this class of stars as a whole. Observations of the continuous spectra of planetary nuclei are of obvious importance in this connection.

(e) The Sun. Recent scientific papers dealing with the Earth's outer atmosphere treat the phenomenon of the solar wind. Certainly its existence is well established, and its influence on the Sun's environment is important. Recently there have been references to what is called the stellar wind. These indications suggest that the Sun and most if not all stars eject atoms from their surfaces to a greater or less degree. If we refer to the Sun's ejection of atoms as a wind or perhaps a light breeze, then the corresponding phenomenon in the vicinity of

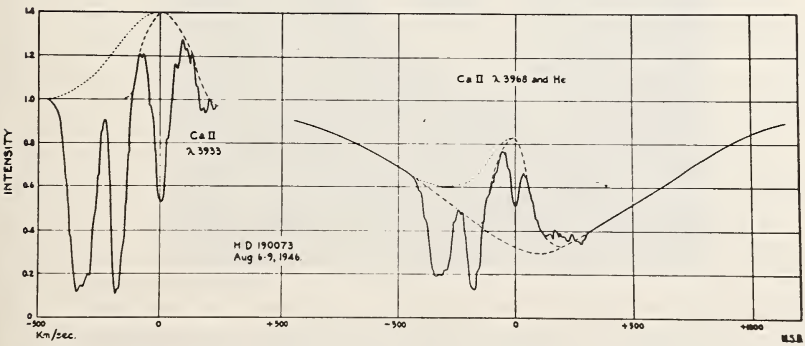

Figure 8. Profiles of Ca II H and K lines of HD190073.

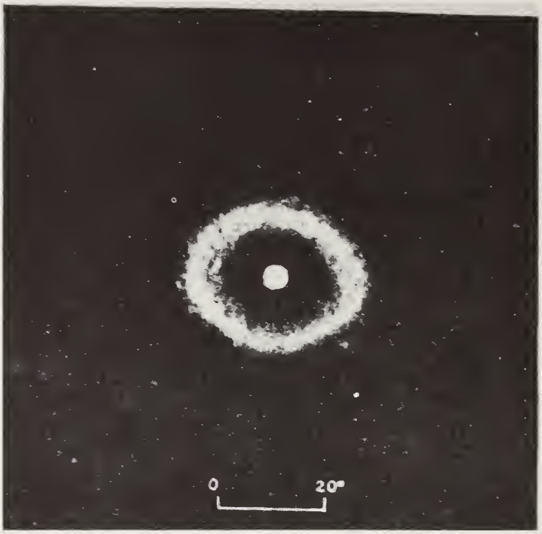

Figure 9. Specimens of planetary nebulae. NGC 2610 and NGC 3242. H. D. Curtis.

a Wolf-Rayet star would be a violent hurricane.

It appears that under certain circumstances the ejection from limited areas of the Sun's surface is greatly increased in volume and velocity. Such temporary bursts of activity are associated with solar flares (hot spots?) and the effects on the terrestrial atmosphere consist of violent magnetic storms and the appearance of hydrogen lines in the spectrum of the aurora. It seems quite possible that phenomena of the general nature of solar flares multiplied many times in intensity and occurring on the surface of a star of high temperature could well give rise to phenomena such as are observed in connection with Wolf-Rayet stars. In this connection it may be pointed out that spectroheliograph observations of the Sun reveal the presence of exceedingly complicated velocity effects in which the motions are not always in the same direction although at times very large volumes of material appear to be leaving the Sun's surface for outer space. Insofar as I am aware, the origin of these sometimes very violent motions is by no means fully understood. If this is the case for a star observed under as favorable conditions as the Sun, it is perhaps not surprising that the causes of the ejection of atoms from Wolf-Rayet and P Cygni stars are still obscure.

(f) Binary Stars. A high proportion of Wolf-Rayet stars are binaries, and it has even been suggested that they are all binaries though this is open to question. The most striking of these binaries studied during the period designated as Phase I is the star HD193576, and it will probably be a long time before its possibilities as a source of scientific information will be exhausted. Previous to 1945, observations of HD193576 had suggested (1) evidence of asymmetrical bands due to tidal action or other causes; (2) evidence that the radius of the Wolf-Rayet envelope is greater than the distance between the two stars; and (3) since primary eclipse occurs when the Wolf-Rayet component is between the O-type component and the observer, it appears that the average surface brightness of the Wolf-Rayet object is lower than that of the O-type star. This again raises the question of the relative temperatures of the two objects and the distribution of energy over the whole observable range of the continuous spectrum.

Figure 10 shows a profile of Hα at a time close to secondary minimum when the O star is between the Wolf-Rayet star and the observer. It is clearly a case of asymmetrical emission with a peculiar central

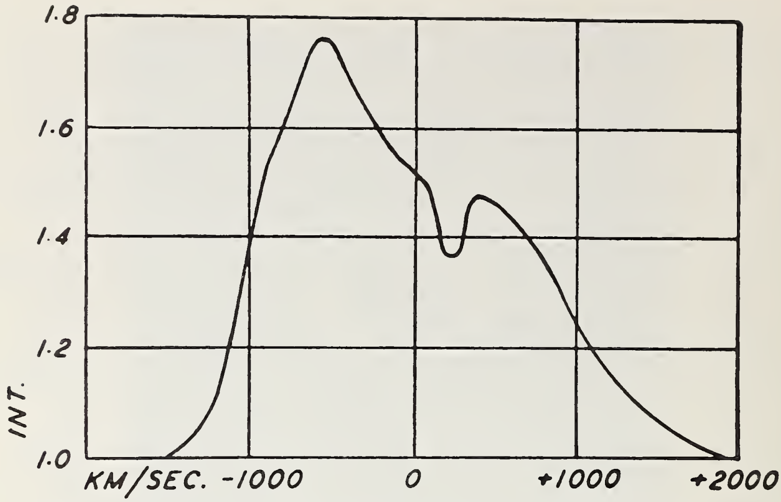

Figure 10. Asymmetric profile of Hα, HD193576.

absorption line. It is important to ascertain whether the absorption line is a self-reversal of the Wolf-Rayet envelope or an aspect of the absorption spectrum of the O-type star. It is also possible that hydrogen is an important contributor to the intensity of this line in addition to He II. It would appear that a set of high dispersion observations of this line covering the entire period of the binary orbit could lead to very valuable information concerning this interesting binary star.

There are numerous other Wolf-Rayet binaries which are now being actively investigated and some of these will undoubtedly be dealt with by speakers at this conference.

(g) Quasi-Stellar Objects. These objects have been regarded by many as the most important astronomical discovery of the twentieth century. All kinds of interesting observations have been made of them; everybody is interested in them and, whenever possible, everybody wants to get in on the act. Certainly I cannot claim any special knowledge of them and I have no desire to rush in where angels such as William Fowler, Thomas Gold, Fred Hoyle and Martin Schmidt fear to tread. Nevertheless, I feel bound to mention the fact that C. R. Lynds has published a spectrum of PHL 5200 which shows the presence of emission bands wider than those of most Wolf-Rayet stars. These bands have strong absorption borders on their violet edges which are suggestive of P Cygni

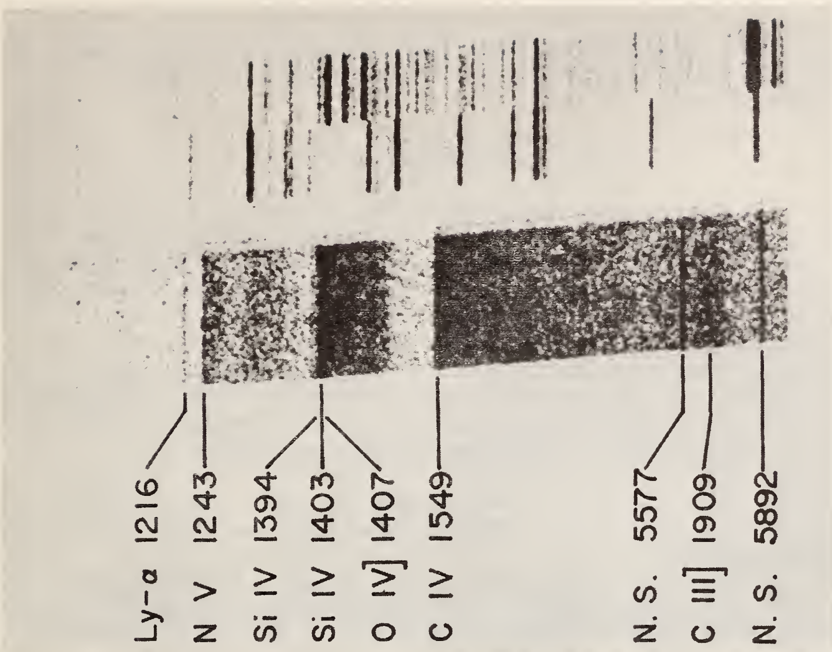

Figure 11. P Cygni spectrum of PHL 5200. C. R. Lynds.

stars. Nearly twenty years ago I was engaged in a study of P Cygni stars which was partly a compilation and partly a record of personal research. If at that time the spectrum published by Lynds had been available, the object PHL 5200 would certainly have been listed among the P Cygni stars. While it would be going a little far to list it among the Wolf-Rayet objects, the great width of the emission bands (of the order of 7800 km/sec) would certainly make it of interest in any discussion of the origin of ejection velocities.

No doubt Lynds and his associates will themselves be developing the consequences of this remarkable discovery. I only wish to suggest that the presence in this object of Wolf-Rayet and P Cygni characteristics may well influence the conclusions on the nature of quasars, if they are indeed a homogeneous group of light sources. It might, for example, give some inkling whether these objects are small, bright galaxies or some sort of gigantic star-like objects. Also when the nature of quasars becomes better known, it might be that in the long run they could give some help in elucidating some of the characteristics of the P Cygni and Wolf-Rayet stars.

Notes on Phase III

Phase I above, to which my discussion has mainly referred, and Phase II, which will be discussed by later speakers, have been concerned with observable phenomena in the atmospheres or envelopes or outer parts of Wolf-Rayet stars. Phase III, mentioned earlier in this discussion, is here arbitrarily defined as referring to the fundamental physical or structural characteristics which cause a star to produce emission lines of the Wolf-Rayet or P Cygni type. This is a problem which, it would appear, can only be tackled from the theoretical angle aided by the reservoir of observational data that has been accumulating for the past hundred years.

It is my impression that liaison between theorists and observational astronomers has improved a good deal since my early years. I seem to remember quite a number of cases where reputably good theory was combined with doubtful observations and vice versa. In the first two or three decades of this century the lack of modern computers often made it difficult for the differences between eminent theoretical people to be resolved leading to impasse and a waste of energy in controversy. We surely have reason to hope that in the future great advances in theoretical studies, aided by the remarkable new computing devices, will make possible (to use a convenient bit of political jargon) a more effective consensus of theory than has ever been possible in the past. In addition, the great increase in the number and quality of observations with giant new telescopes will result in a combination that should offer great possibilities for scientific advancement in this field. I would like to close by expressing the hope that this symposium will give a needed and substantial impetus toward that end.

PART A

THE FEATURES OF THE SYSTEM OF WOLF-RAYET STARS

CHAIRMAN: *Cecilia Payne-Gaposchkin*

Smithsonian Astrophysical Observatory and Harvard College Observatory

INTRODUCTORY SPEAKER: *Lindsey F. Smith*

Department of Astronomy, University of California at Los Angeles

I. INTRODUCTION

My purposes in this summary are: (1) to draw together the available information regarding the luminosities, masses, ages and distributions of the Wolf-Rayet (WR) stars, (2) to determine what correlations exist between these properties, and (3) to see what deductions may be made regarding the origin and evolution of these stars.

When we talk about a "class" of stars, we are implicitly assuming that all stars in that class share certain common characteristics. The stars need not be identical in all their properties, only in those that are chosen to define the class. Observationally the characteristics most commonly used to define a class are the spectral features. We assume that spectral similarity results from similarity in more basic properties of the stars, and we attempt to define the spectral criteria such that this is so. A further division of the stars into subclasses represents an attempt to delineate groups of stars <u>all</u> of whose properties are nearly the same.

I submit that the necessary intrinsic characteristics that we require for a meaningful class are twofold: (A) the stars are at closely related stages of evolution, and (B) the principal mechanisms responsible for the defining spectral characteristics are the same for all stars in the class. By "closely related stages of evolution" I mean either (1) that the stars represent a characteristic phase, e.g., immediately post-main sequence, in the evolution of a group of stars whose other properties (mass, chemical composition) may cover a wide range, or (2) that the different subclasses of the WR class may be arranged in an evolutionary sequence such that each star passes through a series of successive stages in each of which it shows the characteristics of one of the subclasses.

As a working hypothesis, I define the WR class as those stars whose spectra show broad (greater than 4 Å) emission lines of highly ionized helium and either nitrogen or carbon and oxygen; in particular they must have the He II λ4686 line and a broad band between λ4600 and λ4670 made up of either N III and N V or C III and/or C IV lines. I exclude novae, supernovae and the nuclei of planetary nebulae. The subclasses are differentiated according to the dominant ions and the degree of ionization evidenced by the spectra. (This choice is made because we know that, for main-sequence stars, a similar criterion gives us groups of stars with closely similar temperatures.)

I shall now proceed on the assumption that I have a class and subclasses in the sense defined above. But I will pause at intervals to see if there are any data which contradict this hypothesis. Let me say right away that I strongly suspect that the WR stars, as defined above, may not be such a class. In particular I suspect that single WR stars, if they exist, may have different evolutionary histories than do WR stars in binaries and that we may have to define the class more carefully if it is to fulfill criteria (A) and (B) above.

II. CLASSIFICATION

The mode of classification currently applied to WR spectra was adopted by the IAU Commission 29 in 1938 (Beals 1938). The spectra are divided into two sequences, WN and WC, according to the dominance of lines of helium and nitrogen or lines of helium, carbon and oxygen. The separation of the two sequences is fairly complete, but there are a few stars that display intermediate type spectra. The spectral sequences are subdivided by Arabic numerals denoting the relative degree of excitation evidenced by the spectra. The definition of the subdivisions has been the subject of some disagreement over the past few years. At the moment two sets of modifications of the original criteria have been suggested, one by Hiltner and Schild (1966) and one by the author (Smith 1968a). Fortunately there is a fairly clearcut relationship between the three systems; this is given in Table 1. Which system is more closely related to the parameters controlling the spectral differences and is, therefore, the most useful, is not clear at the present time.

In this paper I shall use the revised system defined by Smith (1968a). This system is effectively an extension of the original IAU system; it gives a finer subdivision of the classes (e.g., spectra in the IAU class WC8 are now called WC9, allowing spectra between WC7 and WC8 to be called WC8) and includes objects of higher excitation than were known in 1938. The sequences run from WN3 to WN8 and from WC5 to WC9. Spectra denoted by the lowest Arabic numerals have the highest excitation.

TABLE 1

TRANSFORMATIONS BETWEEN VARIOUS CLASSIFICATION SYSTEMS

Smith	Hiltner-Schild	Beals
WN3	WN4-A	
WN4 (+OB)	WN5-A	WN5
WN4.5 (+OB)	WN5.5-A	
WN5	WN5-B	
WN5 + OB	WN6-A	
WN6	WN6-B	WN6
WN6 + OB	WN6.5-A	
WN7 (+OB)	WN7-A	WN7
WN8 (+OB)	WN8-A	WN8
WC5	WC5	WC5
WC6	WC6	
WC7	WC7	WC7
WC8	WC8	
WC9	WC9	WC8

The presence of a binary may be detected in several ways: (1) We may see a spectrum which is most easily interpreted as a superposition of the spectra of two stars. (2) We may observe periodic variations of the radial velocity. (3) We may observe light variations due to an eclipse. The presence of any of these characteristics does not guarantee that the object is a binary since cases are known in which these phenomena can occur without the presence of a binary (e.g., pulsating variables show variations of light and of radial velocity). A careful evaluation can usually distinguish between the possible interpretations; however, it is traditional that the title "binary" be qualified by an adjective indicating the variety of the evidence. Binaries identified by observations (1) to (3) above are called respectively, "spectrum binaries", "spectroscopic binaries", and "eclipsing binaries". Clearly for any given system we may observe more than one of these phenomena, and accordingly the system may be called a "spectrum and spectroscopic binary" or a "spectroscopic and eclipsing binary".

Among the WR stars we may distinguish two varieties of spectrum binaries. The first includes those whose spectra contain absorption lines (other than violet absorption edges to the emission lines) as well as the characteristic WR emission spectrum. Such objects are consistently found to be spectroscopic binaries as well, and hence we are sure of the correctness of the interpretation of the spectrum in terms of superposition of spectra from two stars. The second variety comprises stars whose spectra show a much stronger continuum with respect to the emission lines (or equivalently, weaker emission lines with respect to the continuum) than do other stars with qualitatively similar emission spectra. We suppose that in these cases the absorption lines in the spectrum of the companion are masked by the emission spectrum of the WR star. Confirmation in the form of observed radial velocity variations is available for a few of the stars of this variety; however in any particular case, certainty of the correctness of the interpretation is lower for these stars than for those in which a definite absorption spectrum is observed. Spectrum binaries are denoted WR + OB or, e.g., WN5 + O7 if the subclasses of the components are known.

A spectroscopic binary may or may not be a spectrum binary, i.e., one star may be so faint that it does not contribute significantly to the spectrum. In the case of a WR star, this means we may find that the velocity derived from an emission line varies, but that there is no evidence from the spectral appearance for the presence of a companion. This clearly opens the possibility that all WR stars may be binaries, but that in many cases the companion has much lower luminosity and mass than the WR star, so that we have noticed neither its contribution to the spectrum nor the change in the radial velocity of the WR star.

Similar considerations apply to eclipsing binaries. However in this case there is little confusion and I need not discuss it further.

I have taken some care to describe the nature of our knowledge about the presence of binary stars, because it seems that one of the most important questions facing us is whether all WR stars are binaries.

The classification system proposed by Hiltner and Schild (1966) divides the WN spectra into two subsequences, denoted A and B according to the strength and width of the emission lines, and assigns Arabic numerals to denote relative degree of excitation within each subsequence. Membership in subsequence B requires broad lines. The subsequence contains only WN5 and WN6 stars. [Although Hiltner and Schild include one spectrum that they classify WN7-B (HD62910), it contains strong lines of both carbon and nitrogen (H. J. Smith 1955, L. F. Smith 1968a) and does not, therefore, truly belong.] Only one star in subsequence B shows any indication of binary nature. Subsequence A contains spectra with narrow emission lines. Many of these stars are obvious spectrum binaries or known spectroscopic binaries. The implication is that all stars in subsequence A are binaries. This implies that the stars in subclasses WN5-B and WN6-B are intrinsically different from all other subclasses, presumably in that they are single. It implies further that all the single WN stars are in subclasses WN5 and WN6. Personally I find these conclusions hard to believe for two reasons: (1) there is one known spectroscopic binary in the B sequence, and (2) there is a very smooth transition from WN5-B to WN6-A and from WN6-B to WN6.5-A. From Table 1 you will see that I have classified these as WN5, WN5 + OB, and WN6, WN6 + OB, respectively. It is, however, a very remarkable fact that in the WN sequence, broad lines are found almost exclusively among the apparently single stars in subclasses WN5 and WN6.

III. LUMINOSITIES

WR stars are very rare objects. In the Galaxy very few are near the Sun. Thus determinations of their distances and the amount of interstellar reddening are extremely difficult. For stars in the Large Magellanic Cloud (LMC), these problems are minimal; the distance modulus of the Cloud is known to within ± 0.2 mag, and interstellar absorption is small. Observations of apparent magnitudes of WR stars in the LMC were first made by Cannon (1924); more complete photographic photometry was carried out by Westerlund and Smith (1964), and narrow-band photoelectric photometry was done by Smith (1968). With one exception, which I will discuss below, no significant spectroscopic differences have been detected between WR stars in the LMC and in the Galaxy (Smith 1968b). Thus we may expect that the absolute magnitudes that we derive for WR stars in the LMC also apply to the WR stars in the Galaxy. The exception referred to is the remarkable fact that stars in subclasses WC6, 7, 8 and 9 are entirely missing from the Magellanic Clouds, and stars in subclass WN6 are rare or absent. I do not believe that this fact invalidates the assumption that the LMC stars are similar to stars in corresponding subclasses in the Galaxy. However it does mean that absolute magnitudes of stars in the "absent" classes must be derived from observations of stars in the Galaxy, and the values are thereby much less certain. Results are given in Table 2.

When determining the magnitudes of WR stars, we need to give some consideration to the wavelength range measured. The emission lines contribute significantly to the total energy output in the visible wavelengths; thus we need to specify which emission lines have been included in any given measurement. The values in Table 2 are derived from narrow-band photometry (Smith 1968b). For WN stars, emission lines are effectively avoided; the absolute magnitudes so derived are close to the absolute visual magnitudes that the stars would have in the broad-band Johnson-Morgan system if the emission lines were absent. For WC stars the v-filter includes a medium strong emission band at about λ5140 due to C III, C II and O V. The colors of stars in the various subclasses of the WN sequence do not differ significantly, and a mean for all subclasses is given; this has been converted from the narrow-band system into the UBV system; again it refers to the continuum without emission lines. Individual emission lines have a considerable effect upon the narrow-band b-v measures for the WC stars; hence

TABLE 2

ABSOLUTE MAGNITUDES AND INTRINSIC COLORS OF WR STARS

Class	$\overline{M_v}$	S.D.	Class	$\overline{M_v}$	S.D.
WN3	$-4\overset{m}{.}5$	$0\overset{m}{.}1$	WC5	$-4\overset{m}{.}4$	$0\overset{m}{.}6$
WN4	-3.9	0.3	WC6	-4.4	
WN5	-4.3	0.1	WC7	-4.4	
WN6	-5.8		WC8	-6.2	
WN7	-6.8	1.0	WC9	-6.2	
WN8	-6.2	0.4			
$\overline{WN}$	$\overline{B-V} = -0\overset{m}{.}08 \pm 0\overset{m}{.}06$				

such b-v measures have no well defined physical meaning and are not reproduced here.

When the values in Table 2 have been determined from observations of stars in the LMC, a standard deviation can be derived; these numbers represent the intrinsic standard deviations of the magnitudes and colors within the subclasses.

For the subclasses that are not represented in the LMC, we can derive the absolute magnitudes for the WN6 and WC7 stars with reasonable confidence from observations of galactic stars (see Smith 1968b). The mean absolute magnitude of WC6 stars has been assumed equal to that of WC5 and WC7 stars, since the latter are equal. The only subclasses for which absolute magnitudes are really uncertain are WC8 and WC9. These values depend only upon observations of γ_1 and γ_2 Velorum. Graham (1965) has determined the distance modulus of γ_1 Velorum by Hβ and UBV photometry. If we assume that γ_2 Velorum is at the same distance as γ_1 Velorum, 460 pc, and that it is unreddened, we obtain an absolute magnitude of -6.6 for γ_2 Velorum. If we assume further that γ_2 Velorum is a binary star containing a WC8 star and a normal main-sequence O7 star ($M_V = -5.2$), we obtain an absolute magnitude of -6.2 for the WC8 star. We have no information regarding the absolute magnitudes of WC9 stars and have simply assumed that they are equal to that of the WC8 star in γ_2 Velorum. $M_V = -6.2$ is surprisingly bright compared to $M_V = -4.4$ for the WC5, 6 and 7 stars, although some of the luminosity may be due to the contribution of emission lines. However, adoption of this luminosity does generate distances for the WC9 stars that are consistent with the simplest interpretation of their asymmetric angular distribution (see Section IV). One further caution regarding the values in Table 2: the absolute magnitude for WN5 stars rests upon two LMC stars which have been classified by photometric criteria. The classification of at least one of these stars should be checked by spectroscopic observations.

Using the values given in Table 2 (Smith 1968c), Wallerstein (1968) has plotted distances versus reddening as derived for galactic WR stars. Compared to similar plots for stars in Hiltner's (1956) catalogue, the values in Table 2 give many more WR stars with large distances and low reddening. Wallerstein points out that this may indicate that some of the stars have lower luminosities than those given in Table 2 and suggests that such stars may be progenitors of planetary nebulae. Alternatively, all of the scatter may be due to the fact that stars at large distances can only be observed if the reddening is small. I would add to this the fact that, plotted on a distance scale, scatter at large distances becomes very large because errors in the distance due to the natural range of luminosity within a subclass are constant in log D not in D.

Earlier attempts to determine the absolute magnitudes of galactic WR stars (Roman 1951, Andrillat 1955, Onderlicka 1958) were handicapped by uncertainties in the distances and reddenings of the stars; they gave rather fainter values than those in Table 2. A more recent attempt by Rublev (1963) to determine the mean absolute magnitudes of galactic WR stars gives values consistent with those in Table 2. Graham (1965) determined distance modulae for young clusters and associations containing WR stars. Resulting absolute magnitudes for WN7 stars are within the range observed in the LMC, but the mean value is fainter. Other WR stars in clusters observed by Graham were used to determine the absolute magnitudes given in Table 2 for WR subclasses that are not represented in the LMC.

Given the absolute magnitudes in Table 2, we notice a surprising fact; the brightest absolute magnitudes are associated with those subclasses displaying the lowest level of excitation in their spectra. This is proven among the WN stars and may also be true among the WC stars. If there is any correlation between the excitation temperature responsible for the emission spectrum and the effective temperature of the star, then the above observation implies that the WN7 and WN8 stars are considerably larger in size than the stars in the higher excitation classes. It is also true that if there is a correlation between excitation temperature and effective temperature, the fainter stars will have larger bolometric corrections, and the bolometric magnitudes of the various subclasses could approach the same value. Thus one of the questions I think we should ask is: What relationship, if any, do we <u>expect</u> between the effective temperature and the excitation temperature?

IV. DISTRIBUTION

In the Magellanic Clouds, we believe (Westerlund and Smith 1964) that we have found all of the WR stars. There are 58 in the LMC and only 2 in the SMC. As has been mentioned in Section III, subclasses WC6, 7, 8 and 9 are missing from the LMC, and WN6 stars are rare or absent. The WR stars in the SMC are both spectrum binaries, WN + OB and WC5 + OB, and both have somewhat peculiar spectra.

In the Galaxy there are 124 known WR stars (Smith 1968a). They are strongly concentrated to the galactic plane (Roberts 1962) and are frequently found in young clusters and associations (Roberts 1958) and in binary systems in which the other component is a young O- or B-star. These facts are confirmed by observations of the WR stars in the Magellanic Clouds (Westerlund and Smith 1964) and imply that the WR stars belong to the extreme Population I. Roberts (1962) also demonstrated that galactic WR stars are strongly concentrated in directions along which we observe spiral arms (which is consistent with their population assignment) and that they are not found in the quadrant centered on the anticenter. Reddish (1967a, 1967b) has noted that they share the latter property with young clusters and associations containing dust-imbedded stars and with Bok globules.

Narrow-band photometry is available for most galactic WR stars (Westerlund 1966, Smith 1968b) in the photometric system in which the absolute magnitudes and intrinsic colors were determined (Table 2). We can, therefore, determine their distances and plot their distribution on the galactic plane (Smith 1968c). The result is shown in Figure 1. The distance of the Sun from the galactic center is taken to be 10 kpc (Arp 1965, Schmidt 1965). New galactic coordinates are marked at the periphery, and the different subclasses of WR stars are represented by different symbols as indicated. We see the WR stars to very great distances. This results from their high luminosity and from their ease of detection on objective prism photographs. Recall, however, that Wallerstein (Section III) suggests that some of the large distances may not be correct.

The distribution of WR stars shown in Figure 1 shows elements of spiral structure as anticipated from the work of Roberts. It also shows that the various subclasses are not distributed in the same way. In particular the WC9 stars show strong concentration towards the galactic center, and the WN6 and WC7 stars also show a tendency to concentrate to the inner regions of the Galaxy.

The presence of distribution differences is most easily verified in terms of the angular distribution. In this way we are free from effects of possible errors in the distance scale. I have counted the numbers of WR stars in each subclass

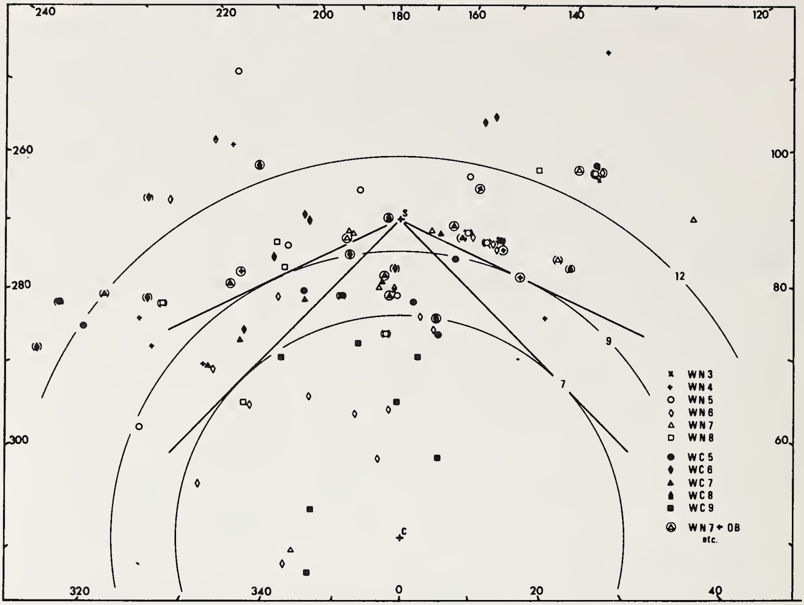

Figure 1: The galactic distribution of Wolf-Rayet stars. New galactic coordinates are given at the periphery. The sun is marked +S; the galactic center is marked +C. The symbols used for the various subclasses are shown on the right.

that are within 60° of the direction of the galactic center, compared them with the numbers of stars in the same subclass that are more than 60° from $\ell^{II} = 0°$, and applied a χ^2 test for the significance of deviations from a common distribution. The procedure was repeated for stars within 45° of $\ell^{II} = 0°$. The differences corresponding to those noted in Figure 1 are significant at the 5% confidence level. The WC9 stars are all found within 45° of $\ell^{II} = 0°$; most of the WN6 and WC7 stars are found within 60° of $\ell^{II} = 0°$; most stars of the other subclasses are found more than 60° from $\ell^{II} = 0°$. (The concentration of the WC7 stars to the inner regions only just satisfies the significance test. It is imperative that this should be re-examined when the completeness of the catalogue is improved in the fainter magnitude intervals.) If the stars are distributed with radial symmetry around the galactic center, it follows that all WC9 stars lie within 7 kpc of the galactic center, that most WN6 and WC7 stars lie within 9 kpc of the galactic center, and that most stars in other subclasses lie more than 9 kpc from the galactic center. No distinction has been made between single stars and binaries.

Thus we find that the population of WR stars varies from the inner to outer regions of the Galaxy. It seems significant that the subclasses that are concentrated towards the galactic center are among those that are rare or absent in the Magellanic Clouds. It is true that WC8 and WC6 stars are also absent from the Magellanic Clouds. There are only three WC8 stars known in the Galaxy; we do not, therefore, have any reliable information regarding their statistical distribution. The WC6 stars are certainly not concentrated towards the galactic center. Thus the correlation is not complete. However it remains true that the population of WR stars in the LMC is very like that in the Galaxy outside 9 kpc from the galactic center, but quite unlike the WR population inside that radius. The outer parts of our Galaxy (more than 12 kpc from the galactic center) show a very sparse population of WR stars; the SMC is more like this region than any other in the Galaxy.

There are other objects in our own and in other galaxies that show variations in properties from the inner to outer regions. In particular, the mean period of cepheid variables in M 31 decreases from 17 days near the center to 7 days in the outermost spiral arm (Baade and Swope 1965). From the work of Kraft and Schmidt (1963) and Kraft (1963), it appears that there is a similar variation in our own Galaxy. The mean period of cepheids in the LMC is close to that of cepheids in the solar neighborhood (Shapley and Nail 1948), while the mean period of cepheids in the SMC is closest to that of cepheids in the outermost parts of our Galaxy (Kraft 1963). Similarly, the ratio of neutral hydrogen to total density in the Galaxy increases from less than 1% at the center to about 9% in the solar neighborhood and then falls off again in the outer parts. The LMC, with a ratio of 9% (Bok 1966), again compares well with the solar neighborhood, while the ratio of 25% found in the Small Magellanic Cloud (Bok 1966) is greater than that found in any part of the Galaxy. (See Table 3.)

Thus it appears that the WR population shares in the effects of some overall change of properties from the center to the edge of the galactic disk and from galaxy to galaxy.

I suggest that all of these phenomena are due to different rates of star formation in the various regions. It is reasonable to suppose that star

TABLE 3

A COMPARISON OF POPULATION I IN THE MAGELLANIC CLOUDS AND IN THE GALAXY

Region (KPC)	WR population[7]	Neutral H / Total mass	Cepheids mode (log P)	Cepheids metal abundance
R < 7	Mostly WC9, WC7, WN6	<< 9%[1]		
7 < R < 9	Mostly WC7, WN6	< 9%[1]	> 0.9?[2]	Higher than solar neighborhood[4]
9 < R < 12	Mostly other subclasses	≈ 9%[1]	0.9[3]	-
LMC	(Mostly other subclasses)	9%[5]	0.9[6]	?
12 < R	Very few	< 9%[1]	0.7[3]	Lower than solar neighborhood[3]
SMC	Very few	≈ 25%[5]	0.5[3]	?

References: 1. M. Schmidt 1965, *Galactic Structure* (Chicago: University of Chicago Press), p. 522. F. J. Kerr and G. Westerhout 1965, *Galactic Structure* (Chicago: University of Chicago Press), p. 196.

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6. H. Shapley and V. McK. Nail 1948, *Proc. American Phil. Soc.*, 92, 310.

7. L. F. Smith 1968, *M.N.R.A.S.*

formation has proceeded most rapidly in the central regions of our Galaxy. This would be expected to result in a depletion of the interstellar hydrogen, as observed, and would presumably result in an increase in the heavy element abundance in the interstellar medium and, therefore, in the young stars. I suggest that in the galactic disk, the rate of star formation and the resulting heavy element abundance in the interstellar medium decreases monotonically from the center of the Galaxy to the outer edges. Reports by Kraft (1963) that the cepheids in the Per-Cas arm appear to be underabundant in metals and by Williams (1966) that cepheids in the Sagittarius arm are richer in metals than those in the Cygnus arm are consistent with this suggestion. Similarly Arp (1962) concludes that clusters originating in the outer regions of the Galaxy have lower metal abundances than those in the inner regions. However Arp (1965) suggests that the metal abundance in the galactic center is only as high as that in the solar neighborhood, whereas the present data suggest that the metal abundance in the galactic center is significantly higher than is found in the solar neighborhood.

It is known from the work of Hofmeister (1965a, b) that the properties of the cepheid population are very sensitive to the abundance of helium and heavier elements. Thus, while the exact effects are not yet known, differences in the chemical composition of stars in the various regions are clearly capable of causing the observed phenomena.

No direct evidence is yet available regarding the chemical compositions of the WR stars. However the present data strongly suggest that the initial chemical composition plays a definitive role in distinguishing the subclasses among the WC stars. Since most subclasses of the WN stars share a common distribution, we conclude that nearly all WN stars, regardless of subclass, share a common initial chemical composition. The exceptions are the stars in subclass WN6, which are more strongly concentrated towards the galactic center, implying that they have a higher initial heavy element abundance than stars in the other WN subclasses.

The hypothesis that the observed distributions are due to variations in chemical composition has the attraction of being the simplest hypothesis that is able to explain all the observed phenomena. It is, in fact, the only hypothesis I have been able to think of that will explain this. According to this hypothesis, the similarity of the LMC to the solar neighborhood and of the SMC to the outer regions of the Galaxy results from a similarity in the primordial density in the respective pairs of regions and thereby in the past and present rates of star formation.

V. ASSOCIATION WITH OB ASSOCIATIONS AND WITH H II REGIONS

Determination of the association of WR stars with other components of Population I has been hindered by lack of distances and consistent spectral classifications for most of the WR stars.

Association of WR stars with clusters and associations was first studied by Roberts (1958), who concluded that about 20% of all galactic WR stars are in clusters or associations and that a higher proportion of the WN stars are found in clusters and associations than of the WC stars. He suggested that WN stars evolve into WC stars.

Reddish (1967b) notes that WR stars are usually found in very young clusters and associations in which he finds circumstellar reddening of the most luminous stars. He concludes that the WR stars are very young objects.

Shajn and Hase (1953) and Bok and Wade (1955) have estimated that 50% of galactic WR stars are associated with nebulosity.

Observing WR stars in the LMC, Westerlund and Smith (1964) found a correlation between the luminosity of the star and the youth of the association in which it was located. WR stars in the 30 Doradus nebula are the most luminous, followed by WR stars in other young associations, followed by WR stars in the field. They conclude that the most luminous WR stars are the youngest and the least luminous are the oldest; they suggest that this represents an evolutionary sequence.

Since we now know that luminosity is correlated with spectral subclass, it is easy to anticipate Roberts' (1958) suggestion: (1) the ages of stars within a given subclass are fairly uniform, and (2) the ages differ from one subclass to another. The following re-assessment of the situation and the conclusions are part of my doctoral thesis and have not been published elsewhere. I therefore give them to you in some detail.

Knowing the distance of the WR stars, we can determine with greater certainty than before, which stars are located within known OB associations. Unfortunately the distances and sizes of the OB associations are still only poorly known. I have used the catalogue of associations given by Kopylow (1958). This catalogue was chosen in preference to others because it makes a concerted effort to distinguish the associations that are strung out along the same line of sight. Since most WR stars are located at considerable distances from the Sun, this precaution is imperative. To make a statistical examination of the frequency with which WR stars occur in clusters and associations, I imposed a limitation of 16.1 mag on the apparent distance modulus to which the WR stars were counted.

The location of known WR stars with respect to H II regions was determined in the northern hemisphere from the Palomar Sky Atlas and in the southern hemisphere from the catalogue and atlas of Rodgers, Campbell and Whitoak (1960). The distances of H II regions are rarely known. A WR star has been assumed to be associated with an H II region if it is centrally located with respect to the nebula or with respect to some substructure of the nebula. The same distance modulus limit was imposed for the WR stars associated with H II regions as for the stars in associations. The nebulae fall into two distinct categories. First, there are large nebulae which are usually associated with an entire OB cluster or association and whose ionization is clearly due to the combined effect of many stars. Secondly, there are small nebulae which I have called "ring" nebulae. Here we observe the WR star at the center of an arc or arcs of nebulosity. It appears to be the sole exciting star for the nebula, and its central location implies that the shape of the nebula is generated by some action of the star.

Table 4 gives the numbers of galactic WR stars within the distance modulus limits, and the frequency with which those stars fall in OB associations or in one of the two varieties of nebulosity. Similar data are given for the WR stars in the Magellanic Clouds, except that no distance modulus limit is necessary and ring nebulae are too small to be detected in the LMC on the plates available to me.

According to the customary philosophy, stars that are found in young associations or in regions of nebulosity are themselves young. It is clear from columns 3 and 4 of Table 4 that stars in some subclasses are found in young regions much more consistently than stars in other subclasses. Thus we may order the subclasses of the WR stars according to the relative ages of the stars. This ordering is shown in Table 5. The WN7 stars are nearly al-

TABLE 4

PERCENTAGES OF WR STARS IN ASSOCIATIONS AND IN H II REGIONS

	Galaxy					LMC		
Class	Limiting magnitude	Total number	Percent in assoc.	Percent in general nebulosity	Percent in ring nebulae	Total number	Percent in assoc.	Percent in nebulosity
WN3	11.6	2	0	0	0	2	0	0
WN4	12.2	1	0	0	0	5	0	20
WN4,5	12.0	1	0	0	0	0		
WN5	11.8	4	25	0	75	2	50	100
WN6	10.3	4	75	25	25	1	100	100
WN7	9.3	3	100	100	0	6	83	83
WN8	9.9	2	50	0	50	3	67	0
WN3,4,5?						13	15	46
WC5	11.7	2	100	0	0	5	60	60
WC6	11.7	4	0	0	0	0		
WC7	11.7	4	25	0	0	0		
WC8	9.9	0				0		
WC9	9.9	2	0	0	0	0		

TABLE 4 (cont.)

		Galaxy				LMC		
Class	Limiting magnitude	Total number	Percent in assoc.	Percent in general nebulosity	Percent in ring nebulae	Total number	Percent in assoc.	Percent in nebulosity
WN3+OB	10.9	0				0		
WN4+OB	11.1	2	50	0	0	4	100	100
WN4.5+OB	11.0	2	50	50(+50?)	0	0		
WN5+OB	10.9	2	100	(100?)	0	0		
WN6+OB	10.0	1	0	100	0	1	100	100
WN7+OB	9.2	2	50	50	0	1	100	100
WN8+OB	9.7	0				0		
WC5+OB	10.9	0				10	90	80
WC6+OB	10.9	1	100	0	0	0		
WC7+OB	10.9	5	60	20(+40?)	0	0		
WC8+OB	9.7	3	0	33(+33?)	0	0		
WC9+OB	9.7	0				0		
OB+WN	~8	0				5	100	100

TABLE 5

RELATIVE AGES OF WR STARS

(Age increasing downwards)

WN7		All binaries?
WN6	WN8	WC5
WN5	WN4	
WN3		WC6,7,8,9

ways found in OB associations that are immersed in nebulosity; the associations in the Carina nebula and in the 30 Doradus nebula and the association Sco OBI are prime examples. Thus the WN7 stars are clearly the youngest stars in the class WR. They are followed by the WN8, WN6 and WC5 stars which are frequently found in OB associations but not in the extremely young associations in which we find the WN7 stars. Next in age are the WN5 and WN4 stars that are only occasionally found in associations. Oldest of all are the WN3 and the WC6, 7, 8 and 9 stars that are almost never found in associations.

Thus among the WN stars those with highest excitation spectra are the oldest, while among the WC stars those with highest excitation spectra are the youngest.

There is some evidence that the spectrum binaries are all very young, in some cases younger than single WR stars of the same WR subclass. This is surprising and important, if true. Unfortunately the distance determinations for spectrum binaries are a great deal more uncertain than for single stars, and this result must be regarded with caution.

It should be noted that the "age" of the star has the normal connotation of the time since the onset of hydrogen burning. The length of the WR phase may, of course, be shorter than the age.

The ring nebulae require separate consideration. Johnson and Hogg (1965) were the first to realize the significance of the relationship between the WR stars and the ring nebulae; they suggested that the shape of the nebula is due to mass loss from the WR stars, i.e., that the ejected matter sweeps up the interstellar material in the stellar vicinity into a thin spheroidal shell.

I have searched the Palomar Atlas for nebulae with sharp filamentary rings. I find that every such nebula not previously identified as a planetary nebula or a supernova remnant surrounds a WR star. In the cases of planetary nebulae and supernovae we believe that the nebula is due to ejection of matter from a central star. It may reasonably be deduced that mass ejection is required for the formation of such nebulae. The presence of nebulae around WR stars thereby represents the most convincing evidence available that at least some WR stars lose a significant amount of matter sometime in their lives.

There are now seven known ring nebulae associated with WR stars. Four of these are shown in Figure 2. They are found only around WN5, WN6 and WN8 stars. In fact every single star of the subclasses WN5, WN6 and WN8 that is not in an association and is sufficiently close that we would observe a nebula if it were present (i.e., within distance modulus 16.1 mag) is found to have such a nebula.

We would not expect to observe a ring nebula around a WR star in an association. An OB association will ionize the surrounding gas so that such a nebula, if present, will not be observed. I con-

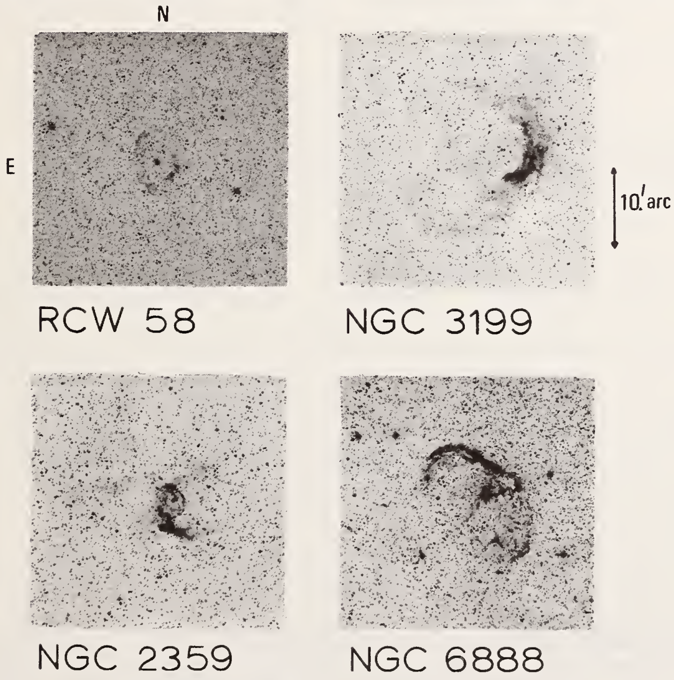

Figure 2. Nebulae associated with Wolf-Rayet stars.

clude that all WN5, 6 and 8 stars are probably capable of forming ring nebulae, i.e., they are losing mass at a significant rate or have done so at a previous stage of their evolution.

What can we say about the other subclasses? Nothing definite. There are three conditions for the formation of an observable ring nebula. (1) The WR star must be losing mass, and the matter ejected must carry a considerable amount of energy; (2) The star must be located in a region where the density of the interstellar medium is reasonably high; (3) The star must not be located in an association. If one of these requirements is not satisfied, it is not possible to say whether the others are.

The three subclasses which do have ring nebulae are all in the three youngest groups in Table 5. Nearly all nearby stars in equally young subclasses are located in associations. The situation with regard to binaries is not clear. Stars in the "older" subclasses may be sufficiently old that they have moved out of the interstellar clouds from which they were formed. Thus in each case requirement (2) or (3) may be unfulfilled, and we can therefore say nothing definite about the mass loss from stars in these subclasses.

If we know the total mass of a ring nebula (e.g., from radio observations) and if we assume (1) uniform ejection of matter at a known rate, (2) a velocity of ejection deduced from the violet absorption edges on the emission lines (Underhill 1968), and (3) the effects of radiation pressure etc. to be negligible, then the requirement of conservation of momentum allows us to determine the age of the nebula. Assuming rates of mass loss between 10^{-5} and 10^{-6} solar masses per year, Johnson and Hogg (1965) obtained ages between 10^4 and 10^5 years for two nebulae. Assuming rates of mass loss between 10^{-4} and 10^{-6} solar masses per year, Smith (1967b) found ages between 10^4 and 10^6 years for five nebulae. If the proposed mechanism is correct, it follows that the age of the nebula is equal to the age of the WR phase of the central star.

VI. MASSES

We are not very certain of the mass of any Wolf-Rayet star. The normal method of mass determination, via spectroscopic and eclipsing binaries, breaks down for these stars because the velocities derived from the emission lines in the spectrum of Wolf-Rayet stars do not usually represent the velocity of the center of mass of the star. This has been emphasized by Smith (1967a) who points out that since different emission lines in the spectrum of a WR star can give velocity curves with different amplitudes, eccentricities and phases, we have no way of knowing which, if any, of the lines best represents the motion of the center of mass of the WR star. It is also pointed out that there appears to be an orientation effect; the derived mass ratios are consistently smaller for systems viewed edge-on than for systems viewed nearly pole-on. The effect is demonstrated in another way by Figure 3, which gives a graph of the values of M_{WR}/M_{OB} against corresponding values of $M_{WR} \sin^3 i$ derived

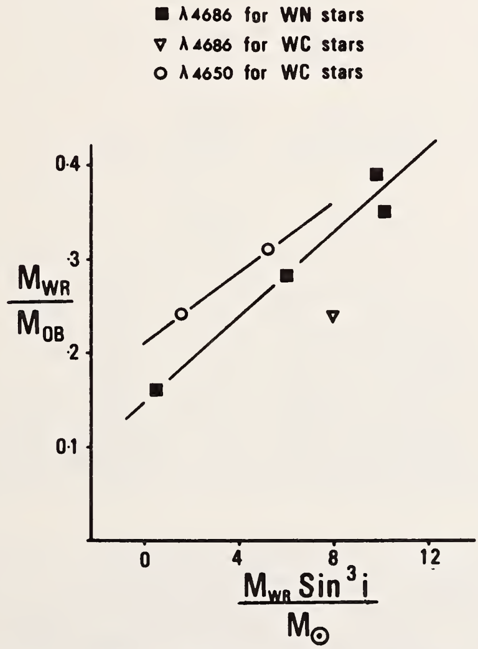

Figure 3. The relationship between derived mass ratios of eclipsing binary systems containing WR stars and $M_{WR} \sin^3 i$. The latter is dominated by the factor $\sin^3 i$ and is thereby a simple measure of the inclination of the orbital plane to the line of sight. The values are taken from Table 6. The emission line used in deriving the plotted values is indicated.

TABLE 6
DATA RELATING TO

HD	MR	Name	Spectral type	λ
152270	65		WC7 + O5-8	4650
186943	94		WN4 + B	4686
				4058
190918	99		WN4.5+O9.5Ia	4686
193928	108		WN6	4686
197406	113		WN7	4686
				4860 }
				4641 }
				4058
168206	85	CV Ser	WC8 + OB	4686
				4650 }
				4442 }
193576	106	V444 Cyg	WN5 + O6	4686 }
				4606 }
				4619 }
	114	CX Cep	WN5	4686
211853	116	eclipsing?	WN6 + BOI:	4686 }
				4603 }
				4058 }
214419	118	CQ Cep	WN7	4686

References:

1. W. A. Hiltner 1944, *Ap. J.*, 99, 273.
2. W. A. Hiltner 1945, *Ap. J.*, 101, 356.
3. W. A. Hiltner 1945, *Ap. J.*, 102, 492.
4. W. A. Hiltner 1948, *Ap. J.*, 108, 56.
5. W. A. Hiltner 1949, *Ap. J.*, 110, 95.
6. W. A. Hiltner 1950, *Ap. J.*, 112, 477.
7. R. M. Hjellming and W. A. Hiltner 1963, *Ap. J.*, 137, 1080.

for WR stars in spectroscopic binaries (i is the angle between the axis of rotation of the binary and the line of sight). The abscissa, $M_{WR} \sin^3 i$, is dominated by the factor $\sin^3 i$; the correlation implies that the derived value of the mass ratio of a binary depends on the inclination of the orbital plane to the line of sight. Since for any given system, the different emission lines can give different values for the velocity amplitude, the derived mass ratio is also dependent upon which emission line is used to represent the motion of the WR star; the emission line used in the orbit

TABLE 6 (cont.)
MASSES OF WOLF-RAYET STARS

M_{WR}/M_{OB}	$M_{WR} \sin^3 i$	f(M)	$M_{WR}/M_\odot$	References Spectro-graphic	References Photo-metric
0.24	1.59	4.29		2,10,13	
0.28	6.0	13.12		2,13	
0.42	3.36	3.92			
0.16	0.51	2.85		12,13	
		4.94		2	
		0.06		13	
		0.09			
		0.24			
0.24	7.95	21.6	≥ 8.0	3,13	7
0.31	5.23	10.0	≥ 5.2		
0.39	9.74	12.75	10.4	11	5,8,9
		5.39		4	4
0.35	10.1	16.01	≥ 10.1	2,13	7
		4.38		1	6

8. G. E. Kron and K. C. Gordon 1943, *Ap. J.*, 97, 311.
9. G. E. Kron and K. C. Gordon 1950, *Ap. J.*, 111, 454.
10. O. Struve 1944, *Ap. J.*, 100, 384.
11. O. C. Wilson 1940, *Ap. J.*, 91, 379.
12. O. C. Wilson 1949, *Ap. J.*, 109, 76.
13. K. Bracher 1967, Thesis, Indiana University.

determinations has been indicated for each point in Figure 3. A clearer correlation is obtained when values of M_{WR}/M_{OB} for the various systems are derived from the same emission line.

An explanation of these phenomena, in particular the inclination effect, may give us considerable insight into the mechanisms operating in the WR atmosphere. Until we have such insight, our determinations of the masses of these stars will remain uncertain.

However we must have some sort of working figure in mind. In Table 6 I have collected the most recent information regarding the apparent mass ratios and masses of WR stars in spectroscopic binaries. The sources of the data are given. In most cases the numbers quoted are those derived by Miss Bracher (1967), who has made spectrographic observations of many of the systems and has recomputed orbital elements for others.

We have three systems that are eclipsing binaries as well as spectroscopic binaries; these contain a WN6 star, a WN5 star and a WC8 star. The mass estimates for the two WN stars are nearly equal: 10.4 $M_\odot$ and $\geq$ 10.1 $M_\odot$. For the WC8 star we have $\geq$ 5 or $\geq$ 8 $M_\odot$ (depending on which emission line is used). The > sign is still included because although we know the inclination is nearly 90° for eclipsing systems, the light curves are not determined sufficiently well to define the value of the inclination, and we know only that the masses are slightly greater than the values given. Thus if we guess that we obtain the correct answers when we observe the system edge-on, we may assert that the WN5 and 6 stars have masses of about 11 $M_\odot$ and that the WC8 stars have masses of about the same or slightly less than those of the WN stars. We should, however, bear in mind that we have found considerable differences between the ages and distributions of the various subclasses. It would be unwise to assume, on the basis of the above data, that, for example, all WN stars have the same mass or that all WN stars are more massive than all WC stars.

A ten-solar mass star on the hydrogen-burning main sequence has an absolute visual magnitude of about -2.5 (Allen 1963). The absolute magnitudes given in Table 2 for WN5, WN6 and WC8 stars are -4.3, -5.8 and -6.2, respectively. Thus these stars appear to be 2 to 4 magnitudes over-luminous for their masses.

VII. INTERPRETATION OF THE CLASSIFICATION SYSTEM

What now can we say about the classification of the WR stars? What parameters seem to be important in the definition of the subclasses?

The distribution of the subclasses suggests that chemical composition is an important parameter; it suggests further that the WC9 stars have the highest initial heavy element abundance, followed by the WC7 and WN6 stars, followed by the remaining subclasses which all have the same initial composition.

We have also found a difference in age among the subclasses. The most likely explanation is that the subclasses represent the same stage of evolution in stars of different masses. This is a situation with which we are familiar; the youngest subclasses originate from the most massive stars, which evolve the most rapidly.

Sequential evolution through the subclasses seems unlikely. It would involve the evolution of WN7 stars, which we always find within young associations, into WN5 stars, which are sometimes found "alone" and still within neutral hydrogen clouds. This does not seem possible.

Let us see if, qualitatively, we can account for the diversity of subclasses via the two parameters, initial mass and initial chemical composition. I suggest, strictly as a working hypothesis, that the WC sequence depends primarily upon initial chemical composition, with the WC9 stars having the highest initial abundance of heavy elements and the WC5 and WC6 stars the lowest.

Most of the WN stars share a common distribution pattern in the Galaxy and, therefore, a common initial chemical composition. I suggest that the WN sequence depends mainly upon the initial masses and present ages of the stars, with the WN7 stars originating from the most massive and the WN3's from the least massive stars.

We have one exception to this scheme in each sequence. Amongst the WC stars, the WC5 and WC6 stars share a common distribution and hence a common initial chemical composition. However, the WC5 stars appear to be significantly younger than the WC6 stars, and this difference must be attributed to a difference in initial mass. Similarly among the WN stars, the WN6 stars appear to be demarcated by initial chemical composition, as indicated by their distribution. These exceptions indicate that if this scheme is approximately correct, then the effects of initial mass and chemical composition are intertwined in a moderately complex manner.

VIII. BINARY STARS

The question we must now ask explicitly is: Are all WR stars binaries, or are some single stars? First let me present to you what seem (to me) to be the most relevant facts.

1. Many WR spectra are clearly composite. Of those that appear to be single, a few are spectro-

scopic binaries. Is it possible that all WR stars are binaries but that the companions of some are of low mass and low luminosity and have no conspicuous effect upon either the spectrum or the velocity of the WR star?

2. The WR components of systems that are spectrum binaries appear to be very similar in spectral characteristics and in luminosity to the WR stars that appear to be single (with the exception of the WN5 and WN6 stars discussed in Section II). I will comment further only on the data relating to the luminosities of these stars.

a. Among the WC5 stars in the LMC (Smith 1968b) the so-called single stars are nearly 1 mag fainter than the faintest binary star. The decrease in contrast between the emission lines and the continuum is qualitatively consistent with the hypothesis that the WR components all have the same luminosity but that the luminosities of the OB components differ.

b. The data noted in (a) clearly imply that a given subclass of WR star may be found with OB-stars of a wide range of luminosities.

c. No inconsistencies have arisen from the assumption that the luminosity of a binary system is the sum of the luminosities of the so-called single WR star of the appropriate subclass and an OB star of the appropriate subclass. (See Smith 1968b.)

1. Evidence in favor of all WR stars being binaries

a. It is likely that the WR stars in binary systems have undergone severe mass exchange. If we also have truly single WR stars, then we have two groups of stars whose evolutions must have been vastly different. The similarities of the spectra <u>and</u> luminosities imply, but by no means prove, that <u>all</u> the stars in a given subclass have the same evolutionary history.

b. Binary nature and mass exchange give us a credible mechanism for creating a small number of very peculiar stars.

2. Evidence against all WR stars being binary stars

a. There is some evidence, as outlined in Section V, that the spectrum binaries are significantly younger than the apparently single WR stars in the same subclass. Could this be a matter of degree rather than kind? Is it possible that a WR star with a more massive companion evolves more quickly or evolves differently?

b. The WR stars associated with ring nebulae all appear to be single stars. (Kuhi (1968) has suggested that HD50896, associated with RCW 11, is a binary, but this is not proven.) Could this be a statistical accident or another effect of degree?

I leave these as questions. But I note that our conclusion on this point will affect our attitude towards the classification system and its interpretation.

IX. EVOLUTIONARY STATUS

From the data summarized here, I think we would still regard WR stars as Population I stars and young objects. However, while stars in some subclasses, e.g., WN7, are extremely young, we may justifiably wonder whether the stars in some other subclasses, e.g., WC9, may be near the age limit for what we generally think of as Population I.

We may, I think, be quite sure that WR stars are not pre-main sequence objects, or at least that they are not <u>all</u> pre-main sequence objects. The fact that stars in some subclasses are fairly old indicates this, but we may argue more precisely as follows. Our mass estimates for WN5 and WN6 stars are about 10 solar masses, yet these stars are not always found in extremely young regions. By contrast, T Tauri stars, which are believed to be pre-main sequence, have masses of the order of 1 solar mass and are <u>only</u> found in the very youngest regions. Since a 10 $M_{\odot}$ star will complete its main sequence contraction much faster than a 1 solar mass star, a pre-main sequence star of 10 solar masses should be found only in regions at least as young as those in which we find T Tauri stars.

It is possible that WN7 stars, which we find in very young regions, are in a pre-main sequence phase; however, this contradicts the assumption that the WR stars are a homogeneous group, and I would suggest that we do not introduce that complication unless it is necessary.

The extreme youth of the WN7 stars indicates that the stars are probably no older than immediately post-main sequences. I think that we may safely assume that the WR stars are not <u>on</u> the hydrogen-burning main sequence; so we must conclude that the

WR stars represent a phase in the evolution of some or all stars that occurs immediately after the stars leave the main sequence.

X. SOURCE OF INSTABILITY

The high velocities observed in the atmospheres of the WR stars, together with the evidence of mass loss, lead us to suspect that the stars are on the verge of instability. What can be the cause of this instability? Several suggestions have been made over the last few years.

Sahade (1958) and Underhill (1966) have suggested that the WR stars are akin to the T Tauri stars and that their instability arises from a similar source whatever that may be. This possibility seems unlikely in the light of evidence presented above that the stars are in a post-main sequence phase.

Limber (1964) suggests that the emission shell is generated by forced rotational instability which produces tangential ejection. The instability is triggered by contraction of a rapidly rotating star. I will leave it to Limber to tell us whether this model can account for many of the properties discussed above.

Recently Paczynski (1967) has suggested that the WR stars are a product of mass exchange between the components of a close binary. He suggests that they are helium burning stars situated near the helium burning main sequence, and that they may be on the verge of pulsational instability due to the temperature sensitivity of the rate of helium burning. He has calculated the evolution of a 16 $M_\odot$ primary to the completion of mass exchange. Kippenhahn and Weigert (1967) have calculated the evolution of a 9 $M_\odot$ primary to its departure from the helium-burning phase; Kippenhahn (1968) has now completed the evolutionary calculations for a 25 $M_\odot$ primary. The detailed correspondence between the models and the observed properties of the WR stars has been worked out by Kippenhahn and the author. In what follows, I adopt the convention used by Paczynski, that the star that is initially the more massive is called the primary throughout.

The evolutionary tracks in the log L, log T_{eff} diagram for the 9 $M_\odot$ star (Kippenhahn and Weigert 1967) and for the 25 $M_\odot$ star (Kippenhahn 1968) are shown in Figure 4. The mass of the secondary has

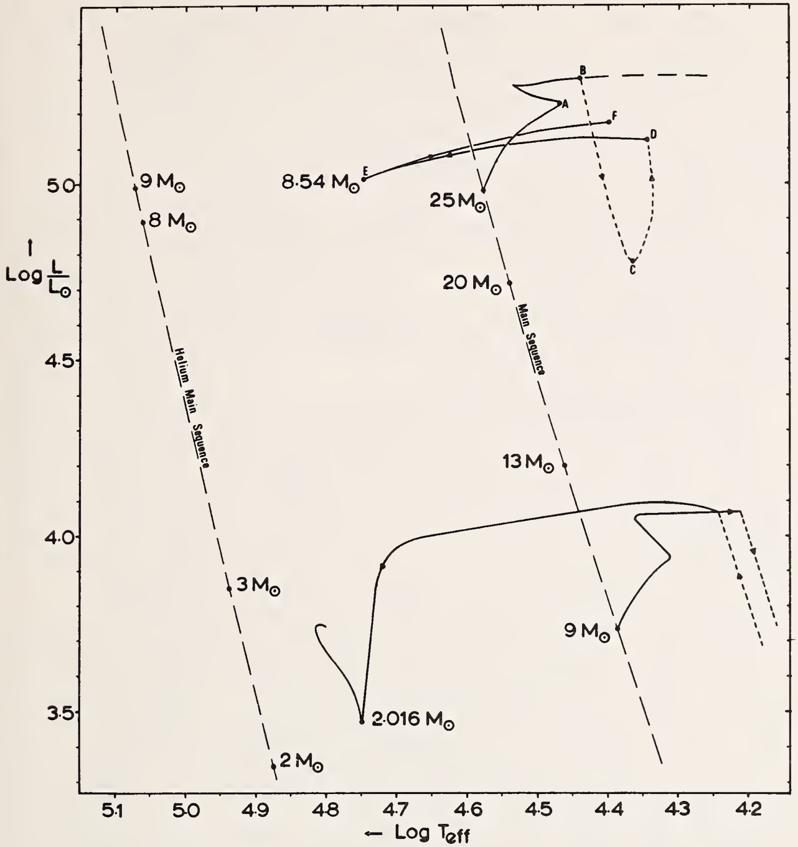

Figure 4. The evolutionary tracks in the log L - log T_{eff} diagram for primary stars in close binaries. In both cases the separation of the binaries is such as to give mass exchange after depletion of hydrogen in the core (Kippenhahn and Weigert 1967 Case B). The upper track for a $25M_\odot$ initial primary is from Kippenhahn (1968). The lower track for a $9M_\odot$ primary is from Kippenhahn and Weigert (1967).

virtually no effect upon the evolution of the primary.

Consider the track shown for the 25 $M_\odot$ star. Point A corresponds to the turn-off point from the main sequence; after this the star moves rapidly along the path shown. If the star were single, it would follow the dashed path to the red giant region. Phases at which the star is moving from left to right are phases in which the envelope is expanding. If the star overflows its critical equipotential before reaching point A, evolution proceeds as described by Kippenhahn and Weigert Case A (1967), and the result is an Algol-type system in which the primary, which is by then the less massive star, is in the subgiant region and fills its critical lobe, while the secondary, which is then the more massive star, is on the main sequence. Agreement between theory and observation for these systems is good and gives confidence that this process does operate frequently and in the manner indicated by the calculations.

If the separation of the binary is slightly wider than that required to give mass exchange before point A, but is close enough that the primary fills its lobe before reaching the equilibrium helium-burning (red giant) phase, we have Kippenhahn and Weigert Case B. In the case shown in Figure 4, the separation was chosen so that the primary filled its critical equipotential lobe at point B. Any matter that overflows this lobe is transferred to the secondary. The primary continues to expand, and rapid mass exchange ensues; the luminosity drops as energy goes into expansion of the outer layers of the star. Mass exchange continues until the primary reaches a configuration in which its equilibrium size (i.e., the size at which helium burning begins in the core) is equal to that of the critical lobe. This will take place just before the star is stripped to its helium core. As it approaches this configuration, mass exchange slows, and the star brightens to approximately its initial luminosity. At point D helium burning begins, the star contracts away from the critical surface, and mass exchange stops. The mass of the star is now a little greater than the mass of the helium core at the time of depletion of hydrogen in the core.

As shell burning increases the size of the helium core, the star moves to the left, eventually settling down at point E where most of the energy is generated in the helium-burning core. The star is near the pure helium main sequence; the presence of a thin hydrogen-rich envelope holds it to the right of that sequence. Notice that the final temperature, 5.5×10^4°K, is the same for the 8.5 $M_\odot$ star that results from evolution of a 25 $M_\odot$ primary as it is for the 2 $M_\odot$ star that results from evolution of a 9 $M_\odot$ primary. The final temperature is very sensitive to the amount of hydrogen-rich material that remains after mass exchange, and this is difficult to calculate; thus the equality shown here may be a coincidence, but it does suggest that the final temperature may be rather insensitive to the mass of the star.

Boury and Ledoux (1965) have shown that a pure helium star is pulsationally unstable for masses greater than 7-8 $M_\odot$. The instability results from the temperature sensitivity of the rate of energy production from helium burning. The product of this class of close binary evolution will always have a thin shell of hydrogen-rich material which will damp the instability; however, it is expected that for a sufficiently massive star, instability will be present. Van der Borght is presently analyzing the stability of such models; results should be available in the near future. If such an instability is present, it may result in shock waves that propagate outwards through the star, producing an inverse temperature gradient and high excitation, as observed in the solar chromosphere.

At point E the system is detached (i.e., neither component fills its critical lobe), and the radius of the 8.5 $M_\odot$ helium star is about 3.5 solar radii. It will remain here for about 5×10^5 years. As the star evolves away from the helium-burning configuration, it expands and again fills its critical lobe, transferring still more of its mass to the secondary. The star is then at point F, with an effective temperature of about 2.5×10^4 °K. Carbon burning will start in the core, and there will be both a helium-burning shell and a hydrogen-burning shell. Thermal pulses originating in the shell sources are expected; these may cause mixing between the layers above and below the shell. Pulsational instability due to temperature sensitivity of the carbon-burning process will almost certainly be present. The star will not be unstable to mass loss as in the evolutionary stage B-C, so it may remain in this configuration, filling its critical lobe, for a time comparable to the core-helium-burning lifetime. Lifetimes in each of the evolutionary phases are given in Table 7. The only phases long enough to be observed are E and F.

The luminosity at point E is slightly greater than that of a pure helium star of the same mass. The excess luminosity is contributed by the hydrogen-burning shell. We may easily extrapolate the results for the 8.5 and 2 $M_\odot$ stars to predict the final luminosities for any final mass and thence for any initial mass. This is shown in Figure 5. At the

TABLE 7

LIFETIMES OF A 25 $M_\odot$ PRIMARY STAR OF A CLOSE BINARY SYSTEM

Stage	Lifetime (years)	Path	Critical lobe filled
H core burning	4,520,000		
H shell burning before mass exchange	190,000	A-B	
Rapid mass exchange; luminosity falling	$\approx$ 300	B-C	yes
Slow mass exchange; luminosity rising	3,860	C-D	yes
He core burning; moving towards He main sequence	62,000	D-E	
He core burning; T_{eff} decreasing	480,000	E-F	
C core burning	480,000?		yes(?)

bottom is plotted the final mass of the primary. Since the final mass is essentially that of the helium core at the time of depletion of hydrogen in the core, the initial mass has a simple relationship to the final mass and is plotted at the top of the diagram. The circles represent pure helium stars after Van der Borght and Meggitt (1963). The diamonds represent the 2 $M_\odot$ and 8.5 $M_\odot$ final products calculated by Kippenhahn and Weigert (1967) and Kippenhahn (1968) respectively. The curve is drawn through the two evolved models, parallel to the curve defined by the helium stars. Assuming the stars radiate like blackbodies at 5.5×10^4 °K, we determine a bolometric correction of 4.60 mag; absolute visual magnitude is plotted at the right. Due to uncertainty in the effective temperature and bolometric correction, the zero point of the absolute visual magnitude is uncertain by about one magnitude. The temperature calculated is an upper limit; thus the absolute visual magnitudes given are fainter limits. Bolometric magnitudes of hydrogen-burning main sequence stars are fainter by about 3 mag. Due to the high tem-

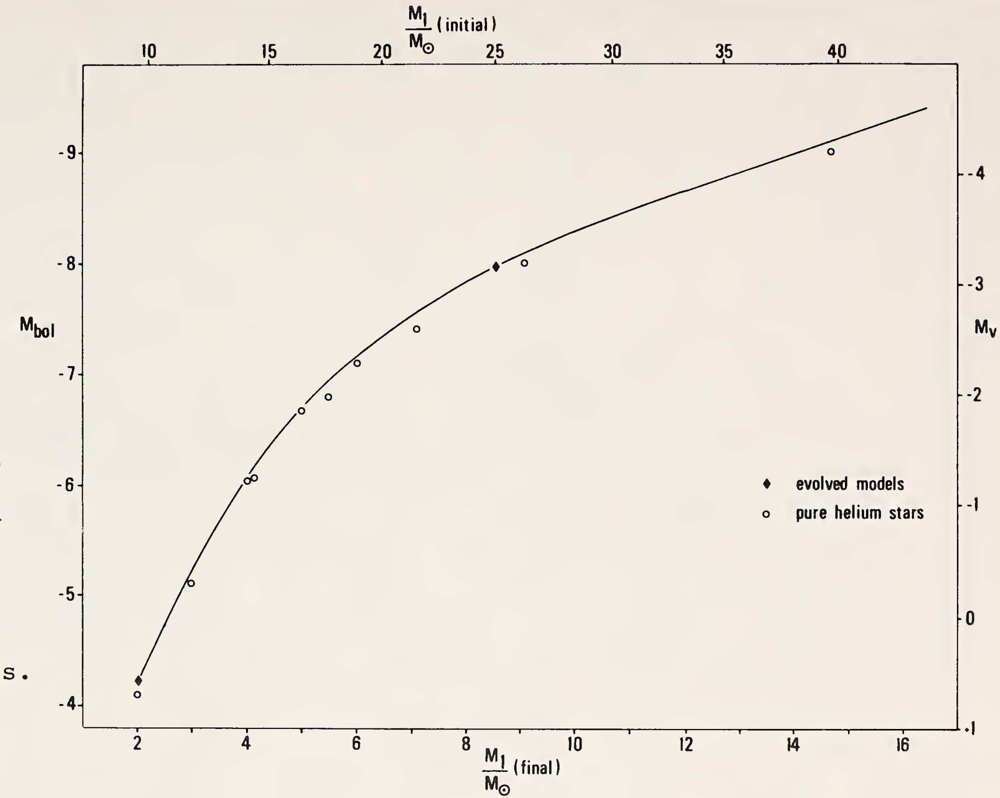

Figure 5. The luminosity of stars in phase E (after completion of mass exchange). Open circles represent pure helium stars after Van der Borght and Meggitt (1963). The filled diamonds represent the evolved models with hydrogen-rich envelopes. The curve is drawn through the diamonds parallel to the curve defined by the circles. Absolute visual magnitudes given on the right are calculated for an effective temperature of 5.5×10^4 °K.

perature of the helium stars, their bolometric corrections are larger. For a temperature of 5.5×10^4 °K the difference in visual absolute magnitude between the helium stars and hydrogen-burning stars of the same masses is about 1 mag.

At phase F, the star is a little more luminous than at E. However the temperature is lower and is about equal to that of a main-sequence star of the same mass. Thus the visual magnitude of the star is more than 3 mag brighter than that of a main-sequence star of the same mass.

We can also predict the final mass and luminosity of the secondary star, which receives the mass shed by the primary and moves up the main sequence, becoming the more massive component of the pair. (The initial main sequence calculated by Kippenhahn (1968) has been used throughout these calculations.) There is, at this point, one serious uncertainty. Mass loss from the system as a whole is likely, since the secondary may find itself with an angular momentum problem (cf., Paczynski and Ziolkowski 1967). We have, at this time, no way of estimating how much mass is likely to be lost from the system. Figure 6 shows two possible cases. The graphs show loci of points of constant ΔM, the difference in magnitude between the final stars, in the sense that if the helium star at phase E is brighter, ΔM is negative. The initial mass of the secondary is plotted on the vertical axis; the initial mass of the primary, on the horizontal. The latter may be replaced by the final mass of the primary, which is given between the two graphs. The upper graph shows the case of no mass loss to the system. For a bolometric correction of 4.60 mag, ΔM is always positive in the mass range for which we have results. If, however, we have overestimated the bolometric correction by, say, 1 mag, then the locus given for ΔM = 1 mag becomes the locus for ΔM = 0 mag, and the two stars have equal visual magnitudes. Similarly, if we consider phase F, the bolometric correction is 2 mag less than used to derive Figure 6, and 2 mag should be subtracted from the values of ΔM attached to the loci. The lower graph shows the resulting loci if only half the mass shed by the primary is captured by the secondary, and the rest is lost to the system. In this case, ΔM is consistently smaller than in the upper graph, and the helium star is often the brighter of the two components. For higher masses of the primary, the luminosity differences tend to favor the helium star.

These, then, are the essential properties pre-

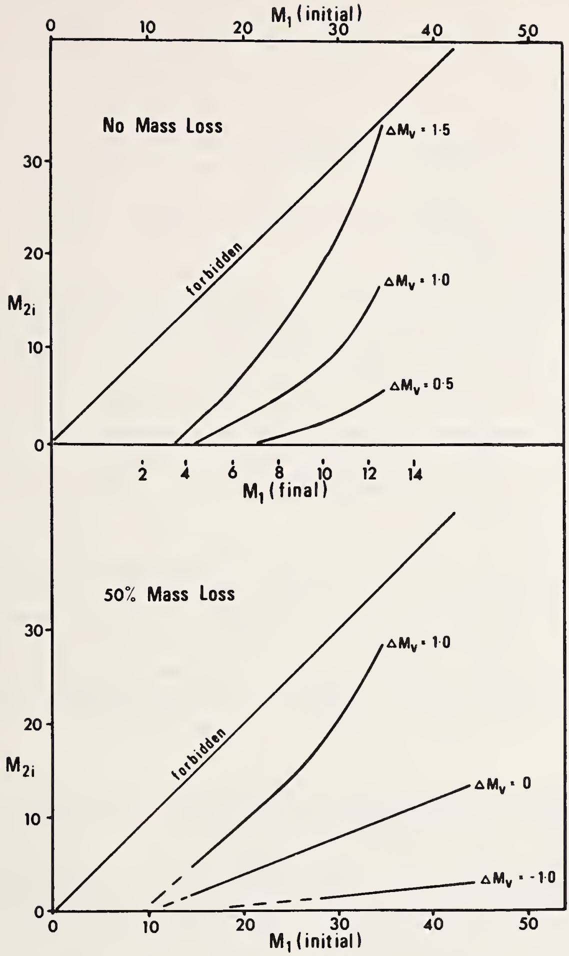

Figure 6. The loci of ΔM (the difference in absolute visual magnitude between the components of the binary) when the primary is in phase E are shown. The upper graph is calculated on the assumption of no mass loss from the system. The lower graph assumes that 50% of the mass shed by the primary is captured by the secondary. The vertical axes give the initial mass of the secondary. The horizontal axes at top and bottom give the initial mass of the primary, and the final mass of the primary is between the two graphs. When the main-sequence star is the most luminous, ΔM is positive.

dicted for the models that result from mass exchange based on the stated assumptions. Let me compare the predictions with the features of the observed properties of WR stars summarized in this report. The theory predicts:

1. Helium stars will occur in binary systems. Unless there is severe mass loss from the system, the helium star will be the less massive component. The companion is expected to look like a normal main-sequence star.

2. The helium star will have a fairly high effective temperature and may have a helium rich atmosphere. In the case of the 8.5 $M_{\odot}$ helium star, the helium concentration in the atmosphere is 46.5% by mass at stage E, and 65% by mass at stage F; these are only slightly greater than normal. However, thermal pulses in stage F may cause mixing of the outer layers and may increase considerably the percentage of helium and heavier elements in the atmosphere.

3. The stars are overluminous for their masses by 3 mag in M_{bol}, by $\geq$ 3 mag in M_{vis} at phase F, and by $\geq$ 1 mag in M_{vis} at phase E. From Figure 5 we see that the predicted absolute visual magnitude for a 10 $M_{\odot}$ star at phase E is -3.5. This is about 1 mag fainter than most of the values given in Table 2. However, considering that the bolometric magnitude used is an upper limit, the agreement is satisfactory.

4. The initial and therefore the final mass of the secondary component is a free parameter. Thus a helium star of given properties can be found with main-sequence stars having a wide range of masses and luminosities, as observed. The helium star can be either the less luminous or the more luminous star of the binary. This allows the possibility that many binaries may go undetected; i.e., the absorption-line spectrum of the main-sequence star may be completely hidden by the stronger emission spectrum of the WR star.

5. The stars are in an immediately post-main sequence phase of evolution.

6. Pulsational instability may exist; if so, it provides a supply of mechanical energy to excite an emission-line spectrum.

7. The final luminosity of the helium star depends only on its mass, which depends only on the initial mass of the star. The age of the star is essentially its main-sequence age, which also depends on its initial mass. Thus we predict that the most luminous helium stars will be the youngest,

as observed among the WN stars.

8. The most luminous helium stars will have the largest radii (in accordance with our empirical results for WN stars), but they do not appear to have significantly higher effective temperatures. This is fortunate since observationally we found the excitation to be anticorrelated with the luminosity.

9. It is probable that the evolution is sensitive to the initial helium abundance. For example, the amount of matter left in the hydrogen-rich envelope may be altered. Such a mechanism may be responsible for the distribution differences that I have interpreted in terms of initial chemical composition. Unfortunately, we cannot at this time be more definite about this point.

10. It has often been suggested that the WR components of binaries are filling their critical Lagrangian lobes. If this is so, then stars at point F appear the most likely candidates for WR stars. Stars at point F have similar luminosities but lower temperatures than stars at point E. Thus the bolometric corrections would be less, the stars would be brighter in the visual spectrum, and the values of ΔM_V in Figure 6 would decrease in favor of the helium stars. Point F also has the advantage of possible exotic atmospheric compositions due to mixing induced by thermal pulses.

XI. DO THE WR STARS FORM A CLASS IN THE SENSE DEFINED IN SECTION I?

The WR class was defined by common spectral features and by the exclusion of planetary nuclei and novae. The question now is: do we have any evidence that the members of the class so defined do not satisfy the criteria stated in Section I, viz: (A) the stars are at closely related stages of evolution, and (B) the principle mechanisms responsible for the defining spectral characteristics are the same for all stars in the class.

First, consider criterion A. It seems a reasonable working hypothesis that the WR stars we know to be members of binaries have suffered mass exchange and have histories very like that sketched in the preceding section. If this is so, are we forced to assume that all WR stars are binaries? Is mass exchange in close binaries the only mechanism that will produce a WR star? Kippenhahn (private communication) suggests that the answer is "not necessarily". The basic property that makes the helium star into a WR star must be its instability. If this instability arises from the temperature-sensitivity of helium or carbon burning, then the specification for a WR star is that it has a helium or carbon core with only a thin overlying layer of hydrogen-rich material. A close binary is a very convenient way of stripping off the outer hydrogen layers to create such a configuration, but there may well be other ways. However, no other mode of envelope stripping is likely to be as fast, or begin as early in the evolution of the star, as that which occurs in a close binary. Hence the observation that binaries have a greater tendency to concentrate to young associations than do single stars, is a crucial one. As I have emphasized, the observation is tentative, because the distances of binaries are so uncertain. We badly need some way to check this possibility.

In this connection, the nuclei of planetary nebulae come to mind. Some of these have spectra that are nearly identical with those of Population I WR stars. Planetary nebulae are believed to represent a post-red-giant stage of evolution. Are we, here, looking at stars that have found a slow way of reaching the critical configuration?

Of the WR subclasses, I think that the WN7 stars are the most likely candidates for non-membership in our hypothetical class of WR objects. The spectra of these WN7 stars are very like those of the Of stars. There is no reason to suppose that Of stars are close binaries. Thus, if we decide that all WR stars are close binaries, the WN7 stars may well be excluded from the class.

We have no direct evidence on criterion (B). However, spectroscopically similar objects that satisfy criterion (A) will most likely also satisfy criterion (B). The main use of the latter criterion is to clarify the relationship between the many objects that are spectroscopically similar; that is, a similar excitation mechanism may produce similar spectra in objects that are unrelated in an evolutionary sense. Thus it is my personal opinion that the planetary nuclei and single and binary WR stars of Population I all satisfy criterion (B), but must be regarded as distinct classes if they result from different evolutions and therefore do not satisfy criterion (A). It seems possible that the spectra of Of stars and of novae result from different sources of excitation and are not related to the first mentioned varieties.

XII. SUMMARY

The correlations between the observed and deduced properties of WR stars and their spectral subclasses are shown schematically in Table 8. The arrows indicate the direction of increase of the property within each sequence. A dashed arrow indicates uncertainty. Note that I have not arranged the WN subclasses in numerical order. Inversion of the order of the WN7 and WN8 stars makes the luminosity and age sequences more nearly monotonic.

TABLE 8

SUMMARY: OVERALL PROPERTIES OF WR STARS

WN3,4,5,6,8,7	Property	WC5,6,7,8,9
←N lines——	Excitation	←C lines——
- - - → ← - - -	Line {strength, width}	←——————
——————→	Luminosity	- - - - - →
- - - - - →	Size	- - - - - →
	Temperature	Cooler?
$\gtrsim$ 10 $M_\odot$	Mass	$\gtrsim$ 5 $M_\odot$
←——————	Age	WC5 < rest
WN6 > rest	Initial abundance of heavy elements	——————→
Immediately post-main sequence	Evolutionary stage	?
WN5,6,8	Ring nebulae	None
WN5,6,8	Strong violet absorption edges	WC9

Note: Direction of arrow indicates increase.

(The exception is subclass WN3 in the luminosity sequence.) Considering only the nitrogen ions, the excitation sequence is still monotonic after this inversion of WN7 and WN8, because the difference between these two classes depends on the strengths of the He I lines.

To some extent the deduced properties depend upon the assumption that we have a uniform class in the sense discussed. If an entire subclass is removed from the class as a whole, the conclusions drawn in this paper and given in Table 8 will not be greatly affected. However, should we have a complete admixture of single and binary stars with very different evolutions, ages, and possibly luminosities, then we should have to reconsider very carefully some of our conclusions.

I have advocated two major interpretations of these observations;

1. The classification system may be interpreted as a two-parameter sequence, initial mass and initial chemical composition. The subclasses of the WC sequence are determined primarily by initial abundance differences among the heavy elements. The subclasses of the WN sequence are controlled primarily by the age (initial mass) of the star. I realize this is an oversimplification.

2. A WR star is a helium- or carbon-burning star, with only a very thin hydrogen-rich envelope. These stars are pulsationally unstable, and this instability generates shock waves that produce an extended atmosphere and an emission-line spectrum.

Mass exchange between the components of a close binary can lead to such a star; WR stars that are members of binary systems have probably gone through such a process. This mode of evolution can explain a major fraction of the observed correlations between luminosity, age, etc.

Whether a single star can also attain a WR configuration by some other process remains undetermined. Another such process would probably be slower than mass exchange, and single WR stars are therefore expected to be older than binary WR stars. Nuclei of planetary nebulae that display WR characteristics may represent the products of slow single-star evolution to a WR phase.

We do not yet know the relationship between the violence of the pulsational instability and other properties of the star. This is probably the vital link between the structural and evolutionary properties of the star and its atmospheric and spectral characteristics.

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DISCUSSION

Chairman: Cecilia Payne-Gaposchkin

Payne-Gaposchkin: We have a number of copies of the Hiltner and Schild paper on classification; as these contain reproductions of spectra, I suggest they be distributed for your reference. [See Part B, Figures 24 - 28, pp. 175 - 179.]

Now before launching into general arguments on the material summarized, I suggest we call for any additional facts not hitherto presented.

Stecher: The large majority of facts available to us on Wolf-Rayet stars have come from observations in the visible spectral region. Observations in the rocket ultraviolet can now provide us with additional information both on the continuum and on the line spectrum.

Our observations were made from an Aerobee rocket carrying a thirteen inch f·10 telescope with an attached photoelectric spectral scanner with three exit slits. The resolution was 10 Å, and the wavelength interval scanned was from 1150 to 4000 Å. The results were telemetered to the ground, recorded on digital tape, and processed in a computer. The whole rocket, with payload attached, was pointed to the programmed stars with an accuracy of ten to twenty seconds of arc, depending on the magnitude of the star. This was accomplished with a star-tracker and a gyroscopic inertial platform. The payload was parachuted to the ground and recovered in good condition. Two scans of 20 seconds duration were made on each programmed star. On this flight we observed α Canis Majoris, ε Canis Majoris, ζ Puppis and γ Velorum. The first two spectra appear to be interpretable in terms of standard atmospheric theory and will not be discussed here. ζ Puppis is an O5f star, and one component of γ Velorum, γ_2 Velorum, is a binary, WC8 + O.

Figure 7 is the spectrum of ζ Puppis in the 1800 to 3100 Å range. The flux is in units of 10^9 ergs $cm^{-2}s^{-1}Å^{-1}$ at the top of the Earth's atmosphere. This wavelength region is free of strong lines, so that by comparing the energy distribution with that of a model atmosphere, we can obtain an effective temperature. Two questions arise: (1) Is the model sufficiently representative of the star to permit us to integrate over the energy distribution in

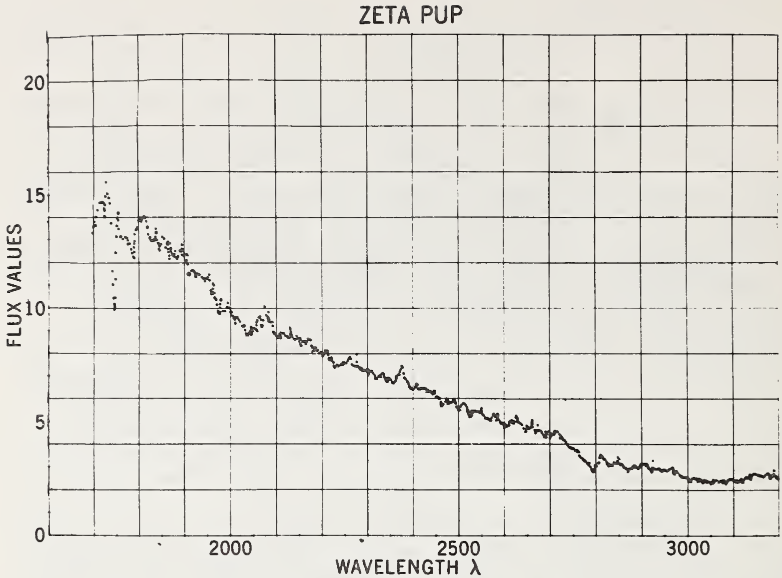

Figure 7. The spectrum of ζ Puppis in the 1800 to 3100 Å range. The flux is in units of 10^9 ergs $cm^{-2}sec^{-1}$ $Å^{-1}$ at the top of the Earth's atmosphere.

unobserved spectral regions, especially below the Lyman limit; i.e., is the physics sufficiently complete? (2) What is the correction for interstellar extinction? Using the continuous models of Mihalas (1965, *Ap. J. Suppl.*, 9, 321.) with no correction for interstellar extinction, we obtained an effective temperature of 2.8×10^4°K, which is of course incorrect. The correction for extinction at 2000 Å is about 5 times the B-V color excess (Stecher, 1965, *Ap. J.*, 142, 1683.). With the most probable color excess, the effective temperature comes out to be 4×10^4°K. At 2000 Å the difference between a 4×10^4°K and a 5×10^4°K model is only one tenth of a magnitude, when the models are normalized to the visual flux. This is because at these temperatures, 2000 Å is still way out at the red end of the Planck curve. Thus an error of +0.02 mag in the color excess could increase the temperature from 4×10^4°K to 5×10^4°K. An error of this size is a distinct possibility. The color excess and temperature of ζ Puppis are important to our discussion, since γ Velorum appears to have the same energy distribution in the 2000 Å region. If we assume, as has been suggested by several investigators, that ζ

Puppis and γ Velorum are physically associated in space, then the correction for interstellar extinction will be the same, and the effective temperatures will both be about 4×10^4 °K.

Figure 8 shows the short wavelength region of ζ Puppis. The remarkable thing to note is the P Cygni type profile in the resonance lines of C IV, Si IV, N V, C III, etc. The excitation of this star is sufficiently high that the N IV line at λ1718 is also in emission with a Doppler-shifted absorption edge. The level is 16 volts up and should be useful in determining the structure of the envelope. The wavelength difference between the emission peak and the blue-shifted absorption minimum in C IV is 18 Å, which corresponds to a velocity of 3400 km/sec. While this is an upper limit, the real velocity, which depends on the model, will certainly be large. The time constant on the amplifier was set to rise to 1/e of the true

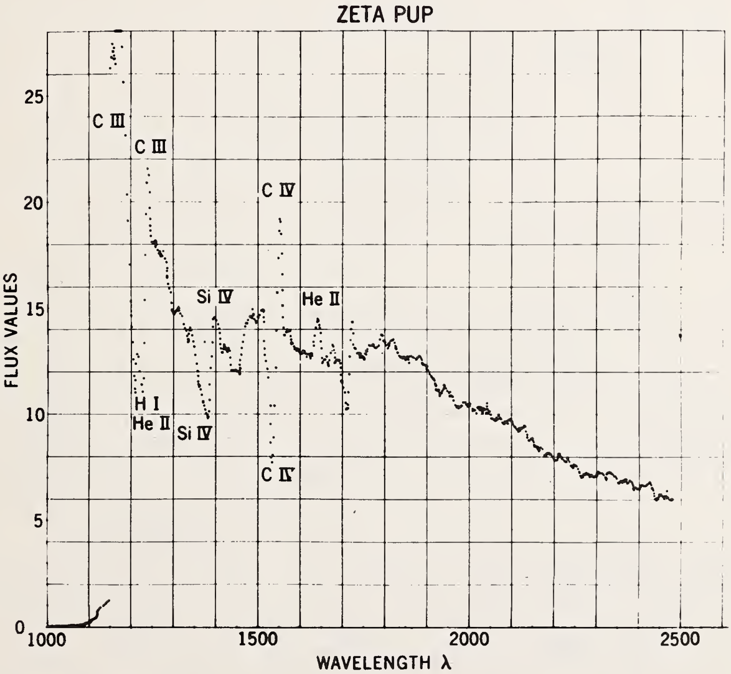

Figure 8. The spectrum of ζ Puppis in the 1200 to the 2400 Å range. The flux is in units of 10^9 ergs cm^{-2} sec^{-1} $Å^{-1}$ at the top of the Earth's atmosphere.

signal level over the scanning bandpass of 10 Å. When corrected for instrumental profile, the center of the absorption line should be quite black, indicating a large steady-state mass loss.

The absorption at λ1216 is due to interstellar hydrogen. The line is formed by radiation damping and can therefore be used to obtain the amount of neutral hydrogen between the Earth and ζ Puppis. The small equivalent width, ∿ 4 Å, indicates a low column density for neutral hydrogen, consistent with the small reddening correction in the visual region. Finally, one should note that in many respects the ultraviolet spectrum of this star is similar to that of a Wolf-Rayet star. In fact, if we were to ignore the wavelength region, this spectrum would meet most of the criteria so far discussed for inclusion among the Wolf-Rayet phenomena (and wavelength region was not one of the criteria).

Having presented the "control star", we proceed to Figure 9 which is the 1800 to 3100 Å scan

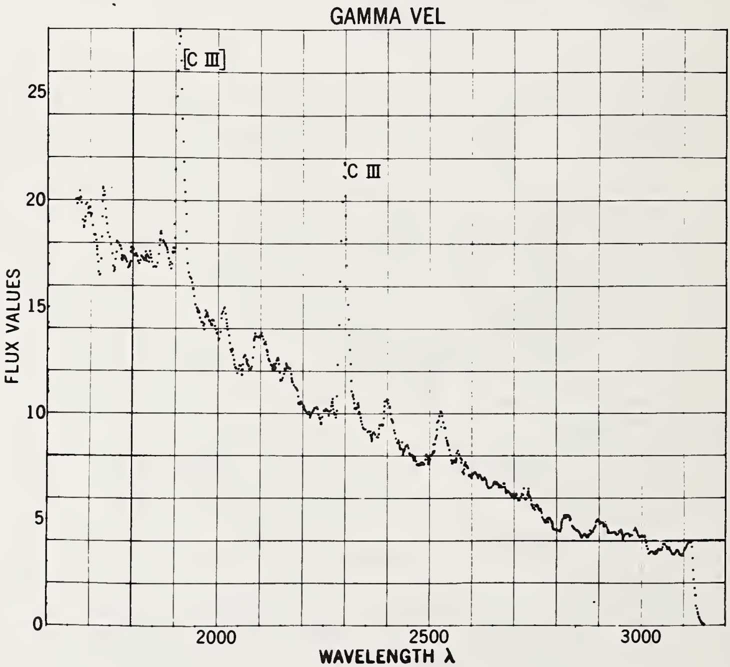

Figure 9. The spectrum of γ Velorum in the 1800 to 3100 Å range. The flux is in units of 10^9 ergs cm^{-2} sec^{-1} $Å^{-1}$ at the top of the Earth's atmosphere.

of γ Velorum. The continuum is less obvious, but a reasonable estimate can still be made. In addition to the Wolf-Rayet continuum there is the O-star and a small contribution from γ_1 Velorum. The lines of the O-star are not noticeable, so it is reasonable to assume that most of the continuum is due to the Wolf-Rayet component itself. With these assumptions, and the same correction for extinction as we applied to ζ Puppis, the effective temperature is 4×10^4°K. This is of course assuming that a hydrogen-helium atmosphere can be applied to this star. If a helium-carbon atmosphere should apply, changes in the far-ultraviolet could alter the temperature. A check against Hanbury Brown's angular diameter, the observable flux, and the flux of the model at λ1900 showed good agreement.

The strong line at λ2296 is the strong permitted ^1D to $^1P^0$ transition in the C III singlets. The even more intense line at λ1909 is the intercombination line of C III. This line has an oscillator strength that is smaller by a factor of a million than that of the permitted line and thus poses the interesting question of the processes of formation. West and I have been looking at this problem, and he will discuss our calculations later. It appears that stimulated emission is necessary to produce the C III line. The required overpopulation of the triplets with respect to the singlets is most likely due to the dilute radiation field in the envelope (Struve and Wurm, 1938, *Ap. J.*, 88, 84.) and/or to mechanisms as yet unspecified. The result is laser action.

The only other line clearly identified in the spectrum is due to C IV at λ2524. The atomic data for the high excitation present in these stars are not sufficiently complete to allow satisfactory identification of most of these lines. It should be remembered that the lines in the ultraviolet contain a considerable amount of energy; the continuum is many times more intense in the UV, and if a line is to show up, it must be intense.

Figure 10 shows the far-ultraviolet spectral scan of γ Velorum. It overlaps the previous spectrum and again shows the λ2296 and λ1909 lines of C III. The Hα line of He II appears to be quite broad. The broad general absorption between λ1700 and λ1300 is due to molecular oxygen in the Earth's atmosphere. There is sufficient information to correct for it, and this is now being done. The C IV line at λ1550 appears both in emission and in absorption, again indicating an expanding envelope,

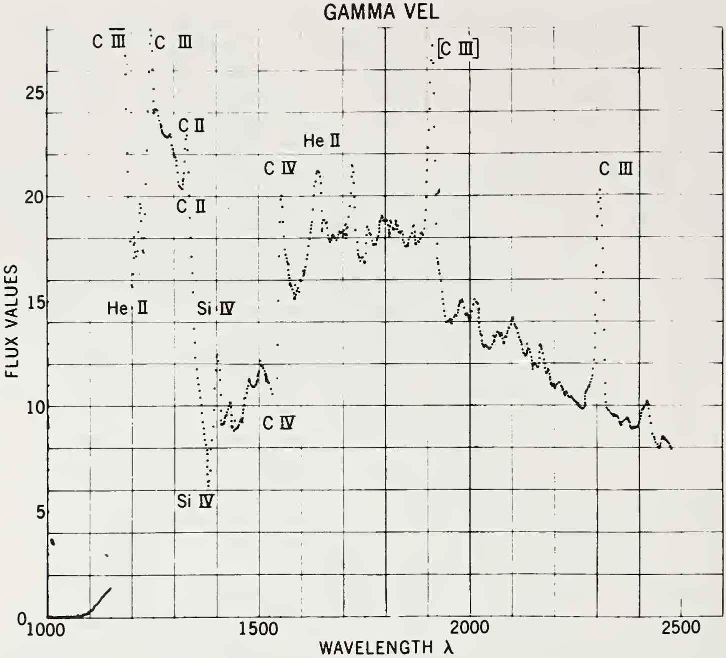

Figure 10. The spectrum of γ Velorum in the 1200 to 2400 Å range. The flux is in units of 10^9 ergs cm^{-2} sec^{-1} $Å^{-1}$ at the top of the Earth's atmosphere.

although the velocity of ∿ 1500 km/sec is less by a factor of two than the velocity in ζ Puppis. This is also evident in the resonance lines of C II, Si IV, etc. Again a large number of lines belonging to the star are unidentified.

One interesting possibility for unidentified lines is the question of the physical reality of the quark. This mathematical particle which has had such great success in SU3 theory may or may not exist in nature. If quarks are physically real they would presumably be produced through cosmic ray reactions in the interstellar medium, would be thermalized, and would be present through star formation in younger stars. At interior temperatures, the quark of charge - 1/3 would bind itself to C, N, and O. The Wolf-Rayet stars offer the most likely possibility of detecting them by observing the spectral lines of carbon of charge 1+2/3, 2+2/3 and of nitrogen of charge 1+2/3, 2+2/3, etc. The long path length through the envelope of the star, coupled with the probable overabundance of these elements, can greatly increase the current upper limit on these particles. Accurate theoretical wavelengths are needed for the quarked atoms, along with more complete multiplet tables.

Nitrogen V appears in absorption in γ Velorum, and N IV is probably present in emission. The lines are weaker than in ζ Puppis but are sufficiently clear to establish the presence of nitrogen in a carbon Wolf-Rayet star. A number of interstellar lines appear in the spectra of both stars: C II, Si II, Al II, O I, Fe II, Ce II. There is also the possibility of autoionization of Al I and Ca I. The equivalent width of hydrogen Lyman-α in γ Velorum is similar to that in ζ Puppis, indicating a low column density of neutral hydrogen and implying the correctness of the interstellar-extinction correction.

Underhill: I have quite a few comments, but I won't make them all at once. First I would like to report on γ_2 Velorum. Ganesh and Bappu (1967, *Kodaikanal Bull.*, 16, No. 183.) have made a radial-velocity study of this binary and find a period of 78.5 days. They determined velocity curves for the Wolf-Rayet component from three different lines, and they present arguments to show that the mass ratio, $M_{WR}/M_{O} = 13/46$, derived from the C III complex at 4652 Å is probably the best value. They also conclude that sin i is about one, or at least that it is greater than 0.8.

Now the spectral class of γ_2 Velorum is, according to Lindsey Smith, WC8 + O7, and her estimates of the absolute magnitudes of the subclasses WC8 and WC9 are based entirely on this system. Using Graham's distance for γ_1 Velorum, (derived from Hβ and UBV photometry), she came up with an absolute magnitude of -6.6 for the system and -6.2 for the WC8 star. Now Ganesh and Bappu note that γ_1 Velorum (HD68243) and γ_2 Velorum (HD68273) are an optical double, that γ_1 Velorum is of MK type B2IV, and that it is 2.4 mag fainter than γ_2 Velorum. If you assume that γ_1 Velorum has the standard absolute magnitude of -3.3 for a B2IV star, and that the absorption and reddening correction are the same for γ_1 and γ_2 Velorum, then the absolute magnitude of the system, γ_2 Velorum, is -5.7. If you assume further that the two components of γ_2 Velorum are equally bright, then the WC8 star has an absolute magnitude of -5, which differs by 1.2 mag from Lindsey's value.

Schild: How did they get the classification B2IV for γ_1 Velorum?

Underhill: They say that an absolute magnitude of -3.3 is ascribed on the basis of the MK classification type assigned. They did not give a source for the classification; I suspect they took it out of the Bright Star Catalogue.

Payne-Gaposchkin: Does the Bright Star Catalogue give a reliable luminosity class?

Schild: Satisfactory MK classifications are available for some of the brightest B-stars, but not for all of the southern OB-stars. This is a crucial point, because you are basing an absolute magnitude estimate on a single spectral and luminosity classification.

Underhill: Well, I just wanted to present this difference in results. Personally I prefer the fainter magnitude because, with the exception of WN7 and WN8 stars which have quite different spectra, most WR stars have absolute magnitudes of the order of -5. The adopted absolute magnitude will certainly make quite a difference to the distribution of WC8 and WC9 stars.

In this connection, I would like to refer to Campbell's hydrogen envelope star, BD+30°3639 or HD184738. It was the original WC8 star, but over the course of years it has been placed with the planetary nebulae. Its apparent visual magnitude is 10.0, and if you accept Seaton's distance, its absolute visual magnitude is -4.9. Admittedly, Seaton warns that his method for determining distances is very uncertain, particularly when the nebula is as dense as this one; but if you want the star to be as bright as -6, you will have to put it a long way off, and then I ask how you are going to see such a small nebula.

Smith: On the subject of the absolute magnitude of γ_2 Velorum: I personally would regard a luminosity for γ_1 Velorum determined from the Hβ photometry of Graham as more reliable than a luminosity derived from a spectral classification of unknown source.

Regarding the absolute magnitude of the WC9 stars: If you drop the absolute magnitude much fainter than -6.2, then you must find another explanation for their peculiar angular distribution. Confinement within 45° of $\ell^{II} = 0°$ implies confinement within 7 kpc of the galactic center (see Figure 1). If the luminosity I gave is correct, then nearly all WC9 stars are sufficiently far away from the Sun to be within 7 kpc of the galactic center. If, on the other hand, the luminosity is fainter than -6.2, then the WC9 stars are closer to the Sun and further than 7 kpc from the galactic center.

You must then explain why they are not found in directions greater than 45° from $\ell^{II} = 0°$.

Underhill: Suppose we are venturesome and agree that WC9 stars are rather older than some of the others. Am I correct in saying that on the whole the stars toward the center of the Galaxy are a bit older than those in the outermost arms such as the arms in the anti-center region of Orion? We are certainly not talking about stars that are <u>very</u> old. Well, the fact that the 6 or 10 WC9 stars that we do observe just happen to be in that quadrant doesn't bother me much: toward the center of the Galaxy you have more arms in which to see them, and after all, the Poisson distribution allows you an uncertainty of the square root of 10 which is 3. So the observed distribution could be just chance. Finally, I think it is essential to draw in the dark, obscuring lanes before concluding anything about the distribution of objects at distances greater than 3 kpc from the Sun.

Smith: But I do see WC7 and WN6 stars more than 45° from $\ell^{II} = 0°$ and at distances comparable to those of WC9 stars.

Consider Anne Underhill's second point, the similarity between Campbell's hydrogen envelope star and the WC9 stars. In the southern hemisphere there is a second, similar object, Henize 99, with a WC9 spectrum plus nebular lines. A spectrogram has been obtained by Louise Webster. Whether these objects are genuine planetary nebulae remains to be determined. However, I have three reasons for asserting that they are different from the stars that we call classical WC9 stars. (1) Stellar emission lines in the spectra of Campbell's star and of H99 are consistently narrower by a factor of about 1.4 than the emission lines in the spectra of classical WC9 stars, i.e., they are about 6 Å as against about 8 Å in WC9 stars. (2) We do not see nebulae around any classical WC9 stars, although three of them have apparent magnitudes brighter than either Campbell's star or H99; nor do we see nebular lines in the spectra of any classical WC9 stars. Thus I assert that the classical WC9 stars do not have nebulae. (We do see nebulae around WR stars, but as I mentioned earlier, these are restricted to three subclasses within the nitrogen sequence.) (3) Whereas all classical WC9 stars are concentrated within 45° of the galactic center, the two stars with nebulae lie in directions outside this range. Furthermore, I think that the distance of Campbell's star is extremely uncertain and that an absolute magnitude based on it is no better than an estimate based on γ_2 Velorum.

Payne-Gaposchkin: You said there was a suggestion of spiral structure in the distribution of galactic WR stars. I wonder if you could show us how it runs.

Smith: There are three nearly circular arms located 10, 8 and 6 kpc from the galactic center. The best fit is obtained with arms inclined about 5° to the tangent direction, in the sense that the arms are trailing (see Smith 1968c).

Payne-Gaposchkin: But these must be "classical" spiral arms, not arms defined by the Wolf-Rayet stars.

Thomas: I am concerned with the distinction between a WR object and the possibly broader range of phenomena which exhibit WR spectra. Lindsey has concentrated on WR objects; she asserts that there are indeed such objects. Let me summarize my own understanding of her picture and ask her to correct and elaborate it. A WR object starts out with a definite chemical composition and mass, and possibly with a definite location relative to other objects. During the course of its evolution, it reaches the WR stage, i.e., its spectrum shows the characteristics which define the WR class. Lindsey asserts that at this stage the star consists of a helium core surrounded by a thin hydrogen shell and that it is overluminous for its mass. The WR spectrum results from the instability of this physical configuration: radial pulsations produce shock waves that mechanically heat the atmosphere.

So there are two questions: (1) How do you produce this physical configuration from the initial conditions in the stars? Is there a certain range in initial mass and chemical composition for which a star is guaranteed to pass through the WR stage? (2) How does this physical configuration act to produce the WR spectrum? What is the special relation between mechanical energy, momentum, and chemical composition that produces the characteristic spectrum? Now you permit variations in spectra within the WR class, namely those variations that correspond to the different subclasses. So you would permit variations in the initial configuration of the star and in the mechanism producing the spectrum. You began by suggesting that the subclasses correspond either to different positions along a single evolutionary track, i.e., to different stages in the evolution of stars of the same initial mass and chemical composition, or to similar positions on different evolutionary tracks, corresponding to differences in initial mass or chemical composition. It is my understanding that you conclude by adopting the second alternative, since you suggest that the WC subclasses correspond to differences in initial chemical composition, and WN subclasses correspond to differences in initial mass. Now in answer to (1), you state that a close binary system will certainly produce the necessary configuration, and without being specific you suggest that there may be other alternatives for a single star. But you also state that the helium-core hydrogen-shell configuration may not be the only way of mechanically heating the atmosphere. So it is not obvious to me that you are claiming that the WR objects are the only class of objects that uniquely produce the WR spectrum.

Smith: I made the hypothesis of a uniform class and showed the arguments for and against it; I do not necessarily believe the hypothesis is correct. I think that to produce a WR spectrum, you need the mechanism which I discussed and that it does not matter whether the star is single, binary, novae or whatever. However to obtain a uniform class, we require that all the stars be at the same evolutionary stage. It is at this point that we have to ask very carefully whether they are binary, single, novae, or nuclei of planetary nebulae. If a star reaches a certain configuration of mass, core size and composition, then it will be, say, a WN7 regardless of how it got there. But if you have different modes of evolution, you may have differences in distribution. For example, single stars are likely to be older, and this will be reflected in their distribution within the Galaxy. Again, you may have different distributions among the subclasses. For example, the central stars of planetary nebulae may represent single stars which have achieved the WR configuration. Since most planetary nuclei are WC stars, it may somehow be easier for a single star to arrive at the WC configuration.

Thomas: In your answers to (1) and (2), you have in no way differentiated between WC and WN stars, either in terms of the model or of the excitation mechanism. You have only specified a helium core surrounded by a hydrogen shell. For all you have said, the difference between the WC and the WN sequences could be just a difference in excitation.

Smith: Yes, that is possible. However, it does appear from the distributions that the WC stars do have higher initial heavy-element abundances (including helium as a heavy element). The most likely suggestion is that the initial heavy-element abundance affects the evolution. In particular it may affect the amount of hydrogen-rich material which remains around the helium core after mass exchange. This, in turn, will almost certainly affect the instability properties and hence will affect the amount of mechanical energy fed into the outer atmosphere. It may also affect the surface composition: once you pass the helium burning stage, you have two thin shell sources, one burning helium, the other hydrogen. These are likely to produce thermal pulses and mixing in the outer layers. The energy content of the thermal pulses and the degree of mixing to the surface will depend on the thickness of the hydrogen-rich envelope, hence on the initial heavy element abundance.

Thomas: You are bringing more carbon up from the interior by differential mixing. Is this the kind of thing Paczynski suggested?

Smith: Yes, it's the kind of thing suggested by Paczynski, although the models that go that far and the detailed suggestions are due to Kippenhahn. There may also be other mechanisms.

Underhill: I have two comments: one is on the assignment of objects to the WR class; the other is on abundance differences. On Beals' first figure, he pointed out one little spot we could not see, under which was written "Oa". He explained that in the early days of spectral classification, these objects were found on objective prism plates, that they were very peculiar, and that there were only a few of them. Now they are called WC and WN, but the point is that we identify each of these stars by its spectrum in a very limited wavelength range, between 4000 and 5000 Å. Indeed if Stecher had shown you the spectrum of ζ Puppis only in the range λ < 2000 Å, you would immediately have said it was a WR star, whereas we all know that in our standard classification system, it's an O5f. So there is no guarantee that we haven't got a mixture of widely differing objects in our WR class. Investigations such as Lindsey's are aimed at trying to separate these objects into smaller groups. But let's not insist on too tight a spectral identification, at least until later in the symposium when we hope to be able to answer a few more questions.

Now regarding abundance differences: Those stars which Lindsey suggests are formed toward the center of the Galaxy are supposed to have more heavy elements than the so-called "normal" population in the solar neighborhood. They are supposed to be old stars. Now to support her arguments relating the abundance of heavy elements to age and galactic distribution, Lindsey has quoted work on the cepheids. There are supposed to be both strong- and weak-line cepheids, and it is often suggested that strong lines imply a greater abundance of heavy elements. And so they do, on the normal LTE approach. Now I've been trying to interpret stellar spectra by standard textbook theories for 20 years, not because I believed the textbooks, but because there wasn't anything else. In 20 years I've computed a lot of models and a lot of lines. Superficially the models look like stars, but when you come to compare them in detail, they aren't stars. And when I look into the physics of what I am doing, and when I talk to the plasma physicists, I find I've been using the wrong theory and getting the wrong answers. For example, the LTE approach leads me to conclude that many more atoms contribute to the formation of a strong line than would be required if the correct theory were used to interpret the spectrum. There is also strong evidence that any star with a slightly extended atmosphere will have broad lines due to motions. So if you say strong lines indicate greater abundances, you are wrong. So I think you are on weak ground in saying there is a definite anomaly in the abundances, either of heavy elements or of helium, in the WR stars.

Thomas: Anne, you know I'm the last person in the world to stifle criticism of the conventional methods of atmospheric analysis, but concerning abundance differences, I think it is essential to differentiate between arguments based on atmospheric analysis and those based on interior models. It seems to me that all Lindsey's arguments are based on the latter.

Payne-Gaposchkin: Some of the arguments about cepheids suggest that their evolutionary paths differ and that this indicates differences in composition.

Smith: Basically the argument is this: we have gross variations of population properties in different galaxies and in different parts of the same galaxy; nothing else will account for these variations as well as the suggestion that they result from differences in initial chemical composition. The fact that the atmospheres also appear to vary in composition provides additional verification of this hypothesis.

Thomas: I thought your argument was based on computations by the interior people, Kippenhahn and Paczynski; if you are now turning to atmospheric analyses, I'll back Anne's objections.

Smith: Mine is an interiors argument. Properties of the cepheid models depend on their initial chemical composition; see, for example, Hoffmeister's models in *Zs. f. Ap.*, 65. She has calculated two sets of evolutionary tracks with different Y's and Z's; it is impossible to tell whether Y or Z is the sensitive parameter. I'm suggesting that the evolution of a WR star is affected in a similar way by its initial chemical composition. Mine is therefore an interiors argument.

Payne-Gaposchkin: Hoffmeister has the extreme and the ordinary Population I in her models, but they were very similar; while the one with low Y was very different. Well, we'll argue about it another time.

Underhill: I am still worried about the helium abundance. Observations by Bappu indicate that there may be H mixed with He II in the Balmer series of some of the well-known Cygnus stars. The fact that you cannot easily observe H is obvious from estimates of electron temperature in these atmospheres. Your gas won't be cool enough to produce strong Balmer lines until you're so far out in the atmosphere you won't see it against the star anyway. So the interior question is wide open; you have nothing against which to check your model. If you're going to start off with a large helium and low hydrogen abundance, the atmosphere has to reflect this. I just don't think there is enough observational evidence to support that position. It may be perfectly true that by starting with a close binary, you can get mass exchange, but how do you know that you don't end up with two O-stars? There is not one bit of evidence that either of the stars will have a Wolf-Rayet spectrum. So the first thing we have to do tomorrow and the next day is to answer the question: How do we produce a WR spectrum?

S. Gaposchkin: I would like to emphasize that V444 Cygni is an almost perfectly determined binary system, both spectroscopically and photometrically. One must therefore accept the values, mass = 9.5 $M_\odot$ and luminosity = -2.8, as well established. So Miss Smith has taken the WR star too bright for this particular subclass.

Smith: The absolute magnitude for the WR component depends on an assumed luminosity ratio.

S. Gaposchkin: False! It is an observed ratio!

Payne-Gaposchkin: We will defer further discussion until after Kuhi has discussed this star tomorrow.

Hanbury Brown: I am going to show you some measurements made on γ_2 Velorum with the stellar interferometer at Narrabri observatory in Australia (see Figure 11). This instrument is an intensity interferometer, and it has the property that the signal to noise ratio is independent of the optical bandwidth, provided only that the optical bandwidth is large compared with the electrical bandwidth. We have exploited this peculiar property to make measurements of the angular diameter of the star in the continuum and in the light of an emission line. The interferometer measures correlation as a function of separation between the two mirrors, or baseline. It can be shown that this correlation is proportional to the square of the fringe visibili-

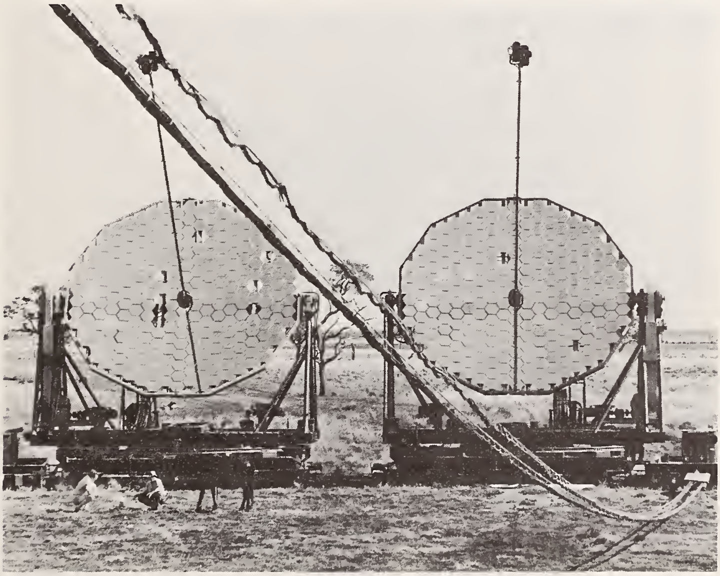

Figure 11. The interferometer at Narrabri Observatory, Australia. Each reflector, 6.5 meters in diameter, is formed by a mosaic of 252 hexagonal glass mirrors approximately 38 cm across and 2 cm thick. The mirrors are not figured to a high precision because it is not necessary to form a conventional image.

ty observed with a classical Michelson interferometer with the same baseline. It follows that from a curve of correlation versus baseline one can readily find the angular diameter of a star.

We made the measurements in the continuum at 4430 Å with a filter bandwidth of ±50 Å, and also in the C III/IV emission line with a filter centered on 4656 Å with a bandwidth of ±12.5 Å. The center wavelength of the latter filter is displaced from the center of the line to allow for the fact that due to aberrations in the optical system, much of the light does not transverse the filter normally. Figure 12 shows the measured values of correlation as a function of the baseline in meters. The error bars on each point represent the uncertainty due to statistical fluctuation of the correlator output. The interpretation of the continuum results (upper part of figure) is complicated by the fact that we are observing a binary star. The correlation from a binary is a function of the angular separation, position angle and relative brightness of the two components, and unless there is some auxiliary optical data, especially about the period, it is very difficult to interpret the re-

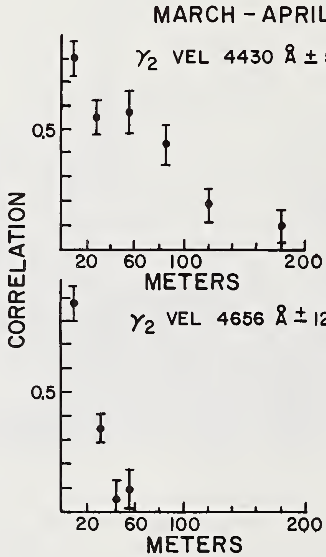

Figure 12. Correlation observed from γ_2 Velorum in the continuum (upper figure) and in an emission line (lower figure).

sults. When the baseline of the interferometer is very short and the angular separation of the binary is not resolved, the measured correlation is proportional to $(L_1 + L_2)^2$, where L_1 and L_2 are the luminosities of the two components. On the other hand, when the baseline is long and the angular separation of the binary is resolved, the correlation is proportional to $(L_1^2 + L_2^2)$ and may therefore be considerably less. In the transitional region between these two conditions, we get all sorts of complicated results which depend in detail on the spacing and position angle of the binary and also on the baseline.

Looking at the results in the continuum, we see at once that we need to reduce the errors by making longer observations; nevertheless, there are some things which can be deduced from them. First we can derive the angular diameter of the star by fitting a theoretical curve (for a uniform disk) to the three points at the longest baselines; we can then repeat this, including the four and five longest baselines, and we can compare the results. We can be reasonably sure that the three, and perhaps four, longest baselines are so long that the separation of the binary will be resolved. At the two shorter baselines we cannot be sure that the binary is resolved, and you can see from the figure that the correlation at the shortest baseline is significantly above a smooth curve through the other points; this suggests that, at the shortest baseline, the binary was not completely resolved, and I shall assume that it was not resolved until the baseline exceeded 50 m. The angular diameter of the brighter component, deduced from the four longest baselines is:

$$\theta \text{ (continuum)} = 0.44 \pm 0.07 \times 10^{-3} \text{ seconds of arc.}$$

I hope you will appreciate that this is a preliminary result, as the observational data were taken only a few weeks ago and have not yet been completely analyzed. From the intercept of the fitted curve with the zero-baseline ordinate, we can find the ratio of the luminosities of the two components of the binary. Our preliminary result is:

$$\Delta m = 1.3 \pm 0.6 \text{ mag.}$$

We cannot, of course, tell from the measurements which of the two stars is the brighter; but roughly speaking, if they differ significantly in brightness, as they appear to do, then the interferometer yields the angular diameter of the brighter star. Thus if the WR component is brighter than the O-star, its angular diameter is given by the value above.

The lower part of Figure 12 shows the correlation observed in the emission line. I should have mentioned before that the correlation is normalized to the value expected from a point source giving the same light flux. Thus the measurements in the emission line point to a single source with an angular diameter of:

$$\theta \text{ (emission line)} = 2.1 \pm 0.3\text{x}10^{-3} \text{ seconds of arc.}$$

Again I must emphasize that the result is preliminary. For example, one must subtract from the "emission line" results the contribution due to the continuum, and this has not been done properly yet. I have not yet had time to do it. Nevertheless, I do not think that the two values of angular diameter which I have quoted will be changed very much by a more complete analysis.

From the results one can see that, assuming that the WR star is the brighter of the two, its apparent diameter in the emission line is about 4.8 times its diameter in the continuum. Also, if we take the value of its parallax given by Allen, 160 pc, then the radius of the WR star is roughly 7.5 $R_\odot$, and that of the emission region is roughly 36 $R_\odot$. Combining the measurements of angular size with measured values of monochromatic flux (f_λ), we find, again very roughly, that the brightness temperature of the WR star at 4430 Å is about $3.1\text{x}10^4$°K, and the brightness temperature of the emission region is about $1.2\text{x}10^4$°K. A more detailed analysis will probably alter these values a little, but I don't think very much.

S. Gaposchkin: Do you have a parallax?

Hanbury Brown: The value of parallax, 160 pc, which I used was taken from Allen's "Astrophysical Quantities", a standard work. I regret that I do not know the original source. I should like to add that one is seriously handicapped in interpreting the continuum observations on this star by lack of spectroscopic data. We badly need to know its period. Miss Underhill has kindly drawn my attention to the paper by Ganesh and Bappu which gives the period as 78.5 days. This measurement could be a great help to us in interpreting our data.

Smith: Graham's distance of 460 pc, determined by Hβ and UBV photometry, is rather larger than Allen's figure of 160 pc, so I would multiply all Hanbury Brown's dimensions by a factor of about 2.5. The luminosity of the system is then -6.6; for the O7 companion, I took $M_V = -5.2$ (Schmidt-Kaler 1965) which leaves -6.2 for the WC8 star. Thus the WR star appears to be the more luminous by about 1 mag, which compares well with the figure of 1.3 ± 0.6 from the interferometric observations.

Payne-Gaposchkin: Can't you get some corroboration from the striking prominence of the bright lines in the combined spectrum?

Smith: Yes, just because the emission spectrum is so prominent, we would not expect the WR star to be as much as 1.3 mag fainter than its companion. But if the distance is indeed 160 pc, the luminosity of the system is approximately -4.2, which is fainter than an O7 star alone. I do not think this is likely.

Thomas: In your summary you suggested a value of 5.0×10^4°K for the photospheric temperature of WR stars. Stecher quoted a similar figure for γ Velorum as a whole. Hanbury Brown's figure is about half of this. Comment?

Hanbury Brown: There is no reddening correction in our determination, and I don't know how large it might be.

Underhill: If you assume that γ_2 Velorum is at Allen's distance of 160 pc - which is where Ganesh and Bappu place it - and that the two components are equally bright, you get an absolute magnitude of -5 for each of them. If you then use Hanbury Brown's figure of 3×10^4°K as an effective temperature, you get a radius in agreement with his results, which are consistent with the idea that a WR star is rather like an O-star.

Thomas: You mean in the continuum and in the atmospheric regions where the continuum is formed?

Underhill: Yes. As far as the continuum is concerned, a WC star appears rather like an O-star. Of course this statement could be challenged by Kuhi tomorrow.

Thomas: So the point at issue is whether the continuum of a WR star behaves roughly like a blackbody at 5×10^4°K, or like a blackbody at half that temperature, or at a greater temperature, or not like a blackbody at all.

Underhill: Certainly not much higher than 5×10^4°K.

Payne-Gaposchkin: Anne, I would like to return to a point that you brought up. You don't seem to think the evidence from the cepheids concerning differences in chemical composition is very convincing. How do you feel about the evidence presented by Preston on RR Lyrae stars?

Underhill: I haven't got the subject at my fingertips, so I would rather not comment.

Schild: Van der Bergh has been studying globular clusters both close to the galactic center and further out. He finds a tendency toward metal enrichment in the stars of those clusters which are close to the center. I can't comment further, as I only heard this reported at a Cal Tech colloquium.

Payne-Gaposchkin: W. W. Morgan's spectra are the best and most convincing evidence of this effect.

Underhill: The basis of all this work is narrow-band photometry. Correlations are sought between color indices and observed spectra, some of which show weak, some strong, lines. The color indices are Strömgren colors, which give relative brightness between say λ4400 and λ4200. Another color is used to select objects that are similar in the longer wavelength region and therefore have the same effective temperature. The blue color index is then correlated with line strength. They presume that because a line is stronger in one set of stars, there is an abundance difference. This is where I rise in wrath. True, in the standard theory the abundance is your only free parameter; I just don't believe the standard theory.

Schild: I'm out of my field here, but I seem to recall that Van der Bergh's work consisted of a complete analysis of HR diagrams for the globular clusters, and that correlations were observed between metallic abundances and the numbers of stars on two horizontal branches. Too bad Van der Bergh is not here to defend himself.

Underhill: His work is almost exactly the same as I described. He plots an HR diagram: b-v narrow-band-color index against apparent magnitude, which is absolute magnitude because in a cluster all the stars are at the same distance. He gets all the stars lying in a certain sequence, and using model atmospheres or the interpretation of the spectra of a few bright stars, he says one turn-off point corresponds to strong-line stars and another to weak-line stars. Having established the relationship for one or two stars in nearby clusters, he then applies it to fainter field stars. The technique is based on an empirical correlation based on a rough interpretation of spectra using rudimentary theory.

Payne-Gaposchkin: Preston's work is not based on clusters, but on bright RR Lyrae stars. How do you evaluate the work of Oke and his collaborators on weak- and strong-line RR Lyrae stars and cepheids, in which you get quite different relations between T_{eff} and B-V for the different stars?

Underhill: Well, down to spectral type A5, we have photometric spectral types and we have absorption-line spectral types. For the photometric types, we have a relation between the slope of the continuum and T_{eff} established by model atmosphere calculations. If you go cooler than A5, log g becomes the dominant factor in determining the slope of the continuum. I think Preston is a good observer and his interpretation gives me confidence. But although continuum spectral types do seem very consistent, there are still people like me who are old-fashioned enough to prefer absorption-line spectral types.

Payne-Gaposchkin: And of course Preston uses the difference between the two kinds of spectral types as a measure of the metal abundance in RR Lyrae stars. I must say that while I am not convinced about the cepheids, I do find Preston's work pretty convincing.

Underhill: Basically the reason we are all getting so excited about weak and strong helium-line B-stars is that the photometric spectral types and the absorption-line spectral types do not correlate in a unique manner. I hesitate to interpret this in terms of abundance differences.

Kuhi: Anne, would you believe abundance differences derived from photoelectric measurements, independently of model atmosphere calculations? These are based on conspicuous differences in the strengths of lines in stars of a given temperature, as defined by narrow-band indices free from absorption lines. I am referring to the work of Spinrad.

Underhill: What I'm saying is that all deductions from photoelectric indices based on an empirical spectroscopic interpretation of the continuum cannot be uniquely related to the absorption-line spectra. I agree that large differences in the line strengths are possible; the question is how to interpret them.

Thomas: Let me clarify this: First, Anne is questioning not the observations but the physical interpretation. It's not a matter of whether you believe photoelectric versus spectrophotometric results, but rather of whether you believe that a difference in line strength necessarily implies a difference in abundance. On the classical LTE theory, it does. I agree with Anne; it is not obvious this theory is applicable, so it is not obvious the interpretation in terms of abundance differences is valid. The second question is whether stars with the same continuum temperature are necessarily similar in atmospheric regions above where the continuum is formed. Your continuum temperature refers only to conditions at the place where the continuum is formed. To assume that these conditions fix uniquely the conditions in the region of line formation is a big assumption. If, for example, the continuum is formed in a region where conditions are controlled by the radiation temperature, and if the line spectrum is formed in a solar-type chromosphere-corona, then the continuum and the lines are formed under wholly different conditions and wholly different mechanisms. There is no obvious reason why there should be any correlation between them. Maybe there is, but this must be proven, not assumed. I think this is the basis of Anne's argument.

Underhill: Thanks Dick. I agree; you've put it very clearly.

Thomas: But I think we have drifted from the main point. Lindsey, you find that the different subclasses of Wolf-Rayet stars have different distributions both within the Galaxy and among extragalactic objects. You then argue along the lines used by Shapley many years ago when he found that the mean periods of cepheids in the central regions of galaxies differed from those in the outer regions. He said, "Maybe this is an indication of gravitational potential or something". The current approach is to interpret it in terms of abundance differences. But this is only one of many approaches. The important observational fact is not that the abundances vary from one part of a galaxy to another, but that some property varies and that this property in some way affects the subclasses of WR stars, just as it affects the mean periods of cepheids or of subclasses of cepheids. Ever since Shapley, people have been looking for this property.

You go on to note that theoretical calculations show that differences in initial chemical composition could account for the differences in the periods of cepheids. So you suggest that we might account for the differences in the WR subclasses in the same way (including, possibly, a difference in initial mass). You stick to this suggestion, because it's the only logical one you can see.

Anne, you are not worried about Lindsey's arguments as such; you are questioning the simple interpretation of differential line strengths in terms of abundance differences, and you are worried because Lindsey's arguments appear to support this interpretation. I share your worries, but if we accept that certain subclasses of WR stars and cepheids are concentrated toward the galactic center, we have either to accept differential abundances or to propose an alternative interpretation.

Smith: I agree with that summary. What has happened over the years is that when we have looked at stars in the halo and at stars in the disk, we have found correlations between line strengths, dynamical orbits, and photometric criteria. We have come to the conclusion that stars in the halo are intrinsically different from stars in the disk. By far the most likely and logical interpretation is that these differences reflect differences in initial chemical composition. It is my impression that this interpretation is generally accepted, i.e., that it is believed that Population II (halo) stars have lower metal abundances than Population I (disk) stars. Now we have also come to think of Population II as being old. In the center of the Galaxy we find stars which we assign to Population II because they are old; we have assumed that they are also underabundant in heavy elements. But the standard criteria by which we estimate metal abundances indicate that the metal abundance in Population II stars in the center of the Galaxy is at least as high as in the solar neighborhood, i.e., the criteria do not have the same validity in the galactic center as in the halo. So whatever it is we are measuring (and metal abundance is the most likely answer) increases from halo to core as well as from halo to solar neighborhood. Furthermore, it begins to look as though it increases from solar neighborhood to core. I emphasize that this is a comparative procedure and does not depend as much as Anne implies on an absolute interpretation via model atmospheres.

Payne-Gaposchkin: One should not define population in both ways. One must define it either by composition or by age. A star of one-tenth the solar mass can be 10^9 years old and still be an original population star.

Smith: I think this is the source of the confusion. We've become accustomed to assuming that old stars have low metal abundances. We must now realize that we have two independent parameters; stars can be old and can have high metal abundances.

Payne-Gaposchkin: It might be a good idea to scrap Populations I and II and attempt to get something a little more precise. Many people base their use of them on one criterion, implying the other, which is extremely dangerous.

Roman: One thing has been bothering me throughout this whole discussion. Anne's argument is based on B-stars and early A-stars and to some extent on RR Lyrae stars. The cepheid arguments, on the other hand, are all based on later type stars. Are we not trying to compare apples and oranges and worrying because they don't compare very well?

Underhill: Could we possibly turn now to the question of the evolutionary stage of Wolf-Rayet stars? Are they in the pre-main sequence contraction phase or the post-main sequence phase? Obviously this is tied up with questions of their binary character and mass exchange, but I would also like to consider the evolution of the single WR star.

Payne-Gaposchkin: Let me start this discussion by raising a question about close binaries. AO Cass, CC Cass, and 29 Can Maj are all early type eclipsing binaries. They all have periods of 3 or 4 days and masses of around 10 $M_{\odot}$. The first two spectra are O9; the third is O7f + O. Why has not one of these become a WR star? Is it going to become one, or has it been one in the past? What fraction of its life should - or might - such a star spend in the Wolf-Rayet phase? From this kind of statistics we ought to be able to verify the idea that Wolf-Rayet stars should occur at some point in the lifetime of early-type binaries.

Smith: For a 25 $M_{\odot}$ primary (Kippenhahn 1968), the fast part of the mass exchange (B to C in Figure 4) takes place in about 300 years; the slow part (C to D), in about 4000 years. For stars of lower mass, it takes a little longer, but not much. Computations have been made for primaries of three different masses: Paczynski started with a 16 $M_{\odot}$ star and ended up with about 6 $M_{\odot}$; Kippenhahn and Weigert (1967) began with 9 $M_{\odot}$ and ended with 2 $M_{\odot}$ Kippenhahn began with 25 $M_{\odot}$ and ended with 8.5 $M_{\odot}$.

Sahade: Paczynski and Kippenhahn started their work on mass exchange between components of close binaries in order to explain the Algol systems, which are binaries consisting of a main sequence A-type star of normal mass and a G- or K-type supergiant of abnormally low mass, sometimes as low as 0.2 $M_\odot$. This is a surprising combination because, assuming the two components of the binary were formed at the same time, we would expect the heavier component to have evolved faster. We conclude, therefore, that the supergiant was initially the heavier component but that at some stage in its evolution it has lost mass to its companion. The computations explain very nicely how these systems can result from mass exchange between components of specified initial mass and separation. Here again the transition appears to be too fast to allow us any possibility of observing it; indeed of all the Algol systems investigated, only one shows any sign of being in the transition stage.

Mrs. Gaposchkin has asked about systems with O-type components: well, if you accept a certain interpretation of the peculiar distribution of radial velocities, the components of AO Cass (O9III + O9III) have approximate masses 18 $M_\odot$ and 16 $M_\odot$; the components of CC Cass (O9IV + O9IV) have masses 19 $M_\odot$ and 9 $M_\odot$. Could these be systems caught in the transition phase to WR phenomenon?

Smith: Yes, that sounds possible. I would like to say a little more about the difference between Algol-type systems and the systems we think produce WR stars. To obtain an Algol-type system, mass exchange must occur before the hydrogen in the core of the primary is depleted. In the Algol stage, which occurs near the end of mass exchange, the original primary is the less massive component, but it still fills its critical lobe, and it still has hydrogen core burning. This is Kippenhahn and Weigert's case A. Now to get a WR star, we need their case B, in which mass exchange does not begin until after the hydrogen in the core is depleted. We then get helium core burning at the completion of mass exchange.

Sahade: On the question of mass exchange, I would like to remark that it is not just a question of starting with two objects of different mass, computing what happens if mass exchange takes place, and being satisfied if one ends up with the desired combination of masses. As Anne Underhill remarked earlier, you must also explain the observed spectra. I would note further that in almost all the theoretical calculations, it has been assumed that the mass lost by one component is acquired by the other. That mass is in fact being lost to the system is shown by the presence of an expanding envelope as indicated by, for example, He I λ3888. Again, we must take into account the velocities of the particles which stream out of the WR star towards the companion. In comparison with those in Algol systems, the velocities involved in WR stars are quite large. In V444 Cyg, for example, they are of the order of 700 km/sec. The third question I should like to raise concerns the effect of radiation pressure from the O-type component on the streaming particles. HD47129, a system with an O8 component, provides evidence that radiation pressure does affect the motion of particles being lost from the companion star. Katherine Bracher was telling me that in one of the objects she investigated, the He I λ3888 absorption line, formed in the large expanding shell, is present during only part of the cycle. The effect of radiation pressure may, therefore, be very important.

Schild: On the subject of evolution, I would like to discuss some recent observations of the association Sco OBI. This association has received little attention to date, because it is quite far south and difficult to observe from the northern hemisphere. It is of special interest because it contains two WR stars and two Of stars with P Cygni-type lines. The work was undertaken jointly with Hiltner at Yerkes Observatory and Sanduleak at Warner and Swasey Observatory. The investigation is based on spectrograms at 88 Å/mm taken for classification purposes at McDonald Observatory and on photometry obtained at Cerro Tololo.

The association Sco OBI lies in the tail of the scorpion at 16^h47^m and -41°38'. It contains, near its southern end, the galactic cluster NGC 6231. The HR diagram of the association shows a turnup at spectral type O9 - O9.5, and some O6 - O8 stars are present. The method of spectroscopic parallaxes gives a distance of 2 kpc and a distance modulus of 11.5 mag. From the luminosities of the early B-supergiants, we infer that the age of the association must be about 5 million years. This agrees well with the age given by Kippenhahn and Smith for a model WR star that has evolved from a 25 $M_\odot$ star.

Figure 13 shows the HR diagram based on the new data. From the luminosity classes, which are shown for the supergiants, and from the absolute magnitudes, it is clear that this association is unusually rich in luminous stars. Whereas Underhill has recently noted that O-stars brighter than absolute visual magnitude -6.0 are very rare in the Galaxy, Sco OBI contains 5 stars of spectral type O6 - O8

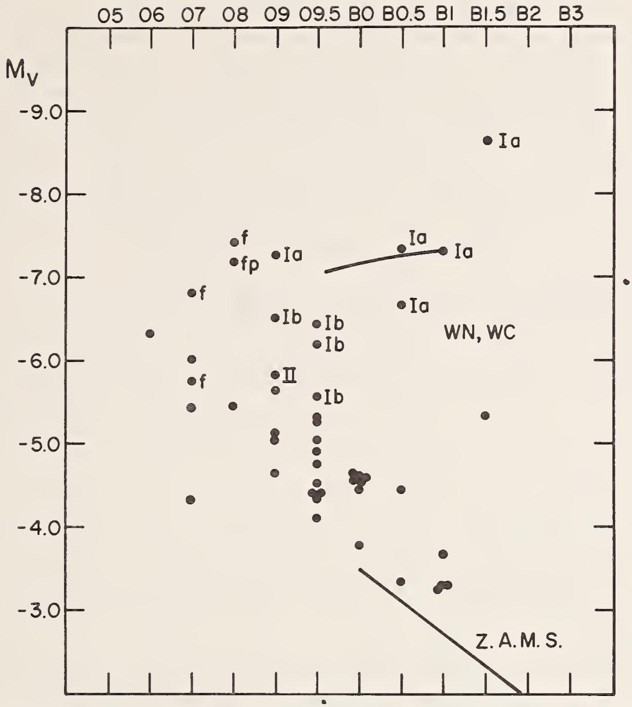

Figure 13. The HR diagram for the association Sco OBI shows a turn-up at spectral type O9 - O9.5, and some O6 - O8 stars are present. The two WR stars are located on the diagram.

brighter than -6.0. Two of these are brighter than -7, and their hydrogen lines show P Cygni profiles; one of them, HD152408, shows other spectral peculiarities as well.

The most luminous star in the association is ζ^1 Sco. Membership of this star has been investigated by Code and Houck. On the basis of the radial velocity of stellar lines as well as the intensity and radial velocity of interstellar lines, they concluded that it is indeed a member. The association contains three additional supergiants. The line drawn in the region of the B-supergiants shows where, according to the models of Stothers, stars of mass 30 $M_\odot$ burn helium. From this we infer that the B-supergiants in Sco OBI are of about 30 $M_\odot$. Since very massive stars evolve at almost constant bolometric luminosity, we infer that ζ^1 Sco is more than 30 $M_\odot$.

There is some indication that, like other young associations, Sco OBI has seen repeated star formation events. The HR diagram shows a fairly well-defined turnup at spectral types O9 - O9.5, and a number of hotter, presumably younger stars are present. In Figure 14, the HR diagram of Sco OBI is compared with that of the association Perseus OBI, which contains the clusters h and χ Persei. From the comparison it appears that we can recognize in Sco OBI a coeval group of stars, which has a turnup at O9 - O9.5 and which is associated with the early B-supergiants. There is also a group of bluer stars which may define a younger coeval group.

Now Blaauw has recently suggested that if a young association contains stars of different ages, the youngest stars will be found in the most compact regions. If then we assume from the HR diagram that the O6 and O7 stars are the youngest, we would expect to find them in the cluster NGC 6231, the most condensed part of the association. This does not, however, seem to be the case. Of the O6 and O7 stars, two are found in the cluster, and the rest are spread over the entire association. Moreover, it does not seem possible to distinguish the O6 - O8 stars on the basis of any independent criteria such as proper motion, radial velocity or spatial distribution.

It is also interesting to compare the locations of the He-burning supergiants. Observations of the

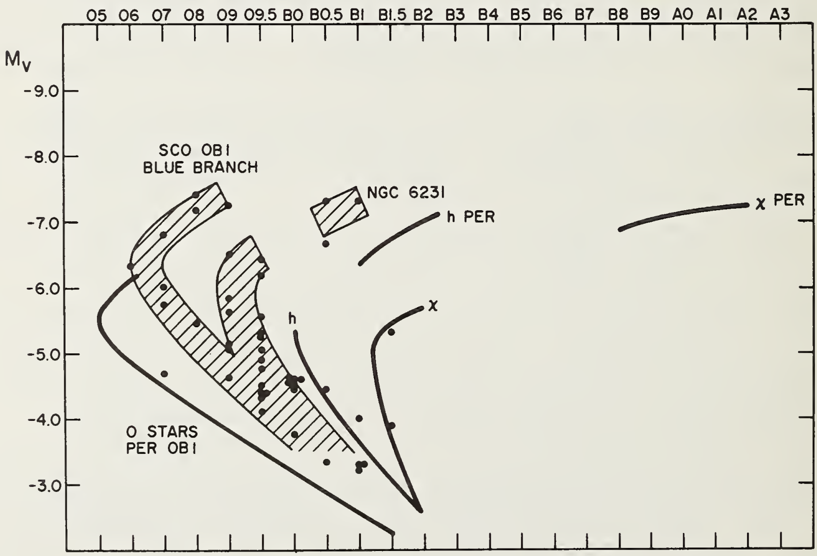

Figure 14. A comparison of the HR diagrams of the associations Sco OBI and Per OBI.

three clusters, χ Pers, h Pers and NGC 6231 (in order of increasing youth), seem to confirm the prediction of stellar models that the sequences of He-burning stars occur at higher effective temperatures and bolometric magnitudes the younger the cluster.

Payne-Gaposchkin: If I remember correctly, the Scorpio association is not associated with a group of red supergiants as are h and χ Persei.

Schild: Correct. This, too, could probably be predicted from stellar models. The stability properties of these very massive He- and C-burning stars are being investigated at Cal Tech. It appears that when stars become very much more massive than about 15 $M_{\odot}$, they cannot live long as red supergiants.

Payne-Gaposchkin: But are you sure the red supergiants in h and χ Persei aren't more massive than this?

Schild: I believe the evolving supergiants in h and χ Persei are certainly less massive than those in NGC 6321. I would also point out that the red supergiants in h and χ Persei are associated with the χ Persei rather than with the h Persei stellar population. This follows from their proper motions and from their distribution in the association; not only do they cluster around χ Persei, but two or three such stars are found in χ Persei itself, whereas none are found in h Persei.

Payne-Gaposchkin: Neither are there WR stars or Of stars in h and χ Persei.

Schild: There are several Of but no WR stars in the Perseus association.

Payne-Gaposchkin: Are they as pronounced as the Of stars near NGC 6231?

Underhill: No, they don't look anything like that. There are a couple in the Scorpius association with tremendous excitation, quite unlike any other Of star I have ever seen. I have spectra of several Of stars in the Perseus association, and they are quite normal. But this brings us back to our original problem: what is the difference in spectral quality between what we call a WR star, an Of star, an O- or a B-star?

Aller: The Of stars in Scorpius certainly don't look anything like WR stars. They have very sharp emission lines.

Underhill: There are only a few emission lines in these objects; not every line is in emission. The dominant emission lines at λ4485 and λ4503 are still unidentified. They are sharp. WR stars have almost the same type of spectrum, except that the lines are much broader and have different shapes. But as Sahade and I remarked earlier, the spectra of T Tauri stars also have characteristics in common with WR stars. I do not imply that T Tauri stars are WR stars; I am just pointing out that while WR stars have a few characteristics in common with O-stars, they have many in common with T Tauri stars. We are inclined to correlate WR stars with O- and Of stars, because they are often found together in associations; but is this getting us any closer to the physics of whatever produces the spectrum?

Schild: I should like to go back to the actual WR stars in this association: HD152270 is a WC7 + O binary in the cluster NGC 6231. HD151932 is of spectral type WN7-A, which means that it lies in the WN sequence containing many binaries. At low dispersion it appears spectroscopically very similar to CQ Cephei, a well-known binary. Struve did not consider it to be a binary, although his radial velocities do show a period of 3.3 days with small amplitude. I think he considered the scatter in the observations to be too large to be certain of its binary character.

For HD152270 we can infer a reddening correction to the UBV photometry from the reddening of nearby stars in the cluster NGC 6231. But because of the emission lines in the WR spectrum, an additional correction must be made if we wish to find the color of the WR continuum. Fortunately, line emission corrections for the U, B and V bands have been derived by Miss Pyper from Lick coudé spectrograms. Since line emission corrections are not available for HD152270, I substituted her values for stars of the same spectral type. The final colors I derived are:

$$B-V = -0.16; \quad U-B = -1.00; \quad M_V = -6.3$$

In exactly the same way, I have derived the following colors for HD151932. (In fact, for WN7 stars, Miss Pyper found that it was unnecessary to correct for emission lines.)

$$B-V = -0.20; \quad U-B = -0.98; \quad M_V = -6.3 \; .$$

In Figure 13, the two WR stars are located on the HR diagram according to their luminosities and B-V colors. To relate the UBV colors to spectral type, I assumed that the continua of these stars have the same intrinsic colors as the continua of early B-supergiants. I made no allowance for the binary character of the stars.

Underhill: I don't see much point in locating these systems on the HR diagram, because the HR diagram is for single stars, and some of these may be binaries.

Smith: Yes, but for stars whose spectra show no evidence of a companion, the contribution of the companion to the luminosity must be small.

Thomas: We're talking about too many things at once here: absolute magnitudes, colors, the effect on each of binary character and how to correct for it, and the difference between WR stars and "associated" objects with respect to all these characteristics. Could someone take these points in order and tell us where we stand on them?

Underhill: Schild has presented information about the Scorpius OBI association - and in particular about the cluster NGC 6231 - to demonstrate that he knows the distance and absolute magnitudes of some of the O-stars to which we keep comparing the WR stars. He used this data to point out characteristics of high luminosity and high excitation temperature. He then considered the two WR stars in the association and drew conclusions on their magnitude and color. The absolute magnitude of HD151932 is -6.3, and since the companion, if any, is very faint, the WN7 star must have an absolute magnitude of at least -6.0. The other pair, HD152270 (WC7 + O), is a very different situation. The absolute magnitude for the system is again -6.3. According to your viewpoint, you can subtract 1 mag or 0.7 mag or nothing to get an absolute magnitude for the WR star of between -6.3 and -5.3. I personally feel there is no justification for a correction of less than 0.7 mag, although I am not sure I would go as far as the 1.0 mag urged by Sergie.

Aller: It is generally accepted that what we observe here are composite spectra: an O-star plus a companion. What we don't know is how much of the continuum comes from the O-star and how much from the WR star. Just looking at them, you get an intuitive feeling that the WR star is drowned out, as Sergie mentioned, by the O-type companion. For example, I think that the components of γ_2 Velorum cannot differ greatly in brightness because, with the exception of the strong carbon lines, the WR lines are swamped by the continuum. There seems to be no general agreement on how to correct for the continuum; if it is a substantial component in the carbon stars, it may also be important in the nitrogen stars.

Underhill: There is one way you can try to separate the two contributions. You can take an O-star line in the UV, where there are not too many emission lines from the WR star, and subtract the two spectra. You say, for example, a single O7 star would give me a line of such and such a shape; the observed one is only half as deep; therefore I must raise the continuum by a factor of two. Jean MacDonald Petrie tried to do this for HD193793 using Victoria spectrograms. She showed that at longer wavelengths, the O-star was the brighter of the two.

Stecher: ζ Puppis and γ Velorum are only about 7° apart. Many people have suggested that they might be associated and could be jointly responsible for the illumination of the Gum nebula. ζ Puppis appears just a few tenths of a magnitude fainter than γ Velorum. The question is, what is the absolute magnitude of an O5 star? It would be interesting to know if these stars were at the same distance, and if so, why they have the same absolute magnitude.

Underhill: I think it's fair to say that nothing much brighter than -4.5 is known. The only Of stars for which you have definite evidence that they are near -6.5 are those mentioned by Schild in Sco OBI. The Of stars in IC 1805 all seem to be double-line spectroscopic binaries. The problem is their distance, but again they come out about -5 or -5.5. I'm not sure what this has to do with ζ Puppis. I've never seen its spectrum, so I don't know whether it is the same as an O5f star in IC 1805.

Roman: Two comments: First, the Cygnus association - which is not so useful because it probably does extend in depth - agrees very nicely with Schild's results for the Sco OBI association in that the Of stars are a magnitude or so brighter than the WR stars. Second, ζ Puppis is, on classification dispersion, a perfectly normal Of star; in fact it is one of the prototype Of stars, and it looks very much like the Of stars in Cygnus.

Stecher: I'd like to discuss reddening corrections and colors. The intrinsic color of a 3×10^4 °K main sequence star is about B-V = -0.32. Now I thought we were all agreed that WR stars are very hot. Hanbury Brown's work and our work all suggest that the temperature of a WR star is greater than 3×10^4 °K. So I am surprised at the low B-V colors found by Schild. Doesn't this suggest a need for greater reddening corrections?

Kuhi: The WR continuous energy distribution over a wide wavelength region is quite peculiar. If you try to fit a blackbody temperature to any spectral region, you find the temperature is a function of the region you choose: the longer the wavelength at which you fit the blackbody curve, the lower the temperature. So B-V measurements cannot be compared with ordinary O-stars.

Smith: My reddening corrections were determined by comparing narrow-band photometric colors with the intrinsic colors determined for WR stars in the Large Magellanic Cloud. I got a reddening correction of 1.6 mag for the WN7 star and 1.1 mag for the WC7 + O7 system. In B-V this would correspond to color excesses of about 0.5 and 0.4 mag respectively.

Schild: My color excess was 0.46 mag, so our values are in good agreement. But there is still a question regarding the intrinsic colors of these stars. Are we justified in comparing the B-V and U-B colors derived from the continua of the WR stars with those for Ia supergiants? This worries me, especially for HD151932. Figure 15 compares the continua of two early-type stars: CQ Cephei, which is a spectroscopic binary and has a spectrum similar to that of HD151932, and ε Orionis which is a B0-supergiant. The stars were unreddened to the same B-V color, using Kuhi's photometry. The two continua are identical over the observable spectral range λ3300 - λ11000.

Underhill: What does that prove? For a star as hot as ε Orionis or any of the O9 supergiants, the energy distribution from 1μ to 3000 Å is very insensitive to the model.

Schild: What model?

Underhill: The model atmosphere. Even for a blackbody, it takes an enormous change in temperature to make any significant difference in the slope of the continuum.

Schild: But Kuhi has shown observationally that the WR continuum is not at all like that of either a blackbody or a main-sequence star.

Underhill: Another objection: it has been suggested that CQ Cephei may be two stars in a common envelope. So all you've got is a body of gas with a rather high excitation temperature and a rather high density. What is the point of comparing it with ε Orionis? Furthermore, we have no idea how to interpret the spectra of early type supergiants or even late B-supergiants. ε Orionis is

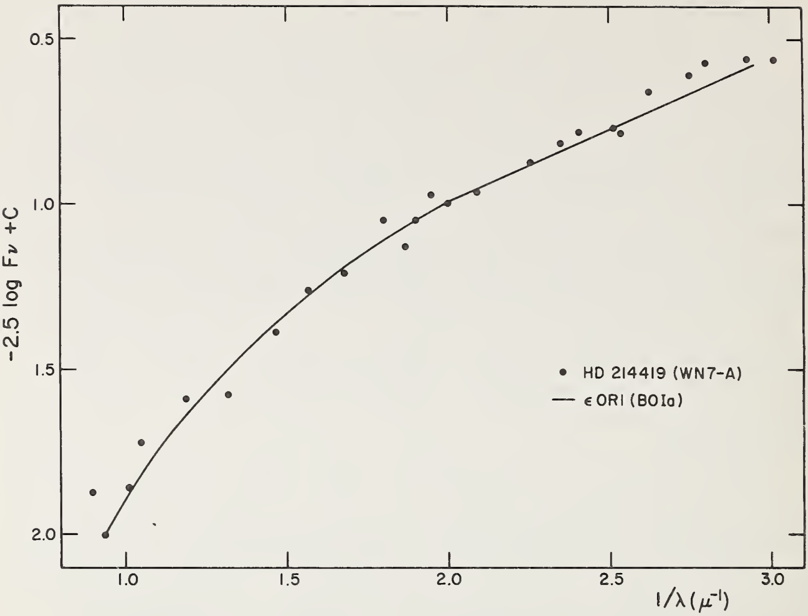

Figure 15. A comparison of the continua of CQ Cephei, HD214419 (WN7-A) and ε Orionis which is a B0 supergiant.

classified as a B0, but when you come down to details, it's nothing like a B0 main-sequence star.

Schild: I agree the continuum is nothing like that of a main-sequence star; that is just the point I wanted to stress. But I disagree that the spectrum of CQ Cephei is all that peculiar; at low dispersion it looks very much like the spectrum of HD151932.

Underhill: But the lines on which HD151932 is classified are sharp Of lines. They indicate a low density, very high excitation gas. There is little evidence that HD151932 is a binary; any companion must be so faint as to have practically no effect on what we see. I think that CQ Cephei, on the other hand, is an eclipsing variable, one of those "two nuclei with common envelope" efforts. You may get low density, high excitation gas streams around a binary system, in which case it will of course emit the same lines, but any such gas streams have very little to do with the stars inside.

Kuhi: There is another interesting point about Schild's slide. The model atmospheres with which I compared my WR observations were necessarily the ones available, namely those for O and B main-sequence stars. Schild, on the other hand, is comparing a WR star with a supergiant, and it is interesting that the continuous energy distributions fit so well.

Kuhi and Schild: Perhaps what we're saying is that the WR envelope is an example of an extended atmosphere. We are then in agreement with Anne that in both WR stars and supergiants, we are dealing with phenomena involving an extended atmosphere.

Thomas: Can you say specifically what you mean by "phenomena involving an extended atmosphere"? I would agree that we are likely to find excitation levels and density scale-heights much exceeding those which, in hydrostatic equilibrium, are associated with your continuum temperatures. Personally, I think the whole atmospheric phenomenon will be explained by large-scale heating and, possibly, a momentum supply. But I am concerned that when you people speak of phenomena involving an extended atmosphere, you are referring only to dilution effects. And while these may be present, they cannot by themselves begin to explain the observed phenomena, particularly if you adopt the classical approach and consider only radiative excitation. But perhaps this subject should be postponed until a later session.

Underhill: We should consider the interpretation of the UBV colors. Quite correctly we make the best possible corrections to get intrinsic B-V and U-V colors for the two WR stars in NGC 6231. Now I don't think anyone will argue strongly against those colors; the corrections for interstellar reddening are reasonable. Then comes the question: Do these intrinsic colors mean anything? Do they relate to spectral types and model atmospheres? It seems to me that the conclusion reached here - which I heartily support - is that they don't mean much. We've got them, but we'd better not interpret them the same way we do for O- and B-stars.

Thomas: I'm lost: why do we need to bring in model atmospheres at this point?

Underhill: Because the interpretation of intrinsic colors in terms of effective temperature is done by means of model atmospheres. I'm inferring that because Schild put his stars on an HR diagram as one would for B-stars, he was prepared to assign them effective temperatures. This is the normal procedure when interpreting an HR diagram.

Schild: That is indeed what I had in mind. May I thank Anne for being so explicit.

Thomas: Then I think we have arrived at the same point we reached in our discussion of "phenomena involving extended atmospheres", namely that point at which we postpone further discussion until such time as we can be more specific about what we are trying to do and the physical basis for doing it.

PART B

A SURVEY OF SPECTROSCOPIC FEATURES OF WOLF-RAYET STARS

CHAIRMAN: *Jorge Sahade*

Observatorio Astronomico, Universidad Nacional de La Plata

INTRODUCTORY SPEAKER: *Leonard V. Kuhi*

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I. INTRODUCTION

The general properties of Wolf-Rayet stars and their spectral classification schemes have already been discussed at length by Lindsey Smith in the first paper of this symposium. I would like therefore to discuss in some detail the spectroscopic features of these stars as determined from the observations. The outstanding characteristics of the Wolf-Rayet stars are the extremely strong broad emission lines of the ions of He, C, N and O and their dichotomy into two apparently separate sequences: one showing predominantly lines of He, C and O; the other, lines of He and N. It is with the nature of these emission lines and the occasional absorption components accompanying them that I will concern myself. In particular their identification, variability, profiles, intensities, possible correlations, and peculiarities are of great interest in providing clues to the understanding of Wolf-Rayet atmospheres. In addition the peculiarities of the continuous energy distribution will be described along with the apparent effects of binary nature on both the emission lines and continuum. Finally a brief description of other objects showing some Wolf-Rayet characteristics will be given. Most of the discussion will be from the observational point of view, but I cannot promise to restrain myself on the theoretical implications whenever I feel these to be of great importance.

II. DETAILED SPECTROSCOPIC FEATURES OF WOLF-RAYET STARS

a. Identification of Spectral Lines

The stronger features of the emission-line spectra have long been adequately identified. Wavelength lists in the normal photographic region were given originally by Beals (1930) and more recently by a variety of authors: Swings (1942), Aller (1943), Smith (1955), and Underhill (1959, 1962, 1968a). The extension of the observations into the near-infrared by Swings and Jose (1950) revealed a wealth of new lines demanding identification, especially in the carbon (WC) sequence. Edlén (1956) used new laboratory data for C II and C III, together with predicted wavelengths from hydrogenic transitions expected from C IV, to successfully complete the identification of nearly all the lines of any consequence showing up in Wolf-Rayet spectra. He also pointed out the remarkable extent to which the spectrum of carbon (especially C IV) is developed in the WC stars. The nitrogen spectrum is rather bleak in comparison. Additional work in the photographic infrared (i.e., out to $\sim\lambda8600$) was reported in a series of papers by Andrillat (1952, 1953, 1957, 1958, 1962) along with that in Smith's thesis (1955). Miller (1954) made use of very low dispersion objective-prism spectra to reach the 1μ region and succeeded in finding the extremely strong line of C III $\lambda9710$ in the WC stars. Code and Bless (1964) obtained a low dispersion spectrum of γ_2 Vel (WC7 + O7) and identified most of the emission lines as being due to C II, C III, He I and He II. With the advent of photoelectric spectrum scanners with photomultipliers capable of observing out to 1μ with no great difficulty, the infrared spectra of five Wolf-Rayet stars were obtained by Kuhi (1966a). The wavelength region from $\lambda8000$ to $\lambda11000$ was scanned with a 10 Å exit slit, and typical results are shown in Figures 1 and 2. The outstanding points are the richness of the spectra of C ions in the WC stars as compared to the absence of N features in the WN stars. In contrast, the WN stars show only very strong lines of He II (especially $\lambda10124$) and He I $\lambda10830$. In addition, following Edlén's work on C IV and using the laboratory data of Glad (1953) and Bockasten (1955), Kuhi also succeeded in identifying most of the weaker lines in this spectral region. In the WN stars one line at $\lambda10430$ remained unidentified, and N III was tentatively suggested as an identification pending further laboratory work.

The results of these investigations can be summarized as follows. Firstly, the spectra of Wolf-Rayet stars represent a very high degree of excitation and ionization. Lines of He I and He II are present in both WN and WC stars, although He II seems to be relatively stronger in WN's than in WC's. In addition, lines of H I are usually overshadowed by those of He II (except in a very few stars) and apparently contribute little to the spectrum. For example the alternate members of the Pickering series of He II blend with lines of the Balmer series of H I. Yet in most stars (e.g.,

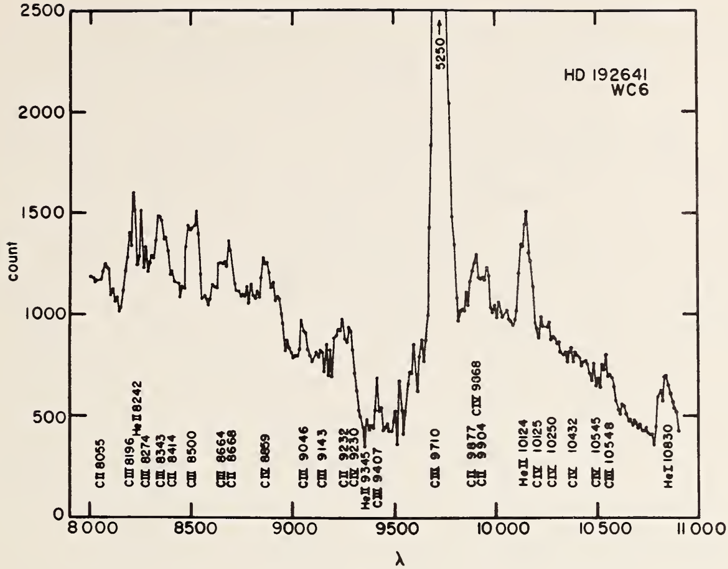

Figure 1. The infrared spectrum of HD192641 obtained with a photoelectric spectrum scanner using a 10 Å exit slit.

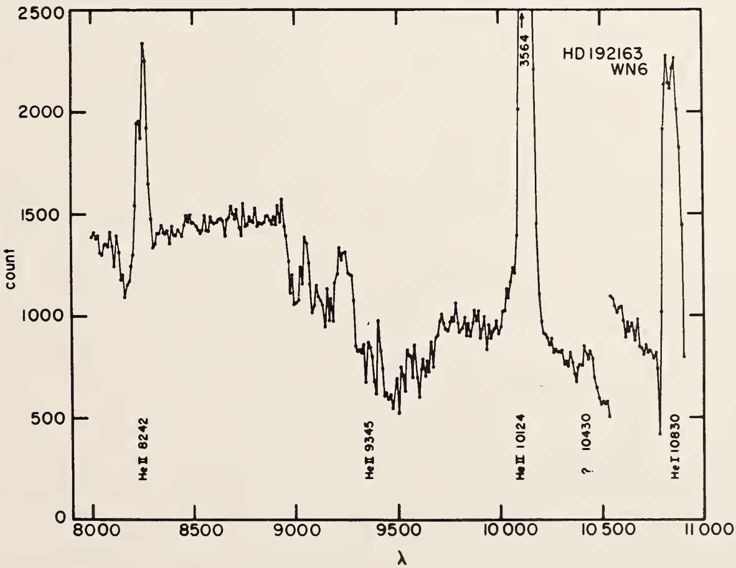

Figure 2. The infrared spectrum of HD192163 obtained as in Figure 1. The break at λ10530 is due to a change in integration time.

HD191765, Underhill 1968a) the Pickering decrement is quite smooth so that any hydrogen contribution must be negligible. Secondly, the spectra of WC stars are dominated by the lines of C II, C III and C IV along with weaker lines of O II, O III, and O IV. The strongest features in a typical WC7 star are C III λλ4650, 5696, 9710, C IV λλ4650, 5808, 7726 and He II λ4686. No strong lines (or even lines of moderate strength) of any ion of nitrogen appear in WC stars. The spectra of the WN stars are not quite so spectacular, having only moderately strong lines of N II, N III, N IV and N V. The strongest features are usually He II λλ3203, 4686, 8242, 10124 (λ9345 is located in a region of very heavy atmospheric water-vapor absorption, and even though clearly present its strength is difficult to measure precisely), N III λλ4100, 4640 and N IV λ7112. Again in general there are no strong lines of ions of carbon or oxygen present in the WN stars, with the sole exception of C IV λ5808 (see below). Thus the two classification sequences (WN and WC) are dramatically borne out by the entire spectral region from λ3100 to λ11000. A few southern stars (classified as Wolf-Rayet stars) which seem to violate this general rule will be discussed later.

We may now consider some finer points in more detail and try specifically to answer the question which has been consistently (and persistently) raised by Underhill (1968b). Is there any concrete evidence for the presence of N in WC's and C in WN's? Let us look first at the line at 5808 Å which occurs with moderate strength in WN's and is one of the stronger lines in WC's. This line has been variously identified as being due to C IV and to N IV. Both Swings (1942) and Aller (1943) identified the line as a blend of C IV λλ5801, 5812. Because Hiltner and Schild (1966) have ascribed it to N IV it is perhaps worthwhile to reproduce Swings' original arguments and settle this issue once and for all. The predicted C IV lines arise from the transitions 3s ^2S - 3p $^2P°$ with wavelengths 5801.3 and 5812.0 Å and intensities 10 and 8 respectively, with the excitation potential of the upper level being 39.5 eV. The blended wavelength would be ∿ 5806 Å, in good agreement with that observed. However the predicted N IV lines involve the transitions 3p' ^3P - 3d' $^3P°$ (with an excitation potential of 63.1 eV), giving rise to wavelengths 5812, 5828, 5846 Å with intensities 5, 5, 15 respectively (according to Swings). The expected blend occurs at 5830 Å which does <u>not</u> agree with the observed wavelength nor can one expect such a high excitation line to appear with much intensity. In addition, recent laboratory work by Hallin (1966) indicates that this predicted multiplet of N IV would be far too weak to have any noticeable effect on the spectrum. Therefore we can only conclude that this line is due to C IV λ5806 and hence that the C IV ion does appear in WN stars (see also Underhill 1968a).

However when we consider the evidence for other ions of carbon or oxygen in WN's and of nitrogen in WC's, the situation is no longer so unambiguous because we are forced to deal with very weak lines or blends with much stronger lines. For example in her paper on HD192103 and HD192163 Underhill (1959) states that N III is definitely present in the WC7 star. However the evidence comes entirely from such statements as "rather too strong for O III, C II, C III, and C IV". There is no clearcut case of an N III line free from blends. She also states that in the WN6 star, O III is definitely present and O IV is probably there. Here the evidence is somewhat more convincing, namely weak lines at λ5506 and λ4797, but it is still not conclusive. Andrillat (1952) has also managed to identify N II in WC stars and C I through IV in WN's. As Edlén points out, the strongest lines of N II in the infrared are λλ7762 and 8439. These lines have not been observed in normal Wolf-Rayet stars, neither in WN's nor in WC's. Furthermore the identification of C III in WN's is extremely tenuous. The far-infrared work has clearly shown that the dominating feature in WC's is C III λ9710 (often with a central intensity greater than ten times the adjacent continuum) and that there is no trace or hint of such a line in the WN's. Therefore we can say definitely that no C III occurs in WN stars. Underhill (1968a) concurs with this and in fact leaves a weak feature at λ5700 in HD191765 unidentified because of the absence of λ9710 in the infrared.

We are then fairly safe in concluding that for the "normal" Wolf-Rayet stars the dichotomy is complete. (The somewhat "peculiar" Wolf-Rayet objects will be discussed later.) These results imply a strong argument in favor of abundance differences being responsible for the two vastly different spectral sequences. Underhill (1957) has, however, suggested that the differences can be interpreted in terms of differing excitation conditions, brought about by a completely different far-ultraviolet energy distribution for the WN's and WC's. One point is still extremely difficult to explain with this picture: How is it possible to construct an atmosphere which is able to distinguish between the 47.24 eV ionization potential of N III and the 47.67 eV of C III so completely that no lines of N III are seen in WC's and no C III in WN's? It is inconceivable to me that no trace of C III λ9710 would be found in WN's if the separation into two sequences is really due to a difference in excitation conditions, since the differences between C III and N III are so minute. The only plausible explanation so far offered is a bona fide abundance difference of carbon and nitrogen. An atmosphere designed to distinguish 47.24 eV from 47.67 eV seems very artificial.

The wavelength identification problem seems now to be basically solved. However a number of weak lines still remain to be identified: examples are λλ7426, 6503, 5700, and weaker lines in the ultraviolet which occur in WC stars, and λλ10430, 5200 in WN's. Presumably further laboratory work on the spectra of multiply ionized ions of common elements will solve this minor problem in the near future.

b. The Continuous Energy Distribution

I would like to discuss here only the recent photoelectric measurements of the continuous energy distribution and to refer you to the literature (cf., the work by Andrillat and Vorontsov-Velyaminov cited by Kuhi 1966a) for earlier photographic results. The outstanding feature of the photographic work was the extremely low values of color temperatures obtained for the Wolf-Rayet stars, i.e., $T_C \sim 7\times10^3$ to 1.6×10^4°K. These temperatures seemed to be incompatible with the high excitation and ionization temperatures indicated by the emission lines. In order to clarify the situation I undertook a study of the brighter Wolf-Rayet stars accessible to Mt. Wilson, using the photoelectric spectrum scanner on the 60- and 100-inch telescopes. The work has been described in detail (Kuhi 1966a) in the literature so that we need only outline it here. Exit slits of 40-50 Å were used in the blue and 50-70 Å in the infrared. Great care was taken in choosing the continuum wavelengths to avoid the emission lines, but even so it was found necessary to make some corrections for their presence in the WC stars. Reddening corrections were made by means of a narrow-band three-color system; i.e., the early type stars lay on a vertical line on a color-color plot and the Wolf-Rayet stars on two essentially parallel reddening lines, the WC's being cooler than the WN's. It was assumed then that the Wolf-Rayet stars could be unreddened to the same point as the location of the early type stars in the two-color diagram. The results can be summarized as follows:

1. The energy distributions of Wolf-Rayet stars do not resemble those of ordinary O- and B-stars.
2. There is a slight ultraviolet excess for the WN's.
3. There is a strong infrared excess for both groups, but especially for the WN's.
4. The WC's seem to be somewhat cooler than the WN's.

The net result of this peculiar energy distribution is that the color temperature assigned to the star depends on the wavelength at which it is measured: The longer the wavelength the lower the color temperature. Typical values for a WN star range from $>10^5$°K at λ3500 to 1.1×10^4°K at λ9500, and for a WC star, from 6.5×10^4°K at λ3500 to 1.5×10^4°K at λ9500. Consequently it proved impossible to fit any theoretical energy distributions to the observations. The stars are clearly quite peculiar. This peculiarity is also evident when one looks at the energy distributions for binary stars in which the OB-type comparison contributes a large fraction of the energy observed. For example HD193514 is dominated by the O-star and hence the continuous energy distribution looks like that of an O-star, whereas in HD214419 both members are equally bright and the resultant distribution looks much more like that of a single Wolf-Rayet star, i.e., quite steep in the ultraviolet and quite a bit flatter in the infrared than for normal O-stars.

Since the 1966 work was published, Hayes (1967) has recalibrated the absolute energy distribution of Vega and other standards. He finds a somewhat larger Balmer discontinuity and a steeper slope in the infrared than was shown by the Oke (1964) calibration on which the Wolf-Rayet continuous energy distributions were based. Since this new calibration has led to much more consistent results for A- and B-stars, I have applied the correction factors to the 1966 data and present the results in Tables 1 and 2. These tables give the unreddened fluxes in mag per unit frequency interval. A few typical curves for single and binary stars are plotted in Figure 3 along with an O9V and a B0Ia star. The new calibration has two effects on the earlier re-

TABLE 1

UNREDDENED FLUXES: WN STARS

1/λ	HD4004 WN5	9974 WN3	50896 WN5	186943 WN4 +B	187282 WN4	190918 WN4.5 +O9.5Ia
3.012	+0.18	+0.46	+0.47	+0.52	+0.18	+0.84
2.932	0.24	0.49	0.46	0.59	0.24	0.55
2.800	0.35	0.56	0.57	0.62	0.41	0.58
2.750	0.38	0.65	0.62	0.67	0.44	0.67
2.632	0.40	0.58	0.56	0.58	0.45	0.57
2.545	0.49	0.70	0.61	0.63	0.53	0.60
2.522	0.50	0.72	0.59	0.65	0.53	1.64
2.410	0.66	0.75	0.69	0.73	0.66	1.65
2.350	0.75	0.84	0.75	0.76	0.72	1.68
2.259	0.82	0.84	0.80	0.83	0.78	1.80
2.089	0.91	1.01	0.92	0.94	0.85	0.94
2.000	1.00	1.00	1.00	1.00	1.00	1.00
1.949	1.01	1.12	0.96	1.02	1.07	1.05
1.900	1.11	1.06	1.05	1.07	1.15	1.04
1.866	1.35	1.16	1.11	1.17	1.16	1.16
1.800	+1.36	1.23	1.11	1.18	1.17	1.17
1.681	--	1.32	1.22	1.31	1.32	1.32
1.570	--	1.44	1.30	1.42	1.47	1.40
1.471	--	1.57	1.37	1.53	1.54	1.62
1.322	--	1.64	1.51	1.52	1.87	1.76
1.190	--	1.87	1.62	1.83	1.99	1.93
1.049	--	2.06	1.76	1.98	2.19	2.14
1.012	--	2.13	1.78	2.17	2.25	2.19
0.934	--	+2.09	1.85	+2.11	+2.38	2.33
0.900	--	--	+1.90	--	--	+2.44

Note: The unreddened flux is given as $-2.5 \log F_\nu$ + const. and is normalized to 1.00 at λ5000.

TABLE 1 (cont.)

UNREDDENED FLUXES: WN STARS

191765 WN6	191765 WN6	193077 WN5 (+OB)	193576 WN5 +O6	193928 WN5 +OB	211853 WN6 +B0I	214419 WN7 +O7	219460 WN4.5 +B0
+0.38	+0.41	+0.45	+0.50	+0.48	+0.43	+0.43	+0.60
0.38	0.50	0.59	0.56	0.54	0.63	0.44	0.65
0.52	0.58	0.60	0.70	0.71	0.62	0.46	0.79
0.51	0.60	0.66	0.69	0.61	0.65	0.51	0.73
0.52	0.54	0.66	0.63	0.64	0.63	0.52	0.70
0.60	0.67	0.61	0.61	0.68	0.66	0.58	0.71
0.63	0.60	0.60	0.63	0.70	0.72	0.61	0.71
0.76	0.73	0.67	0.70	0.66	0.71	0.65	0.74
0.72	0.87	0.69	0.71	0.75	0.74	0.68	0.78
0.88	0.91	0.81	0.79	0.76	0.82	0.78	0.83
1.00	1.06	0.92	0.95	0.94	0.87	0.93	0.94
1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
1.04	1.03	1.05	1.07	1.02	1.06	0.99	0.98
1.11	1.11	1.02	1.09	1.06	1.08	1.10	1.11
1.21	1.28	1.11	1.17	1.15	1.15	1.19	1.16
1.27	1.30	1.19	1.26	1.17	1.18	1.13	1.21
1.37	1.46	1.33	1.35	1.28	1.30	1.35	1.37
1.43	1.58	1.45	1.45	1.38	1.40	1.44	1.45
1.59	1.69	1.61	1.51	1.48	1.56	1.61	1.58
1.74	1.88	1.78	1.80	1.64	1.69	1.86	1.78
1.92	2.04	1.94	1.94	1.79	1.84	1.93	1.93
2.07	2.17	2.24	2.08	2.19	2.20	2.11	2.17
2.11	2.35	2.26	2.25	2.13	2.18	2.26	2.20
2.13	2.45	2.35	2.43	2.32	2.32	2.43	2.37
+2.26	+2.49	+2.38	+2.54	+2.29	+2.38	+2.31	+2.30

sults: (1) Comparisons made on a relative basis are not changed, i.e., the observed energy distribution of a single Wolf-Rayet star is still peculiar when compared to the observed distribution of a normal O-star. The ultraviolet and infrared excesses clearly remain. (2) Calculations and comparisons made on an absolute basis are quite different, i.e., the estimates of color temperatures will all be modified; being slightly lower in the ultraviolet and slightly higher in the infrared. The overall effect of the dependence of color temperature on wavelength is still present. The disagreement between the observations and the predicted energy distribution for $T_{eff} \sim 5\times10^4$°K and log g = 4.0 is now somewhat smaller in the ultraviolet and photographic but is

TABLE 2

UNREDDENED FLUXES: WC STARS

1/λ	HD16523 WC6	17638 WC6	165763 WC5	168206 WC8 +B0	192103 WC8	192641 WC7 +Be	193793 WC7 +O5
3.012	+0.25	+0.35	+0.47	+0.33	+0.45	0.48	0.52
2.850	0.55	0.66	0.60	0.40	0.73	0.65	0.83
2.734	0.51	0.51	0.53	0.51	0.64	0.62	0.67
2.621	0.54	0.62	0.60	0.53	0.54	0.54	0.57
2.484	0.62	0.65	0.70	0.62	0.57	0.62	0.64
2.342	0.67	0.66	0.67	0.69	0.68	0.74	0.75
3.025	0.97	1.02	0.96	0.97	1.00	0.90	0.92
1.980	1.00	1.00	1.00	1.00	1.00	1.00	1.00
1.925	1.05	1.10	1.02	0.99	1.04	0.97	0.94
1.866	1.01	1.12	1.04	1.10	1.08	1.09	0.99
1.808	1.20	1.27	1.13	1.15	1.24	1.19	1.13
1.669	1.30	1.39	1.27	1.32	1.28	1.27	1.27
1.385	1.62	1.60	1.48	1.56	1.59	1.54	1.47
1.250	1.76	1.70	1.56	1.57	1.73	1.66	1.60
1.098	1.86	1.95	1.78	1.92	1.93	1.91	1.78
1.056	2.01	2.02	1.79	1.93	2.00	1.94	1.84
0.950	+2.36	2.27	1.95	2.12	2.16	2.16	2.04
0.898	--	+2.26	+2.07	+2.14	+2.36	+2.08	+2.13

still quite large at 1μ (∿30%). Thus one can safely conclude that the Wolf-Rayet stars are peculiar in their continuous energy distributions when compared to normal OB main sequence stars. However as shown in Figure 3, the differences between Wolf-Rayet stars and OB supergiants may not be so great. This point is in need of further investigation.

Finally we note that the depth of secondary eclipse in V444 Cygni (O-star in front) in the continuum is an increasing function of wavelength ranging from ∿ 0.14 mag at λ3300 to ∿ 0.19 mag at λ11000. Kuhi (1968) has shown that this result is most readily accounted for by assuming that the Wolf-Rayet star is intrinsically brighter in the infrared than the O-star. He is able to reproduce reasonably well the dependence of eclipse depth on wavelength by using the observed energy distribution of a single WN5 star to compute the brightness

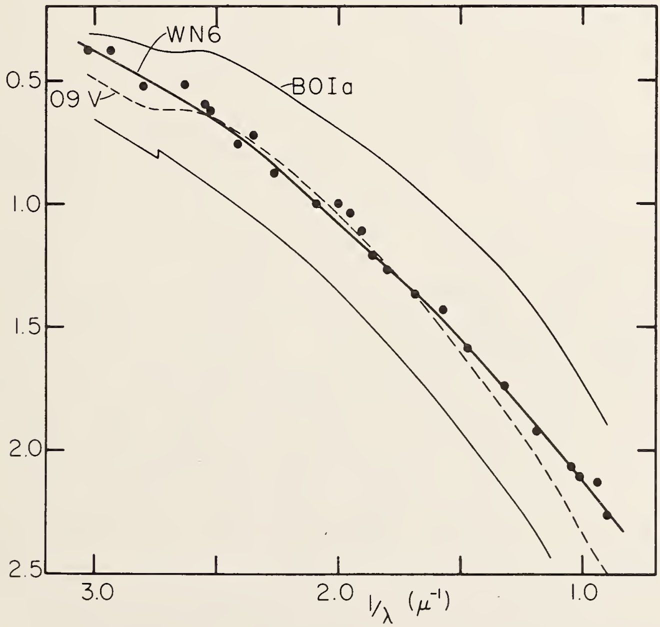

Figure 3. The continuous energy distribution of a WN6 star compared with that of the O9V star 10 Lacertae (thin dashed line), the B0Ia star, ε Orionis (thin solid line), and an unblanketed model atmosphere for $T_{eff} = 5\times10^4$ °K (unlabelled thin solid line). The units are mag per unit frequency interval on the ordinate and inverse microns on the abscissa.

ratio as a function of wavelength. Therefore we are able to conclude that the infrared excess is an intrinisic property of Wolf-Rayet stars.

c. Line Profiles

The emission-line profiles for both WC and WN stars fall into two groups, those that have violet-displaced absorption components and those that do not. The lines of the first group seem to show some tendency toward the classical flat-topped profiles expected from a spherically symmetric envelope expanding with constant velocity (see Beals 1930), but in general the lines of both groups show rather steep sides with rounded tops. The determination of these emission-line profiles by ordinary photographic means is extremely difficult for the stronger lines. The intensity ranges encountered (e.g., central intensities 10 or more times greater than the adjacent continuum) greatly surpass the workable range of photographic emulsions, and hence most of the photographically determined profiles cannot be very accurate. No such problem presents itself for the lines of moderate strength, and photographic profiles should be quite reliable.

For the stronger lines photoelectric spectrum scanning provides a convenient and reliable means of obtaining profiles. Unfortunately very little work has yet been done in this area: my own results for a few lines seem to be all that is available with sufficient resolution to be at all comparable to the photographic profiles. Figures 4-6 illustrate photoelectric emission profiles for the λ4600-λ4700 region for a number of Wolf-Rayet stars. They were obtained with a 2 Å exit slit with the Oke scanner used on the 100-inch reflector at Mt. Wilson. The extreme range in intensity is clearly evident: the peak intensity of C III λ4650 in HD165763 is $\sim$ 20 times the continuum intensity; the total width at half-intensity is $\sim$ 55 Å. The spectral types on the figures are on the Beals (1930) system. It would be more appropriate to use the newer system of Hiltner and Schild (1966) or L. Smith (1966): The three WC stars would then be HD165763 (WC6), HD192641 (WC7), and HD192103 (WC8 pec) on the former system, and WC5, WC7 + Be, and WC8 (+ OB) on the latter. In either case a clear trend in increasing width of the λ4650 feature with earlier type is apparent from Figure 4. There is also evidence for a displaced absorption component in

HD192103 even at this rather low resolution (2 Å). Figure 5 shows three WN stars HD193077 (WN5 + OB), HD192163 (WN6) and HD191765 (WN6) (L. Smith's spectral types). The important things to note are the changes in the relative strength of N V λ4605-22 and N III λ4640 between types WN5 and WN6 and the lack of any apparent correlation between the line width and spectral type. Figure 6 shows three more stars, HD193793 (WC7p + O5), HD190918 (WN4.5 + O9.5 Ia) and HD228766 (WN7 + O) (L. Smith's spectral

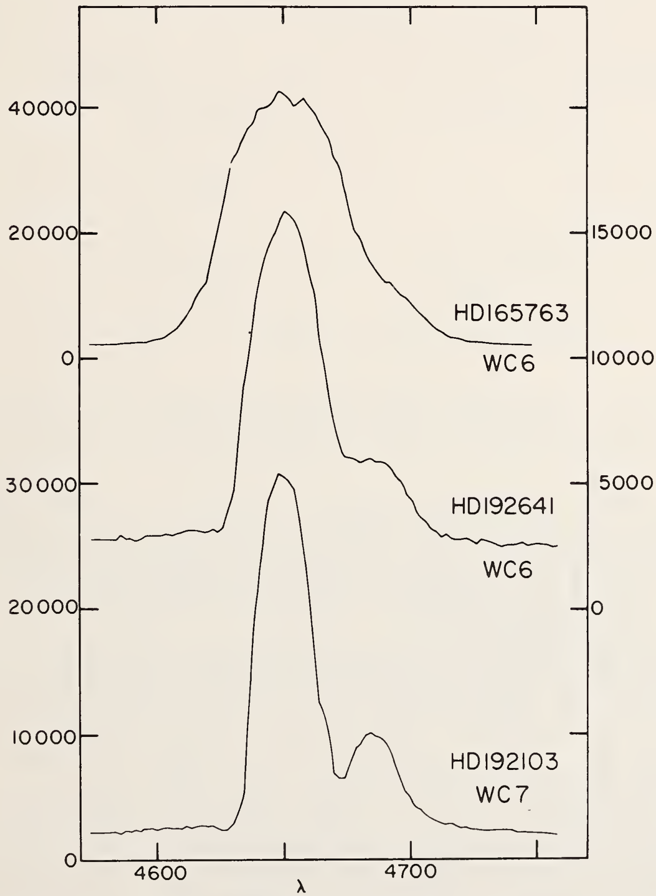

Figure 4. Photoelectric line profiles of λ4650 complex for WC_o stars obtained with a spectrum scanner using a 2 Å exit slit. The ordinate gives the total number of counts (proportional to intensity); the scales on the left refer to HD165763 and HD192103 and that on the right refers to HD192641.

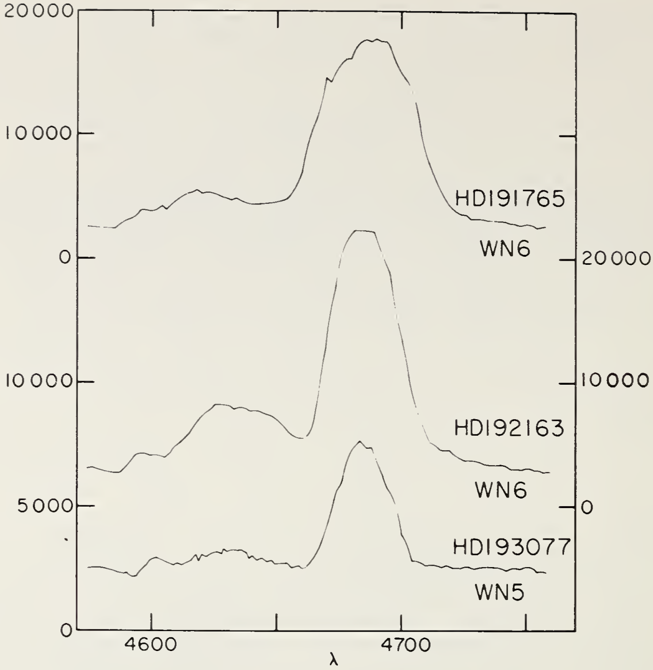

Figure 5. Photoelectric line profiles of λ4650 complex for WN stars. Other data same as Figure 4.

types). Again evidence for an absorption component is visible in HD193793, and the profile itself seems almost flat-topped. For the other two stars there is clearly no advantage in using photoelectric methods since considerable detail is lost because of the low resolution.

Finally we present photoelectric profiles for the He I λ10830 region obtained with a 4 Å exit slit using the prime-focus scanner on the 200-inch reflector at Palomar. The stars scanned were HD192103 (WC8 + OB), HD192641 (WC7 + Be), HD192163 (WN6) and HD193077 (WN5 + OB); the results are shown in Figures 7 and 8. All the lines show the strong violet-displaced absorption core similar to that seen in He I λ3889. But in addition we note that the four stars have four completely different profiles: HD192163 has the classical flat-topped profile, HD192103 has a profile similar to non-metastable lines, and the other two show something in between. There also seems to be some central fine structure which is greater than the expected fluctuations from the counting statistics. In this

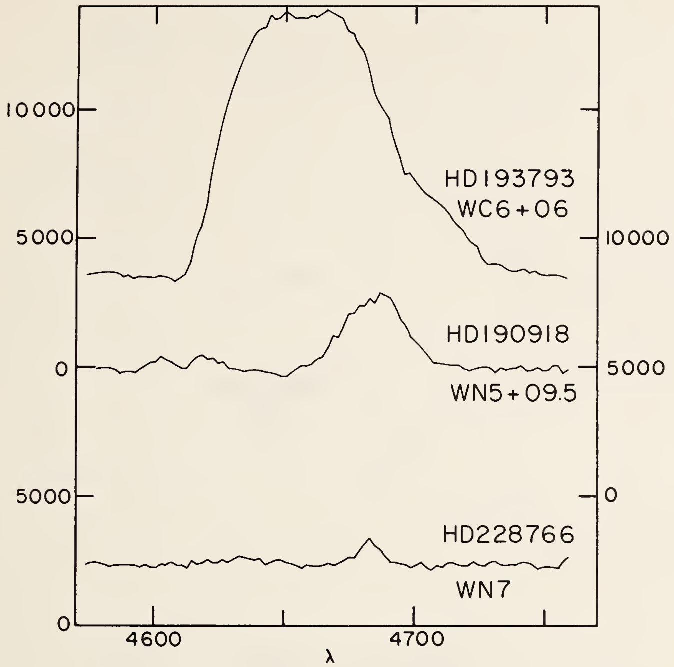

Figure 6. Photoelectric line profiles of λ4650 complex for other WR stars. Other data same as Figure 4.

region of the spectrum the advantages of the scanner technique are obvious, and it is to be hoped that more accurate profiles of the strong infrared lines will soon become available. Good examples of photographic profiles have been given by Underhill (1968a, 1959) and are illustrated in the figures accompanying her presentation at this symposium. References to earlier work are given in Underhill's review paper (1968b).

One can summarize the photographic and photoelectric data by saying that the typical emission-line profile is quite symmetrical and is of the form $\exp(-\Delta\lambda^2/\Delta\lambda_o^2)$, where $\Delta\lambda_o$ is a suitable constant for the line and is related to the velocity of the atoms. In general for lines of the same ion, the width at half-intensity is proportional to the wavelength, e.g., He II λλ3202, 4686 and 6560 in HD191765 are 25, 41 and 55 Å wide respectively. This implies that $\Delta\lambda/\lambda$ is approximately constant and hence that the chief broadening mechanism is simply Doppler broadening due to the motions of the

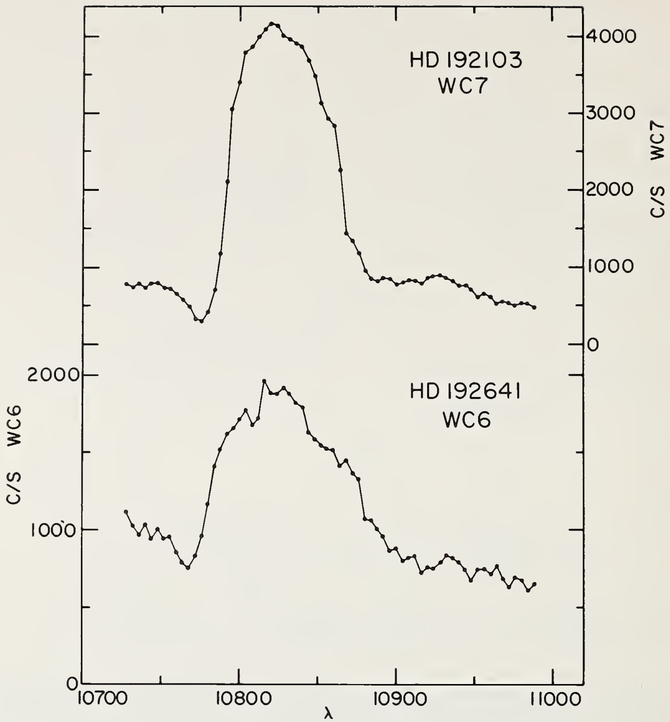

Figure 7. Photoelectric line profiles of λ10830 for WC stars obtained with a spectrum scanner using a 4 Å exit slit. The ordinate gives the number of counts per second.

atoms. The corresponding velocities for these three lines are 2340, 2620 and 2590 km/sec respectively, which indicates that the correlation is not one to one and hence that some other broadening agent is also operating. Beals pointed this out in 1929 when he noted that the width increases with wavelength at a rate slightly greater than predicted by pure Doppler broadening. In addition the wings of lines such as He II λ4686 and λ3203 in HD192103 and HD192163 (Underhill 1959) are somewhat greater than those of a Gaussian profile, again suggesting the presence of an additional broadening agent. Stark broadening has been proposed (Johnson 1954) but is clearly ruled out for several reasons: (1) Lines of different series of He II would have different shapes and widths, since the Stark effect would be greatest on those lines having a larger value of n for the lower state. However no such

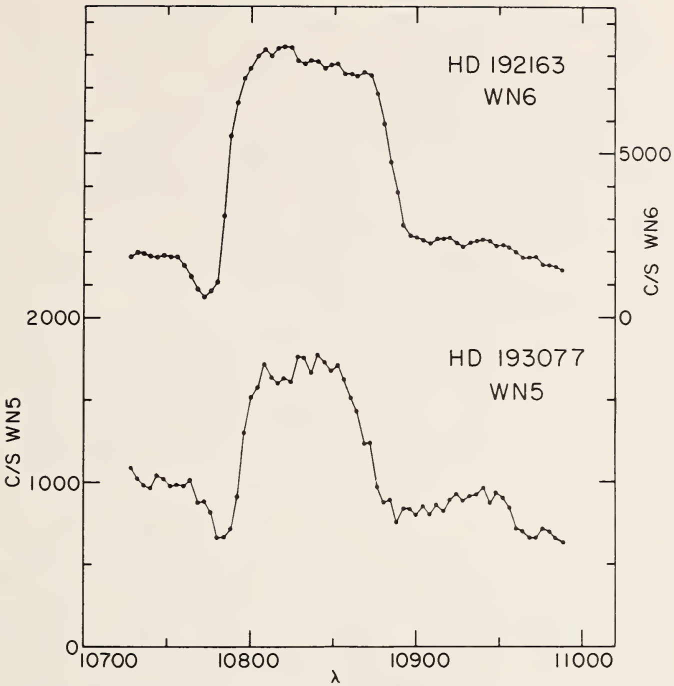

Figure 8. Photoelectric line profiles of λ10830 for WN stars. Other data same as Figure 7.

effect is observed. (2) Hydrogen-like transitions (i.e., between principal quantum numbers) are observed to quite high values of n in C III and especially in C IV, which has an unusually well-developed spectrum. For example C IV lines from the 6-n, 7-n, and 8-n series are identified in WC stars. In the presence of any appreciable ionic field these lines would not be visible since they are exceedingly sensitive to Stark effect.

The work of Münch on the absorption lines of the O-type companion of V444 Cyg (1950) gives convincing evidence of the presence of electron scattering in Wolf-Rayet atmospheres. This had already been suggested (Kopal and Shapley 1946) from the eclipse data obtained by Kron and Gordon (1950), which revealed a very broad primary eclipse compared to a much narrower secondary eclipse. Münch noted that the absorption lines from the O-star were much broader and shallower when the Wolf-Rayet star was in front (primary minimum) than at other phases. He interpreted this as being due to electron scattering both in the Wolf-Rayet envelope and in the O-star atmosphere itself. An optical depth $\tau \simeq 0.5$ for the scattering "layer" was found necessary to match the observed changes in the profile of H10. Münch also suggested that electron scattering produces an appreciable broadening in the emission lines formed in the deeper layers of the Wolf-Rayet envelope where the optical depth would be sufficiently large. In this way he is able to reproduce roughly the profile of He II λ4686 in V444 Cygni with electron scattering producing about one-half of the observed width.

Underhill has criticized this result on several occasions (1959, 1968a) by stating that any absorption line should undergo similar broadening. For example the violet-displaced absorption components of He I (and for that matter any of the observed absorption components) are much sharper than any emission line in the same star. The most likely conclusion is then that the absorption component is formed in a part of the envelope that is basically outside the electron scattering region. This she dismisses as an unlikely hypothesis. However I would like to suggest that this is not so unlikely a possibility, especially for He I λλ3889 and 10830. Firstly, these absorption lines arise from metastable levels $2\ ^3S - 3\ ^3P$ and $2\ ^3S - 2\ ^3P$ respectively; as is well known, these lines (as well as others of higher excitation) are considerably enhanced under conditions of moderate dilution ($W \leq 0.01$) because of the buildup of the population of the $2\ ^3S$ metastable level. Secondly, in the region where electron scattering is efficient (i.e., $\tau \simeq 0.5$), the material density must be fairly high ($\sim 10^{12}/cm^3$) so that collisions must play a significant role (the importance of collisions has been discussed most recently by Code and Bless 1964 and Bappu 1967) and hence act to depopulate the metastable level. These two factors imply that the He I absorption lines are formed at some distance from the star and quite possibly outside the bulk of the electron-scattering region. The other absorption components that are most often seen are additional lines from the He I triplets and the blends of C III λ4650 and N IV λ3483. These all arise from levels which can be expected to be overpopulated by dilution effects, and again the absorption features are quite sharp in contrast to the broad emission. Sharp absorption components are also seen occasionally for N V λ4609 and C IV λ5806; these lines arise from normal levels of quite high excitation, and Underhill (1968b) suggests that they are due to large gf values and the large abundance of the ion. The latter arises because of the high ionization potential required to reach the next stage of ionization. Flat-topped profiles, when observed, are usually found among these lines. However the four different He I λ10830 profiles illustrated earlier indicate that we are not dealing with the case of a transparent envelope expanding with constant velocity. The presence of the "sharp" absorption components also implies that the extremely chaotic conditions closer to the star have been somehow ironed out and that we are left with a net outward flow of material moving at velocities ranging from ∿ 500 to 2700 km/sec. These velocities are similar to the large velocities of expansion (∿ 2000 km/sec) derived by Morton (1967) from the violet-displaced absorption components observed in the resonance lines of C IV, N V and Si IV in OB supergiants. Perhaps the same basic mechanism is at work here, but the conditions in the Wolf-Rayet inner envelope are clearly much more extreme.

We should now discuss the various correlations found for the emission-line widths. For a given star, lines from highly ionized ions are much narrower than those from ions of a lower degree of ionization. In WN stars for example this is very clearly evident when one compares N V λ4609 to N IV λ3483. The correlation is not so well defined for lines of different elements. This range in width among different ions of the same element is most readily interpreted in terms of stratification in the Wolf-Rayet atmosphere. However the nature of this stratification has not been settled. Beals originally suggested that the temperature decreased outwards, so that the N V lines (the narrowest) were formed in much deeper layers than those of N III (quite broad). This in turn implied that the atoms were accelerated to higher velocities at greater distances from the star. Münch's discussion of electron scattering however implies just the opposite; a large optical depth produces a large width, hence the broadest lines must be formed close to the star, and the temperature increases outwards. Since electron scattering does not account for the entire width (the remainder being Doppler), this picture also implies decelerating motions in the envelope. Thomas (1949) has proposed an envelope supported by large-scale turbulence, in which the lines of highly ionized ions occur higher in the atmosphere than those of less highly ionized ions. A more attractive picture (because it does not predict unobserved occultation effects) has been suggested by Code and Bless (1964). They propose that the line widths are due to the intrinsic velocity range in large prominences and that collisional ionization and excitation are extremely important. The most highly ionized ions then would have the lowest kinetic energy and hence the narrowest emission lines. No stratification is needed. These models will be discussed in detail by Underhill in the following paper.

The second correlation is well defined only for the WC stars: the earlier the spectral type the broader are the emission lines. For example the widths of C III-IV λ4650 are 85, 45, 35 and 10 Å for L. Smith's types WC5, WC6, WC7 and WC9 respectively. Presumably this correlation is related to the increase in temperatures encountered with earlier spectral types. However what the exact mechanism or connection may be is not at all clear. It cannot be due to higher temperature: the widths are much too large for this if the temperatures involved are of the order of 5×10^4 to 10^5°K. The higher excitation (earlier type) and larger widths may be caused by the same primary mechanism; for example increased flux of high energy particles, as already suggested by Code and Bless (1964), could impart high kinetic energies (hence Doppler motions) to all stages of ionization while also raising the general level of ionization. Clearly a great deal of work remains to be done in this area. There is no clearcut correlation for WN stars although L. Smith (1966) does note that "the strength and width of lines vary markedly from class to class, increasing from WN7 through WN8 and WN6, reaching a peak between WN6 and WN5, and declining through WN4 and WN3". However the variation from star to star and from ion to ion is large so that no definite conclusions can be reached.

The third correlation was discovered by Hiltner and Schild (1966) in setting up their new classification scheme. The WN stars fall into two groups, one with broad emission lines, the other with relatively narrow lines. Aside from one star, all the stars in the latter group are known binaries; the reverse is true in the former group. No good explanation is available for this correlation, although Underhill (1968b) has suggested that the presence of a binary companion somehow damps out the large velocities encountered in the Wolf-Rayet atmosphere of a single star. However this cannot be the entire answer because of two difficulties: (1) There seems to be no such distinction between

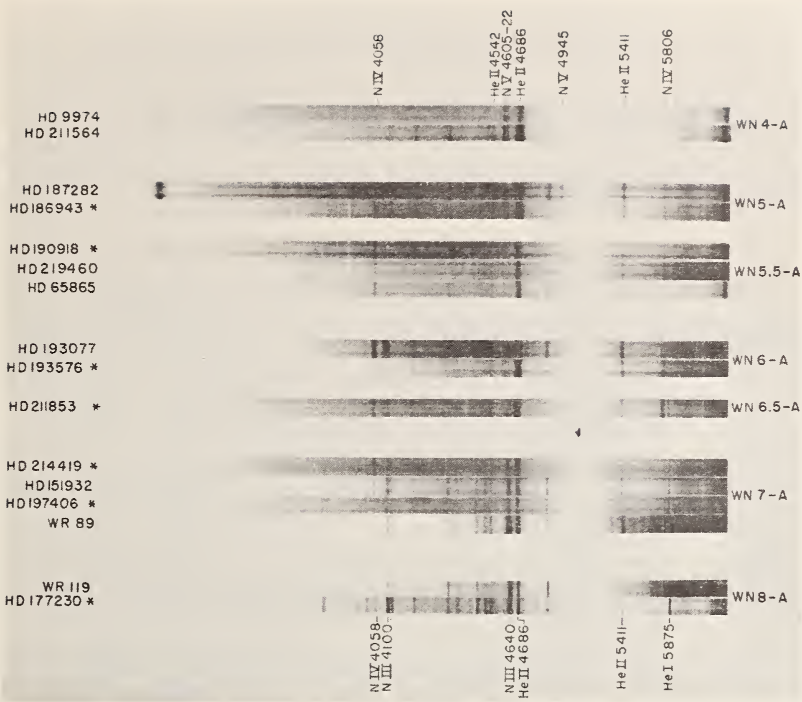

Figure 9. A comparison of spectra of binary and single WR stars taken from Hiltner and Schild (1966).

the binary and single stars of the WC sequence; if a binary damping mechanism were operating, surely it would have some effect on the WC binaries as well. (2) We might expect some additional correlation between line width and orbital separation (i.e., period) if the effect is really due to the companion. The greater the separation, the less would be the influence of the companion and hence the broader the emission lines produced. Figure 9 seems to show just the opposite effect, if any.

For Hiltner and Schild's WN5-WN5.5 stars, HD186943 shows a much broader He II λ4686 line than HD190918, even though the periods are 9.55 and 85.0 days respectively. Again in their group WN6-WN6.5, HD193576 (P = 4.21 days) shows a somewhat broader He II line than HD211853 (P = 6.69 days). Finally in group WN7, CQ Cep (P = 1.64 days) shows a broader line of He II than HD197406 (P = 4.32 days). Thus a reverse correlation can be established; namely, the shorter the period of a binary in a given spectral class, the broader its emission lines.

There are therefore two counteracting effects: (1) The narrower lines displayed by WN binaries in general and when compared to single WN's, and (2) the broader lines displayed by the close binaries in a given spectral class. Interaction effects obviously become more important for close binaries, but no real understanding of their result on Wolf-Rayet stars exists.

d. Line Intensities

The test of any theory for the Wolf-Rayet atmosphere is the reproduction of its emission-line spectrum, i.e., not only the line profiles but also the relative intensities of the lines from different ions. Therefore it is of great importance to know the observed emission-line intensities with good accuracy. Unfortunately the problems of large intensity ranges, already discussed in connection with line profiles, also arise in the photographic measurements of line intensities. There is in addition a severe blending problem (especially for the WC stars) due to the presence of many weak lines, so that it is extremely difficult to isolate the intensities of individual lines. The large range in intensity can be handled photoelectrically (although very little has been done), but there is no easy solution for the blending. Consequently the meaning of line-intensity lists is not too clear.

Nevertheless comprehensive studies have been carried out by a number of people (Plaskett 1924, Beals 1930, Swings 1942). Aller (1943) gives photographic intensities for eight Wolf-Rayet stars for the spectral range $\lambda3200$ to $\lambda6100$. Swings and Jose (1950) extended the work into the photographic infrared ($\sim \lambda8600$) for eight stars (but only three in common with Aller). H. Smith (1955) presented a great deal of data for southern Wolf-Rayet stars to supplement that from the fairly well studied northern stars. He also pointed out the notoriously poor agreement among line intensities determined by three different observers for the same star, factors of two being not at all uncommon. More recently Underhill (1968a) has provided line intensities for HD191765, and Kuhi (1966a) has determined photoelectric intensities for a few stars in the far-infrared ($\lambda8000$ to $\lambda11000$). Most of the other work has been qualitative with the strengths usually listed as weak, moderate, strong, etc. We list in Tables 3 and 4 some new photoelectric measurements of the intensities of the stronger far-

TABLE 3

INFRARED LINE INTENSITIES: WN STARS

Star	Spectral type	He II 8240	He II 10124	He I 10830	Pγ 10938
HD 4004	WN5	58	384	384	93
6327	W(He)	19	225	-	42
9974	WN3	6	116	-	-
50896	WN5	60	371	294	-
56925	WN5	58	346	106	-
65865	WN4.5	18	84	33	-
165688	WN6	40	317	403	170
MR 89	WN7	26	106	400	46
HD 177230	WN8	19	33	384	36
186943	WN4 + B	17	110	47	22
187282	WN4	-	179	38	23
190918	WN4.5 + O9.5Ia	1	38	15	23
191765	WN6	47	322	297	64
192163	WN6	45	285	326	69
193077	WN5 (+OB)	13	97	66	17
228766	WN7 + O	1	29	41	14
193576	WN5 + O6	14	150	882	29
193928	WN5 + OB	14	221	161	32
197406	WN7	12	38	69	9
MR 114	WN5 + OB	27	156	81	32
HD 211564	WN3	25	180	19	8
211853	WN6 + B0I	11	91	91	16
MR 119	WN8	9	8	166	47
HD 214419	WN7 + O7	8	99	56	8
219460	WN4.5 + B0	11	64	32	8

Notes: 1. The spectral types are those of Lindsey Smith (1966) and are somewhat different from the older IAU types.

2. The intensity is given in terms of the continuum flux per unit wavelength at the central wavelength of the line and refers to the total intensity above the continuum measured in the exit slit (96 Å for $\lambda<9344$ and 128 Å for $\lambda>9344$). Thus it may include a contribution from nearby weaker lines.

TABLE 4

INFRARED LINE INTENSITIES: WC STARS

Star	Spectral type	He II 8240	C III 8350	C III 8500	C III 8664	C IV 8860	C III 9710	C II,IV 9890	He II 10124	C III,IV 10544	He I 10830	Pγ 10938
HD 16523	WC6	95	71	62	51	48	587	13	170	55	154	60
17638	WC6	86	61	58	45	48	544	10	166	51	225	60
MR 82	WC9	83	117	66	73	58	120	24	8	6	179	32
HD 165763	WC5	82	50	33	22	21	536	6	149	29	115	60
168206	WC8 + B0	25	34	29	27	27	211	–	20	15	93	13
169010	WC5	106	76	61	60	91	553	32	177	58	195	72
MR 90	WC9	65	104	50	63	53	160	35	9	6	282	46
HD 190002	WC7	59	71	65	60	48	384	–	94	35	97	44
192103	WC8 (+OB)	62	64	45	28	14	627	24	93	36	295	37
192641	WC7 + Be	36	35	28	22	19	425	37	67	29	100	5
193793	WC7 + O5	25	23	11	10	10	238	5	64	26	93	43
195177	WC5p (+OB)	58	41	39	35	60	304	28	133	74	61	28
213409	WC5	100	86	79	58	60	730	32	177	81	211	47

Note: Units of intensity are the same as in Table 3.

infrared lines for 38 stars accessible to the 120-inch reflector at Lick Observatory. These intensities were obtained with the Wampler scanner using exit slits of 96 Å (for $\lambda < 9344$ Å) and 128 Å (for $\lambda > 9344$ Å) and represent the total intensity of line emission in the wavelength interval measured. The tables give this intensity in terms of the continuum intensity (per unit wavelength) at the central wavelength. This procedure avoids calibration and reddening problems so that the absolute fluxes can always be obtained once the flux in the continuum is adequately known. For example Underhill (1968a) has used Kuhi's (1966b) continuous energy distribution to obtain absolute line fluxes for HD191765.

We can now make use of these measurements to estimate the total energy involved in the emission lines as compared to that in the continuum. If we ignore the known and suspected binaries and allow $\sim$ 10% correction for fainter lines not measured, we arrive at the data in Table 5. The quantity tabulated is the ratio, r, of line flux to continuum flux in the region λ8000-λ11000. The ratios for each spectral class are the means for all the single stars measured in that class. A number of hitherto single stars were not included in the means because their intensities seemed too low; they may be binaries even though they have not been detected as such in the normal photographic region of the spectrum.

TABLE 5

FRACTION OF ENERGY IN EMISSION LINES (λ8000-λ11000)

Spectral type	Number of stars	r
WN3	1	0.088
WN4	1	0.095
WN5	3	0.261
WN6	3	0.291
WN7	1	0.212
WN8	1	0.173
WC5	3	0.519
WC6	2	0.499
WC7	1	0.351
WC8	1	0.486
WC9	2	0.298

Examples are HD9974, 65865, 197406 and MR119. It should also be noted that two of the WC5 stars have ∿ 20% lower intensities of C III λ9710 than HD213409, but all three were used in the mean. The results clearly demonstrate the relatively large amount of energy radiated in the emission lines even in this wavelength region where the spectrum is not so rich as in the ultraviolet. Furthermore the WC stars are about twice as extravagant in general as the WN's. The relatively low values of r for types WN4 and WN3 are due to the lack of any strong lines; most of the expected lines of N IV and N V are in the ultraviolet (near and far). We can also make use of Underhill's data for HD191765 (WN6) to estimate r for the regions λ3100 to λ4900 and λ5300 to λ6800. Again allowing 10% for weak lines we find r = 0.712 and 0.266 respectively. If we allow r = 0.250 for the wavelength regions not covered here by definite measurements, we find that over the range λ3100 to λ11000 the emission lines contain 0.373 as much energy as the continuum. The value for a typical WC star would be at least twice as large, judged by the appearance of their spectra. When more precise measurements become available, similar calculations can be performed for all Wolf-Rayet stars. The implications of this result and those of Table 4 are extremely important: there is a tremendous amount of energy involved in the production of the emission lines, not only in the actual transitions producing the lines but also in the energy required to produce the high degree of ionization, and any theory must take this into account. Again, aside from some applications of the Zanstra mechanism (which assumes conditions obviously not fulfilled in Wolf-Rayet stars), no theoretical discussion has considered this aspect of the problem.

e. *Variations in Line Intensities and Profiles*

The greatest changes in the intensity and shape of both emission lines and absorption components occur in the spectra of binary systems. However small irregular changes have also been reported for a number of single Wolf-Rayet stars. We will discuss the latter here and defer the binaries to Section III. H. Smith (1955) has observed variations in the weaker lines of HD92740 and HD93131 (both WN7); the usually conspicuous He II λ4340 and λ4200 practically vanish on occasion. Also the violet-displaced absorption components of Si IV λ4088 and N V λ4605 change erratically. Bappu (1951) has reported similar variations for N V λ4603, N III λ4640, He II λ4541 and He I λ4471 in HD191765, along with apparent central reversals in He II λ4686 and He II λ5411. He also reported intensity variations in HD192163, but his results have not been confirmed by Underhill (1966). Struve (1944) has noted changes in the strengths of the absorption components of N V λ4603, λ4620 and He II λ4541 in HD151932 (WN7). All of these observations emphasize the similarity between WN7 stars and Of stars, which undergo changes in the lines of N III and N IV (Oke 1954).

The only other single Wolf-Rayet star to show spectral variations is HD50896. Wilson (1948)noted variable radial velocities and profiles of N IV λ4058. H. Smith (1955) also noted peculiar variations in absorption components and the presence of central absorptions in most lines when N IV λ4058 was bright and did not have a central absorption. More recently Barbon, Bertola, Ciatti and Margini (1965) have commented on the strong atmospheric activity implied by the observed spectral changes. However we must bear in mind that these variations are very similar to those observed in binaries and that HD50896 may yet prove to be a binary.

The evidence for intrinsic variability of emission lines in single stars is very incomplete. The meager evidence which is available suggests some slight variability but nothing so drastic as the changes observed in other types of stars (e.g., T Tauri stars). Variations observed incidently with the photoelectric spectrum scanner are usually of the order of 10% or less. No systematic study of these variations has yet been undertaken.

III. THE EFFECT OF BINARY NATURE ON SPECTRA

A large fraction of Wolf-Rayet stars are binaries with an early-type companion. The binary nature is detected by variable radial velocities, by presence of stellar absorption features, and by the general "drowning" of emission lines in the continuum of a hot star. Examples of the last effect can be seen in Tables 3 and 4: compare for example the intensity of He II λ10124 in HD211853 (WN6 + B0I) to that in HD165688 (WN6), 91 to 317! Several general comments concerning the effects of binary nature have already been made, so that I would now like to describe in some detail the spectral changes observed in binaries.

The most studied Wolf-Rayet binary is the eclipsing system V444 Cygni (HD193576, WN5 + O6, P = 4.21 days), discovered by Gaposchkin (1941). The light curve has been discussed by Kron and Gordon (1943, 1950) and by Kopal and Shapley (1946). The most detailed spectroscopic discussions are by Wilson (1940, 1942) and by Münch (1950). We will concern ourselves chiefly with the spectroscopic details discovered by Münch and with additional features observed photoelectrically by Kuhi (1968).

The major changes in the emission lines are as follows: The He II λ4686 has a symmetrical profile at both elongations but is distinctly asymmetrical to opposite sides during primary and secondary eclipse. Figure 10 is a photoelectric profile of the line at secondary eclipse (Ø = 0.0) and at quadrature (Ø ≃ 0.75). Sahade (1957) has noted the appearance of a sharp emission feature superimposed on the broader line during both eclipses. He suggests that this new feature is responsible for the asymmetry and that it arises from gas streaming between the two stars. No such feature is seen photoelectrically, but it may be

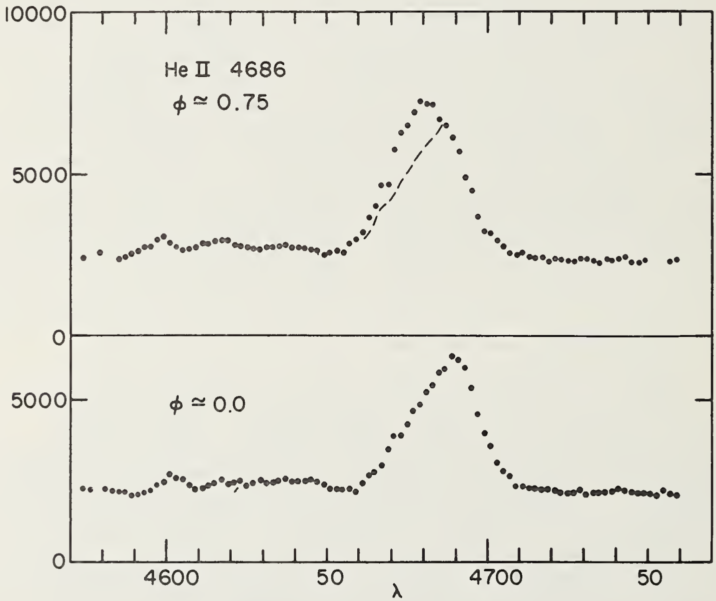

Figure 10. Photoelectric line profile of He II λ4686 in V444 Cygni at secondary eclipse (ϕ = 0.0) and at elongation (ϕ = 0.75). Note the distinct asymmetry at ϕ = 0.0; the asymmetry is in the opposite sense at ϕ = 0.5, i.e., at primary eclipse.

wiped out by the low resolution (a 2 Å exit slit). The N III lines also undergo remarkable changes: λ4542 (blended with He II) appears stronger at primary minimum than at secondary and is weakest at elongations. On the other hand λ4640 is stronger at secondary minimum than at primary. Also very remarkable is the appearance of an absorption component (v = -340 km/sec) in N IV λ3483 only at primary minimum. It is not present outside eclipse! Other lines of N IV do not show this component (as might be expected since λ3483 is the only N IV line to show an absorption component in other stars); instead λ4058 is strongest at primary minimum and is more often distorted by an overlying fine absorption feature (which persists for less than 6 hours) when the Wolf-Rayet star is farthest away from the observer. The N V λλ4603, 4619 lines weaken considerably at times of conjunction, and the accompanying violet absorption edges also change markedly. They are strongest at primary minimum, weaker at elongation, and disappear completely at secondary eclipse. The emission lines of He I are very broad, and the absorption component of λ3889 is strongest around primary eclipse, but its intensity is quite variable. Its variations however bear no relation to the variations observed in the other spectral features. Clearly it must be formed at a fairly large distance from the star, as discussed earlier. Similar variations of He I λ3889 have also been found in other binaries (e.g., Hiltner 1944, 1945). Münch tried to estimate the intensity changes due to eclipse but did not succeed (because of insufficient accuracy), other than to note that He II λ4686 decreased by ∿ 12% at primary minimum and N V λ4603 weakened considerably at conjunctions. Hiltner (1950) also noted the very peculiar behavior of He II λ4686 in CQ Cep (HD214419, WN7 + O7, P = 1.64 days): the light curve in λ4686 showed two maxima coinciding with conjunctions, and the intensity was greater by 4% at primary minimum than at secondary. He also concluded that the intensity was intrinsically variable.

Kuhi (1968) attempted to resolve the situation by obtaining light curves through secondary eclipse in all of the stronger emission lines. He planned to use the O-star as an occulting disk to cover up successive regions of the Wolf-Rayet envelope and, by comparing the shapes and depths of the eclipse curves for different ions, to determine the stratification (if any) of the envelope. Thus if the temperature decreased outwards, lines of N V would undergo a much sharper and deeper eclipse than those of N III and vice versa. The results, however, did not reveal the stratification but only served to point out the extreme complexity of the system. A few such light curves are shown in Figures 11 to 15. Several effects are immediately clear: (1) There is no correlation between eclipse depth and ionization potential for a given ion. (2) Individual

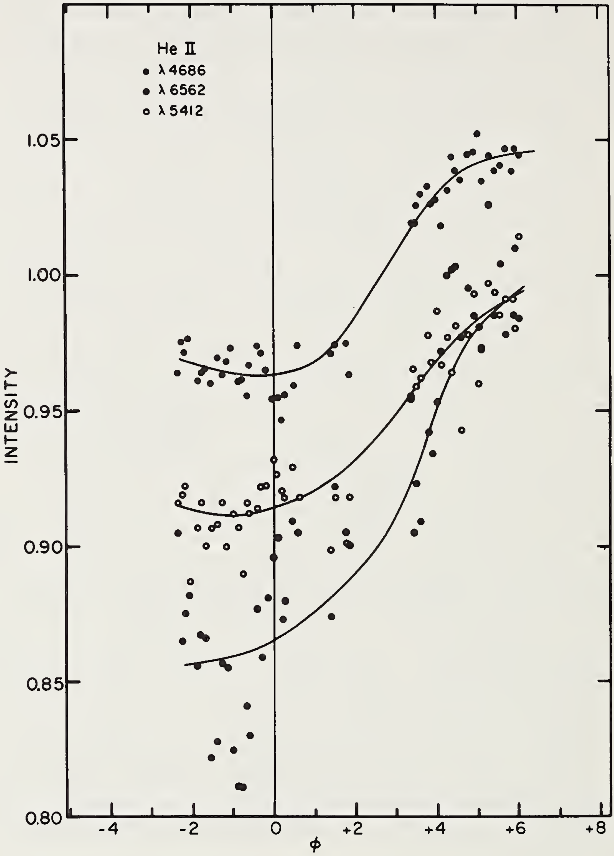

Figure 11. Eclipse curves of He II lines at secondary minimum of V444 Cygni. The phase ϕ is measured in hours from secondary minimum as determined from the continuum light curve. The intensity is that of the line only and has been normalized to 1.000 at $\phi = +30.0$. The mean observed errors are $\sim \pm 0.005$, ± 0.039 and ± 0.014 for λλ4686, 5412, and 6562 respectively.

lines of the same ion do not have the same eclipse curve. (3) Some lines (e.g., N III λ4540) undergo extremely peculiar behavior not showing a true eclipse curve at all. (4) Other lines (e.g., N III λ4100) have a very asymmetric light curve. In addition all lines undergo a decrease in intensity at primary eclipse (Wolf-Rayet star in front) which is often much larger than that at secondary eclipse (e.g., N V λ4609: 50% decrease at primary and ∿ 30% at secondary). The intrinsic variability of the lines was also clearly demonstrated: N IV λ7112 decreased by 30% over 21 days, N III λ4540 increased by 7%, and N V λ4609 decreased by 11% in the same

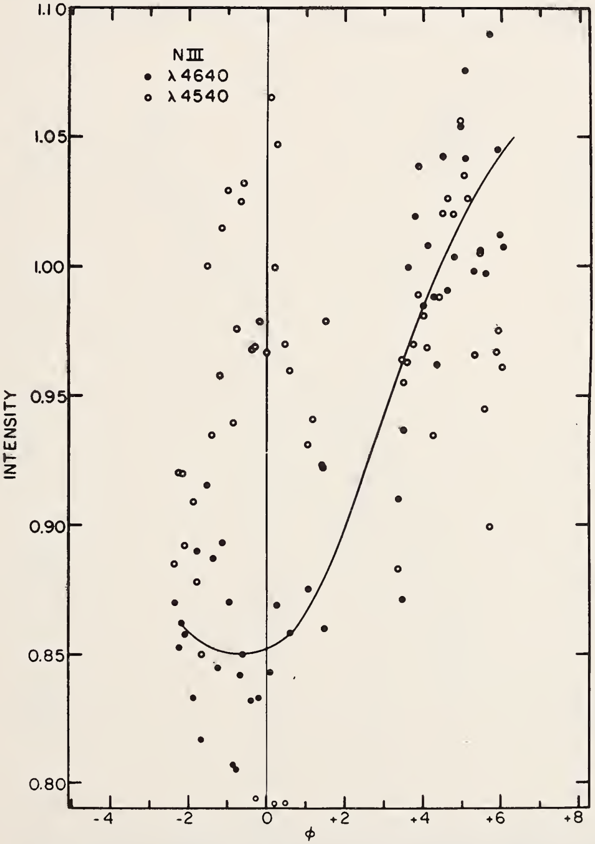

Figure 12. Eclipse curves of N III lines at secondary minimum. Intensity units and phase as in Figure 11. The mean observed errors for both lines are ∿ ± 0.045. Note the peculiar behavior of λ4540.

time. This strongly points to the random nature of the process responsible for the production of the emission lines. The interpretation of these results in terms of stratification effects proved to be impossible because of several additional complicating factors implied by the data. Firstly a large fraction of the light in the emission lines comes from the side of the Wolf-Rayet star facing the O-star, and there is a strong indication that much of this is concentrated in a stream between the two stars. Secondly, various lines (e.g., He II λ4686) were found to increase in intensity as secondary minimum was approached, and some of this increase was interpreted as additional excitation produced by

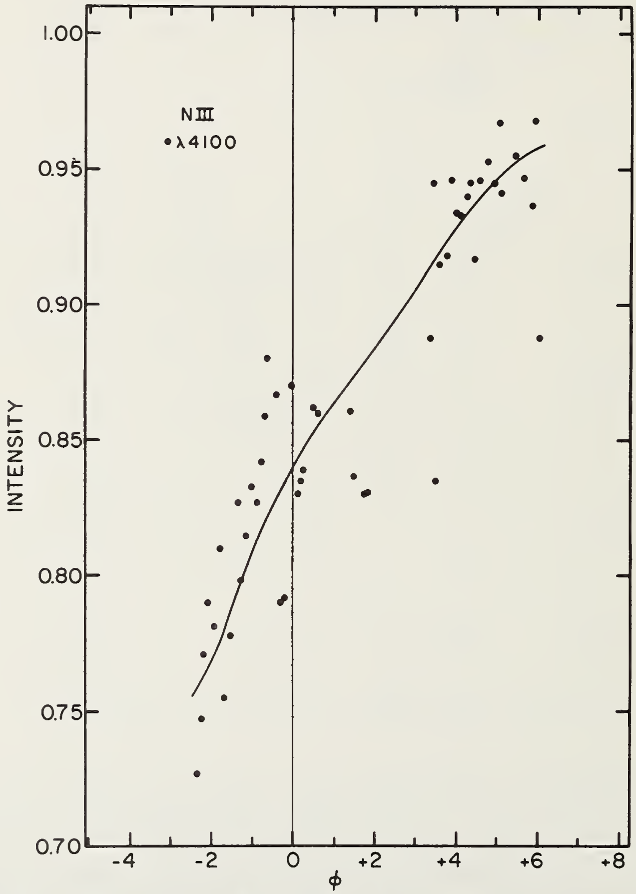

Figure 13. Eclipse curve of N III λ4100. Units as in Figure 11. The mean observed error is ∿ ± 0.036. Note the asymmetry of the light curve.

the O-star. Thirdly, the Wolf-Rayet envelope is optically thick in many of the emission lines, so that additional non-geometrical effects are produced such as the appearance of absorption lines and large decreases in intensity at primary eclipse (e.g., larger than can be explained just by occultation by the WN star). Finally in such an envelope the electron scatterers act as secondary sources of line emission and effectively enlarge the area in which a particular emission line appears to be formed. Hence no insight can be gained as to the original size of the N V λ4609 emitting region, and therefore the stratification question

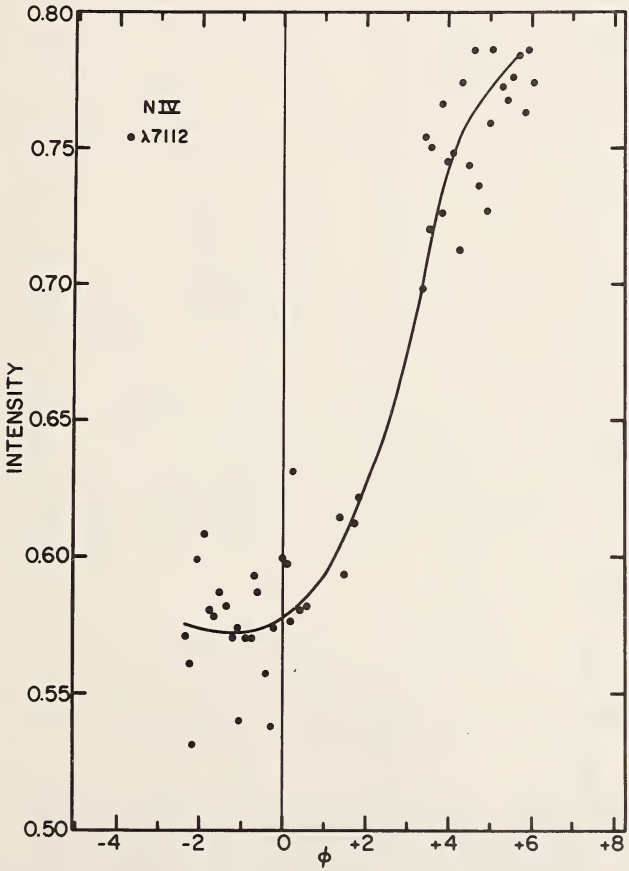

Figure 14. Eclipse curve of N IV λ7112. Intensity units and phase as in Figure 11. The mean observed error is ∿ ± 0.014.

is still not settled. The final resolution seems to require simultaneous photoelectric intensity measurements and coudé spectra obtained throughout one epoch. The simultaneous measurements are necessary to disentangle geometric and physical effects; observations at one epoch are to avoid complications from the intrinsic variability of the emission lines. Another system showing complex behavior is γ_2 Velorum, a southern WC star which has not been studied very extensively. H. Smith (1955) reports narrow central absorptions in most of the emission lines from H I to O V. He con-

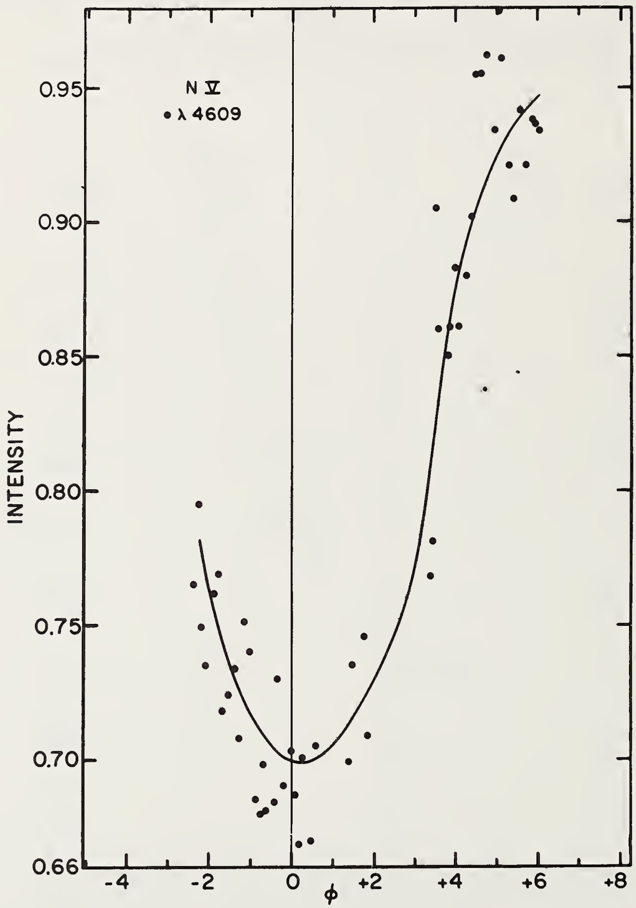

Figure 15. Eclipse curve of N V λ4609. Intensity units and phase as in Figure 11. The mean observed error is ∿ ± 0.035.

firmed Perrine's (1918) observations of the behavior of violet-displaced absorption components of He I which appear and disappear on a time scale of days. At times double absorption lines are visible. The extremely curious thing is that the normal absorption component of He I λ3889 remained unaffected by this behavior at a velocity of - 1320 km/sec compared to -480 to -960 km/sec for the other components. This behavior is quite rare and has been observed only in 1918 by Perrine and in 1953 by Smith. No explanation has been offered. Bappu and Sinvhal (1955) have investigated the behavior of He II λλ6560, 5411, He I λ5857 and N IV λ4058 in CQ Cephei and find it similar to that already noted for He II λ4686. In addition Ganesh and Bappu (1968) have described marked changes in line profiles of He II λ4686, λ4200 and N IV λ4058 in HD193928, HD186943, and HD211853. Thus changes in emission line intensities and profiles seem to be a common characteristic of Wolf-Rayet binaries along with variations in the He I λ3889 absorption component.

We should also discuss the effects of a Wolf-Rayet binary system on the O-star. The effects on the absorption lines (Münch 1950) have already been discussed. One might expect the O-type companions to be somewhat peculiar, but Beals (1934) stated that they seemed to be no different than ordinary O-and B-stars. If mass exchange does occur during post-main sequence evolution, the line strengths of carbon, nitrogren and oxygen may be somewhat peculiar. This depends critically, of course, on what part of the Wolf-Rayet component is exchanged. If the outer regions only take part, then the line strengths of the O-star should be normal. Preliminary results (Kuhi and Conti) indicate that they are indeed normal.

IV. INTERMEDIATE OBJECTS

a. Wolf-Rayet Stars Showing Lines of both Carbon and Nitrogen

A number of southern Wolf-Rayet stars described by H. Smith (1955) apparently show strong lines of both carbon and nitrogen. HD62910 (WN6-C7) shows the normal nitrogen spectrum together with strong emission lines of C III λ6735, λ5696, λ4326, C IV λ5805, C III-IV λ4650 and O V λ5590, λ3760. HD90657 (WN4-C + OB) shows C III λ4650 and N III

λ4640 of roughly equal strength. Finally HD104994 (WN3) is the strangest of all. It represents the highest state of excitation observed in a WN star. The lines of N V λ4603, λ4620 equal He II λ4686 in strength; other N V lines are extremely strong. In addition O VI $\lambda\lambda$3811, 3834 are quite strong. This is the only WN star for which they are seen; usually they are conspicuous only in WC6 stars. Such unusual stars (Smith has several other interesting peculiar objects, but these three should illustrate the point) do not fit into the simple picture of two separate sequences but instead demand a very specific explanation. Can they be explained by special excitation conditions or by post-main sequence evolution? No satisfactory answers have yet appeared, but we should not forget these strange stars when we comfortably extoll the virtues of one theory over another.

b. Stars Showing Wolf-Rayet Features and Nebular Lines

HD184738 (WC8, Campbell's hydrogen-envelope star) is the most famous of the very few stars in this group. It has been extensively discussed by Campbell (1918), Stoy (1935), Aller (1943), Struve and Swings (1940), Swings and Jose (1950), H. Smith (1955) and Andrillat (1958). It shows a WC8 nucleus, but the total intensities and line widths are much less than for normal WC8 stars. The nebular lines are extremely strong: [N II] λ6580, 5755, [O II] λ3727, [S III] λ6312 are all present. Figure 16 shows the infrared spectrum of this object from λ6000 to λ11000 as obtained with a photoelectric spectrum scanner with a 10 Å exit slit. The strength of the Hα blend is fantastically large, the central intensity being $\sim$ 80 times as strong as the adjacent continuum. The details of the Hα profile are shown in Figure 17. The forbidden lines also dominate the infrared spectrum: [S II] $\lambda\lambda$6717, 6731, 10284, 10320, 10370, [S III] $\lambda\lambda$9069, 9532 and [O II] λ7320. The He I λ10830 line is also quite strong. The weaker lines are due to C II, C III and H I. There is some evidence for N II in the star: it may be a contributor to the blend at λ8440. Because of the nebular lines and surrounding nebula this star has often been called a planetary nebula, but it is not at all clear what it is. A related object is HD167362, which also shows a strong nebular spectrum and a stellar spectrum similar to that of HD184738. Again nitrogen is strong in the neb-

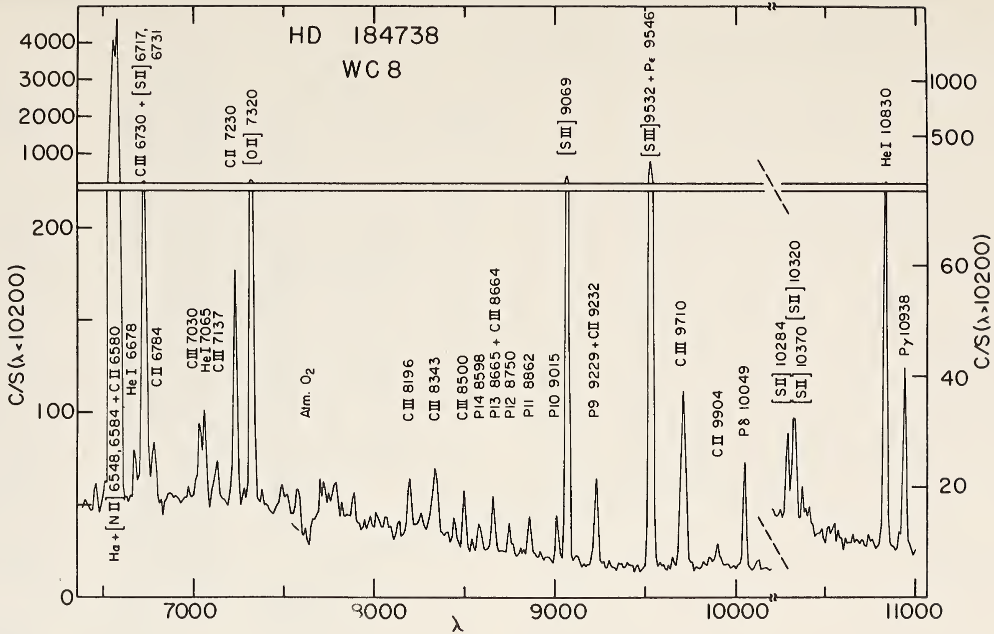

Figure 16. Infrared spectrum of HD184738 (Campbell's hydrogen-envelope star) obtained with a photoelectric spectrum scanner with a 10 Å exit slit. The ordinate gives the counts per second; there is a break in the scale at λ10200. Note the extreme strength of the nebular lines.

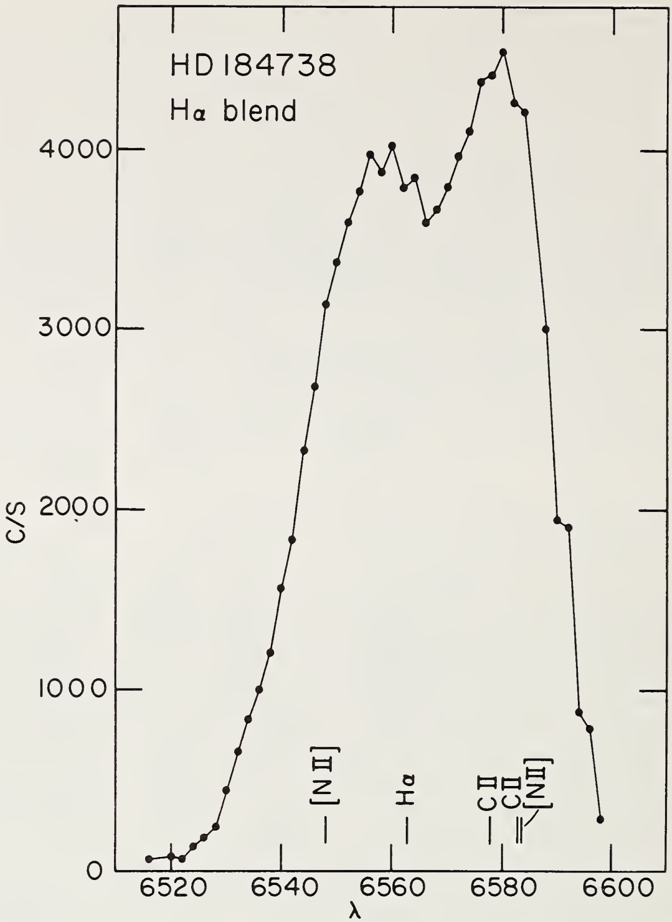

Figure 17. Photoelectric line profile of the λ6560 complex in HD184738 obtained with a spectrum scanner using a 2 Å exit slit. The profile is a blend of nebular lines of H, [N II] and stellar lines of C II.

ula but absent from the nucleus. Finally a most peculiar object, NGC 6543, somewhat akin to the above two stars, displays emission lines of carbon and nitrogen in both the nebular and nuclear spectra. These objects may be the intermediate stage between planetaries and Wolf-Rayet stars, but this hypothesis leads to great difficulties with the masses of the central stars and those of normal Wolf-Rayet stars.

c. Central Stars of Planetary Nebulae

Extremely little is known about the Wolf-Rayet stars comprising the nuclei of planetary nebulae. In fact work in progress by L. Smith and Aller suggests that the spectra of a majority of stars formerly called "Wolf-Rayet" do not resemble the spectra of normal Wolf-Rayet stars. Many of the nuclei are Of stars which naturally have quite sharp lines. A large number of them are peculiar, showing broad emission bands near C III-IV λ4650, He II λ4686 and O VI λλ3811-3834 and differing in structure from star to star. The other emission lines are very weak. The presence of O VI again implies conditions of very high excitation, much higher in fact than those in normal Wolf-Rayet stars. Six stars remain which are basically similar to classical Wolf-Rayet stars. One of these (HD184738) has already been discussed under (b). Smith and Aller note two points about these six stars: (1) they have consistently narrower lines, and (2) there is a greater tendency to find moderately strong lines of <u>both</u> carbon and nitrogen than in ordinary Wolf-Rayet stars. Thus there are still basic differences between these two groups which must be reconciled with any theory.

The implied masses of the central stars are much smaller than those of normal Wolf-Rayet stars ($\sim 1\ M_\odot$ instead of $\sim 10\ M_\odot$). Hence we must conclude that the Wolf-Rayet phenomenon is attributable to the physical conditions in the atmosphere (but still perhaps induced as a consequence of some stage of stellar evolution) and is <u>not</u> the exclusive property of a certain mass range of stars.

The objects discussed very briefly in these sections emphasize the embarrassing fact already pointed out that we really do not understand the nature of Wolf-Rayet stars. I hope that this symposium will help alleviate this dismal state of affairs.

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DISCUSSION

Chairman: Jorge Sahade

Sahade: In summarizing the spectral features of WR stars, Kuhi has divided his material into a number of topics: (1) The possible overlap between the WC and WN sequences. (2) The distribution of energy in the continuum. (3) Line profiles. (4) Correlations between the various characteristics listed in items (1) to (3). (5) The relative amounts of energy radiated in lines and continuum. (6) Variations in line intensity. (7) Influence of binary character. (8) Other objects exhibiting WR and quasi-WR phenomena.

I suggest that starting with item (1), we follow this sequence in our discussion. Let me remind you, however, of Kuhi's suggestion that with the exception of C IV λ5805, there are no strong lines of C or O in WN stars, and that there are no even moderately strong lines of N in WC stars. The only exceptions are several southern WR objects that show both C and N lines. As Kuhi discussed these exceptions in item (8), I suggest we combine items (1) and (8) in our discussion.

Underhill: First I would say that O V is definitely present in WN stars, and O IV is probable. The spectrum of HD191765 provides a good example: The He II λ5411 line appears to lie at λ5427, whereas other lines show no such shift. The He II line must, therefore, be a blend, and when you look in the multiplet tables, you find that indeed one of the strongest multiplets of O V comes in just the right position.

The same spectrum provides support for Kuhi's argument that C IV is present in WN spectra. The C IV lines at λλ5801 and 5812 would account nicely for the λ5806 emission, and the alternative suggestion of N IV doesn't really compete. Whereas Hallin actually predicted one multiplet of N IV observed in this spectrum, he didn't even observe the λ5806 emission in his plasma source. It can, therefore, hardly be strong enough to produce the observed feature in the WR star.

Second, I am convinced that N III is definitely present in the late WC subclasses. Kuhi has said quite correctly that my evidence for N III in HD192103 (WC7 or WC8) is not strong, but I say it is not so weak that you can ignore it. The evidence is stronger in Campbell's hydrogen-envelope star, (WC9 on the present system), where there is no alternative identification for λ4634 but N III. I would not suggest the presence of N in WC5 and WC6 stars, but I think it is definitely present in the later WC's.

Aller: The forbidden lines of N II λλ6548 and 6584 are strong in the planetary nebula surrounding Campbell's star, so is it really so surprising to find nitrogen in the stellar spectrum?

Underhill: It's no surprise to me, but people insist on forcing nitrogen out of these stars when it's got to be there. Kuhi suggested that because the ionization potentials of C III and N III are nearly equal, it is difficult to understand why one should be present and the other not unless there are very real abundance differences. But in an emission spectrum, the recombination spectrum is not necessarily governed by the ionization potential. Other things such as recombination coefficients and the question of what upper ions are present can also effect the spectrum, so I don't think his remark is terribly relevant.

Kuhi: I have already commented at length on these objects. I agree with Anne that WC stars contain nitrogen. But perhaps we are belaboring this point. The main point is that we have two distinct sequences: one has strong lines of nitrogen, the other of carbon and oxygen. I do not think it is particularly significant that we find some nitrogen in stars of the carbon sequence.

Taken as a whole, the data on Campbell's star, on planetary nuclei and on the southern WR stars lead me to believe that we are dealing with a "WR phenomenon", something which occurs because the conditions in these objects are right for its occurrence. I do not know what these conditions are, but they do not appear to be uniquely related to a given type or mass of star. We find them occurring in very massive and luminous objects similar to O-stars, and we find them in less massive objects such as the nuclei of planetary nebulae.

Aller: I have a few remarks concerning the nuclei of planetary nebulae, based on an observational program conducted by Lindsey Smith and myself. I want to emphasize that planetary nuclei include a number of different spectral classes. Some show only continuum; some, such as the nucleus of NGC 6508, show absorption lines like O-stars; some are like Of stars, and some like WR stars. The distinction between the Of and WR stars is - or should be - that those spectra showing narrow emission lines are consistently called Of. The WR-type, i.e., those with broad emission lines, can then be divided into two distinct groups: those such as NGC 40 (Figures 18 and 19) and Campbell's star (Figure 20) whose spectra resemble the classical WC stars; and those such as NGC 246 whose spectra are dominated by O VI λλ3811 and 3834, C III-IV λ4650, and He II λ4686.

I call this second group the O VI sequence because its members show a continuous variation in the strength and structure of the O VI lines (Figure 21). At one extreme is the nucleus of NGC 246, which shows a continuous spectrum with a few absorption lines and two sharp but faint emission lines at λ3811 and λ3834. The O VI lines are somewhat stronger in objects like the nucleus of NGC 2371-2, which has faint broad emission lines topped by sharp emission peaks. In IC 2003, the O VI lines are still rather weak, while in IC 1747 they are just apparent, and in NGC 7026 they are prominent. In NGC 6751 they are moderately intense, while in NGC 6905, they are very strong and blended into a single band. Still more extreme than NGC 6905 are objects like NGC 5189 in which the O VI lines are enormously strong and completely dominate the spectrum. This star has been studied by Blanco, Kunkel and Hiltner, who suggest that it may be the optical counterpart of the x-ray source Centaurus XR-2. A second object, with a similar spectrum but with no associated nebulosity, has been identified with another x-ray source GX3+1.

It seems likely, therefore, that the O VI sequence is one of increasing excitation and that the most highly excited members may be strong x-ray sources. I conclude that it is quite distinct from any type of sequence found in the classical WR stars, and I think it offers interesting possibilities for speculation.

Schild: Is there any correlation between the properties of the stars in the O VI sequence and such things as surface brightness and true diameter?

Aller: The statistics are so limited that it is impossible to derive correlations. NGC 6905 and NGC 2371-2, for example, are large nebulae, whereas NGC 7026 and IC 1747 are binucleated. And while the excitation level of the nebular spectrum is often rather high, the O VI group does not include the highest excitation nebulae. Even NGC 5189 does not have very high excitation.

Johnson: Do you find any abundance differences between the central stars and their associated neb-

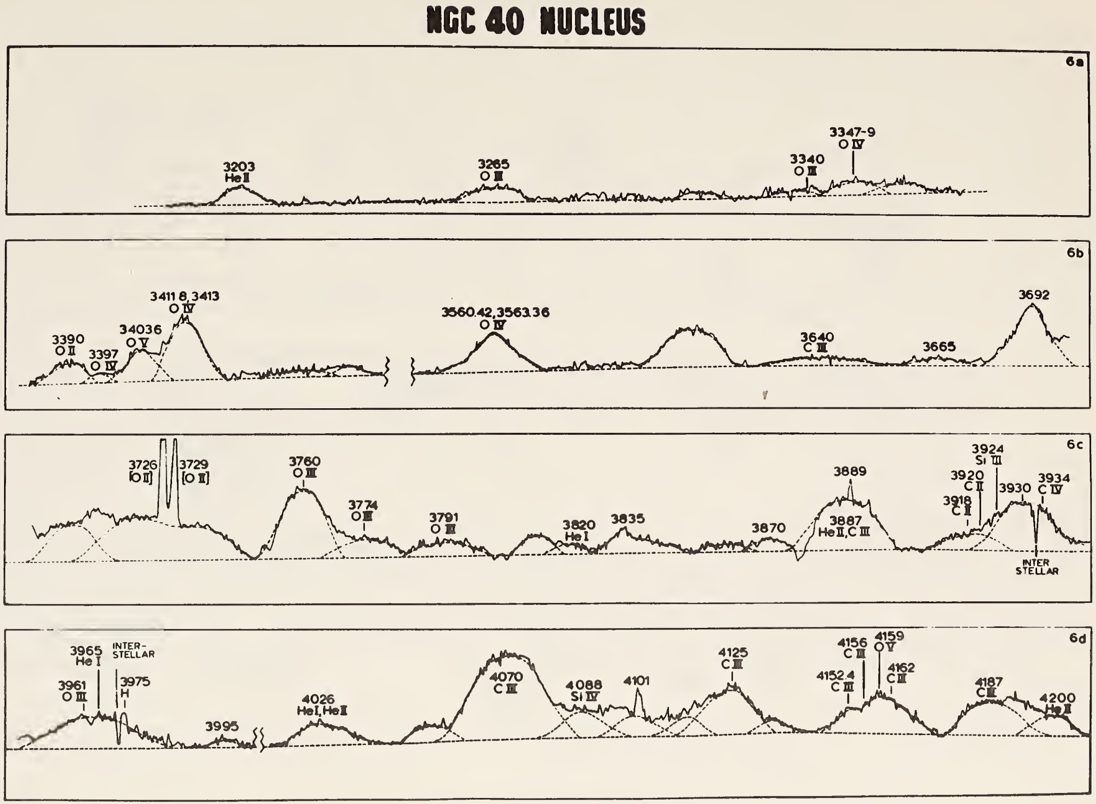

Figure 18. Tracings of the spectrum of the nucleus of NGC 40 from λ3200 to λ4200. The estimated level of the continuum and the estimated resolution of the broad bands into components are shown by dashed lines.

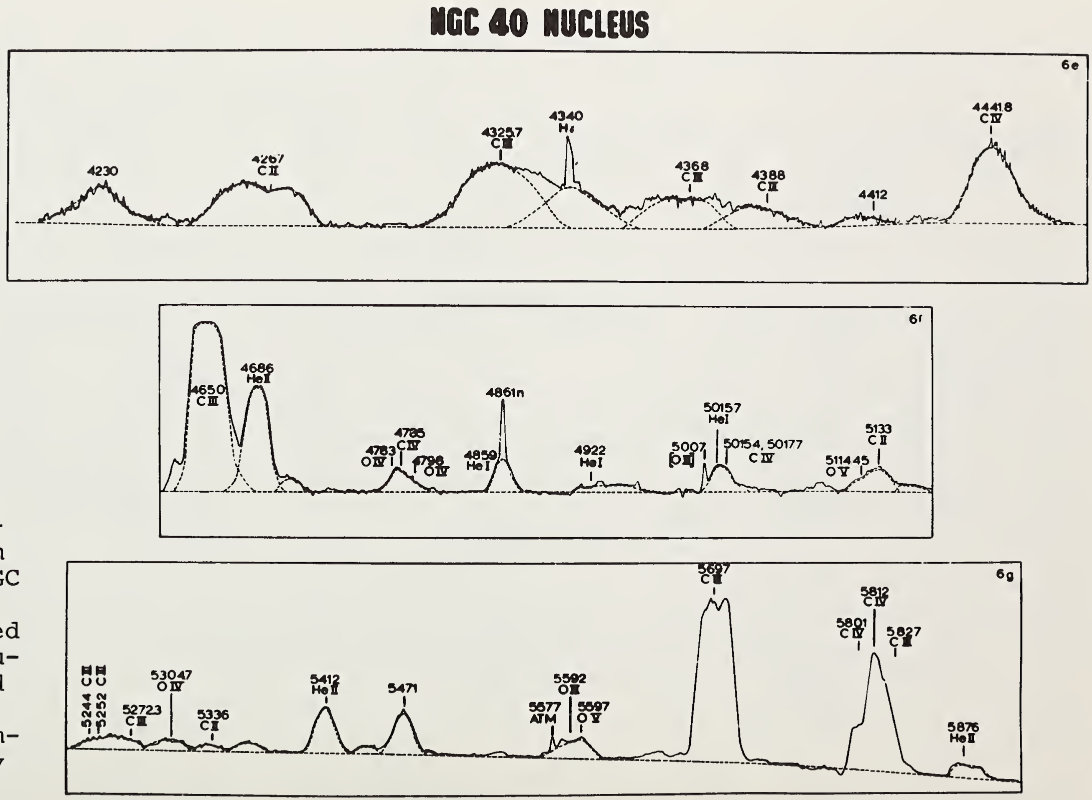

Figure 19. Tracings of the spectrum of the nucleus of NGC 40 from λ4200 to λ5900. The estimated level of the continuum and the estimated resolution of the broad bands into components are shown by dashed lines.

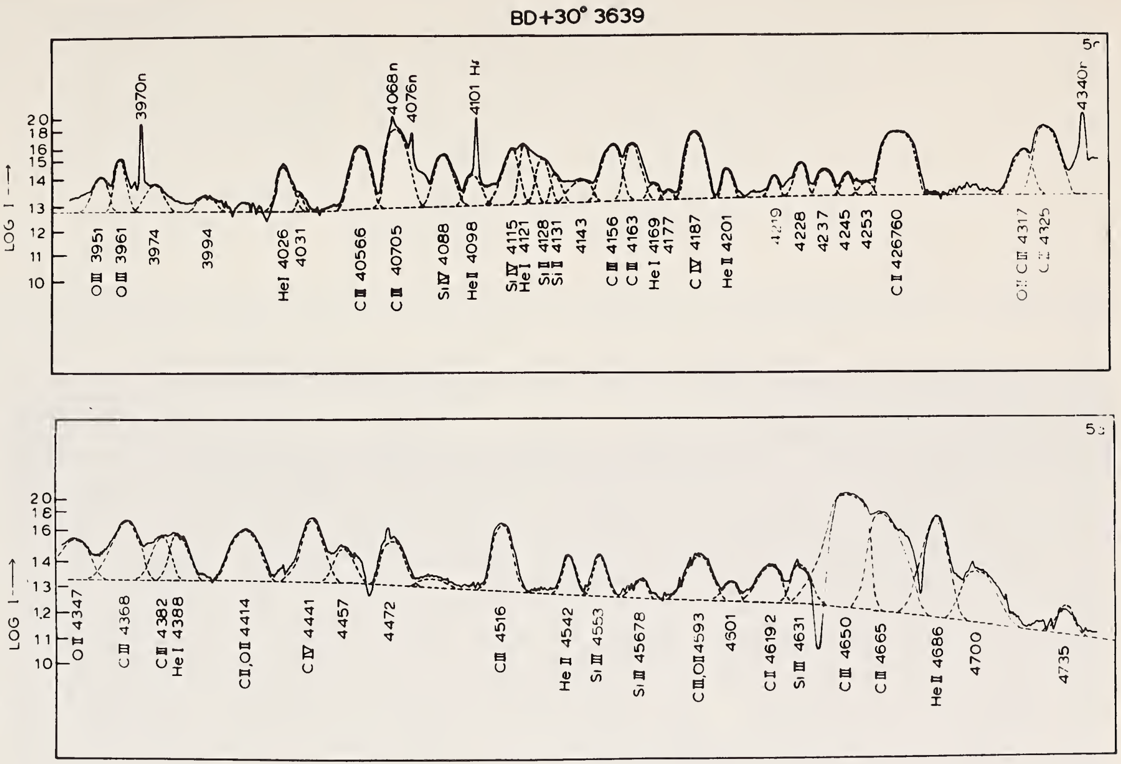

Figure 20. Tracings of the spectrum of Campbell's hydrogen-envelope star, BD+30°3639, from λ3950 to λ4740. The estimated level of the continuum and the estimated resolution of the broad bands into components are shown by dashed lines.

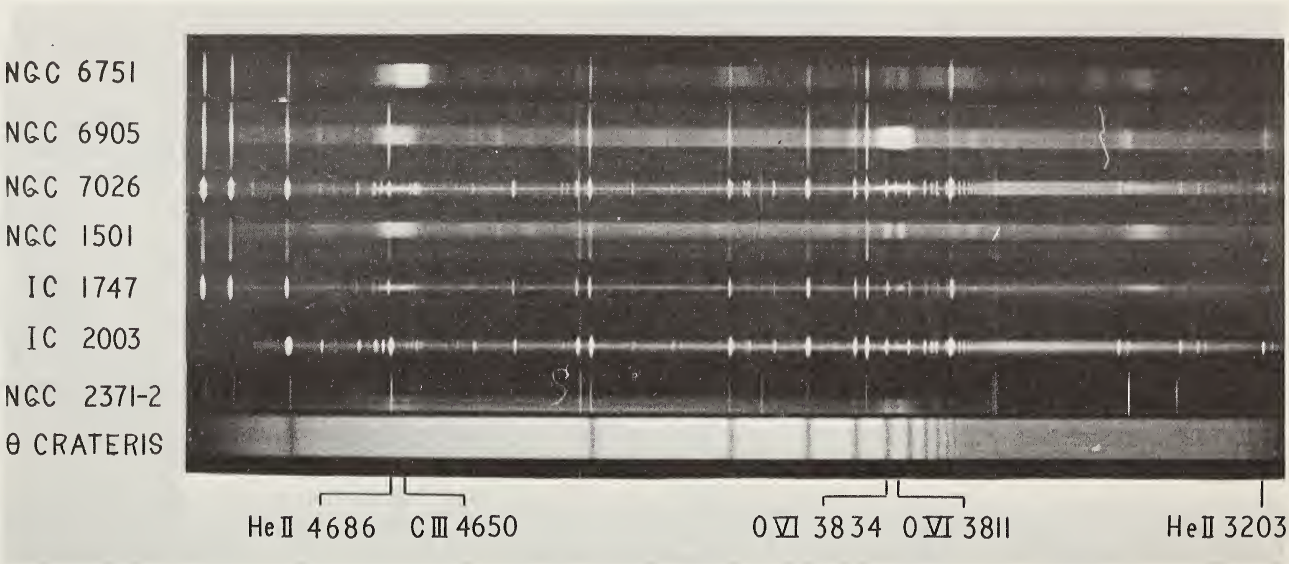

Figure 21. Spectra of planetary nuclei with WR characteristics. The spectrum of NGC 6751 is very similar to that of a classical WC7 star. The others have no counterparts among classical WR stars and comprise a new class of star which we name the O VI sequence. The spectrum of a B9 star, θ Crateris, is given for comparison. The spectra were taken with the 120-inch telescope of Lick Observatory.

ulae? In particular, I am interested in the H and He abundances: Do you find a higher He abundance in the nucleus than in the nebula?

Aller: I might be able to answer that if you could tell me how to analyze the spectrum of a WR star or planetary nucleus. For the nebulae, the analysis is straightforward in principle, although uncertainties in cross sections and computational details make it more difficult in practice. For the central stars, however, we know very little about the formation of spectral lines, and in addition there is the problem of disentangling the hydrogen spectrum of the star from that of the nebula. Current evidence suggests that there are no appreciable abundance differences between the planetary nebulae and their central stars. Perhaps the best approach would be through the absorption lines, since the absorption line objects seem to be very much like ordinary O-stars. However, Anne Underhill tells us that we can't even interpret these spectra.

Wrubel: Is there any indication from their position in the Galaxy that all these objects belong to the same population? Is the distribution of these WR nuclei consistent with that of the classical WR stars as outlined by Lindsey Smith?

Aller: Again the statistics are bad: we have very few objects, and those that we have are very faint. The low dispersion spectra were all taken at the prime focus of the 120-inch telescope. Even with electronic image converters, there is only a limited number of objects we can reach in the time available. The answer to your question seems to lie in more telescope time.

Smith: I have two comments on the differences between planetary nuclei and classical WR stars. First, the differences in line width mentioned by Kuhi show up very clearly among the low-excitation spectra (e.g., BD+30°3639 and NGC 40 as compared with HD164270 and HD192103), but in the high-excitation spectrum of the nucleus of NGC 6751, the widths are comparable with those of classical WC6 spectra.

Second, the nuclei of planetary nebulae are believed to be of about one solar mass, whereas the WR stars are about 10 $M_\odot$. I have suggested that the mechanism responsible for the WR spectrum is an instability in the He- or C-burning core of the star. The helium instability sets in only above 7 or 8 $M_\odot$, so if the same mechanism is to account for the spectra of planetary nuclei, they must have a C-burning core. Theoretical calculations suggest that a pure C-burning star is unstable at any mass, but a hydrogen envelope would tend to stabilize the system. So it seems reasonable to suggest that the WR phenomenon in planetary nuclei is due to C-burning instability.

Thomas: RR Lyrae stars are pulsationally unstable and are of about one solar mass. How do they fit into your generalization? If you assert (1) that the evolutionary path of a star is fixed by its initial mass and chemical composition, and (2) that all stars within a certain range of these parameters will go through a WR stage, how do you interpret the difference between RR Lyrae stars and planetary nuclei? Is it a difference in initial chemical composition? Or do they represent different phases in the evolution of stars of the same initial mass and composition? Or again is it possible that they started with different masses (and possibly different compositions) and that the heavier one, following a different evolutionary track, has lost mass, so that the present equality in mass is a result rather than an initial condition? Both RR Lyrae stars and planetary nuclei are single stars, so it can't be a binary effect.

Smith: It is a different form of instability in the two cases. For RR Lyrae stars, as for cepheid variables, it is K-instability due to the variation of opacity with temperature and pressure. For the WR stars, I am suggesting ε-instability, due to the variation of energy production with temperature and pressure. This difference certainly reflects a difference in the present structure of the two classes of stars. How they attained this structure is another matter. It is not, however, clear to me that it is useful to compare these quite different objects.

In this connection, I should like to add a postscript to my discussion yesterday on the distribution of WR stars and its possible interpretation in terms of initial chemical composition. I then made no distinction between binary and single stars. Now you will recall from Schild's discussion that the association Sco OBI contains a WN7 star and a WC7 binary. Since the two stars are in the same association, they must have essentially the same age and initial chemical composition (although Schild did indicate there is some evidence for small variations in these parameters). The fact that single WC7 stars do not appear in associations tells us they are fairly old. Yet here we have at least one binary WC7 star that is definitely very young. I do not think we can escape the conclusion that binary WC7 stars are younger than single WC7 stars. If this is true for one subclass, it is reasonable to assume it is true for all subclasses. Hence the binary stars and the single stars are basically different and may have different distributions in the Galaxy. While I still favor differences in initial chemical composition as a working hypothesis, the matter does need re-evaluation.

Thomas: Then let me continue the point I was making on instabilities. Suppose I assert that the WR phenomenon reflects an atmospheric state (regardless of the mechanism by which that state is produced) rather than a unique evolutionary stage in a particular class of objects. And suppose further that this atmospheric state reflects a mechanical heating and that the variation in the WR phenomena reflects variations both in the character of the heating and the character of the object (e.g., composition and gravity). Then the kind, the degree, and the result of the instability must be considered carefully. What distinguishes the WR phase of the planetary nucleus, with its stable broad emission lines, from the cepheid spectrum of the RR Lyrae star, with its sporadic, narrow emission lines? Both these stars are of one solar mass, and both have mechanical heating from radial pulsation. Is it the "background" state of the atmosphere, as determined by the size of the radiative flux? Is it the chemical composition of the atmosphere? Is it the period of the mechanical pulsation? Is it the total energy and momentum carried in the mechanical pulsation? Superficially, the WR instability appears to be violent, whereas that of the RR Lyrae stars, the cepheids, the long period variables and the Sun are mild. But the terms "mild" and "violent" must be much more precisely defined.

J. Cox: I would like to comment on the physical distinction between the two kinds of instability. In the RR Lyrae stars and classical cepheids, the instability is caused by ionization in the stellar envelope. Since there is a limit to how much an element can be ionized, there is a natural limit to the amplitude of the pulsations driven by this energy source. In the WR stars, on the other hand, the pulsations are supposed to be driven by nuclear energy sources in the core of the star. Whether or not there exists a natural limitation to the amplitude of such pulsations is not yet known. It is a crucial point. One of our students, Ziebarth, is investigating this problem in connection with the upper mass limit of the main sequence stars, which owes its origin to the same kind of instability mechanism.

Thomas: I was thinking not just of the amplitude of the radial pulsation, but also of the magnitude of the mechanical energy delivered to the atmosphere. We know that in the Sun we get comparatively narrow, high-excitation emission lines in the rocket UV. Some years ago Mrs. Gaposchkin classified the quality of this spectrum as WC6. We have also heard Stecher comment on the great width of emission lines in the rocket UV in ζ Puppis, and he says this is also true of other supergiants. Now suppose we were to increase by the same factor the amounts of material and of mechanical heating in the solar chromosphere-corona. Would we then get broad emission lines similar to those in WR stars? Or would we have to increase the supply of mechanical energy by more than we increase the atmospheric mass? We have several parameters: energy available (amplitude and period of pulsation), energy delivered (actually dissipated in the atmosphere), excitation level at some particular place in the atmosphere (chromosphere-corona, observed only in rocket UV or eclipse), and excitation level in the great mass of the atmosphere (disk spectrum observed in visual region).

Underhill: The interesting thing about WR stars is that they give no visible evidence of pulsation in the sense that RR Lyrae stars and classical cepheids do. Kuhi has some evidence of erratic variations of small amplitude; but if there were pulsations, we would expect light variations and a period.

Thomas: Pulsations of the kind discussed by Kippenhahn and Paczynski (as summarized by Lindsey) have periods of minutes only. Would you detect such short periods from the observations presently available? Second, it is not the pulsation as such but the mechanical energy which will affect our interpretation of the spectrum. If the pulsations are of sufficiently high frequency that their effect is that of a statistically constant supply of mechanical energy, it is not obvious that we would observe a light variation.

Leung: On the subject of the amplitudes and variations in the radial pulsations of RR Lyrae stars and classical cepheids, I would like to draw your attention to an interesting correlation. For classical cepheids, RR Lyrae stars, δ Scuti and dwarf cepheids, there seems to be a strong correlation between surface gravity "g" and the slope "a" of the pulsation. We define "a" by $\Delta m = a\,\Delta(RV)$, where Δm is the amplitude of the light variation, and $\Delta(RV)$ is the amplitude of the radial velocity variation. The larger the value of "g", the smaller the value of "a". If this relation were to hold for WR stars, the very small amplitude of the light variation would imply a small value of "a", which would require a large value of "g".

Underhill: There is no evidence that WR stars have particularly large values of "g". If you take their average mass and Hanbury Brown's estimate of their radius, you come up with log g ∿ 4.

Leung: Since we know nothing about the mechanism of instability in WR stars, I just wanted to point out what would happen if you related them to RR Lyrae stars and classical cepheids. Personally I doubt that the mechanism is the same in WR stars as in the others.

Schild: I would like to make a point about the separation of the WC stars into two groups. Figure 24 from the Hiltner and Schild atlas [Figures 24-28 appear at the end of the discussion, pp. 175-179.] shows WC stars of higher excitation; Figure 25, those of lower excitation. These objects were classified without any knowledge of the identity of the stars. If you compare the strengths and widths of the emission features around λ4000, you will see that they appear sharper and stronger relative to the continuum in Figure 25. Whereas Figure 24 consists of ordinary WR stars of WC type, Figure 25 includes many peculiar objects such as the nucleus of NGC 40 and Campbell's hydrogen envelope star. These differences cannot be explained simply as temperature or excitation effects. I just want to emphasize that wholly empirically there are differences in appearance between the classical WR objects and those associated with planetary nebulae.

Sahade: We are all agreed that there are two sequences of Wolf-Rayet stars. Kuhi and Miss Smith argue strongly that they are differentiated by differences in chemical composition; Miss Underhill suggested in 1958 that they could arise from differences in excitation. I should like to hear some discussion on this point.

Underhill: I think any such discussion should be deferred until session C, when I have summarized the various processes that must be taken into consideration.

Stephenson: As we are considering Kuhi's categories (1) and (8) together, let me suggest that since old novae show broad lines, He II emission and hot continua, they should be included among "objects exhibiting quasi-WR phenomena".

Smith: If we believe that the characteristic spectrum of the WR stars is due to excitation of the outer envelope by a shock-wave, then we may hypothesize that in novae and supernovae you get similar characteristics because again you have shock-waves running through the extended outer regions of the star.

Underhill: This epitomizes the fact that spectroscopic phenomena, as defined by spectroscopic class, are not uniquely related to the parameters basic to theories of stellar structure, i.e., mass, composition and effective temperature. Our aim in interpreting stellar spectra has been to derive these parameters empirically from a knowledge of spectral type. When we try to interpret B- and O-spectra, we find that somewhere around spectral type B2, we begin to run out of a unique relationship between spectral type and basic parameters. The fact that we are now discussing quasi-WR phenomena as distinct from WR stars means that we have completely run out. We have created a spectral class that represents a physical situation in a plasma; the dominant characteristics of that situation are not uniquely related to mass, radius and total radiation field. In other words, the WR spectrum alone cannot be used to establish the interior structure of the object producing it. We have got to find some other feature in the stellar radiation field that can be used as a criterion of structure.

Aller: I wonder how many novae really fit into this category of objects in which the continuum is less highly excited than the line spectrum and in which absorption lines appear on the violet edge of emission bands. And furthermore, conventional novae are very shortlived phenomena, whereas the WR objects endure for many years.

Underhill: We will have to define our criteria more carefully. An important property of WR stars is that they are steady. We could eliminate novae immediately by requiring that the star not change rapidly with time.

Thomas: I am not sure you want to, Anne. I think Stephenson's emphasis on old novae was well taken. When you start considering whether the two sequences of WR stars reflect differences in composition or differences in excitation, you must remember that Henry Smith (1955, Thesis) found one old nova that had changed from WN to WC or vice versa. So I think we are demonstrating that while we may have WR objects, we also have WR phenomena. Lindsey has summarized one possible mechanism for producing WR objects, namely that by which mass exchange in close binaries results in an unstable He- or C-burning core. Possibly there are other ways to produce WR objects. I think that old novae and other objects exhibiting a quasi-WR spectrum may give us a clue to the excitation problem as a guide to the variety of ways in which a quasi-WR phenomenon - and thus perhaps the WR-phenomenon - may be produced. They may also give us a clue to the origin of the two WR sequences.

Regarding Anne's comments on the relation between stellar spectra and the parameters basic to theories of stellar structure, I of course agree enthusiastically. Twenty years of trying to develop a non-LTE diagnostic stellar spectroscopy have been based on just this viewpoint and were inspired to a large extent by emission line objects such as WR stars. But the first problem is to determine what parameters are needed to describe the spectrum; we cannot simply assume that we know what they are.

S. Gaposchkin: Let's not forget that many - possibly all - old novae are binaries. Perhaps the analogy with WR objects or phenomena goes even deeper.

Sahade: If there is no more discussion on this point, I suggest we turn to Kuhi's second item, namely the distribution of energy in the continuum.

Underhill: Kuhi's results present great problems. If we consider hydrogen-helium atmospheres, we can predict the intensity distribution from models computed by Mihalas, Strom, and myself. In the wavelength range we considered, you cannot change the intensity distribution by going to higher temperatures; it is almost insensitive to the details of the model. Yet the spectra which Kuhi has measured and has corrected as well as possible for interstellar reddening show a significantly different distribution. What can you do? You have no alternative but to postulate some unknown process.

Thomas: You agree wholeheartedly with our objections to the use of conventional models for the interpretation of line spectra. Why don't you take the next logical step and question the assumptions underlying your continuum models? In addition to those of LTE and radiative equilibrium, which you have questioned, you have one strong assumption that you have not questioned - that of hydrostatic equilibrium. You will remember the work of Kosirev and Chandrasekhar during the thirties: They studied the effect of arbitrary variations in density (such as those that might result if hydrostatic equilibrium were dropped) on the distribution of spectral energy. They happened to be looking for UV excesses, but that is not the crucial point. I think we had better stop talking about an atmosphere in hydrostatic and radiative equilibrium, characterized by the two parameters "g" and T_{eff}.

Kuhi: Are there any models available for OB supergiants?

Underhill: No. You can't get hydrostatic equilibrium, and computers only work with hydrostatic equilibrium.

Kuhi: Okay, so that only emphasizes the point I made yesterday about the similarity in the energy distributions of OB supergiants and WR stars. And it of course makes Dick's point all the more clear.

Stecher: Hunger and Klinglesmith have recently analyzed the helium stars and found them to contain 40% C by mass. If the WR stars are overabundant in C and He, then the C-absorption in the continuum will change the atmospheric structure and the radiation field in the far-UV. In principle this suggests a method for determining the C-abundance.

Underhill: Yes, to get a major change in the intensity distribution, you do have to move away from a predominantly H-atmosphere in hydrostatic equilibrium. But I would not like to use the continuous spectrum for an interpretation of the line spectrum. I am sure we are observing two quite different plasmas. An interpretation of the plasma responsible for the continuous spectrum is not likely to be valid for the plasma in which the emission lines are formed.

Sahade: We now move along to Kuhi's third point: line profiles. He has shown us that most of the profiles are Gaussian, but that some of them, especially those with violet absorption edges, are flat-topped, and that many of those that are flat-topped are just those lines that we would expect to show the effects of diluted radiation.

Nariai: Do you mean by Gaussian profiles that you expect turbulent motion of the order of 1000 km/sec? I am afraid such turbulent motion cannot exist because it would have a decay time of the order of 1 sec. You had better interpret the profiles in terms of a velocity gradient in the expansion velocity. Flat-topped lines may be formed in a region where the velocity is almost constant, while Gaussian profiles originate in a region where the expansion velocity changes rapidly.

Kuhi: I don't know that it is turbulent velocity, but there are two reasons for suggesting that it might be. Firstly, the line profile is roughly symmetric and roughly Gaussian. If I ignore any radiation transfer problem, I find that random velocities from 500 to 2000 km/sec are required to give this shape. Secondly, where absorption components exist for such lines, their violet displacements are of the same size.

Thomas: In a cynical kind of way, I would remind you that just as most emission gradients appear to be exponential, so most line profiles appear to be Gaussian, at least until you start trying to fit them in self-consistent, numerical detail. You usually manage to find deviations once you know what you are looking for. The physical problem in WR stars is to decide whether there is indeed a random distribution of velocities at each point in the atmosphere, or whether the atmosphere is sufficiently transparent that the symmetric reflection of a systematic velocity gradient gives the impression of randomness. Clearly there are two important physical questions, both of which we have been trying to answer for the past 20 years. (1) Can there exist random motions so violently supersonic as these velocities suggest? This was Nariai's question, just as it was mine a long time ago when I proposed an electron temperature of 10^6°K for a WR atmosphere. (2) What is the line opacity in the atmosphere? So far we have been held up in answering these questions by our lack of a complete set of solutions for the combined aerodynamical and astrophysical treatment of energy dissipation and radiation transfer in such an atmosphere. We have been working on this piecemeal for twenty years.

A. Cox: Perhaps Kuhi would review the observational evidence for a "turbulent" shell with the velocities just mentioned.

Kuhi: The evidence is indirect. Any simple model of a WR atmosphere fails. If we assume pure radial expansion, we find from eclipsing systems that the size of the envelope is something like 5 times the stellar radius. But if the envelope were that small, we would expect to observe occultation effects, i.e., the emission lines should appear asymmetric because the red-shifted wing, formed in the receding part of the envelope, would be hidden by the disk of the star. No such asymmetries are observed. If we try forced rotational equatorial ejection, such as Limber has suggested, we again run into difficulties with profiles.

But if we assume that by some chance the envelope is really larger, we should then expect to find phase differences between the time of eclipse as measured photometrically and the time as measured from velocity curves. No such differences exist. So we ask: How can we avoid these difficulties? And one answer seems to be turbulence because then any given line is the resultant of many components across the disk of the star.

A. Cox: What about these lines that show asymmetries at the eclipse phases?

Kuhi: That is a phenomenon associated directly with the fact that you have two stars and should not be confused with the case of single stars.

Limber: As has been mentioned, turbulence with such velocities is violently supersonic; the dissipation of energy would be enormous and would lead to very great difficulties.

Underhill: It is misleading to use the word turbulence in the sense that it is used by aerodynamicists. What we really mean is a distribution of velocities that appears from a great distance to have the form $\exp(-v^2/b^2)$. There is absolutely no doubt that the most probable velocity "b" is of the order of 10^3 km/sec. This corresponds to a temperature of some 10^7°K, which is foolish.

Thomas: We're not saying a thing here about turbulence that wasn't said twenty years ago. One of the things we had hoped to get from this symposium was an answer to the question: Has there been sufficient change in the phenomenological boundary conditions or in our theoretical understanding of aerodynamical and astrophysical problems to give us a better understanding of the physical situation in WR stars? So far in this symposium we have heard a few encouraging items: (1) The suggestion that oscillations of a He- or C-core could provide a source of mechanical energy; (2) A good deal of evidence that T_e is between 3×10^4°K and 5×10^4°K in the region where the continuum is formed; (3) Confirmation that the radius of the region in which the lines are formed is roughly a factor of five larger than the radius as measured in the continuum. (4) Information on the rocket-UV spectrum, which agrees well with results obtained in the visible; (5) Arguments in support of differences in both chemical composition and excitation as an explanation for the existence of two sequences: WN and WC. On the one hand, differences in taxonomic properties are interpreted in terms of differences in initial composition; on the other hand, there is an overlap of N features in WC objects and of C features in WN objects.

Underhill: Kuhi and I have been presenting you with just such information on boundary conditions. It is information that has existed for only a few years and even then for only a few stars.

Let's turn to the flat-topped profiles. These are not prominent in the ordinary WR stars. One of the sources of confusion in the early days was the happenstance that Beals chose to study C III λ5696 in HD193793, which turns out to be about the best flat-topped profile in existence. Naturally he said, "expanding atmosphere". Other people, looking at other profiles, reached other conclusions. There were two viewpoints in head-on collision, and both seemed perfectly correct.

Beals: I think if you used the ends of the flat-topped profiles to try to derive the velocity distribution, you could get a Gaussian distribution there too, although none of these interpretations is very firm.

May I now ask why the violet absorption edges are so strong in C III and He I, and whether they are formed close in to the star or farther out? I presume you would say He I is farther out than He II because it shows an absorption edge, which we attribute to dilution effects in the He I metastable lines.

Kuhi: You must be careful in your conclusion. You might say that the He I metastable lines are formed farther out because of dilution, but you can't take the next step and assume this implies T_e decreases outwards. We don't know that.

Gebbie: Are the absorption edges confined to any particular classes of WR stars, and if so is there any correlation between these classes and the classes which tend to produce small ring nebulae?

Smith: I believe the absorption edges are strongest and most consistently present in the spectra of stars in precisely those subclasses which are associated with ring nebulae, i.e., the single WN5, WN6 and WN8 stars. In the WC sequence, they occur most frequently in the WC9 spectra.

Thomas: Katharine and Lindsey are talking about a correlation between absorption edges and nebulosity; Kuhi has said that absorption edges are usually associated with flat-topped profiles. Are we to conclude that there is a correlation between nebulosity and flat-topped profiles?

Kuhi: What I said was: Absorption edges tend to occur in lines which show flat-topped profiles, if we see flat-topped profiles. I don't want to go beyond that statement.

Underhill: What you see is a variety of combinations. Flat-topped profiles with absorption edges tend to show the characteristics of an extended atmosphere. Actually they are Of lines. There are also flat-topped profiles, such as C III λ5696, which do not generally have absorption edges. Finally, there are lines with absorption edges which do not have distinguishable flat-tops. I think this occurs when two lines at the same wavelength are blended.

Smith: The most conspicuous absorption line in the spectra of WN5, WN6 and WN8 stars is the violet edge of He I λ3889. Is that line consistently flat-topped?

Underhill: It is flat-topped, but there is a rounded emission of He II right in its middle, so you can only see it when it's strong.

Payne-Gaposchkin: λ5875 is flat-topped and shows a nice absorption edge; and it doesn't have a He II line on top of it.

Kuhi: There are several different processes involved in producing these absorption lines. There is one group, which includes λλ3889 and 10830, with a lower metastable level, and there are other lines, about which Anne will speak in the next session, that arise from normal permitted levels.

Thomas: I am trying to be as simpleminded as possible in order to see which simpleminded arguments hold and which evaporate. In essence you and Lindsey are saying: Beals' original suggestion of a simple expanding atmosphere is pretty good for some stars, because on the one hand we see nebulosity which suggests an expanding atmosphere or ejected shell, and on the other hand we see absorption edges which suggest the same thing. The question is whether these particular stars are exceptional and, if not, why we do not see this kind of double evidence of an expanding atmosphere in all WR stars.

Sahade: I think we should remember that we probably have at least two kinds of envelopes in WR binaries: the thick envelope which surrounds the WR stars, and an expanding extended envelope. The different profiles may be connected through this model.

Now let us turn to Kuhi's fourth point, the various correlations. First we consider the line-broadening mechanism. Kuhi has pointed out that $\Delta\lambda/\lambda$ is roughly constant for lines of a given ion, which suggests that Doppler broadening is the chief mechanism. We have already discussed some aspects of velocity fields. Kuhi also summarized the various suggestions about the effect of electron scattering. Is there any further discussion?

Underhill: From an empirical standpoint there is little to discuss. The correlation "$\Delta\lambda/\lambda$ roughly constant" is simply true. I intend to give an interpretive discussion in the next session. Briefly, the point I shall make is that you must be very careful in choosing your lines and in assigning them to specific parts of your model. That is why the interpretations just discussed - expanding atmospheres and random velocity fields - can both be partially true. Certain strong lines will have contributions from many parts of the atmosphere; certain other lines will be formed only in shell-type conditions.

Beals: I am interested in the question of accelerated versus decelerated expansion. Do I understand that Kuhi thinks the atoms are decelerated outward? If so, what is the mechanism? Is gravity sufficient in these rather large envelopes, or do we need something like collisions between the atoms and the surrounding envelope?

Kuhi: My interpretation depends on whether you assume the temperature increases or decreases outward.

Stecher: Pikelner suggested some 20 years ago that radiation pressure in the resonance lines might levitate the atmosphere. One of his students has applied it to WR stars and gotten reasonable electron densities in the outer atmosphere. Solomon and Lucy have been applying it to mass loss from supergiants. I believe the ionization will increase outward due to the decrease in electron density.

Thomas: The idea that radiation pressure in resonance lines can act to drag out the atmosphere goes back to Milne and his suggestion about Ca^+ in the solar atmosphere. I am not sure that it has ever been successfully embodied in a wholly self-consistent theory. And it is not obvious that the ionization will increase with an outward decrease in electron density. The coronal type of collisional ionization equilibrium is independent of density. I agree with Anne that many of these questions can be answered only in the context of a completely self-consistent model. All these jigsaw pieces must be put together into a complete mosaic.

Sahade: Kuhi's second correlation is that between line width and spectral type in WC stars: the broader the lines, the earlier the spectral type.

Kuhi: This is not really discussible; the line widths are used to define the WC subclasses.

Sahade: Then we go on to the third correlation: a decrease in line width with increasing ionization level.

Thomas: This goes back to Beals' first work and is one of the oldest correlations in the literature. I took it literally in my model of the WR atmosphere as an extended chromosphere-corona supported by mechanical heating. In such a model you would expect T_e, and hence the ionization, to increase outward, at least initially. Then the observed correlation would suggest an outward decrease in the expansion velocity, or, depending upon how you interpret the line broadening, in velocity gradient. But whatever the interpretation, I regard it as one of the most significant pieces of empirical evidence, or boundary conditions, that we have. I was glad to hear Kuhi reaffirm it.

Underhill: I don't believe it has been established observationally. It is not based on enough quantitative information. I think it is just a happenstance.

Thomas: Happenstance means it exists, but you don't understand why. And here the important question is whether it exists, regardless of why.

Kuhi: Well, I think it is true in some cases. There are certainly spectra in which the lines of N III, N IV and N V are really quite different. But as Anne has pointed out, there are exceptions. I am afraid this will always be our problem with WR stars. There are exceptions to almost anything you can find.

Underhill: I think it was Mrs. Gaposchkin who first remarked that classifying WR stars is a pointless task: you end up with one class, one star. According to Lindsey Smith, we have 124 WR objects in our galaxy and 24 different classes of objects. That gives you about 5 objects per class. I'm dead against classification atlases: every time I take a high-dispersion plate, I find that some line which has been described as, say, N IV or C IV, is a general muck of 10 or 15 lines. Empirical classifications may be perfectly consistent, but I don't think they are accurate enough to establish this sort of correlation.

Sahade: So we go on to the fourth correlation, that between sharp-line objects in the WN sequence (Hiltner and Schild class A) and binary stars.

Thomas: Can you be more precise? Is the difference between sharp and broad lines a matter of 10%, a factor of 2, or a factor of 5?

Kuhi: Possibly a factor of 2; nowhere near a factor of 5.

Underhill: This is one of the points that requires further investigation and quantitative measurement. Just how sharp are these lines? What is the real meaning of conclusions based on low-dispersion spectra? But it is a very difficult observational problem to get 20 Å/mm spectra of these objects.

Payne-Gaposchkin: But you do not deny the scientific use of the division; it is so wonderfully physical. All sharp lines happen to be binaries. When people saw this for the first time, they saw something important.

Roman: I'm somewhat confused on looking at the Hiltner and Schild Atlas. [The Hiltner and Schild Atlas, Figures 24-28, appears at the end of the discussion, pp. 175-179.] Only about half the stars in the sharp-line WN sequence (WN-A) (Figure 26) appear to be binaries. In general, the single stars in this sequence seem to have broader lines than the binaries, but nothing like as broad as those in the broad-line sequence (Figure 27), one of which is in fact a binary. Then of the two WN8's the single star appears to have slightly sharper lines than the binary. So I'm a bit confused as to the facts.

Underhill: But that's just the problem. When you compare lines of a binary with those of a single star, the width can be misleading. You should in fact compare the half-widths not of the actual but of the normalized profiles.

Schild: As I said before, the spectrograms were arranged in sequences on the basis of their appearance. It was then observed that a large number of objects in one sequence were binaries. That is all there is to say.

Sahade: Let us now go on to Kuhi's fifth item: the relative amounts of energy radiated in the lines and in the continuum.

Gebbie: In view of the large amount of energy in the emission lines, I am curious about the physical significance of a visual magnitude that explicitly excludes these lines. Could this account, in part, for the anti-correlation between excitation and luminosity obtained by Lindsey?

Underhill: If we assume this energy is a form of the UV radiation, our bolometric correction will account for it just as for a normal star. The strength of the emission lines can perhaps be regarded as a conversion of the bolometric correction.

Gebbie: But these are not necessarily recombination lines.

Underhill: Some are; some aren't. Those that are, represent a good fraction of the conversion.

Thomas: Not obvious at all. You are assuming a mechanism. Suppose we buy the following picture: a continuum corresponding to about 5×10^4°K formed in a photosphere, and emission lines formed in a random-velocity shell heated by mechanical energy. We then have two kinds of energy supply: the continuum will refer to radiative processes, and the lines to collisional. So the continuum absolute magnitude and line absolute magnitude will refer to two different processes.

Underhill: I agree. To get a meaningful total flux, or bolometric magnitude, you should include both the mechanical and radiative energy. The term "effective temperature", as it is usually used, is misleading. The mechanical flux is usually neglected, as its contribution is small compared with the radiative. How much of the energy in the lines comes from the far-UV radiation field, and how much from the mechanical flux is, I agree, an open question.

Gebbie: Then is it meaningful to exclude from the absolute magnitude a source which may contain half the energy?

Smith: My change in luminosity with subclass amounted to some two magnitudes between WN3 and WN8. Diane Pyper gives a mean correction for the influence of the emission lines on the magnitudes of WN stars that amounts to 0.2 mag. Kuhi's data would give corrections amounting to between 0.1 and 0.3 mag. Thus the two corrections are in good agreement and would be an order of magnitude too small to account for the anti-correlation between excitation and luminosity.

Kuhi: Let me emphasize that the values I gave in my summary from my own measurements covered only the region λλ8000-11000. To derive the ratios 37% for WN stars and 70% for WC stars, I combined my data with that of Anne Underhill.

Underhill: These corrections are only for M_V, whereas it is the bolometric magnitude that is fundamental to theories of stellar structure. Implicitly we assume the bolometric correction for WR stars is about the same as for O-stars. But even if it is, we still don't know the correction for O-stars to within half a magnitude. And if the emission lines are excited by mechanical energy, we may be off by another half a magnitude or more. On the other hand, we must realize that while lines may produce 70% of the energy in the visible region, that is still only a small percentage of the total energy emitted by the WR star down to or below 912 Å. We are looking at these stars in the faintest part of their continua.

Thomas: Could I ask if the following is a fair summary of what you are saying: We observe M_V for the continuum. First, we want to correct it to include the energy emitted in the lines, so that we can estimate the total radiation in the visible region. Then, from this small visible tail, we want to infer the size of the dog - the bolometric magnitude. Your bolometric correction is 2-3 mag, Lindsey?

Smith: Less than 4.6 mag for Kippenhahn's He-burning stars; about 2.5 mag for the same stars in the C-burning phase.

Thomas: So you are really saying that the bolometric corrections are so large and so insensitive to the details of the model that no matter what model you use - including just a blackbody - the introduction of a supply of mechanical energy can have no significant effect. Right? Then this is an assertion which is basic to our entire discussion and which must be checked. I remind you the WR star is likely to be the most extreme freak of all freaks, and it is not impossible that it may deviate from this simple assumption.

Underhill: You are right in your summary of what we do; but I don't think the situation is so weird when you begin to look at it; it behaves like physics should.

Johnson: We've heard that a large fraction of the energy is radiated in the emission lines and that in binaries, a large part of the line-producing region lies between the stars. Is it possible that this same region produces a reasonable part of the continuum by free-free emission?

Kuhi: My remark that a large part of the line-producing region lies between the stars referred only to V444 Cygni, whereas the figures on the energy radiated in the emission lines referred only to single stars.

Sahade: Now when we turn to Kuhi's sixth point, the variation in line intensities, we must distinguish between lines in single stars and those in binaries. The latter should be included in item (7).

Aller: I want to mention HD45166, because it is usually quoted in connection with intrinsic variations in line intensities in WR stars. Apart from this star, most discussions concentrate on Of stars where strong variations in line intensities are well established. Using Harvard plates many years ago, Carol Anger Piene found tremendous variations in the nitrogen lines near λλ4634-4640. The spectrum of something like a B-star, with well-marked hydrogen lines, is also apparent. The star is evidently a binary, but there is no simple periodicity. There is still some question whether it is really a WR or an Of star. (Personally, I don't think we have yet settled the question of the dividing line between the two.) The star resembles an Of star in the sharpness of its lines, but the character of the spectrum is that of a WR star. I do not believe these variations in the nitrogen lines have anything to do with the binary character.

Sahade: If there is nothing more on non-binary effects, we will turn to the binaries, item (7).

Smith: Kuhi found that in V444 Cygni the secondary minimum (WR star eclipsed) is deeper in the red than in the photographic. This may be due to the infrared excess of the WR star, but how would you interpret Hiltner's result (1950, *Ap. J.*, 112, 477) that in CQ Cephei we observe the opposite effect, i.e., the secondary minimum is deeper in the UV than in the visual?

Underhill: Those were broad-band observations, 1000 Å or more wide.

Kuhi: Then we can't say anything definite; broad-band observations include the effects of emission lines. If we are to compare CQ Cephei and V444 Cygni, we need narrow-band work.

Hjellming: I'd like to comment on a WR eclipsing system that I think has been much neglected. This is HD168206, CV Ser, found by Gaposchkin many years ago. The system has a visual magnitude of 9.14 and a spectral type, according to Smith's classification, of WC8 + B0. It is the only eclipsing system known in the WC sequence. Figure 22 shows three light curves. The upper curve was

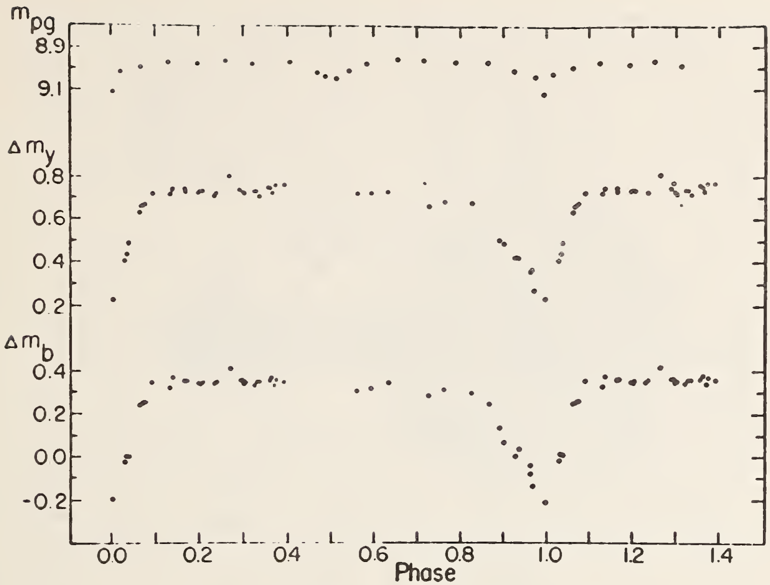

Figure 22. Photometric observations of CV Ser. The upper curve was obtained by Gaposchkin (1949), and the lower curves are new observations. Note the depth and width asymmetry of the primary eclipse. The period is 29.640 days.

presented by Gaposchkin when he first showed it was an eclipsing system: from this data it has a 0.14 mag primary eclipse and a 0.08 mag secondary eclipse. About 5 years ago, in the course of a photometric survey of binary characteristics in WR stars, I re-observed this system for about 6 weeks. The lower two curves show the results of my broad-band UBV photometry. The middle curve shows the variation in the yellow; the lower curve, the results in the blue. There are several points of interest. First, the primary eclipse (WR star in front) is much deeper than that observed by Gaposchkin: the blue shows a depth of 0.55 mag, and the yellow, 0.49 mag. At first we attributed the discrepancy to the fact that Gaposchkin had used a slightly erroneous period furnished by Hiltner. However, there is a faint possibility that the envelope has actually changed, that the primary eclipse is deeper because the WR envelope is bigger. The second point of interest is the difference in the depth of the eclipse in the two colors: the blue eclipse is deeper than the yellow by about 0.06 mag, which is well above the errors of the photometry. Studies of the variation in emission lines during eclipses would be of great interest in connection with this system. The system has a period of 29.640 days and takes about a week to go through primary eclipse, so detailed spectroscopic studies could be done in leisure. Finally, if you wonder why I have no data on the secondary eclipse, it is presumably because it occurred during a period of bad weather.

Kuhi: First, although bright, the system is still a magnitude fainter than V444 Cygni, so the observing time required for a given accuracy is greater. Second, the star has been observed photoelectrically at Lick in the hope of detecting the secondary eclipse which is, of course, the one that should be observed for changes in the intensity of emission lines. No secondary eclipse has been detected to within the accuracy of the UBV photometry, i.e., to something like 0.01 mag. It is still possible you might find an eclipse in some of the emission lines, and it would be especially worthwhile to pursue this. However, as we do not know the exact date of the secondary to within two days, I would hesitate to ask for time on a large telescope. Perhaps it would be worthwhile to try a smaller telescope, using narrow band filters (e.g., on λ4686 of He II) and integrating over longer periods of time. Then if something happens, we can go to the 120-inch.

S. Gaposchkin: I am delighted to see you are working on this system. As you mentioned, it is one of the few examples of a WC eclipsing binary. So if we can tie down this secondary minimum, we can determine the sizes and masses of the two components. I personally find you spectroscopists the wildest and most entertaining group of all the astronomers. I think this is because spectra are enigmatic phenomena. Struve once told me that he can take one spectral plate, work on it for an entire year, and get something out of it. This contrasts with my own need of 1000 plates to get the right answer for an eclipsing system. One plate gives the spectroscopist a year's work; 1000 plates give me one set of numbers.

Regarding Kuhi's statement that his observations of binaries are a mess, I think it is a most revealing mess. He does great injustice to his work. I think he can be the first to give a real structure, in a graphic way, to a real WR star. Figure 23 shows an exact dimensional picture of V444 Cygni; the inner structure of the star is

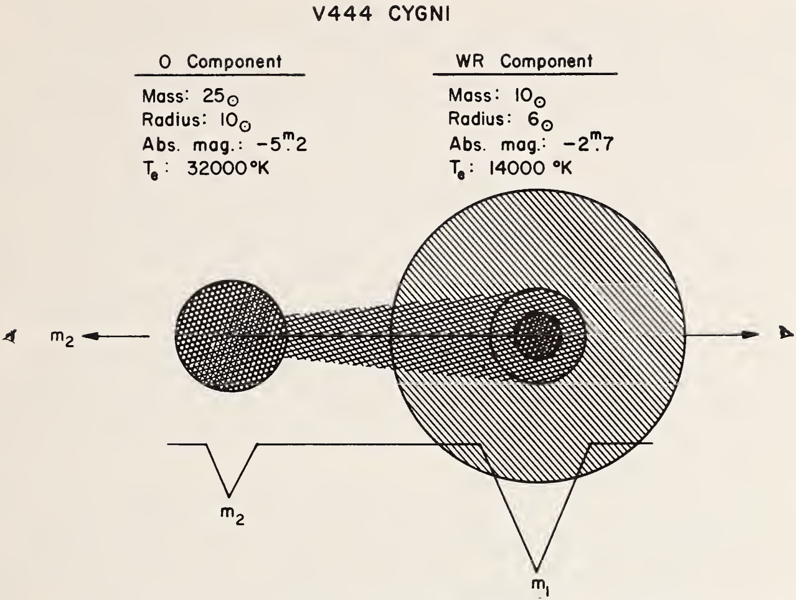

Figure 23. The best and the only unambiguously determined WR + O system as illustrated by Sergie Gaposchkin.

schematic; the eclipse light curve is below. Kuhi, you should be able to use your spectroscopic data to come up with an equally precise picture of the structure.

Kuhi: The real problem, however, is that I do not have geometrical effects alone to consider. There are all those other things I mentioned in my talk that confuse the situation. I wish it were possible to interpret eclipse profiles in terms of geometry alone.

Sahade: In considering the WR binaries, we are discussing two main problems. One is connected with the He I lines, which show dilution effects; the other is connected with the emission-line shape. There is the strong evidence that the asymmetries of structure observed in emission lines (e.g., He II λ4686) come from material located between the two stars. This structure has been observed several times in γ_2 Vel, the first time by Perrine.

Hanbury Brown: The measurements of Bappu and Ganesh, which show an orbital velocity ranging from +200km/sec to -100km/sec and a period of 78.5 days, were taken in the C III-IV emission complex at λ4652. We measured the apparent angular size of the system in this same line and found it to be about 5 times the size of the star. We therefore appear to have a region 5 times the stellar diameter orbiting in a binary system. Is this regarded as a reasonable model?

Underhill: The whole star is orbiting at this speed. The extended atmosphere moves with the star. That is perfectly normal for an early type binary.

Thomas: When Anne says this is perfectly normal, she means it is normal by assumption. In all such discussions, including those on mass exchange, the detailed aerodynamics of the problem are generally neglected. Clearly, there is an interesting set of problems to be solved. Possibly we are being unduly optimistic in supposing we can interpret the spectral phenomena without simultaneously solving the aerodynamic problem.

Sahade: Let me remind you of Kuhi's remark that He I λ3888 is always observed as a strong line and is always greatly displaced - by the same amount - to the ultraviolet. In stars like γ_2 Velorum, the line has several components less displaced to the violet, the position and number of which undergo very pronounced changes. *Sky and Telescope* of 1956-57 contains a reproduction of consecutive plates of γ_2 Velorum, and you can see how fast these changes occur. A similar thing happens in the well-known peculiar system of β Lyrae, which is also surrounded by a large expanding envelope, as indicated by the presence of a violet-displaced line of He I λ3888, again with several less-displaced components.

Regarding the He II λ4686 emission, γ_2 Velorum also displays a structure which may be connected, as in V444 Cygni, with matter streaming toward the companion star. The structure of λ4686 in V444 Cygni is not simple. Sometimes the relatively narrow, superimposed emission looks double, and sometimes there is even a sharp absorption cutting in. In γ_2 Velorum, the feature is even more complicated.

Kuhi: I must say that in V444 Cygni nothing really correlated with anything; once you tried to correct for secondary effects, there were no clear-cut correlations. There was no clear cut correlation between eclipse curves (shape and depth) and ionization potential. Individual lines from different ions behaved differently, as did different lines from the same ion. I think the situation with CQ Cephei is worse. Indeed, I think CQ Cephei will probably confuse the interpretation of WR stars more than it will solve it because it is a contact binary and we are dealing with the interaction between two stars.

Underhill: The purpose of this symposium is to try to establish a body of observational facts on which to base our theory and interpretation. So although these complicated systems can be used for masses and radii, I think we should put them aside and concentrate on a select group of quiet, well-behaved objects.

Sahade: Yes, but what if all WR stars turn out to be binaries? At this point we are discussing binaries; if they happen to be complicated, I cannot help that. But to judge from Kuhi's comment, I think we have exhausted this subject.

Schmidt-Kaler: As a postscript to this and to Lindsey's summary in section A, I would like to suggest a possible connection between nebulosities and a new type of stellar aggregate which we have discovered and called "stellar rings" [*Veroff. Bochum*, No. 1, in press]. A stellar ring is a cluster of stars which appears as a regular elliptical ring with a very sharp outer boundary. The thickness of the ring is about 1/30 the minor diameter. The number of stars involved averages 70 and may go up to 200, so the density is considerably higher than that of the general stellar field. For six objects with photometric distance moduli, we obtain a unique minor diameter of 7.1 psc and ages of between 0.5 and 5×10^6 years. We believe these rings must be the result of an expansion process.

In searching the Palomar Sky Survey prints for the precursors of stellar rings, we found a number of emission objects that display the same characteristics as the rings, except that they are gaseous instead of stellar. Their properties are summarized in Table 6. One might interpret these data as suggesting that these nebulae represent a continuous transition from the simple ionization front around a WR star, to the appearance of a shock front, to the formation of stars in a stellar ring, and finally to the decline of the ionization, leaving only the longer-lived stellar ring. Thus I would suggest that the WR stars act as a kind of "blasting cap" for star formation.

Johnson: The Russian astronomer Dolidze has found that emission-line stars are concentrated around supernova remnants - IC 443 was mentioned. He applied similar reasoning to the production of stars in compressed gases outside the expanding shell, only here it was a supernova that did the triggering.

TABLE 6

RING TYPE H II REGIONS

Sharpless number	Exciting star	Distance	Minor diameter	Description
308	HD 50896 WN5	1.59 kpc (Smith) 0.69 } open cluster 0.63 } Cr 121 (Schmidt-Kaler 1961, Feinstein 1968) 0.66 kpc adopted	5.8 pc ±0.9	An H II region with a very sharp ionization front, but almost no density increase to the edge (no ring).
162= NGC 7635	BD+60°2522 O7f	3.2 kpc (MK) 5.2 kpc (RV) 4.2 adopted	3.3 ±0.7	H II region with a strong and very sharp ionization front. A few stars embedded in it.
298= NGC 2359	HD56925 WN5	6.9 kpc (Smith) 5.1 (RV) 6.0 adopted	6.8 pc ±0.4	Same as Sh 162, embedded in the H II shell appears a ring-type star chain.
105= NGC 6888	HD 192163 WN6	2.29 kpc (Smith)	7.1	An elliptical filamentary nebula with a very sharp boundary. A little irregularity of form in the NW; in the E the edge of the nebula is suggested by very faint gaseous filaments only, but the elliptical figure is continued by a regular elliptical star chain.
284	not known			This is <u>not</u> a ring type H II region, however; it shows the same very regular and sharp filamentary boundaries.

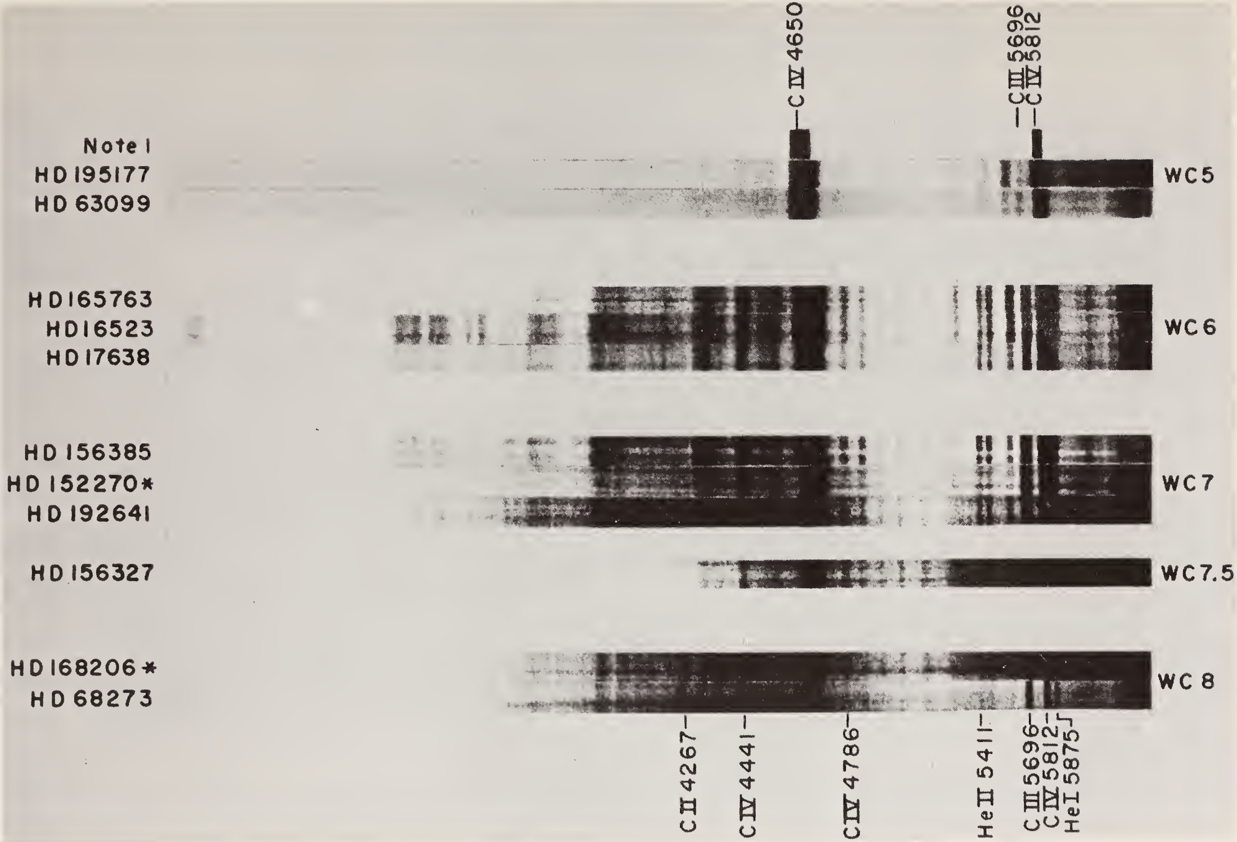

Figure 24. The WC sequence. Note 1. (1900) $\alpha = 21^h 46^m.4$; $\delta = +50°13'$.

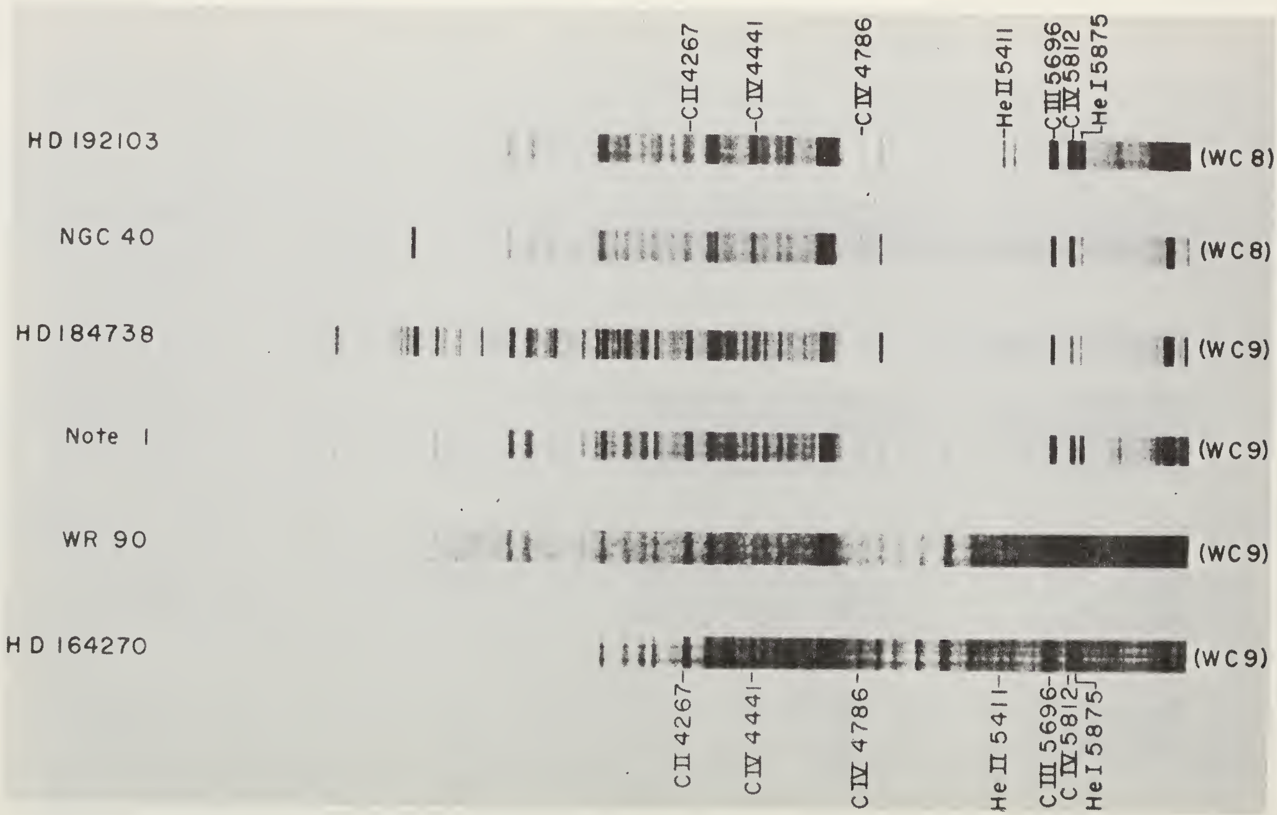

Figure 25. Wolf-Rayet carbon stars differing from the sequence illustrated in Figure 24. Note 1. (1875) $\alpha = 18^h31^m.1$; $-10° + 13'$. HD184738 is BD+30°3639, Campbell's hydrogen-envelope star.

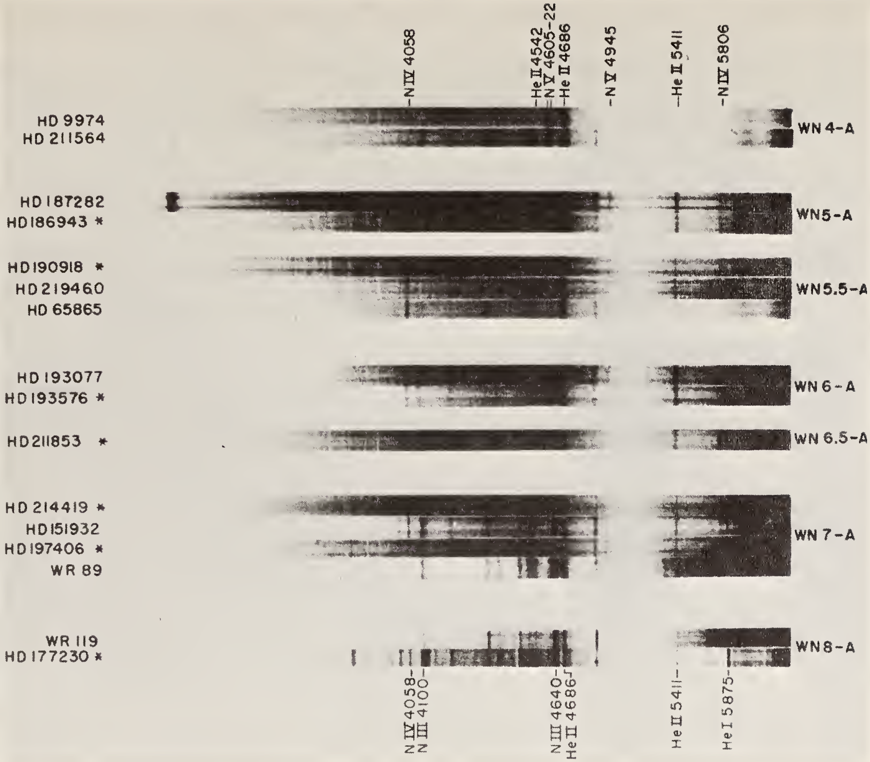

Figure 26. The WN-A sequence. The WR numbers refer to Roberts' (1962) catalogue. The spectrogram of WR 89 was taken with one-half the dispersion of the others. An asterisk indicates that the star is a known binary. HD9974, HD193077, and HD151932 may also be binary.

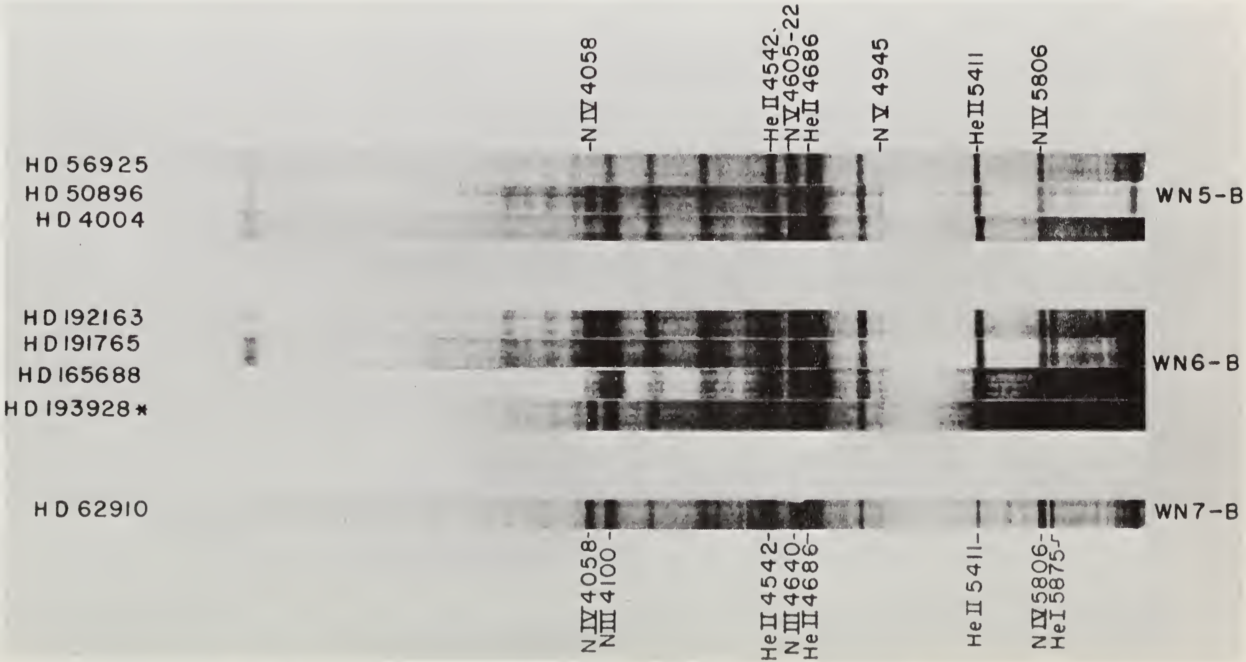

Figure 27. The WN-B sequence. HD193928 is the only known binary in the group. P = 21.6 days.

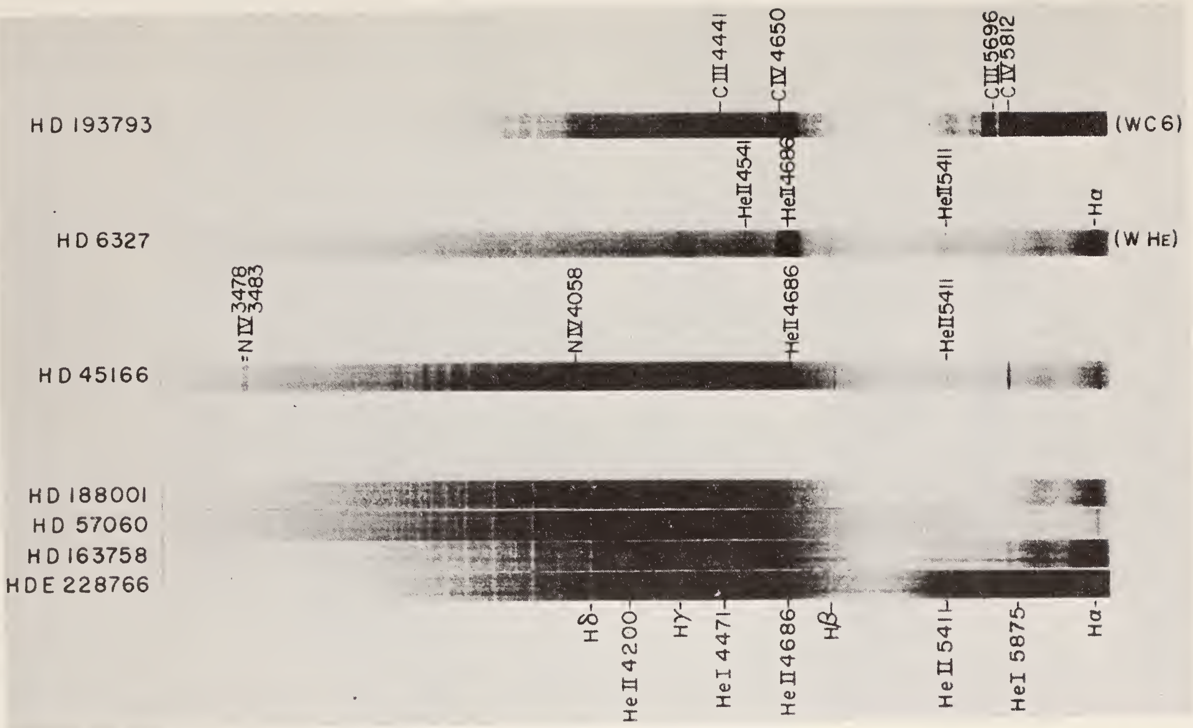

Figure 28. Miscellaneous stars. HD6327 seems to be Wolf-Rayet star without either nitrogen or carbon. HD45166 is a star often associated with Wolf-Rayet stars. Four Of stars are illustrated for comparison.

PART C

SPECTROSCOPIC DIAGNOSTICS, INTERPRETATION, AND ATMOSPHERIC MODELS

CHAIRMAN: *Lawrence H. Aller*

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INTRODUCTORY SPEAKER: *Anne B. Underhill*

Sonnenborgh Observatory, Utrecht

I. INTRODUCTION

The Wolf-Rayet stars have been selected as a group according to certain conspicuous characteristics of their spectra, which consist of a faint continuous spectrum crossed by rather strong, broad emission lines from He I, He II, C III, C IV, N III, N IV, N V, O III, O IV and O V. Other emission lines are present, but the most characteristic lines upon which the spectral classification system is based belong to these spectra. A few particular absorption lines are seen. The absorption lines are always displaced towards shorter wavelengths with respect to the emission lines, which usually have wavelengths consistent with the expected radial-velocity component of the peculiar motion of the star. In this discussion variations in the spectrum due to orbital motion in a binary system and to blending with the spectrum of a companion will be considered to be of secondary importance. They offer merely an extra source of confusion which should be avoided so far as possible by a careful selection of objects for study.

An attempt will be made to show what the observed spectroscopic details imply concerning the physical conditions in the atmospheres of Wolf-Rayet stars. Of themselves these conclusions say nothing about the evolutionary stage of Wolf-Rayet stars. However, when the information about atmospheric conditions is coupled with other astronomical information about Wolf-Rayet stars and about other stars, it should be possible to draw a self-consistent picture of the position of Wolf-Rayet stars in the general pattern of star formation and evolution. The present discussion will conclude with a few remarks on this subject.

Spectral classes give an empirically arranged ordering of stars according to changes of a few characteristic features in the spectra of the stars. One may not assume without further investigation that the selected criteria lead to classes arranged monotonically according to a theoretically significant parameter such as the effective temperature of the star. One of the goals of the present discussion is to see if it is possible to find criteria which do arrange Wolf-Rayet stars in a sequence according to effective temperature. Effective temperature is selected as basic because it is a significant parameter in the theories of stellar structure and evolution.

To go from observed spectral detail to effective temperature implies possession of a satisfactory theoretical understanding of the formation of stellar spectra. We do not have this understanding at present. Much of what will be presented here is an attempt to formulate the chief problems in understanding Wolf-Rayet spectra. More questions arise than can be answered. However, progress will have been made if we can establish some of the significant questions.

In Section II a brief summary is given of the relevant spectroscopic information. Full references for this material can be found in other papers presented at this conference and in Underhill (1968). The chief aspects of the spectroscopic diagnostic process are discussed in Section III, while in Section IV a qualitative model of a Wolf-Rayet atmosphere is presented. Clues concerning the evolutionary stage of the Wolf-Rayet stars are discussed in Section V.

II. THE TYPES OF SPECTROSCOPIC INFORMATION

a. The Continuous Spectrum

In comparison to O-stars the WN stars have a definite ultraviolet excess which begins near 4000 Å; the WC stars have only a slight ultraviolet excess, if any. Through the green-blue-violet spectral region the relative energy distribution in the continuous spectrum of Wolf-Rayet stars is very similar to that of O-stars. Both WN and WC stars seem to have a small infrared excess. It is difficult to say how large this excess is because the correction for interstellar reddening is a critical factor in determining the true infrared excess.

From a study of eclipse depths in different wavelengths at secondary minimum Kuhi (1968) has shown that the Wolf-Rayet component of V444 Cygni definitely brightens in the infrared with respect to its O-type companion. This binary system contains gas streams. One cannot say surely whether the brightening in the infrared is associated with the Wolf-Rayet star itself or with gas in the binary system lying in the neighborhood of the Wolf-Rayet star. Such gas would be expected to radiate a free-free continuum in the infrared.

b. The Line Spectrum

It is a significant fact that the emission and absorption lines of Wolf-Rayet spectra cannot be regarded as being formed in one body of gas at one representative temperature and pressure. Rather one must acknowledge that the observed spectrum is a composite of features which are formed under quite different circumstances. Progress in understanding the meaning of Wolf-Rayet spectra is dependent on the correct grouping of lines together for interpretive purposes. Successful analysis is hindered by the fact that we have only a limited spectral range accessible for study. The available information can be conveniently grouped under four headings.

1. Shapes of the emission lines

(i) There are a few flat-topped emission lines which in some cases are accompanied by a shortward-displaced absorption component. These line shapes can be explained quite satisfactorily by the classical theory of a spherical expanding atmosphere. In WC stars the line C III λ5696 gives an outstanding example of a flat-topped profile not accompanied by an absorption component. In WN stars, and in some WC stars, the lines He I λ5876 and λ3888 have a flat-topped emission profile accompanied by a strong shortward-displaced absorption component. In WN6 stars the line N IV λ4057 appears to be broad and flat-topped in emission, but there is no clear absorption component. On the other hand the rounded emission line from the multiplet N IV λ3478-82 is usually accompanied by a strong shortward-displaced absorption component. In WC stars the rounded emission line from the C III λ4650 multiplet behaves in a similar manner to N IV λ3478-82. One cannot isolate a flat-topped emission line for either of these multiplets.

The lines mentioned above are the chief lines in Wolf-Rayet spectra that have profiles that can be explained, at least in part, by the simple hypothesis of a spherical expanding envelope. It is worthy of note that most of these lines appear in emission in Of stars as a result of particular processes or are known to be strengthened in absorption by dilution effects.

The spectrum profile of HD193793 (WC6 + O) in the region 5630 to 5960 Å is shown in Figure 1. The flat-topped profile of C III λ5696 is clearly

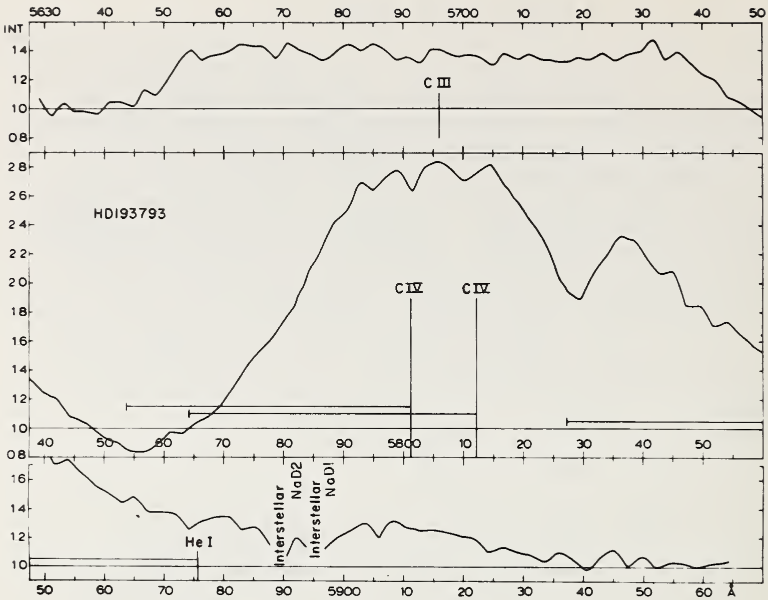

Figure 1. The spectrum profile of HD193793, WC6 + O, between 5630 and 5960 Å.

evident and also the flat-topped profile and displaced absorption of He I λ5876. The C IV lines are discussed below. The spectrum of this binary star has been chosen to illustrate the features formed in the expanding atmosphere because the Wolf-Rayet spectrum is seen blended with the essentially continuous O-type spectrum. Consequently the intensity of the emission lines is nowhere so great with respect to the continuous spectrum that the photographic photometry is seriously in error. The flat tops are real.

The flat-topped lines in the spectrum of HD191765, WN6, may be seen in Figures 2 and 3. The flat character of the He I λ5876 emission is striking; that of the weak emission due to He I λ3888 is less conspicuous, although the shortward-displaced absorption core is strong. The broad rather flat strong feature due to N IV λ4057 is not seriously distorted by blends. In addition to the components indicated in the diagram, the Si IV lines λ4088 and λ4116 contribute strongly to the blend at 4100 Å.

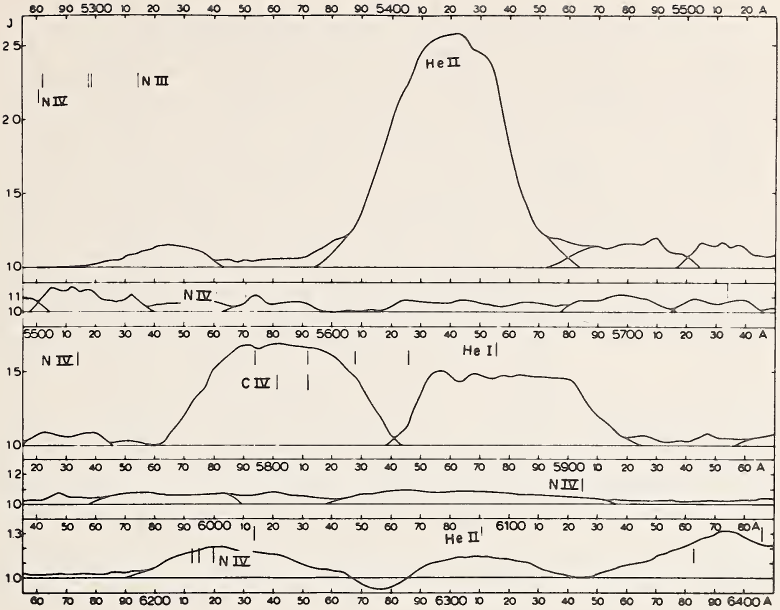

Figure 2. The spectrum profile of HD191765, WN6, between 5300 and 6400 Å.

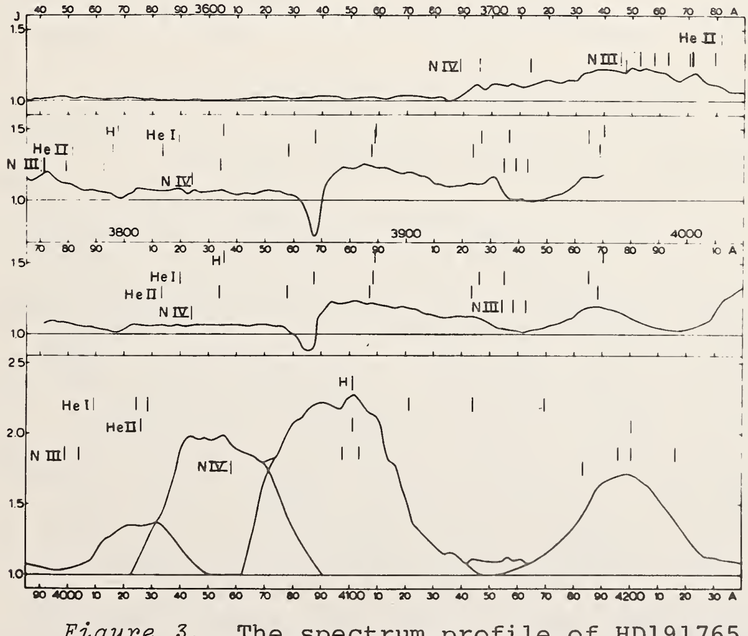

Figure 3. The spectrum profile of HD191765, WN6, between 3540 and 4200 Å.

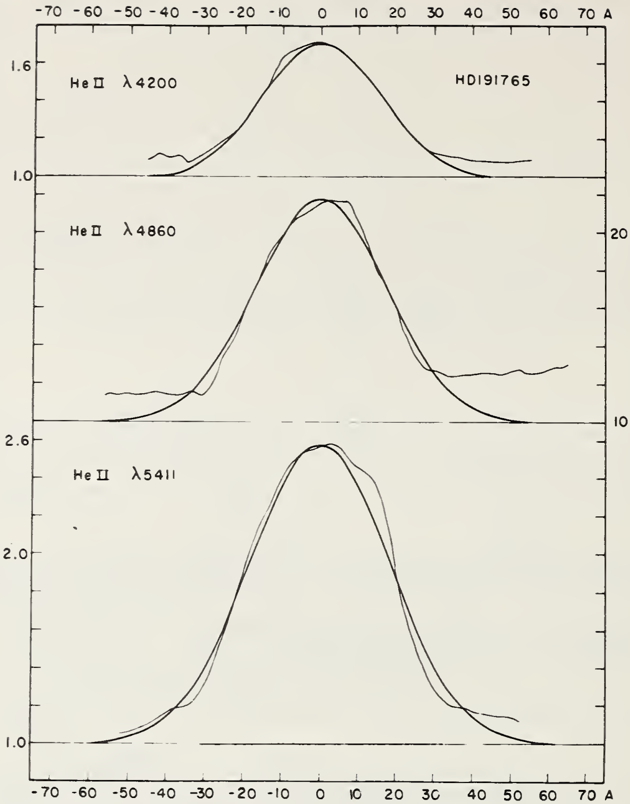

Figure 4. Some He II profiles in the spectrum of HD191765 fitted by a Gaussian profile.

(ii) Most of the emission lines in Wolf-Rayet spectra have a rounded shape that can be approximated rather well by a Gaussian function. In many cases the apparent "line" is a blend of several components. In the case of the He II lines however the blending is not severe. Some typical He II profiles in the spectrum of HD191765 can be seen in Figures 2 and 3. Some of the He II profiles are shown fitted by a Gaussian profile in Figure 4. The total width at half-intensity of the He II lines of HD191765 in velocity units remains essentially constant at about 2380 km/sec for the whole spectrum from λ3203 to λ6683. This width is the same for lines of the n = 3, 4 and 5 series. This rounded shape is the usual case for emission lines in Wolf-Rayet spectra. The flat-topped lines form an exception.

(iii) The emission lines of the WN7 stars are much sharper than those of most Wolf-Rayet stars, resembling in this respect the emission lines of Of stars more closely than those of an average Wolf-Rayet star. This point is illustrated by Figures 5 and 6, which show part of the spectrum profile of three WN7 stars and of the WN6 star HD191765, as well as by the photographs of spectra presented by Hiltner and Schild (1966) and by Smith (1968). Each of the stars HD93131 and HD92740 seems to be accompanied by a companion, for absorption cores are visible at Hγ and Hδ, although this point has not been noticed by those doing spectral classification.

Hiltner and Schild have noticed that the emission lines in WN binaries are usually sharper than those of WN stars not known to have a companion. A point to be considered when interpreting Wolf-Rayet spectra, and in particular those classified as WN7 and WN8, is whether some of the emission lines are formed in gaseous streams or in an extended envelope around the complete (binary) system. A similar problem is posed by the spectra of Wolf-Rayet stars

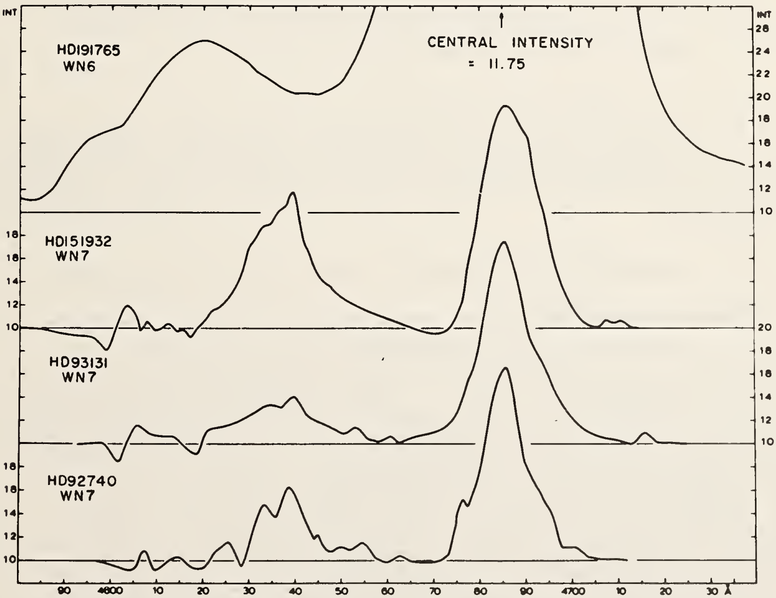

Figure 5. A comparison of the spectrum profiles of three WN7 stars with that of a WN6 star in the spectral region 4580 to 4740 Å.

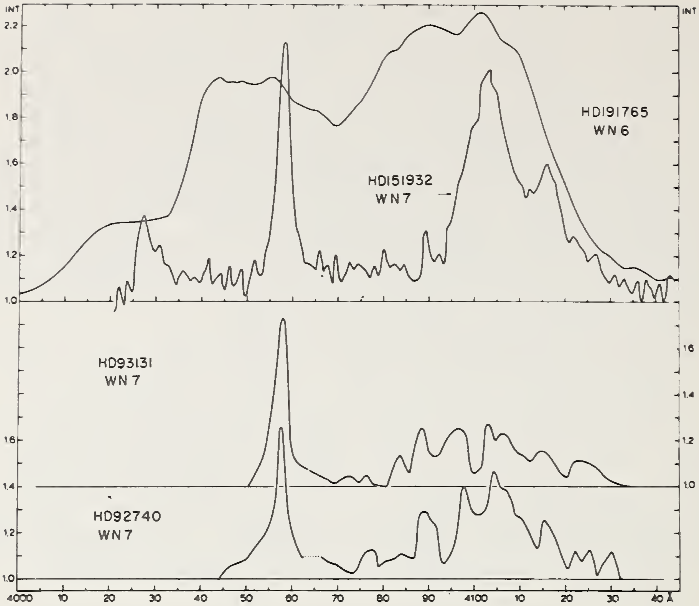

Figure 6. A comparison of the spectrum profiles of three WN7 stars with that of a WN7 star in the region 4000 to 4140 Å.

which are the nuclei of planetary nebulae. For purposes of interpretation, one must separate the lines formed in the nebula from those formed in the stellar atmosphere.

2. *Shortward-displaced absorption lines from normal levels.*

Shortward-displaced absorption lines are most simply interpreted as being due to absorption in that part of an expanding shell which is projected against the stellar disk. Some lines are known to be strengthened in absorption owing to dilution effects in an extended atmosphere. Dilution effects occur because there are metastable levels in the term scheme of the ion. Such lines are the most conspicuous absorption lines in Wolf-Rayet spectra. However a few lines from normal levels are also seen as shortward-displaced absorption components. The most conspicuous lines are C IV λ5801 and λ5812 in WC stars (see Figure 1) and N V λ4603 and λ4620 in WN stars (see Figure 5). Occasionally in WN stars He II λ4686 and λ3203 and the strongest lines of the Pickering series appear as shortward-displaced absorption components. Each of these absorption lines originates from a "terminal" ion; either the ion is the last possible ion which has an absorption line spectrum, or a great amount of energy is needed to reach the next stage of ionization. The ionization potential of the fourth ion of carbon is 391.986 volts, while that of the fifth ion of nitrogen is 551.925 volts. Hence the absorption spectra of He II, C IV and N V persist through a wider range in temperature than the absorption spectra from lower ions. Shortward-displaced absorption components are not seen accompanying all lines in Wolf-Rayet spectra.

Since the absorption lines C IV λ5801, λ5812, N V λ4603, λ4620 and He II λ4686, λ5411, etc. are subordinate lines from normal levels, qualitative reasoning suggests that the electron temperature in the part of the atmosphere where these lines are formed varies directly as the excitation potential (see Table 1) of the lower level. The O VI lines have not yet been observed in absorption in Wolf-Rayet spectra to my knowledge. The C IV lines are observed in absorption only in WC stars. These arguments suggest that the electron temperature in the expanding shell of a WC star is lower than in the expanding shell of a WN star.

3. Long series of lines from hydrogen-like spectra

According to the arguments of Inglis and Teller (1939), if the net charge on the nucleus or core of the ion is Z, the electron density is related to the last visible line of a Rydberg series as follows:

$$\log N_e = 23.26 - 7.5 \log n + 4.5 \log Z.$$

TABLE 1

Spectrum	Level	E. P.	Lines
C IV	3^2S	37.54 volts	5801, 5812
He II	n=4	51.00	5411, 4541
N V	3^2S	56.54	4603, 4620
O VI	3^2S	79.33	3811, 3834

Thus if the hydrogen series breaks off at n = 16, the He II series will break off at n = 24 and the C IV series at n = 37. Long series of He II and of C IV lines are seen in Wolf-Rayet spectra, but this does not mean that the electron density is particularly low. If the hydrogen series breaks off at n = 16, the electron density is about 10^{14}. The available observations do not permit a precise estimate of the electron density in this way because it is impossible to distinguish the Rydberg series of He II and C IV to sufficiently high values of n.

4. The composition of the spectrum

It is well known that lines from the ions of carbon and oxygen dominate the spectrum of WC stars, whereas lines of the nitrogen ions dominate the spectra of WN stars. Both types of Wolf-Rayet stars contain lines from He I and He II, while the hydrogen lines seem to be significant only in the stars of types WC8 and WC9. Since the lines of the Balmer series of hydrogen blend with lines of the Pickering series of He II, it is difficult to separate the contribution of H from that of He II. If the decrement from Hα to λ5411 to Hβ to λ4541 to Hγ is fairly smooth, one may assume that the contribution due to hydrogen is small.

Careful inspection of moderate dispersion spectrograms of Wolf-Rayet stars has shown that weak lines of the nitrogen ions are present in WC spectra, while lines of O V and of O IV are quite prominent in WN spectra. The C IV blend λλ5801, 5812 is definitely present in WN stars, though this blend is much weaker than it is in WC stars. The corresponding multiplet of O VI at 3811 and 3834 Å appears in emission in a few Wolf-Rayet spectra; some of these stars have been classified as WC, others as WN. Three stars showing these lines are HD115473 (WC5), γ_2 Velorum (WC8 + O7), and HD104994 (WN3).

There is no sound reason for thinking that the observed relative intensities of the emission lines in Wolf-Rayet spectra indicate gross abundance differences between the various spectral classes. Quite clearly a detailed theory of spectrum formation must be developed before any conclusions about abundances can be drawn. The particular excitation processes which are active in the atmospheres must be considered in detail.

III. SPECTROSCOPIC DIAGNOSTICS AND INTERPRETATION

At present very little quantitative data about Wolf-Rayet spectra is available for analysis and no sophisticated and fully satisfactory theories of spectrum formation exist to turn this data into quantitative parameters such as electron temperatures, electron densities, abundances and state of motion of the atmosphere. In this section some qualitative arguments will be reviewed which indicate with what temperature and pressure range we seem to be concerned, and a brief summary will be given of the physical representations which have been considered. The geometric properties of the atmosphere (shape of the atmosphere and its state of motion) also have an important influence on the observed spectrum.

a. Evidence Concerning the Temperatures of Wolf-Rayet Stars

1. From the continuous spectrum

The energy distribution in the continuous spectrum between 3300 Å and 10000 Å, when corrected for interstellar reddening, is rather similar to that of early B-stars or O-stars. Photometric arguments similar to those used for OB-stars, the observed magnitudes having been corrected for the emission bands, suggest that the color temperatures of WC stars are near 2×10^4°K while those of WN stars are near 3.8×10^4°K (Pyper 1966). When the color temperature is greater than 2×10^4°K, the energy distribution between about 4000 Å and 7000 Å is not an accurate indicator of temperature; the color temperatures might be 10^4°K higher, certainly not much lower.

The ultraviolet excesses of the WN stars and the possible infrared excess cannot be explained by the usual theories of color temperature. Bless, Fischel and Stecher (1968), and Wallerstein (1968) who expands an idea due to Nariai (1967), have made the interesting suggestion that some Wolf-Rayet stars may be surrounded by a hot corona generated by the expanding atmosphere colliding with the surrounding nebula or with the interstellar medium. It is postulated that x rays are generated in this corona, and evidence has been compiled that some degree of correlation exists between Wolf-Rayet stars and x-ray sources. The x rays will be accompanied by high-speed electrons, and it seems possible that the observed ultraviolet excesses of the WN stars may be a result of the interactions between radiation and matter in the postulated corona. Since the spectra of T Tauri stars also have some characteristics similar to the spectra of Wolf-Rayet stars (ultraviolet excesses, broad emission lines typical of temperatures near 10^4 °K, and a few shortward-displaced absorption cores), one may infer that in these cases, too, we are observing the effects of fast moving material impinging on an interstellar medium or on a shell of not too low density. A detailed physical picture of what may occur must still be developed and tested.

The ejection velocity of the shell from the Wolf-Rayet, Of or T Tauri star cannot be wholly responsible for the ultraviolet excess, since WC stars have as large velocities of ejection as WN stars, and both are greater than those of T Tauri stars; yet only the T Tauri stars and WN stars are observed to have definite ultraviolet excesses. In the case of WN stars the ultraviolet excesses cannot be attributed to emission in the Balmer continuum. Bless, Fischel and Stecher have suggested that the expanding atmospheres around Of stars might also generate hot coronae and x rays. However, Of stars are not known to possess significant ultraviolet excesses, although some of them appear to possess infrared excesses.

2. *From the line spectrum*

In Section II, *b*, 2, it was noted that since shortward-displaced absorption components of C IV λλ5801,12 are seen in WC spectra but not in WN spectra, whereas shortward-displaced absorption components of N V λλ4603,20 are seen in WN spectra but not in WC spectra, the electron temperature in the expanding atmospheres of WC stars is lower than that in the expanding atmospheres of WN stars. In addition somewhat qualitative arguments have been given by Underhill (1958), indicating that the observed difference in composition of WC spectra from WN spectra may be interpreted as evidence for a different effective temperature of the underlying star, the effective temperatures of WC stars being about 3×10^4 °K while those of WN stars may be as high as 5×10^4 °K.

The great complexity of WC spectra is largely due to the presence of the strong, rich spectra of C III, O III and O IV. On the whole WN spectra contain fewer lines, and if the well-known N III multiplet λλ4634,41 is set aside as being excited chiefly in an outer nebula, the remaining strong lines of N IV, N V, O IV and O V give an impression that the level of excitation in WN atmospheres is higher than that in WC atmospheres.

Using the idea that the spectra are collisionally excited, Bappu (1968) has estimated excitation temperatures for five Wolf-Rayet stars. His results are given in Table 2.

The electron temperatures in the atmospheres of Wolf-Rayet stars are not known well. The most probable range seems to be 2.4×10^4°K to about 5×10^4°K in the part of the atmosphere where the rounded emission lines are formed. Estimates made by others can be found in the references given in Section III, *c*, 2.

b. Evidence Concerning the Electron Density in Wolf-Rayet Atmospheres

In Section II, *b*, 3 it was pointed out that the Inglis-Teller formula allowed one to place a loose upper limit of 10^{14} on the electron density. No sharp forbidden lines, such as are observed for planetary nebulae, appear in the spectra of Wolf-Rayet stars. Furthermore one does not observe sharp, narrow absorption lines. The electron density is probably well above that in planetary nebulae. Model atmosphere calculations for O-and B-stars suggest that electron densities in the neighborhood of 10^{11} to 10^{13} may be appropriate for the inner parts of Wolf-Rayet atmospheres. Wallerstein

TABLE 2

HD192163	WN6	32 400°K	from lines of He II
HD165763	WC6	56 900	from lines of C IV
HD192641	WC7 + Be	51 600	from lines of C IV
HD192103	WC7	38 000	from lines of C IV
HD184738	WC8	24 000	from lines of C IV

(1968) estimates 10^{11} from the x-ray intensity of a source which he relates to HD211853.

c. *The Types of Physical Process to be Considered*

Three aspects of the interactions between radiation, atoms and ions that lead to the phenomenon we call a Wolf-Rayet spectrum must be considered. The first is the mechanism of spectral line broadening, while the second is the radiative transfer process itself, and the third is the geometry of the situation.

1. *Physical mechanisms of line broadening*

Three major processes may be considered: Stark effect, Doppler broadening, and the effects of non-coherent electron scattering. Because the shapes of the He II lines appear to be independent of the lower quantum number (n = 3, 4 or 5) and of the upper quantum number (n = 4 to 14 or so), one may conclude that Stark broadening is not an important mechanism. If the electron densities lie in the range 10^{11} to 10^{13} as surmised, significant Stark broadening would not be expected. The fact that the shapes of unblended lines in any one spectrum have the same halfwidth and shape, on a velocity scale, and the fact that most of the lines have a rounded, Gaussian shape make it seem probable that Doppler broadening is an important cause of the observed line shapes. If large motions exist in a Wolf-Rayet atmosphere, say a Gaussian distribution of velocities with a most probable velocity of the order of 1000 km/sec, the atmosphere would be optically thin, and profiles of the observed shape might result. However the broadening resulting from electron scattering will also produce a rather rounded profile from an originally fairly sharp emission line. One critical point to be determined is how great an opacity of electrons would be required to produce the observed line shapes from an initially sharp line. It is true that the emission lines are usually rather wide in Wolf-Rayet stars (this width varies considerably from star to star), but the absorption lines, He I λ3888 in particular, are not very wide. One can perhaps limit the amount of electron scattering which is permitted by making sure that the absorption lines remain as narrow as they are observed. This problem of the balance between Doppler broadening and broadening by electron scattering cannot be solved without considering the geometrical figures of the atmosphere.

2. The radiative transfer processes

The first attempts to analyze Wolf-Rayet spectra in order to find temperatures started from the extreme hypotheses of nebular conditions (Beals 1934, 1940) and of thermodynamic equilibrium (Aller 1943), with only radiative processes considered to be of importance. The results were unsatisfactory and contradictory, and it was evident that more sophisticated theories would have to be developed. Attempts were made to improve the purely radiative theories by Zanstra and Weenen (1950), Miyamoto (1952) and by Rublev (1964). It became evident that the spectrum formation process in Wolf-Rayet atmospheres was more closely akin to that in Be stars or shell stars than to that in planetary nebulae. In the purely radiative theories it is usually assumed that the quantity and quality of the exciting and ionizing radiation at wavelengths below the Lyman limit can be described by a single temperature and a blackbody distribution. A consistent solution is sought to account for the observed equivalent widths of a few emission lines in terms of an effective temperature and a set of abundances. Little or no attention is paid to the shapes of the lines.

The importance of collisional excitation processes was emphasized by Thomas (1949). Quantitative application of these ideas has been done by Bappu (1958, 1968). The problem has been looked at in some detail by Code and Bless (1964) who have shown that protons and alpha particles moving with velocities of the order of 1000 km/sec have sufficient energy to generate the observed excitation. Collisional excitation is an attractive way of obtaining the observed wide range of excitation which appears in nearly every Wolf-Rayet spectrum.

It is difficult to interpret Wolf-Rayet spectra accurately and in a quantitative manner, for one must separate the spectrum into groups of lines which are formed under the same atmospheric conditions. Many years ago Cecilia Payne-Gaposhkin (1935) drew attention to the fact that Wolf-Rayet spectra are a collection of spectra from different sources. How to effect the needed separation is not yet certain. Great practical difficulties exist because there are, indeed, very few lines which are not seriously blended, and very little quantitative spectrophotometry has been done using spectra of adequate spectral purity.

3. The geometrical aspects

The concept of a simple, spherical, low density expanding atmosphere (Beals 1930, Chandrasekhar 1934) seems to be adequate for interpreting the shapes of a few lines. However as O. C. Wilson (1942) demonstrated by considering binary stars, the expanding-shell hypothesis is inadequate for explaining most of the lines in Wolf-Rayet spectra. One must consider that the rounded emission lines are formed in a rather compact atmosphere near the stellar photosphere. The effects on the line profiles of various geometric shapes and of various velocity fields have been explored by Bappu and Menzel (1954); Rublev (1960, 1962, 1963) has used Sobolov's treatment of the moving atmosphere problem to explore further. The fact that some shortward-displaced absorption components and some flat-topped emission lines exist seems to be definite evidence that matter is streaming from Wolf-Rayet stars. This streaming appears to be steady; the expansion velocity in any one star does not change. Velocities have been observed from a few hundred km/sec to about 2800 km/sec. Radiation pressure in the far-ultraviolet resonance lines may be the driving force. This question has not been thoroughly explored.

IV. A MODEL OF A TYPICAL WOLF-RAYET ATMOSPHERE

The spectroscopic detail observed in apparently single Wolf-Rayet stars such as HD192103 (WC7), HD191765 and HD192163 (WN6), can be understood, at least qualitatively, in terms of the following model which consists of three parts.

(i) First there is a photosphere which radiates in the continuous spectrum like an O-star. For a WC7 star the effective temperature may be about 3×10^4 °K, while for WN6 stars the effective temperature is probably at least 4×10^4 °K. A separate source of radiation, perhaps a hot corona, is required to generate the ultraviolet excess of the WN stars. Since most Wolf-Rayet stars are about as bright visually as O-stars (i.e., $-4 \leq M_V \leq -5$), the photosphere must have a radius like that of an O-

star. Seven to ten solar radii seems to be a reasonable range.

(ii) Second there is an inner compact atmosphere where all the emission lines with rounded profiles are formed. This atmosphere is rather opaque in line frequencies but transparent in continuum frequencies between 3000 and 10000 Å. If the width of the lines is chiefly due to Doppler broadening, the ions in this part of the atmosphere have motions such that $\langle v \rangle$ is of the order of 1000 km/sec. The thickness of the atmosphere may be one to two solar radii. The particle density probably lies in the range 10^{11} to 10^{14} particles per cm^3.

(iii) Outermost there is an expanding low-density atmosphere in which dilution effects and monochromatic fluorescence effects occur. This atmosphere can be considered to be due to the boiling off of the inner atmosphere. The evaporation could be generated by the Maxwell tail of the velocity distribution in the inner atmosphere. A typical radius is 17 solar radii (cf., the dimensions estimated for the eclipsing variable V444 Cygni). In WC stars the level of excitation in the expanding shell is 30 to 40 volts; in WN stars it is 50 to 60 volts.

The WN7 and WN8 stars appear to be very luminous, $M_V \sim -6.5$, like the OB-supergiants and P Cygni. Insufficient information exists to sketch a useful model.

V. CLUES REGARDING THE EVOLUTIONARY STAGE OF WOLF-RAYET STARS

The visual absolute magnitudes of most Wolf-Rayet stars lie between -4 and -5. Only the WN7 and WN8 stars appear to be as bright as -6.5. If the bolometric corrections are about the same as for O-stars, about 3 magnitudes, the Wolf-Rayet stars in general are about as bright bolometrically as O-stars, while the WN7 and WN8 stars are similar to OB-supergiants. From the seven double-lined spectroscopic binaries for which a mass ratio has been estimated, it is quite clear that the mass of the Wolf-Rayet star is about 1/3 that of its early B- or O-type companion. In no case is the mass greater than that of the O-or B-star, or even equal to it. This means that the mass of a Wolf-Rayet star probably falls in the range 4 to 10 solar masses. Nothing is known directly about the masses of WN7 and WN8 stars.

Arguments concerning the distribution of Wolf-Rayet stars put their ages at 10^8 years or less. Arguments concerning the sizes of the interstellar ring nebulae observed around some stars place their ages in the range 10^5 to 10^6 years. These discussions suggest that WC stars are older than WN stars.

The characteristic spectra of Wolf-Rayet stars are a direct result of the interactions between radiation and matter in plasmas having particle densities of the order of 10^{11} to 10^{14}, electron temperatures of the order of 3×10^4°K to 5×10^4°K, and a random velocity field with $\langle v \rangle$ of the order of 1000 km/sec, as well as a low-density gas streaming outward with a velocity between about 500 and 2800 km/sec. The temperature and velocity vary somewhat from star to star.

Since the normal Wolf-Rayet spectra have many details in common with both T Tauri stars, which are generally acknowledged to be in the first stages of contraction, and with the central stars of planetary nebulae, which are generally acknowledged to be at an advanced stage of evolution, it is clear that the characteristic appearance of Wolf-Rayet spectra is not a sufficient criterion to establish the evolutionary stage of these stars. The typical emission-line spectrum is merely a statement that the physical conditions described in the preceding paragraph exist in a plasma around the star. This set of conditions, apparently, can come into being at more than one stage in the lifetime of the star. What process makes just those physical conditions appear in the outer layers of a star with the result that the spectrum is of "Wolf-Rayet type" is at present unknown.

The above arguments concerning the ages of Wolf-Rayet stars and the fact that the Wolf-Rayet star is always the least massive star of the binary system (in those cases when two spectra are observed) make it seem probable that the Wolf-Rayet phase occurs while the star is contracting to the main sequence. On the other hand, it is possible that the mass ratio near 1/3 is the result of mass exchange in a close binary system. Then one must conclude that the Wolf-Rayet phase corresponds to a stage fairly far along in the evolution of a star. Those Wolf-Rayet stars which are the nuclei of planetary nebulae must be considered to be at a late stage of evolution. These stars seem to be different from "normal" Wolf-Rayet stars. The meager evidence about their visual absolute magnitudes places M_V near 0 or +1, and their radii appear to be of the order of the solar radius.

If one excludes the central stars of planetary nebulae from the discussion, then two choices are open for the evolutionary stage of Wolf-Rayet stars: either (1) they are still contracting to the main sequence, or (2) they are in a post-main sequence stage of development following mass exchange in a binary system. There appears to be no sure way of deciding between these alternatives on the basis of spectrum alone. It is true that the observed visual absolute magnitudes imply higher bolometric absolute magnitudes than the theory of contracting stars of known mass predicts. However this conclusion is true only if the assumed bolometric correction of about 3 magnitudes is correct. It is necessary to determine the bolometric corrections for Wolf-Rayet stars.

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DISCUSSION

Chairman: Lawrence H. Aller

[*Ed. comment:* The original plan of the symposium was that Part B should contain all the empirical material on spectral features and that Part C should be a digest of ideas on (1) the atmospheric conditions required to produce such spectra and (2) the models and physical effects required to produce the atmospheric conditions. Unfortunately, the literature contains few detailed quantitative investigations of the kind required for Part C. Miss Underhill therefore thought it best to explore the observational consequences of various suggestions for the interpretation of specific spectral features in terms of isolated atmospheric conditions. Hence much of her discussion supplemented Kuhi's in presenting further observational material illustrating her points.]

Aller: We shall start by considering any additional observational material relating to the problem of interpretation. Then we can continue to discuss the interpretation of the spectrum, and finally we can consider the models.

As the first item under "observational material", I would like to mention an observation by Gordon Wares and myself of the object η Carinae, which bears on the effect of electron scattering in an extended envelope. This object consists of a brilliantly red nucleus (which has a soft appearance under the best seeing conditions) 2 to 2.5" in diameter and a surrounding shell of about 10" in diameter. The spectrum of the stellar nucleus has been studied by many observers (see, e.g., Thackeray 1953, *M.N.R.A.S.*, 113, 211; Aller and Dunham 1966, *Ap. J.*, 146, 126; Rogers and Searle 1967, *M.N.R.A.S.*, 135, 90). It consists of a background continuum, the remarkable properties of which have been studied by Rogers and Searle; broad emission lines largely of hydrogen and ionized iron; sharp emission lines superposed on the broad emission lines; and occasional absorption lines on the violet edges of the emission lines. There is a number of forbidden lines of Fe^+, Ne^+ and S^+, but none of O^+. The broad emission lines and violet absorption edges resemble the spectra of Wolf-Rayet stars, but the latter show no counterpart of the sharp emission lines.

It is generally stated in the literature that the spectrum of the shell resembles that of the nucleus, so we decided to attempt a quantitative comparison. Figure 7 shows a series of spectra secured with a dispersion of 39 Å/mm with the 60-inch reflector at Cerro Tololo. The longest exposure on the nucleus is about 7 minutes and that on the shell (which is flanked by the companion spectrum) is about 33 minutes. This plate covers the region from 5015 to 4070 Å. Additional spectrograms cover the region 4100 to 3200 Å and 5000 to 6700 Å. The light from the shell has been regarded as nuclear light scattered by electrons, and we feel that this explanation must be the correct one. The emission lines in the shell spectrum all appear to be derived from the central nucleus. The sharp lines of the core are gone; nothing is left but diffuse features. The absorption lines, on the other hand, appear to be strengthened in the shell, which has thus Wolf-Rayetized the spectrum of the η Carinae nucleus. We appear to have here a graphic demonstration of the importance of electron scattering, which has been suggested by Münch, by Kopal and Shapley, and by Thomas as a significant mechanism in Wolf-Rayet stars. A quantitative study of these spectrograms is in progress.

Smith: You've shown that the violet absorption edges are more intense when you look toward the shell than when you look straight at the central star. But we've been interpreting these absorption edges as coming from the part of the shell between us and the star; so why should they be stronger when you're not looking at the star?

Aller: They are stronger only relative to the emission in the adjacent continuum. In an absolute sense, in terms of equivalent widths, they are weaker. This of course involves a radiative trans-

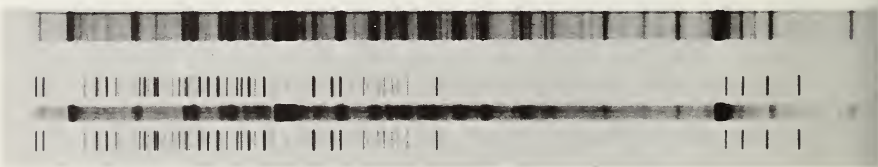

Figure 7. A comparison of spectra of the central nucleus of η Carinae (top) and of the outer shell (bottom), taken with the 60-inch telescope at Cerro Tololo. Wavelength increases to the right.

fer problem; one has to interpret line shapes as well as intensities. We're trying to do the photometry on these emission and absorption profiles now, in order to compare the two effects. I think the detailed quantitative interpretation will require the solution of a transfer problem.

Thomas: If we buy the conventional picture of an absorption line formed only in the material between the stellar disk and the observer, then an absorption edge will appear in the shell spectrum only if it has been scattered through 90°. What you are saying is that the effect of electron scattering is greater on the continuum and on the emission line than on the absorption edge - the absorption is washed out less than the emission.

Aller: That would be an obvious interpretation, but one also has to worry about excitation of lines in the outer shell. The reason we don't think there is much direct excitation of Fe II is the absence of lines with sharp cores in the shell spectrum.

Smith: Are you telling me that the conventional picture with the absorption core coming just from that part between us and the star is an utterly unrealistic simplification? We must include the scattered light from the shell?

Underhill: It's not utterly unrealistic. But in this case there seems to be a rather dense shell which is optically thick in strong lines such as hydrogen; hence they will be self-absorbed. If you look at very strong lines in an iron arc spectrum, you'll find self-absorbed cores, the shape of which depends upon the motion. Here we have exactly the same thing. The shell is optically thick - very thick - in the hydrogen lines; you cannot assume it has the same thickness in all lines. It's very simple.

Payne-Gaposchkin: Some facts on η Carinae should be mentioned in connection with the production of these line profiles. The first observation of η Carinae, made in 1887, by Agnes Clark, showed bright lines, whereas a spectrum taken at Harvard in 1889 showed only absorption lines. It resembled an F-supergiant with rather hazy lines. Some of the lines, such as those of hydrogen, showed faint bright edges.

Aller: The surrounding shell is usually interpreted as the result of an outburst in 1843 when the star reached maximum brightness. Unfortunately we have no spectroscopic observations so we really don't know what happened. The outburst in 1895 was less violent. The object has apparently settled down now to the spectrum I described. I have very carefully referred to this as an object rather than a star because of its large diameter; what it is, we don't know. If the density in the shell were higher, it might have resembled a WR star.

Wrubel: One type of datum that is obviously missing and that might contribute to our knowledge of electron scattering is measurements of the polarization of the emission lines. Kuhi has already said this is beyond his techniques at present. Does anyone else have any information?

Johnson: Hiltner obtained polarization data on V444 Cyg some years ago, but I don't know of any work since then.

Kuhi: Hiltner's work was all in the continuum. What about the lines?

Johnson: Apparently there is no such work. Polarization measurements can be made quite accurately now, so someone should certainly go into this problem. It might settle the question of electron scattering in these stars.

Aller: Possibly we could now say something about radio and x-ray observations of WR stars.

Pecker-Wimel: At the symposium on planetary nebulae last year, Davies announced that two normal WR stars had been observed at 11 cm. This should be very interesting to us because it should correspond at least to conditions in the solar chromosphere.

Hjellming: Was the resolution sufficiently good to be sure the radiation was not coming from a nebular shell?

Pecker-Wimel: They were two normal WR stars without nebulosity.

Aller: This could certainly be very significant because of the associated suggestion that WR stars are x-ray sources. We have already discussed this in the last session with respect to the two southern objects with strong O VI emission lines.

Stecher: This is an important question which might tell us something definitive about WR stars. The thermal expansion we have been talking about corresponds to 10^7°K. Eventually the particles will run into the interstellar medium and should produce x rays. There is also the possibility of line emission in the x-ray region, which I think we are just able to tackle instrumentally.

Thomas: Let's be very clear what you are talking about here. When you speak of a thermal expansion at 10^7°K, I assume you are referring to my figure of 20 years ago, when I said that <u>if</u> you want the WR atmosphere to be maintained in hydrostatic equilibrium with <u>a</u> temperature, it has to be 10^7°K. But I also said I didn't believe this temperature; I preferred instead a non-static atmosphere supported by some sort of jets. But this points up our real problem in trying to infer atmospheric structure from excitation levels. In the visual spectrum of the Sun, for example, there is nothing to indicate temperatures of the order of 10^6°K in the corona. And it is hard to infer from thermal effects at 10^6°K the existence of x rays and cosmic rays, such as are observed to come from the Sun. So for the Sun, you have a choice of looking for thermal emission of x rays in localized "very hot spots", or of looking for some non-thermal-emission processes of the sort that Biermann and Lüst have been seeking for many years. The same problem arises in WR stars, and this is why I have been trying to pin you down on excitation levels in various parts of the spectrum as a reflection of excitation levels in various parts of the atmosphere. Anne Underhill says she doesn't want to go higher than 5×10^4°K in the continuum. In the visual spectral regions of the line spectrum, we see N V and possibly O VI. I don't recall Stecher pointing out higher ionization than this in the rocket UV for $\lambda \gtrsim 1150$ Å. So if I assume collisional ionization in the usual non-LTE approach, the line excitation corresponds to some 2 to 3×10^5°K. But of course if T_e can be 10^6°K in the Sun, it is hard to imagine that with the evidence of an even greater supply of mechanical energy in WR stars we don't get T_e at least this high. The question is, how much higher? There has been much speculation on the kind of equilibrium configurations that can be reached as a function of mechanical energy input and atmospheric opacity, but I don't think any definitive conclusions have been reached. So if you want to start talking about x-ray sources, we are left with the same problem we have for the Sun. We will have to investigate atmospheric models as a function of mechanical energy input and of the radiative input, which fixes the photospheric model. Until this is done, we have no basis for distinguishing between thermal and non-thermal explanations of what we observe.

Stecher: The Doppler shift for, say, C IV corresponds to 0.25 meV C IV particles.

Thomas: What counts for excitation is differential motions. Don't forget that you won't get 0.25 meV collisional excitation from 0.25 meV heavy particles. It's the old story of heavy particle excitation not being as efficient as electron excitation.

Pecker-Wimel: We don't need to be so erudite. The WR stars with x-ray spectra have O VI; if they have O VI, that means they can have C V and you can get plenty of x rays from C V.

Smith: I would like to clarify this question of the presence of O VI lines in WR spectra. It has been said here that O VI is only observed in the spectra of a very few stars. However, in the Hiltner-Schild Atlas (Figure 24) [Figures 24-28 appear at the end of Part B, pp. 175-179], you can clearly see a pair of emission lines at λλ3811 and 3834 which correspond to the O VI doublet. You can see that they are present in the WC6 and WC7 spectra; in my spectra of WC5 stars they are also present.

Underhill: There are also possible blends of O III and O IV. I remember studying HD192103, and there were many lines in that region. I could not convince myself that the O VI lines were definitely present among the other lines, most of which were due to the less-ionized O-ions. I agree with Lindsey that in some of the hotter stars, O VI looks more definite.

Smith: HD192103 is a WC8 star, and I am not suggesting the presence of the O VI doublet in that star. I went through Edlen's list (*Vistas in Astronomy*, 2, 1456) and the revised multiplet tables, and I concluded that this λλ3811, 3834 pair is a blend of O VI and He II. Since the other emission lines in the same He II series on either side are very weak, the observed emission must be dominantly O VI. The appearance of the O VI doublet is similar in the WC5, 6 and 7 subclasses, becoming stronger as you go from WC7 to WC5 and are stronger still in the planetary nuclei mentioned by Aller.

Johnson: I would like to re-emphasize Aller's point that the spectra of the two proposed x-ray sources, NGC 5189 and GX3+1, are dominated by the O VI doublet and by He II λ4686. Since these objects are associated with x rays, they must either be very hot or contain a very hot source of radiation. Furthermore, the lines are very broad; I recall something like 10000 km/sec being mentioned.

I would like to discuss the problem of classifying a WR star that has neither C nor N lines visible in the spectrum. Maybe they are not WR stars, and we have to discuss them as examples of O VI. It is my impression that we have never reached an absolutely rigorous method of dropping stars into a slot labelled "WR star".

Thomas: I disagree. At the outset, I proposed four criteria that were broad enough to include all the objects we have just been discussing, yet specific enough to exclude a number of others on which questions have been raised at various times during this symposium. Whether you like my four criteria is another matter; but they do seem to me to summarize the unique features of the WR category of objects or spectra.

But let's return to the critical question. In Anne's summary, she stated that O VI was present in only two stars. Now I gather that if Lindsey is right, it has been observed in many more. If you are right, Lindsey, how many stars show O VI?

Smith: Everything hotter than WC7. That means 26 known classical WR stars in the Galaxy and 15 in the LMC, including binaries in which the O VI lines may not always be visible due to "drowning".

Schild: I must disagree on one point, and that concerns the He WR star HD6327 (Figure 28). It shows all the features of WR stars except that it does not show any lines of C, N, or O, at least in the blue and violet regions. We have only one spectrogram covering the region about λλ3400-6500, and we see nothing but He.

Thomas: It's not a WR star by my definition; it may be by yours.

Payne-Gaposchkin: You can vaguely see quite a number of absorption lines on your spectrum; have they been measured and identified?

Schild: As I recall, all the absorption lines are interstellar.

Johnson: It is interesting to me that no one is concerned about the absence of interstellar absorption lines in WR spectra. If they aren't present, could it be that many of these stars are closer than we think or that they lie in special regions of interstellar space?

A. Cox: Given the evidence for very high excitation levels in the region of line formation and for rather high T_e in the region of formation of the continuum, it would be interesting to know what little you can get from spectra about the hydrogen abundance. Perhaps there is significant mass-exchange and perhaps it leads to changes in internal abundance that affect the evolutionary track of the star; any information we could get about the hydrogen abundance would be useful as a check on these speculations.

Underhill: All you can say is that in the cooler WR stars - certainly in the WC7, 8, and 9 stars - you do get bumps in the Balmer decrement. So you might possibly say there is a little hydrogen there. I do not know of sufficient material for the WN7 or 8 stars to be able to check this. I've tried to do this in the broad-line WN6 stars, and the decrement was very slow and smooth, so I conclude the lines are certainly 90% He II. But again this only says H is there; I don't know how you would estimate how much.

Thomas: How can you expect to observe H in such hot objects? How far out in the atmosphere must you go before things get cooler; to the interstellar medium?

Underhill: That's the problem, because the H you observe is essentially in the interstellar medium.

A. Cox: Another point. You say that the WN stars have hotter photospheres than the WC stars. Does this have anything to do with the hydrogen abundance in the photosphere; is it a model atmosphere problem?

Underhill: I don't think so, but the model atmospheres on which I am basing my arguments are essentially hydrogen models. If you make a He model, and compute the intensity distribution, it's going to come out about the same, because the opacity still drops off as ν^{-3}. The He opacities come from the n=3 and 4 levels of He I and He II, and these are treated as hydrogen-like.

A. Cox: The point is that the ionization equilibria for H and He have a different temperature dependence. I would think that different relative amounts of H and He would give you different effective temperatures. Do I gather it is possible to say that H has an abundance somewhere from 0 to 70%, but we just don't know what?

Aller: If you admit HD45166 is a WR star - maybe Anne won't - then H is certainly present. It seems to be on the borderline with the Of stars, and I don't think we can easily reach any conclusion because of its extremely high temperature. But I would expect to find some evidence for H in some of these objects.

Beals: I have made some measurements of the Balmer decrement, and I am still convinced that there is a considerable amount of H in the WR stars.

Stecher: If there is no H, must there not be some other sources of opacity? The models of Hunger and Klinglesmith show that C is important.

Thomas: I am bothered by your dependence on the decrements of lines. It seems to me you are ignoring the transfer problem, hence the problem of line formation. Do I understand that you are simply looking at the observed decrement, comparing it to the He recombination spectrum you would expect from a thin atmosphere, and deciding on the presence or absence of H according to whether the two agree?

Underhill: Suppose we do assume a thin atmosphere. Suppose further we have a ratio of Hα to Hβ of between 5 and 3. Then we conclude there is hydrogen. On the other hand, suppose we have a ratio of between 4 and 2. Then we have two choices: Either there is very little hydrogen, or there are appreciable self-absorption effects. In other words, if we have a flat decrement, there is no unique interpretation. But if we have a steep decrement, I'll gamble and say there is a fair amount of hydrogen.

Thomas: This is indeed about as crude an analysis as it is possible to make. It ignores all the physics of line formation. If there is any opacity at all in the atmosphere or any collisional excitation I would not put much trust in such arguments.

Aller: It's quite evident that there is a lot of self-absorption in these stars, as I emphasized in my 1943 paper. You have to compare the Pickering and the Balmer series and the Balmer decrement as best you can, and also other lines for which good f-values are available. There is also a further point concerning the infrared. Have Low or others working in that region done anything on WR stars?

Kuhi: I have suggested to them that they look at these objects, but I don't know if they have found anything yet.

Aller: So the infrared is one of the things we should be studying with a large telescope. Then there is the related question of the infrared and ultraviolet excesses.

Kuhi: The first thing I'll do after this symposium is apply the new calibration of Hayes to these excesses. I think we can remove some of the infrared excess, but it's going to be difficult to remove all the ultraviolet. I refer to the excess with respect to a normal O-type star. The second point I want to make is that the comparison between the WR stars and the supergiants should be carried a little further. It is possible that whatever is producing the infrared excess in the WR star is also producing the same effect in OB-type supergiants. To the best of my knowledge, no one is studying this problem.

Smith: Let me see if I understand the current situation. In the old days, before the correction, the models agreed with OB-stars and not with WR stars. Now the models agree with WR stars, but not with OB-stars.

Underhill: No. The models never agreed in detail with OB-stars. There were always small but disturbing discrepancies, which started people worrying about the absolute calibration. This is now being straightened out.

Smith: Does this mean the energy distribution of a WR star is the same as of an OB-star?

Underhill: Not quite, although it is quite close. The WN's have a definite UV excess, while the WC's track quite closely. The point I want to make about the WR stars is that as recently as 10 years ago, people were using photographic photometry to get temperatures of 1.2×10^4°K. These values are now recognized to be greatly in error. Now suppose you make a standard model atmosphere for 5×10^4°K. If you include line-blanketing it gets chopped off below λ1500. Stecher's observations show that in the region λλ1500-3000, the model fits the observations very well. What we have just been talking about, however, is the region $\lambda \gtrsim 3300$ Å. In this region the O-stars and the WC's track quite well, but the WN's go much too high. It's hard to say what the small corrections may be at the infrared end of the spectrum, as the blackbody curve is small here, and small deviations correspond to large changes in effective temperature.

Stecher: The interstellar-grains people should be able to help us with the infrared correction. In my own spectra, I don't see the usual lines of an O-star; thus I conclude that I'm looking at a WR star. I think the best value for the temperature is about 4×10^4°K.

Roman: I'm still confused about the UV excess. WR stars appear to resemble supergiants: First, do we have any good comparisons between WR stars and supergiants? Second, do we have good models for supergiants? Finally, is the great number of emission lines found in the visible part of the WC spectrum related to the apparent differences between the WC's and the WN's in the UV?

Underhill: The answer to all three questions is no. To determine the continuum for the WC's is very difficult, although Kuhi has solved it fairly well. What convinces me of a real UV excess in WN's is that I can get a WN spectra down to λ3100 even when I cannot get spectra of WC's or O-stars.

Aller: I am puzzled by the alleged connection between the UV excess in WN stars and in T Tauri stars.

Kuhi: The UV excess in T Tauri occurs down around the Balmer continuum, increases in intensity toward the UV, and then drops off. In the brighter T Tauri stars, the UV excess correlates with the Hα emission, suggesting a Balmer origin. However, Walker has some spectra of fainter T Tauri stars which introduce a serious difficulty: whereas the Balmer lines decrease in intensity and disappear long before the Balmer continuum is reached, the UV continuous emission remains. In T Tauri stars there is also a sort of blue continuous emission which acts to fill in the absorption lines and makes them difficult to see. So I don't know if one can really say the WN and T Tauri color excesses are related.

Bessell: The color excesses for the T Tauri stars are determined with respect to the energy distribution of an F- or G-star. So a T Tauri star with a UV excess is an object like a G-star with enhanced UV radiation. This suggests a model for the T Tauri star of a cool star surrounded by a hot shell, which is similar to the WR model of a hot star surrounded by an even hotter shell. However the recent dynamical model of a T Tauri star constructed by Larson at Cal Tech is just the opposite - a hot star surrounded by a thick cool envelope. So perhaps we should think of a blue star with a red deficit, rather than a red star with a blue excess.

Underhill: But in addition to the shell features the T Tauri stars show a very definite underlying absorption line spectrum of G-or F-spectral type. Perhaps I introduced a red herring in pointing out the similarities between them and WR stars. My point was simply that both types of objects appear to have an expanding extended shell; they differ in their levels of excitation.

Bessell: But you really introduced T Tauri stars to support the hypothesis that WR stars are also coming onto the main sequence.

Underhill: I introduced them because I feel we know nothing about the kind of spectrum produced by an expanded, highly turbulent atmosphere. I asked myself what I would see if I dropped the excitation level in a WR star: the answer is the Ca^+ H and K lines and the H lines; they will be broad, and they may have displaced absorption cores - hence the T Tauri stars. I agree that the UV excesses are not a good point of comparison, since that of the T Tauri stars comes from the Balmer emission which doesn't exist in the WR stars.

Aller: If there is no further explicit discussion on the continuum, I suggest we move on to the interpretation of the lines, where we have three basic points: the relation between intensity and the population of energy levels; the question of thick versus thin atmospheric shells, which is connected with the problem of self-reversals; and the problem of radiative transfer in a moving atmosphere, where you may also have curvature effects.

Thomas: I am disappointed that thus far in the discussion of the interpretation of line spectra we have heard nothing that wasn't talked about 20 years ago. Anne's discussion of Bappu's 1968 work on T_{ex} is simply a rehash of the critique I made in 1948 of Aller's 1943 work. What we have done in these 20 years is develop quantitative methods for discussing stellar atmospheres and their spectra in terms of the actual physical situation and not simply an assumed set of physical relations. In order to establish some guide for our analysis, let me go back to my introductory remarks of the first day and ask you to consider what really characterizes the WR spectrum, makes it unique.

Three things stand out in the WR spectrum: (1) It is an emission-line spectrum; (2) the lines are very broad; (3) the excitation level of the lines is much greater than that of the continuum. I don't know of any classical stellar model that predicts such a situation. The closest situation for which we do have models is the solar chromosphere-corona, and these models are highly non-classical. If I want to produce emission lines, I must introduce a mechanism that either puts more energy into the lines than the continuum, or removes more energy from the continuum than the lines. The Schuster mechanism does the latter, but Katharine Gebbie and I have shown that it will not be very effective under these conditions. Various kinds of fluorescence can do the former, but whereas Anne relies heavily on them, I believe they are too fortuitous to explain the whole WR spectrum. The same objection applies to explanations in terms of nebular-type recombination spectra, even if such spectra were not already ruled out by point (3).

Underhill: I don't see how you can make the last remark when we know nothing about the continuum.

Thomas: But you are the one who won't let me push the continuum above 5×10^4°K; indeed most of the arguments here favor something closer to 3 to 4×10^4°K. You can't produce a radiation-induced recombination spectrum involving N V and O VI from the radiation field produced by a photosphere at $T_e < 5\times10^4$°K. So you are forced to adopt a chromosphere-corona type model in which mechanical heating is superposed on a continuous radiation field, and in which T_e increases outward through some part of the atmosphere and reaches values of at least 2 to 5×10^5°K. If we believe the Shapley-Kopal densities for V444 Cyg and Hanbury Brown's estimate of radii of the line-producing regions, T_e reaches these values in regions where the electron densities are 10^{12} to 10^{14}.

Underhill: I said the apparent color temperature in the visual was 3 to 4×10^4°K. If you had a continuum at 10^5°K, how would it differ in relative energy distribution over that short wavelength range from what we showed here? And if you construct a photosphere at 5×10^5°K with the densities we believe are correct, you are going to need an enormously high gravity to hold it together.

Thomas: A photosphere is defined as $\tau=1$ in the continuum; if T_e were 5×10^5°K at such a depth, I would see it in some way. I don't. And in all the models to which you constantly refer, you have electron temperatures of $< 5\times10^4$°K. This is not some color temperature but a kinetic temperature, a parameter you use to compute gas pressure.

So the atmospheric configuration we have to investigate is clear; maybe we don't know how it got that way, but we have rough estimates of what it is: T_e is between 5×10^4°K and 10^7°K and increases outward through at least part of the atmosphere. The run of density is much less certain, but n_e is certainly much higher in the region of line formation than in the solar chromosphere-corona. So you probably want to consider $n_e \lesssim 10^{14}$ in the region of line formation and considerably higher values where the continuum is formed. Whether the He I spectrum can give information on dilution effects, and hence on n_e, at still greater heights in the atmosphere remains to be demonstrated. But unless something intervenes, the opacities in all the lines considered will be quite high, and solutions of the transfer equation will be required in order to discuss line profiles. But of course something does intervene, as evidenced by point (2), the breadth of the emission lines: there are large velocity fields. We cannot be sure what they do to the opacity because so far we haven't been able to decide between a simple expanding atmosphere, a differentially expanding atmosphere, and an atmosphere with random motions. So the opacity is one of the things to be determined, and it is directly related to the distribution of velocities over the atmosphere.

The problem we face in investigating the WR atmosphere is essentially the same as the one we faced some years ago, and still face today, in studying the outer solar atmosphere: what distribution of T_e and n_e will give something like the observed spectrum? The added complication for the WR stars is the velocity distributions - macroscopic and microscopic. The added simplification is the increase in our store of knowledge and experience in how to approach a non-LTE situation. If we could neglect the effect of velocity fields on the opacity, the problem would be straightforward: we would assume some distribution of T_e and n_e and we would compute the spectrum. The techniques are well known; they are trivial for the thin atmosphere and somewhat more difficult for the thicker atmosphere. When we take into account the effect of velocity fields on the opacity, we require solutions of transfer problems in moving media; both the opacity and source terms in the source function must be coupled to the velocity field. It is this kind of approach, rather than one based on the standard atmospheric models, that we should be considering here. After we have carried out these quasi-empirical investigations on the kinds of distributions of T_e, n_e, and V that might produce the observed spectrum, we can proceed to investigate the energy balance. This is the stage we have now reached in the outer solar atmosphere. In applying the standard atmospheric models, you have assumed at the outset that you know the answer to the last question.

Pecker-Wimel: You must make some assumption about the mechanism of excitation and about T_r in the continuum.

Thomas: The first is not necessary; all likely processes can easily be included. I agree with the necessity to assume something about T_r, but I presumed this could be obtained from observations in the visual and rocket UV.

Underhill: Bappu gives arguments to justify his assumption that the non-LTE factors b_k satisfy the relation $b_k/b_j = 1$. And actually the important parts of T_r are in the region $\lambda < 228$ Å where they cannot be observed.

Thomas: It is unnecessary to assume anything about the b_k's; I would not trust any a priori assumption. Agreed on T_r, but we seem to be gaining rapidly in empirical knowledge; let's give NASA a little more time and we'll have the answer.

Pecker-Wimel: I am working on such problems now, trying to include the velocity field. But I think many of your arguments hold only for the 2-level atom, and that the procedure becomes difficult for more complicated atoms. It is very difficult to separate the various excitation processes, especially when you have mechanical heating and are uncertain about the continuous radiation field.

Thomas: Well, you and I have a couple of papers where we show how, in principle, this can be done. It necessitates the ability to identify the important processes populating any given level of the atom. A number of people seem to be progressing rapidly with numerical solutions to these problems, mostly without velocity fields, it is true. But I do think we now have an insight we did not have some years ago into the physical effects of the various parts of the source function: the scattering term and the source and sink terms.

Underhill: You can't really do this because you don't know the cross sections with the necessary accuracy. And furthermore, you have these particular processes of which I have managed to isolate two or three. But there is no guarantee there aren't many more of them as yet unrecognized.

Thomas: Our experience is to the contrary, but we won't know until we try. The problem in which we have the least experience is that involving moving media. Charlotte Pecker-Wimel has been working on it; so has Dave Hummer; possibly we could ask them to outline their thinking.

Pecker-Wimel: Most of my results are still on the machine; I had hoped to bring them along, but they were not ready. I would only emphasize that you cannot choose, a priori, a distribution $n_e(r)$ without being sure that it is consistent with $v(r)$.

Hummer: From the work George Rybicki and I have been doing on line formation in expanding atmospheres, I have gained some insight into how the different kinds of profiles can be interpreted. Our work involves numerical solution of the non-LTE line transfer problem with noncoherent scattering in plane parallel slabs in which radial velocity, temperature, density or any other atmospheric parameter can vary arbitrarily with depth. Since I have had planetary nebulae primarily in mind in this work, we have always taken the mid-plane of the slab to be stationary with respect to an observer outside. The velocity on the near side increases toward the observer, while that on the other side increases away from the observer. We find that the usual double-peaked intensity profile is distorted by a reduction of the intensity on the blue side, so that an apparent redshift occurs. This is, of course, the effect first obtained by Chandrasekhar and later by many others for absorption lines; the intensity in the blue is reduced, leading to an apparently blue-shifted absorption line. The explanation is quite simple: one sees to greater depths in the red than in the blue, and for the situations considered so far the source function is greater at large depths than near the surface. An interesting consequence of this explanation is that if one had a strong enough temperature increase toward the observer, again with the velocity increasing in the same direction, one should see blue-shifted emission and red-shifted absorption lines. It is worth emphasizing that the red shift has nothing to do with the receding rear part of the slab, for even when this part is stationary, the same type of profiles are obtained.

The importance of this work for the theory of WR stars seems to me to be two-fold. First of all, we can develop some feeling for the kinds of profiles that are associated with various types of atmospheric motion, including optical depth effects. Second, when definite atmospheric models are proposed, we can test them to see if the correct line profiles are indeed observed. I would like to take spectra of the type shown us by Anne Underhill and, on the basis of the intuition we have now developed, pick out those lines which are formed by a radiative mechanism in a turbulent situation and, in the case of the flat-topped emission lines, those which are formed in a thin atmosphere situation. I think it is clear that these latter arise because of the large velocity gradients. The point is that it is now possible to sit down and make a good guess at a kinematical picture for each line.

Smith: I have tracings of the spectrum of HD50896 (WN5) that show a definite central reversal in the He II $\lambda 4686$ line. Figure 8 shows the asymmetry of the profile.

Hummer: That is exactly what you would expect. What would be a central self-reversal in a static atmosphere becomes distorted in an expanding atmosphere. The blue wing simply drops away.

Underhill: The great problem in interpreting these things is that it is difficult to be sure from

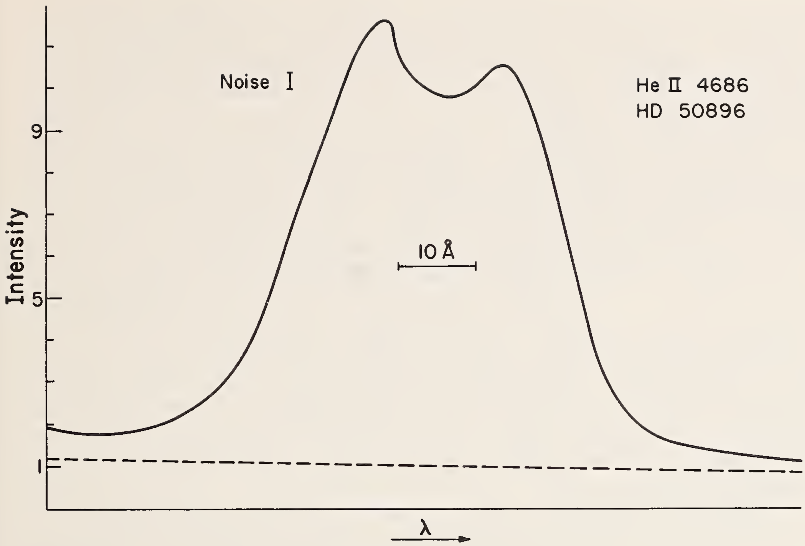

Figure 8. The line profile of He II λ4686 in HD50896 from a 9-minute exposure at 16 Å/mm taken 26 September 1967, with the Coudé spectrograph of the 120-inch telescope of Lick Observatory. All other strong lines on this spectrogram show asymmetric profiles similar to that of λ4686 but with less pronounced maxima on the redward side. This is a transient phenomenon; a plate from the previous night shows normal Gaussian profiles.

your observations whether they are real effects, or whether the tops of the profiles are simply grass. In photographic photometry you cannot get away from grass being about 3% of the intensity.

Smith: These are photographic observations, but the grass is significantly less than the depth of the self-reversal. Nor is there a possibility of confusion resulting from a blend with N III λ4640. The center of that line would lie significantly outside the λ4686 profile, and it is not present. The exposure time was about 10 min at 16 Å/mm. The spectrum looks like that of a single star, but Kuhi thinks it may be a binary. I have seen one other profile like this, a C III line in Campbell's star. It was slightly asymmetric but not so much so as this. On the whole these profiles are quite rare.

Underhill: I would emphasize that the main problem is to be sure you have short exposure, high resolution spectra. There is a note by Hutchings in

Observatory on photoelectric scans of Hα and Hβ in Be stars, Of stars and B supergiants. They see definite changes in profile which you will not see photographically because you are integrating over a longer period of time.

Payne-Gaposchkin: It is worth noting that some novae emission profiles exhibit what looks like self-reversal until you notice that the shape is shared by the forbidden lines. This makes it unlikely that the phenomenon is really self-reversal.

Thomas: Charlotte Pecker-Wimel and I had a note in *Ap. J.* a couple years ago on this phenomenon in the [Fe X] red line. I agree it is hard to imagine that such effects come from self-absorption, but we managed to convince ourselves that this was the explanation. There was a suggestion some years ago that novae ejected two globs of material symmetrically, but we gave reasons for excluding this interpretation.

Sahade: I think it is dangerous to base any conclusions on the λ4686 line. As we have heard, it is a rather complicated line in binaries. I think we must look for other lines in which to study self-reversal effects.

Thomas: That may be true, but when you have an observation that leads directly to some physics, you try your best to use it. The existence of a central self-reversal in an emission line tells you immediately that the optical thickness of the layer producing the line exceeds about 10. Asymmetry in such a line tells you immediately that you have a velocity gradient.

Aller: We have known for some 25 years that most of these lines are strong lines formed in an optically thick atmosphere.

Thomas: I am not aware that this is well known. What we have known is that the ratios of the line intensities are not the ratios of the f-values. But these are two quite separate things, and the one does not imply the other.

Underhill: We definitely know these atmospheres are thick. When you have such strong lines in atmospheres of such small dimensions, these atmospheres must be optically thick. O. C. Wilson showed in 1942 that the dimensions of these atmospheres were small. I suggest He II λ4200 is a better line than λ4686 for diagnosing the atmosphere, because it is not particularly strong, whereas λ4686 is probably formed outside the main part of the atmosphere.

Thomas: Everything depends upon the velocity distribution. If you take 10 $R_\odot$ for the radius of the line-producing atmosphere and a particle concentration of 10^{12}, you get a line-of-sight particle concentration of 10^{24}. Even if the absorption coefficients were small, this would give a high opacity <u>if</u> there were no differential velocity. If there is <u>a</u> differential velocity you must compute the opacities very carefully. I see no sound basis for your conclusions, and this is why I lay such stress on things like the self-reversed cores, which do provide a real, physical basis for inferring something about the opacity.

West: I would like to describe a microscopic calculation of the type we have just been discussing, which has been made by Stecher and myself and is aimed at interpreting the C III spectrum in the rocket-UV observations of γ Vel presented earlier in this symposium by Stecher. The calculations are preliminary. By making various simplifying assumptions we are trying to get some feeling for what physical effects are important. This should be regarded as only a progress report.

The strongest emission line in the spectrum was identified as the C III intercombination line λ1909. According to Code and Bless (1964, *Ap. J.*, 139, 787), the C III ion has the most completely developed emission spectrum in this WC8 star. It would not be surprising, therefore, to find strong C III lines in the UV. However, the λ1909 line is the transition from the ground state of the triplets 2p $^3P^0$ to the ground state of the ion, $2s^2$ $^1S^0$. Garstang and Shamey (1967, *Ap. J.*, 148, 665) give the oscillator strength as $gf = 3.1\times10^{-7}$ or A = 190 sec^{-1}. This low transition probability contrasts with the observed equivalent width of about 14 Å (minimum value, uncorrected for instrumental profile) to provide an interesting problem in the mechanism of line formation.

In order to discuss quantitatively the physical processes that may be operative, we need a rough model for the emitting region of the envelope. We assume the WR star to have a photospheric radius of 5 solar radii, and we consider the line to be formed in a moving envelope whose outer radius is 5 times the photospheric one. (These assumptions are consistent with the results reported at this symposium by Hanbury Brown.) We assume the velocity field in the envelope to consist of a spherically symmetric radial expansion, with zero velocity at the lower boundary of the envelope (which coincides with the outer edge of the photosphere) and a linear outward increase reaching 1500 km/sec at the outer boundary of the envelope. We assume the only random motions in the moving envelope to be thermal, and we assume $T_e = 5 \times 10^4$ °K everywhere in the envelope. We assume the density to decrease outward from the lower boundary as r^{-2}, where r is the radius. We explore a range of values for the density at the base of the moving envelope. Finally, for the continuum radiation field at the top of the photosphere, we take the results from Mihalas' continuum model of a 4×10^4 °K star (1965, *Ap. J. Suppl.*, IX, 321). We assume there is no continuous opacity in the envelope.

Given this simple model, we could now proceed to investigate the line-formation process by solving simultaneously the equations of statistical equilibrium and of radiative transfer to obtain values for the source function and the line profile throughout the atmosphere. Such a procedure would be more complicated, for two reasons, than those which now exist in the literature. First, even in this simple model, there are very strong differential velocity fields. Second, the atmosphere departs severely from a plane-parallel model. As mentioned, this present investigation is preliminary and exploratory, so we proceed to make three further simplifications.

First, we assume the total line profile to be the superposition of profiles from a number of atmospheric elements, each of half-width equal to one thermal Doppler half-width, and each centered at a frequency corresponding to the line-of-sight velocity with respect to the observer. Because of the velocity gradient, the contributions from other regions of the atmosphere will be shifted out of the frequency band emitted by the atmospheric element considered. The dimensions of the contributing atmospheric region are fixed by:

$$z_2 - z_1 = 2u/(dv/dz) = \text{constant} \tag{1}$$

because of the assumptions of an isothermal envelope and a linear velocity gradient. The line-of-sight coordinate is z, u is the thermal velocity, and v is the mass-motion velocity.

Second, when we observe an emission line, we must decide whether it arises wholly from a geometrical effect because the envelope is larger than the photosphere, or whether it is intrinsic and arises because the envelope between the observer and photosphere emits more in the line than it absorbs from the continuum at the same wavelength. In the present simplified calculation, we consider the question of intrinsic emission. Thus we restrict our attention to the emission from that part of the envelope lying between us and the photospheric disk. We will then underestimate the energy in the emission line, relative to the observations which refer to the entire envelope.

Third, rather than computing the source function from the equation of radiative transfer, we proceed by assigning it various values and computing the effect on the emergent radiation. We assume a ν-independent source function, so that we obtain its value by assuming the population ratio of upper and lower levels.

Under these conditions the equation of radiative transfer along the line-of-sight from the stellar disk through the envelope to the observer becomes:

$$\frac{dI_{21}}{d\tau_{21}} = I_{21} - S_{21} \tag{2}$$

where

$$S_{21} = E_{21}/K_{12} \tag{3}$$

$$E_{21} = N_2 A_{21} ch/8\pi u \tag{4}$$

$$K_{12} = (N_1 B_{12} - N_2 B_{21}) ch/8\pi u \tag{5}$$

where u is the thermal velocity of carbon; N_j is the occupation number of the level j; and the A_{21}, B_{12}, and B_{21} are the Milne form of the Einstein transition probabilities.

Equation (2) integrates to give:

$$I_{21} = I^{\circ}_{21}(\tau)\ e^{-\tau} + \int_0^{\tau} S_{21}\ e^{-\tau}\ d\tau\ , \tag{6}$$

and under the assumption of a constant value for the source function, S_{21}, equation (6) becomes:

$$I_{21} = I^{\circ}_{21}(\tau)\ e^{-\tau} + S_{21}(1 - e^{-\tau})\ . \tag{7}$$

We assume there is no spectral line at the top of the photosphere, so I°_{21} is just the continuous radiation field given by Mihalas' computations. Thus the quantity of interest is I_{21}/I°_{21}, the ratio of the emission in the line to that in the continuum. Clearly, from the restrictions set by the assumptions, we will get only the center and the blue part of the line profile; the red part will be occulted by the photospheric disk. In Figure 9 we plot the value of I_{21}/I°_{21} at the line-center for various assumed values of (N_1+N_2) at the base of the envelope and for various assumed values of N_2/N_1. The value of τ follows directly from equation (1) and these two assumptions on the values of the N_j:

$$\tau = K_{21}\ 2u/(dv/dz) \quad . \tag{8}$$

For convenience, we also give the values for b_2/b_1, the ratio of the departures from a Boltzmann distribution.

The observed quantity is the ratio of total flux in the line to that in the continuum, F_{21}/F°_{21}; so as mentioned earlier the observed and computed quantities are not strictly comparable. Let us, however, ask what conclusion we could draw if they were comparable.

The observed value for F_{21}/F°_{21} is at least 4 and could increase to 7 for an infinitely narrow line corrected for instrumental profile. Taking the conventional H/C abundance ratio and assuming all C occurs as C^{++}, the curve in Figure 9 for $(N_1+N_2) = 3.2\times10^{8}/cm^{3}$ represents an H density of $10^{12}/cm^{3}$ in the envelope. So if this curve indeed represents the conditions in the envelope, we require $N_2/N_1 \geq 14$ to reproduce the minimum observed ratio of line to continuum. Such a value for N_2/N_1 requires a population inversion of the two energy levels; the absorption coefficient becomes negative; and a laser action will amplify the radiation in the line. If such a situation exists, it is indeed very interesting. Its existence depends upon the large N_2/N_1 ratio; the next problem would be to explain it.

To explain the observations other than by a large N_2/N_1 ratio requires that one increase by a factor of 5 either the H density at the base of the envelope or the C abundance ratio - or that one change the underlying assumptions. While the C abundance remains open to controversy in WR stars, $10^{12}/cm^{3}$ is thought to be an upper limit to the electron density because collisional de-excitation would dominate spontaneous emission at higher densities.

Of the assumptions underlying the theory, that which allows I_{21}/I°_{21} to be compared with F_{21}/F°_{21} is

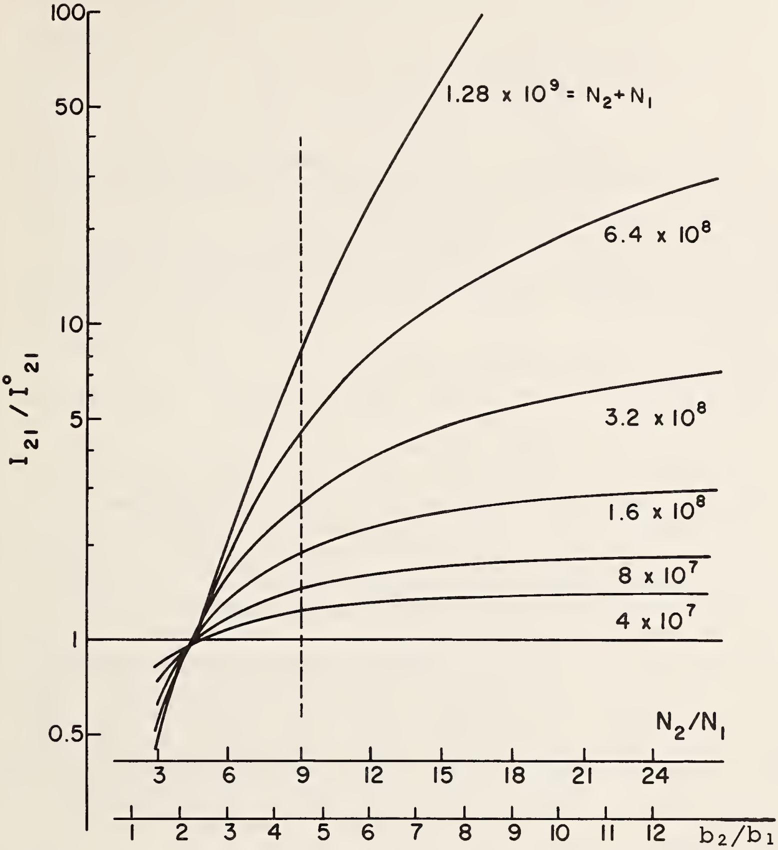

Figure 9. The ratio of the emission in the line to that in the continuum at the line center for various assumed values of ($N_1 + N_2$) at the base of the envelope.

the most open to question. We are now in the process of refining our calculations to eliminate the necessity for this comparison. Our present point is simply that these results have raised the interesting possibility of laser action, and that they should be followed up by more detailed calculations.

For reference, I should mention that we used the following values in our computations:

$$I^{\circ}_{21} = 4.78\times10^{-3} \text{ ergs cm}^{-2} \text{ sec}^{-1} \nu^{-1}$$

$$u = 1.0\times10^{6} \text{ cm sec}^{-1}$$

$$2u/(dv/dz) = 1.9\times10^{10} \text{ cm} .$$

Hummer: What is it that makes this transition alone show laser action? How does it differ from the other similar transitions?

West: It is reasonable to expect that you could get an overpopulation in a metastable level because the ways of getting out of it are few and slow.

Hummer: But there are a lot of other atoms with similar metastable states.

Underhill: Not atoms with abundant ions that give you observable lines.

Stecher: On the observational side, λ1175, which arises from the $^3P^0$ level, is very strong. It is seen in emission and absorption, Doppler shifted about 5 Å. This indicates a considerable population of the triplets in the envelope. The collisional depopulation comes at about 10^{12} electrons/cm^3. We don't have an exact value since we do not know the cross section.

Underhill: You do have to maintain an appreciable population in that metastable level of C III. It is the upper level for the λ1909 transition, but it is also the lower level for the λ4650 absorption line, which is one of the most prominent features in the spectrum. This makes me think the densities should be low; I also believe 10^{12} is an absolute upper limit.

Stecher: I might add that there are other data for a number of other recombination lines. We have not yet analyzed it, but I think a number of interesting things may develop when we do. In particular we find N IV at λ1488 and O V at λ1216. There is the possibility that the O V line is exciting He II λ4686.

Underhill: I was hoping you had data on the O V lines at λ2750.

Stecher: There is only one WR star we can observe with rockets; we need a satellite to observe the others.

Aller: These WR stars seem to provide such a formidable array of difficult problems, all of which must be solved simultaneously, that I wonder if it would not be wise to select some simpler objects that exhibit at least some of the spectral features observed in WR objects and see whether we might learn something about the physical processes involved. I have in mind the P Cygni stars, which are similar to the WR stars, but with simpler line profiles; the profiles have rounded emission components with violet absorption edges. It is normally thought that the P Cygni stars and the WR stars can be readily differentiated from one another. Beals has studied this problem; I wonder if he would like to comment.

Beals: I was particularly interested by Miss Underhill's mention of a number of stars with narrow lines. These are the WR stars of type WN7, and I believe that HD151932 is one of the group. But had we not defined the category of WR stars, I feel sure these objects would have been classified as P Cygni. Indeed I am not sure that they should not have been classified as P Cygni. I am inclined to say that there is no sharp line between WR and P Cygni stars. I agree that the P Cygni phenomenon may shed light on the WR phenomenon, and I feel that the two should certainly be studied together. There are 50 or more of them altogether.

Aller: Two points should be mentioned; First, the excitation level in the P Cygni stars is generally lower than in the WR stars, so the theoretical treatment may be easier. Second, some objects are observed to flop back and forth between a P Cygni spectrum and something else; I refer in particular to S Doradus, which every now and then has a nice P Cygni spectrum. Such variation makes things less agreeable.

Underhill: Mart de Groot, at Utrecht, is working on P Cygni profiles for his thesis. This work won't be available for about a year, so I might just summarize the highlights. P Cygni itself is classified as B1q and is of a much lower level of excitation than the WR stars; the hydrogen lines for example stand out in its spectrum. In those line profiles associated with expanding shells, the emission peaks are less striking than in a WR spectrum, and the absorption peaks dominate the line. I think that whether the line is dominated by emission or absorption peaks depends very much upon the level of excitation in the shell. In the hydrogen lines you get a sharp emission peak and a strong absorption peak, sometimes two absorption peaks. The amount of the absorption varies with time, while I believe the emission peak stays fairly constant. In the He I lines you get much more absorption and hardly any emission. You do find a few lines with only emission, which just means that there are not enough atoms in the lower levels to give absorption in the shells; most of the lines you observe are subordinate lines.

In P Cygni spectra, there are no double emission peaks such as you get in Be stars. P Cygni stars give the feeling of a spherical expanding atmosphere; like WR stars they give the impression of spherical symmetry. Be stars and novae, on the other hand, give the feeling that things happen in a plane, that they are more nearly cylindrically symmetric.

Hummer: What characteristics of the spectra tell you that?

Underhill: The only way of getting two separate emission pips is from a rotating star in which material is concentrated in a plane.

Hummer: But this depends on the amount of material present; I can see your point if the material is only marginally thick.

Underhill: Be stars and novae shells are only marginally thick; shell stars are thicker.

Beals: The complex of lines, which apparently exists in P Cygni as a temporary phenomenon, seems to be permanent in HD190073. It is one of the most fascinating lines I've seen in any star, and I think it is changing in detail but not in general outline. It is an undisplaced emission line with a central absorption minimum and two strong absorption lines on the violet edge. You frequently get this sort of thing in novae, where it is easily explained in terms of two moving shells. In this star, however, it has been like this for at least 30 years and probably much longer. HD190073 is between 7th and 8th magnitude; I'm delighted to have a chance to mention it here in the hope that someone with modern equipment will observe it again.

Pecker-Wimel: Is this profile observed in many lines or in only one?

Beals: There are profiles available for the hydrogen lines, and they show three absorption components corresponding in velocity to those of Ca II. The hydrogen lines don't look at all like the Ca II lines, and although the velocities are roughly the same, the various components of the line do not have the same relative intensities.

Johnson: From what you said about the very great life of this double absorption feature, I gather you don't necessarily believe it can be explained wholly by a classical expanding shell. Are you suggesting that the doubleness of the feature implies something unique?

Beals: I tried to explain the profile in terms of the acceleration of atoms. I assumed that the large-velocity component arises from atoms close to the star and that as the material moves out, it experiences different rates of deceleration. When it is absorbing strongly, it is moving outwards with a uniform velocity or with a very small deceleration; when the absorption is weaker, the material is decelerating more rapidly; and when it reaches zero velocity, there is a shell surrounding the star at some great distance, showing an emission line with a central absorption. This was the only model I could think of; I am not particularly happy with it; if I really believed it, I would not be making an extra effort to get other people to study the star and explain the profile.

Aller: FU Orionis is a star that suddenly brightened and evolved a P Cygni spectrum that has just remained ever since.

Underhill: Beals' discussion points out that the major difference between the novae on the one hand and the WR and P Cygni stars on the other is that the latter show a spectrum constant in time. The displaced absorption component indicates you are seeing an outward flow of gas, but you are not always seeing the same atoms; you are seeing the gas at a particular geometrical point where the density and excitation conditions are such as to produce the line. You have a steady-state velocity flow. Novae, on the other hand, do not reach equilibrium; they may go through a phase showing the same spectrum as WR stars, but it is a transient phase. A similar, possibly intermediate situation holds for the shell stars. A shell star may give evidence of a steady-state outward streaming for a long time and then suddenly change; you will then have evidence of both a stationary and an outward moving shell. In 48 Librae, for example, the shell lines have given a radial velocity variation with a period of apparently 10 years. But recently the star has gone crazy and shown negative velocities. I took a spectrum at Kitt Peak that showed double Na D lines: a strong, rather sharp line and a fuzzy patch to the violet; you obviously have two pieces of expanding gas.

Aller: Doesn't the supergiant A star, α Cyg, show a P Cygni profile for Hα? The line spectrum is otherwise constant in time, although the atmosphere itself shows oscillations.

Beals: I think the P Cygni characteristics of Hα in α Cyg are somewhat variable.

Nariai: Kikuchi has taken spectra of Hα in β Orionis and finds that its profile varies with a period of about 20 days.

Schild: This raises a question I hoped to ask of Beals. From looking at a fair number of spectra of very luminous stars, it is my impression that many of the 50 or so stars he identified as P Cyg type have spectra that are fairly normal for high-luminosity stars except that they show a P Cyg profile at Hα and perhaps a few of the higher Balmer lines. Could you tell me how many other spectra you know that show P Cygni profiles in He and other lines besides H?

Beals: I would say that the number of stars showing spectra similar to P Cyg right from one end of the spectrum to the other is so small as to be negligible. I think your remark about P Cyg characteristics being imposed on normal spectra is probably correct. But although we associate P Cyg type spectra with rather high luminosity stars, I don't think all the P Cygni stars are highly luminous; indeed I think some are not highly luminous at all.

Schild: Just to be very clear, let me amplify my last remark. There are one or two highly luminous stars which do have really abnormal spectra; P Cygni is not unique. You have already mentioned the two stars, HD151804 and 152408, in the association Sco OBI. These do have very pronounced spectral peculiarities as well as peculiar P Cygni profiles in the hydrogen lines.

Payne-Gaposchkin: The companion of o Ceti is a very low luminosity star, visible only when the long period variable is faint, yet it is a kind of P Cygni star - a bright line, high temperature star. Also, is there not a suggestion somewhere in the literature that P Cygni is an eclipsing binary?

Underhill: Two Russian astronomers reported in the *Variable Star Bulletin* that they had found a half-day period in the luminosity of P Cygni and suggested it might be a W Ursa Majoris star. de Groot has measured radial velocities on all his spectra, and can show that they do not follow a half-day period. He found velocity variations of much longer period in some of the hydrogen lines; we think this is an atmospheric oscillation.

Pecker-Wimel: We have from time to time spoken of the similarities between Of and WN stars. Is there any evidence of fast rotation in WN stars?

Underhill: Rotation can be determined only from observations of absorption lines that exhibit the well-known dish shapes. We have not detected any such absorption lines. Therefore I know of no way of detecting rotation.

Pecker-Wimel: I have heard rather vaguely of one attempt. When WN stars and Of stars in the Large Magellanic Cloud were plotted on the HR diagram, the WN stars fell in that region of the diagram where rotation was observed. So I am wondering whether you might be getting a blend of rotation and expansion in the observed WR profiles. The profiles computed by Sobelev included both rotation and expansion and showed that you could get a central dip in such an emission line.

Underhill: Yes, if you have an optically thin atmosphere that is extended. A typical Be star has a shell, and the atmosphere is spinning, so you get two separate emission lines. But I don't understand the investigation you reported; I don't believe we know that the Of stars are rotating. It is true that some of them have very broad lines, and when we have an absorption line 4 Å wide with a 10 percent central absorption, it is a nice point to prove that the broadening is due to rotation and not to micro- or macroturbulence. All the observed lines from O-stars have this characteristic shape; they indicate rotational velocities of less than 200 to 300 km/sec. But again the interpretation depends very much on what you assume for the source function in the atmosphere. The usual procedure, which we know is poor, is to assume Kirchoff's law and to match the line profile by introducing microturbulence as a fudge factor. To be more realistic, we should introduce scattering into the source function. This would give us a broad, strong line for considerably smaller rotational velocities. So until we can separate the detailed physical processes producing the line, I think it is hard to separate the various kinds of velocity fields.

Limber: Now that rotation has been introduced, I should like to comment on my model of a few years ago in which I tried to interpret the WR phenomenon in terms of a rotational forced ejection. Though I am not completely convinced of the model, I am not yet ready to abandon it. I do not intend to discuss the suggestion in detail; you can read the literature (1964, *Ap. J.*, 139, 1251). I would like however to draw your attention to several characteristics of the WR phenomenon which, it seems to me, are crucial to our understanding any model of a WR star. Several of these characteristics involve questions of rotation and rotational effects, and they may clarify the importance of such effects both in the model of the star at the WR stage and in the evolutionary history leading to that stage.

First there is the simple kinetic picture of the WR star. Our first-generation model by Beals consisted of a very extended, expanding atmosphere. This simple picture had to be changed for two reasons: First, the absence of a transit-time effect (phase difference in time of eclipse as observed photometrically and as observed in the velocity curves) meant that the region of line emission could not exceed the region of continuum emission by more than a factor of about 5 in radius. Second, the occultation effect (asymmetric line profile), which should be present in a model of simple expansion, was also absent. So we have Kuhi's second-generation model, which consists of an inner chaotic region with random velocities of several thousand km/sec, where the strong emission lines are produced, surmounted by the low-density expanding region. Now I would point out an alternative second-generation model that is equally consistent with the absence of transit-time and occultation effects. Here the chaotic velocities in the inner region are replaced by rotational velocities that decrease outward. So we must make a decision between these two alternative kinetic models, both of which provide the differential motions necessary to explain the line widths.

There are two kinds of evidence. One is the geometry of the emitting regions; the other is the binary character of many of these stars. The geometry, which involves the distribution of mass as well as of excitation, is determined by the rotational instability which in turn is affected by the presence of a companion. Consider the single star. At speeds such that the centrifugal and gravitational forces on a mass element at the equator are equal, rotation can readily move mass into a circumstellar envelope, given an appropriate viscous agent. Thus the centrifugal acceleration provides support for the extended atmosphere. Moreover, if the rotation is differential, as may be the case in the photosphere and must be the case in the envelope, the velocity gradients can provide significant mechanical energy for exciting the envelope. Thus, for example, it is possible that some of the chromospheric effects we find in main-sequence F- and G-stars result from differential rotation. The correlation that O. C. Wilson finds between chromospheric activity and stellar age may well result from the reduction with time of differential rotation and thus of energy for chromospheric activity.

Rotation seems generally to be a source of instability in stellar evolution. A star contracting to the main sequence may well face a rotational crisis. In early post-main sequence evolution it appears that the Crampin-Hoyle mechanism can bring rapidly rotating stars to another crisis, thus producing Be stars. In later post-main sequence evolution, as Paczynski and others have suggested, contraction may again bring at least some classes of stars to a rotational-instability crisis.

Now consider the binary aspect. Here we have another source of instability which produces mass loss, as has been discussed. I think one very important piece of evidence is the rather sharp boundary to the region where emission takes place. In eclipsing systems, the WR component appears to be about twice as large as an eclipsing object as an eclipsed. In the system V444 Cyg, from which the Krons drew this conclusion, I note that the boundary of this inner emitting envelope lies very close to that of the inner Lagrangian surface about the WR component. This may be coincidence, and the boundary of the emitting region may not be as sharp as the Krons suggest, but it is an interesting fact. It suggests that the inner Lagrangian boundary may play a significant role in fixing the boundary of the emitting region. This can be true only if the velocities in the vicinity of the inner Lagrangian boundary are small relative to a frame rotating with the orbital motion. So there should be no very large velocities in the vicinity of the inner Lagrangian boundary.

Thomas: So you are suggesting that if in the region of line emission there are any high-velocity chaotic motions or any large differential rotation velocities, they must vanish at the outer edge of this region. Then a large expansion velocity cannot have been reached at this boundary.

Limber: Right. This implies that the velocity of expansion is not large here, nor are there any random motions with large velocities.

It has been suggested that WR stars in close binaries have narrower lines than single WR stars. One might speculate that less kinetic activity is required in order to get mass ejection from a WR star in a close binary than from a single WR star. It would be easier to ease matter through the inner Lagrangian point for the binary than to expel it from a single star.

Thomas: You mean that when a WR star has a companion, it somehow knows it doesn't have to have as much internal instability, or whatever produces the "chaotic shell", as when it is single?

Limber: What the mechanism of instability is, I do not know. But I suggest that if the WR star is a member of a close binary, it can produce the required mass flux with less instability. I recall Miss Smith mentioning that WR stars in close binaries are not associated with ring nebulae. One might infer from this that any mass lost by a WR star in a close binary is transferred to the companion and not lost to the system.

Kandel: Thomas has asked how the WR star knows it doesn't have to be as unstable when it has a companion. Possibly it gets this information through the boundary conditions. Suppose you have some sort of overstability as the exciting mechanism; then the amplitude at which everything becomes steady may be considerably lower in binary systems when you have a companion to siphon off the mass. In practice the boundary condition might impose a certain flux of mass and momentum at a given radius from the star. This radius would be much smaller for a binary system.

Limber: Let me comment on the observed longitude dependence of the spectroscopic properties of WR stars in binary systems. Hiltner found twenty years ago that in CQ Ceph, He II λ4686 is strongest at primary and secondary eclipse and weakest at the two elongations. This suggests several alternatives for the distribution of the emitting material; in all of them it is spherically asymmetric and located in a region where it is influenced by both stars. Such effects suggest that the geometries of the excitation and velocity fields are fixed with respect to a frame rotating with the orbital motion. The effects of the O-type companion are not necessarily overpowered by those of the WR star. If this logic is substantiated, we may be able to rule out some possible excitation mechanisms and geometries.

Finally, I would like to remark on a point that does not deal directly with rotation or binary character: the relation between other objects and WR stars, with respect to our models of WR stars. Consider the Of stars and the nuclei of planetary nebulae. None of us believes the Of stars represent a late stage of evolution accompanied by pulsational instability in a star that is essentially pure helium or carbon. If the relation between the Of and the WR stars is superficial, we have no problem; but if the Of phenomenon is essentially the same as the WR phenomenon, we should worry. The same holds for the nuclei of planetary nebulae. If the binary characteristic is vital to the WR phenomenon, and if planetary nuclei are not binary, it is awkward to identify one with the other. These difficulties cause me to believe that in the models so far proposed, we do not understand the WR phenomenon. I believe the possibility of rotation should not be ignored. At the same time, I am certain that the details are considerably more complicated than I suggested several years ago.

Underhill: I would feel much better if you had been more explicit about those possibilities for excitation and geometry that you say may be ruled out by the effects you summarized. And I would also caution against pushing too far such comments as yours on the Of stars. I believe the apparent relation of Of stars to WR stars is simply a spectroscopic chance. In Of stars we observe many of the lines in absorption that we observe only in emission in the WR stars. We can guess the excitation temperatures required to form these lines in absorption, and this is in fact part of our basis for saying the electron temperatures in WR stars are 3 to 4×10^4°K. But before using the properties of one spectrum to reject a model for the other, we must be aware of the differences as well as the similarities between the two.

Nariai: If we are discussing general models for mass ejection, I would like to suggest radiation pressure as a possible mechanism. The equation of motion for one-dimensional stationary flow, including a radiation field, is:

$$v(dv/dr) = \int (K_\nu F_\nu/c)d\nu - \rho^{-1}(dP_g/dr) - GM/r^2$$

where v is velocity; K_ν and F_ν are the monochromatic absorption coefficient and the radiation flux respectively; P_g is the gas pressure; and M is the mass of the star. If the radiation term dominates, we obtain an order-of-magnitude estimate by setting all the other terms on the right equal to zero and integrating. We get $v \sim 100$ km/sec for $T_r = 3\times10^4$°K and $R = 10^7$ km. This is a little smaller than the observed values of v, but possibly it can be increased if the integration over ν is properly treated.

In this mechanism, a part of the radiation flux is consumed in driving the material up to 1000 km/sec and in pushing it out of the star's potential. Typical values are $L_{rad} = 10^{38}$, $L_{K.E.} = 3\times10^{36}$, and $L_G = 10^{36}$ erg/sec for $M = 5\ M_\odot$, $R = 7\ R_\odot$, $T_e = 3\times10^4$°K,

v = 1000 km/sec and $dM/dt = 10^{-5} M_\odot$/year. $L_{K.E.} = 3\times10^{36}$ erg/sec corresponds to $F_{K.E.} = 10^{12}$ erg/cm^2/sec. We cannot get such a large value other than by the conversion of radiation into kinetic energy flux. For example, a maximum acoustical energy generation from the convective zone is 10^8 erg/cm^2/sec for normal composition at 6×10^3°K, and 10^9 for a helium rich atmosphere at 1.3×10^4°K. There is no convection zone due to ionization of He II around $T_e \sim 3\times10^4$°K and log g < 4. As pointed out by Miss Smith, shock waves due to nuclear instability are a fascinating mechanism. They may be able to carry the required energy, but the flux is reduced by 1/e in a distance of the order of 10^9 to 10^{10} cm, which is pretty small compared with the size of the envelope, 10^{12} cm. Then the velocity is expected to rise steeply near the surface and to decrease gradually toward outer space, which makes it a little difficult to reproduce the observed profile.

PART D

A SUMMARY OF PROBLEMS, IDEAS, AND CONCLUSIONS ON THE PHYSICAL STRUCTURE OF THE WOLF-RAYET STARS

INTRODUCTORY SPEAKER: *Richard N. Thomas*

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I. INTRODUCTION

In each of the three preceding days, we have summarized and discussed one of the three broad aspects into which we divided the study of objects exhibiting WR spectra: taxonomy, spectral features, and the interpretation of spectral features. Today we shall try to synthesize these discussions to see what problems we can pose, what ideas have come up, and what conclusions we can reach on the physical structure of the Wolf-Rayet stars. I propose to start this synthesis by outlining what I think has come out of these discussions. At the beginning of the symposium, I presented several questions which it seemed to me we must answer before we can claim any real understanding of the physics of these stars. Naturally my outline here will be guided by these questions. But by presenting them as an introduction rather than a conclusion, I am giving you the opportunity to criticize and correct those aspects which arise from my personal myopia. The object of such an outline is the same as that of the summary-introductions on the preceding days - to set up a skeleton containing the form of the subject, which you can alter and flesh out. Possibly from such individual views of the last three days, we can produce some kind of multi-stereoscopic picture that has coherence.

At the beginning of the symposium, I proposed that within the broad category of objects that exhibit some variety of WR spectrum, we set up two subcategories: (A) "classical WR stars" whose spectra - the "pure" WR spectra - are sufficiently well defined to be unambiguously identified, and (B) "quasi-WR objects" whose spectra resemble the "pure" WR spectra sufficiently closely to suggest some common characteristics in their atmospheres. I suggested two alternatives for a physical interpretation of the classical WR stars. Either they are a distinct kind of star, whose physical properties lie within narrow limits, or they represent a particular atmospheric configuration which can occur in many types of stars and objects with widely differing physical properties. The first alternative would imply that the classical WR stars are distinct "WR objects", with a distinct internal as well as atmospheric structure. The second would simply describe an atmospheric configuration, independent of internal structure. The question of "objects" versus "phenomena" has been discussed extensively in this symposium. So our first question is:

1. Can we, on the basis of spectra alone, establish a distinct class whose members will have some or all of their other physical properties lying within some limited range?

Our second question follows immediately from the first and already implies some measure of physical insight:

2. Can we identify a class of quasi-WR objects by certain spectral features, common to them and to classical WR stars, that imply some common atmospheric conditions?

The first question can be investigated wholly empirically; to answer the second question, we must pose a third.

3. What are the unique properties of the classical WR atmosphere that produce the pure WR spectrum?

To answer question (3) we would normally begin by isolating the class of WR objects or phenomena and then proceed to investigate the state of their atmospheres and the mechanisms which produce and maintain these states. If the classical WR stars are indeed objects, we would ask: What is unique about their internal structure that produces such an atmosphere? If, on the other hand, they are phenomena, we must ask: What do the atmospheres of such a wide variety of objects have in common that produces the WR spectrum? But as has been suggested several times during this symposium, there is another approach to question (3). We could study the quasi-WR objects and, by comparing them with normal stars and with the classical WR stars, isolate those properties responsible for the WR features. We could then try to determine whether these same features were fundamental in producing the pure WR spectrum.

Finally, of course, we would hope that given the answers to questions (1) to (3), we would be able to answer the final two questions in the sequence of our understanding of the WR stars:

4. What is the complete structure of the WR object - if indeed such an object exists - and how does this structure give rise to the atmospheric features?

5. What are the complete structures of the quasi-WR objects, and again how do they lead to the atmospheric features producing the quasi-WR spectrum?

The symposium was implicitly organized on the logical pattern of these questions. Looking back over the three days, I think we might have been more explicit about it. But if we now follow this pattern in the summarizing session we may be able to distinguish some success as well as some glaring omissions.

II. SUMMARY

1. *Taxonomy of classical WR Stars*

During the course of this symposium we have from time to time heard the plea that we should not be too dogmatic in our assignment of stars to, or exclusion of stars from, the WR class. This has been especially true when we have tried to distinguish between pure and quasi-WR spectra. We have been told that high dispersion spectra show such individuality that each star effectively defines its own subclass. The implication is that the WR category is not distinct and isolated, but rather that it lies toward one end of a possibly multidimensional chain, of which its subclasses form a number of links, but to which sequences of other objects showing quasi-WR spectra also belong.

I would be the last to argue against such a picture, particularly in view of my long-standing assertion that a WR star simply represents an extreme example of an atmosphere fed by mechanical energy and momentum. But we must not allow a preoccupation with minor differences to divert us from the basic problems. The general WR class is characterized by features unique with respect to normal stars and with respect to classical models of stellar atmospheres. The characteristics which define the subclasses are perhaps less unusual, except for those spectroscopic features which differentiate the WC and WN sequences. So it is on the unique spectral features of the general WR class that we must concentrate in trying to isolate unambiguously those physical effects which produce them. A first step in this direction is to inquire into the non-spectral characteristics of the WR and quasi-WR stars and their relation to the spectral features.

With respect to WR and quasi-WR objects, we are, in a sense, in a period of spectroscopic diagnostics analogous to the era prior to the identification of the physical parameters that determine the "normal" spectral sequence. Behind much of our difficulty in interpreting WR spectra is the implicit assumption that we know what these parameters are. Over and over again during this symposium we have heard such remarks as: "One of our goals is to arrange WR stars in a sequence of T_{eff} because T_{eff} is a parameter basic to stellar structure and evolution". Again during comparisons between stellar types and between various kinds of luminosities we have continuously evoked a location on the HR diagram. I quote an interchange:

Underhill: ...because the interpretation of intrinsic colors in terms of effective temperatures is done by means of model atmospheres. I'm inferring that because Schild put his stars on an HR diagram as one would B stars, he was prepared to assign them effective temperatures. This is the normal procedure when interpreting an HR diagram.

Schild: That is indeed what I had in mind.

May I remind you of the two advances - one theoretical, one empirical - on which the classical interpretation of stellar spectra is based and of the order that their adoption brought to the array of facts on stellar classification. The theoretical advance was Fowler's assertion that the spectral sequence could be understood in terms of thermodynamic equilibrium populations for the excitation and ionization levels, i.e., in terms of temperature and density. The empirical advance was the relation between luminosity and spectral class, which amounts to a specification of temperature in terms of the energy in the radiation field. Thus the temperature and occupation numbers of all energy states follow directly from the total luminosity. We can construct model atmospheres for each spectral type, and assuming that luminosity, radius, and gravity suffice to characterize such atmospheres, we can construct the corresponding interior models. Conversely, we can try to infer these parameters from the spectrum and from them infer the interior models.

By returning to this pre-model era of spectroscopic diagnostics, we are seeking, in our study of the WR and quasi-WR objects, just that kind of key provided by the Fowler and the HR relations.

So in this summary of the taxonomy of the WR objects, I repeat my list of those spectral features which unambiguously characterize the WR category. In each debate over the inclusion of a given object, we find that one or more of these four criteria have been violated. Each time we try to apply a "standard" model atmosphere to a WR object, we find one or more of these features are incompatible with the model.

1. The spectrum consists almost wholly of emission lines. When absorption lines occur, they occur as satellites at the violet edges of the emission lines.

2. The emission lines are very broad. Interpreted as Doppler-broadening, the widths correspond to differential motions of some hundreds to thousands of km/sec and are not necessarily the same for all ions.

3. The lines in any one star represent a wide range of excitation and ionization. The excitation level of the line spectrum is generally much higher than that of the continuum as estimated from its spectral energy distribution.

4. The spectrum falls into one of two groups. Either it shows strong lines from carbon and oxygen, or it shows strong lines from nitrogen. Both groups show strong lines of helium plus other weaker lines.

Now it seems to me that given these four criteria, I already have a basis on which to answer question (3) and to make some inferences on question (4). Also, I now know what to look for in trying to answer question (2) and to establish a category of quasi Wolf-Rayet objects. But we will come back to these points later.

In our original plan for the symposium, Lindsey Smith's review paper was to have been aimed at question (1). She posed the questions somewhat differently. Instead of considering both classical WR stars and quasi-WR objects, she restricted herself to the former and considered whether a WR object exists. She attempted to establish the existence of common physical properties within subclasses and to clarify the meaning of the various subclasses by identifying each subclass with an evolutionary stage of WR objects. She attempted to decide whether these subclasses represented successive evolutionary stages of initially similar objects or alternative configurations of objects with somewhat different initial conditions. In the first alternative, the WR class as a whole comprises objects of like properties; in the second, this is true only for the subclasses. Thus a third possibility, that the subclasses represent different aspects of a general WR phenomenon in different kinds of objects, was a priori excluded. In supporting her conclusions, she impinged heavily on question (4), the structure of WR objects and its influence on those atmospheric phenomena that give rise to the WR spectrum. But I reserve comment on this last aspect of her summary until I have considered questions (2) and (3).

We come now to the significance and the homogeneity of membership within the subclasses. It would be pointless for me to try to summarize the criteria proposed for the various classification schemes. Lindsey's differs from the others chiefly in the absolute luminosities attached to the subclasses, and in the emphasis on the presence or absence of various lines, e.g., O VI. You may wish to push these points further. After listening to Kuhi's remarks, it seems to me that the outstanding inadequacy lies in our knowledge of the continuum characteristics as a function of subclass. Both Lindsey and Kuhi agree on two points: first, that although there are few data for comparison of the two sequences, the continuum temperature appears to be lower in the WC than in the WN class; and second, that the energy radiated in the lines is greater in the WC, relative to the continuum, than in the WN class. Now suppose we were to argue that the mechanism producing the continuous spectrum differs from that producing the line spectrum. Then these 2 points could be interpreted as a difference not so much in the "intensity" of the line-producing mechanism as in the "intensity" of the continuum-producing mechanism. For those stars investigated in detail, the energy distribution in the continuum seems to depart significantly from that predicted by the theoretical main-sequence models and also from that of main-sequence stars. Indeed it is the supergiants which WR stars seem to resemble most closely. So again we have evidence that the general WR class is internally homogeneous and that it differs from the class of normal stars: it does, however, seem to have a closer relation to certain other exceptional types.

As for the conventional physical properties of mass and luminosity, we again have little evidence; but what we do have suggests a first-order difference in the masses in the WC and WN sequences, with the WN the more massive. There is disagreement on the reality of the very brightest magnitudes (~ -6) for the WR stars, but no strong evidence for a difference between the two sequences.

In both the WC and WN sequences the luminosities seem to increase toward later subclasses, whereas the excitation of the line spectrum seems to increase toward the earlier classes. So if one assumes that line excitation correlates with energy in the continuum - and this is not at all obvious - the size of the object must, as Lindsey suggests, increase toward the later subclasses. Finally, Lindsey finds evidence for an association between ring nebulae and certain of the WN subclasses, but no such association with WC objects.

From these data, I end up with the feeling that there is homogeneity of physical properties within each of the subclasses, but that these properties change significantly from one subclass to another. This conclusion is reinforced by Lindsey's suggestion that the WC subclasses arise from differences in initial chemical composition while the WN subclasses arise from differences in initial mass. These are points for you to discuss. From the strong differences of opinion already expressed at various times during the symposium, I am sure that at least some of you will have something to say on the question of the homogeneity of physical properties within subclasses and the variation from one subclass to another.

I again stress that we have had no detailed discussion of the taxonomy of quasi-WR objects, nor have we discussed their spectra or structure. This represents a glaring neglect of what could be a powerful approach to the understanding of WR stars. I am therefore putting quasi-WR objects at the head of the list for today's discussion.

2. *Specification of Quasi-WR Objects*

I would recommend as candidates all stars with broad emission lines and all stars with some pattern similar to that of the two WR sequences. Thus we should certainly include P Cygni stars, some stages of novae, nuclei of planetary nebulae, and possibly the rocket-UV spectrum of the Sun and, by inference, of many other stars.

3. *Spectral Characteristics of the WR Atmosphere*

This question was to be covered from two complementary aspects: Kuhi was to summarize the empirical information from which we might establish relationships between spectral and atmospheric features; Anne was to summarize the diagnostic attempts at interpreting spectral lines in terms of atmospheric structure; from this we might possibly have expected to deduce something about the conditions under which the spectrum is produced.

To me, three things stand out from Kuhi's talk because they seem to be important boundary conditions on any inference we may make about the state of the atmosphere.

First, the breadth of the spectral emission lines still seems inescapably linked to a velocity dispersion of some sort. In the naive, thin-atmosphere interpretation, the existence of two definite kinds of spectral lines - flat-topped profiles, often accompanied by violet absorption edges, and more-or-less Gaussian profiles - is evidence for both expansion and very high velocity random motions. And indeed this is precisely the basis for Kuhi's schematic model of the atmosphere: a photosphere surmounted by a "turbulent" region surmounted in turn by an expanding region.

Second, our one piece of evidence for stratification, Beals' old correlation between line width and ionization stage, is still strongly supported by Kuhi.

But third, the binary character of many of the best observed systems seems to play havoc with the stratification problem. The behavior of lines during eclipses emphasizes the point of Hanbury Brown's question: Is it possible that an atmosphere which extends to about 1/3 the separation of the components can follow the star around in its orbit? Can we discuss it in terms of model-atmosphere computations for homogeneous static atmospheres?

From this it would appear that we have no clearcut, unambiguous information on the distribution of material, excitation, or electron temperature in the atmosphere. Indeed, I am sure you will have noticed how careful Kuhi has been throughout the discussions to avoid committing himself on any of these subjects. But I personally think we have a great deal of information, although clearly not as much as we would like. First, no one seems to dispute that $T_e \gtrsim 5\times10^4$°K in the region where the continuum is formed nor that lines of N V and O VI appear in at least some spectra. Since these lines cannot be formed at 5×10^4°K, T_e must increase outward, at least initially. Second, we have data on V444 Cyg from the analysis by Mrs. Shapley and Kopal, which gives a rough distribution of density with height over part of the atmosphere. As I showed long ago, this puts stringent conditions on the atmospheric model. Third, we have Kuhi's suggestion that those lines with violet absorption edges are formed farther out in the atmosphere than the other emission lines. He bases this on (1) the fact that these absorption components are sharper than any emission line and may therefore be formed outside the electron scattering part of the atmosphere, and (2) the fact that these absorption components arise from levels strengthened by dilution effects, which increase with increasing distance from the star. I think this suggestion deserves considerable attention since it could give important information on the distribution of excitation and on the region of expansion. Finally, the rocket-UV observations allow us to explore the distribution in depth of excitation and ionization. As in similar solar investigations, this may at least provide limits on the kinds of models that are compatible with the data.

Turning to the diagnostic aspects and the summary by Anne Underhill, I find a slight difference of opinion between Anne and myself. Anne believes that everything is now too mixed up to conclude much of anything. I, on the other hand, believe that we have significantly narrowed the alternatives for the atmospheric structure and also for the causes of this structure. Let me quickly summarize the arguments I gave during the discussion of her paper.

1. The spectrum consists mainly of emission lines; absorption lines occur only as satellites at the violet absorption edges of the emission. This is a stringent condition. Of the three ways to produce an intrinsic emission line, I have rejected two (a Schuster-type mechanism and a fluorescent mechanism), leaving only the possibility of a chromosphere-corona type atmosphere in the whole region of line formation. The alternative to an intrinsic emission line is one arising wholly from the geometry of an extended atmosphere.

2. The fact that the excitation level of the line spectrum exceeds that of the continuum ensures that the lines are not formed, as in planetary nebulae, by a recombination spectrum. The situation resembles that of a chromosphere-corona configuration.

3. The breadth and shape of the line profiles also support the chromosphere-corona model. First, the observed density gradients (V444 Cygni) and atmospheric extensions imply either a dynamic support of the atmosphere or a temperature of about 10^7°K. Random motions of the type suggested by the profiles ensure mechanical dissipation of energy and hence an energy supply. So while the emission lines could conceivably be explained wholly by the geometry of an extended atmosphere, the extended atmosphere itself requires just that chromosphere-corona configuration that it was introduced to obviate.

I conclude that the WR atmospheric configuration is simply that of a greatly enhanced chromosphere-corona: enhanced in terms of the size of the mechanical energy and momentum effects and in the amount of mass involved in these effects. Given this, we can comment on question (2) as well as on question (3). The feature of the WR object that produces its distinctive atmosphere is a high velocity field of mechanical energy; the features we should look for in the spectra of quasi-WR objects are just those that accompany such a supply of mechanical energy.

To clarify our picture of the WR and quasi-WR objects, we will want to study:

1. The aerodynamics of motions of 100 to 1000 km/sec in a gas whose density is $\leq$ about 10^{14} particles/cm^3. In particular, we will want to inquire into the diagnostic separation, in optically thin and thick atmospheres, of random motions from motions with a unidirectional velocity gradient along a radius.

2. Problems of radiative transfer under such conditions.

3. Problems of statistical equilibrium under such conditions.

This now brings us to Lindsey Smith's remarks on question (4).

4. The Structure of a WR Object and its Effect on the Atmospheric Features

We must distinguish carefully between Lindsey Smith's specific suggestion for a possible structure, and the general kind of structure implied by the physical nature of her suggestions. The specific suggestions are a blend of the work by Paczynski and his collaborators, and by Kippenhahn and his collaborators, as applied by Lindsey to support her inferences on the significance of spectral subclasses. They also follow from the observation that a great many of the WR stars - all of them, according to some conjectures - are components of close binary systems, of which the other member is an early type star usually of class O.

The arguments rest on three properties of the atmospheric model: (1) the requirement of a source of mechanical energy; (2) the division into C and N sequences, which can plausibly be interpreted as a difference in chemical composition; and (3) the difference in ages, luminosities, and levels of excitation between the subclasses and between the sequences, coupled with evidence that the WR stars are not very old objects. The arguments can be summarized as follows: Because the primary component of a close binary loses mass to its companion, it will evolve rapidly to a configuration with a thin outer hydrogen shell and a He-burning core. It will then be overluminous for its mass. Such a helium configuration is probably vibrationally unstable and if so, this instability could provide the required supply of mechanical energy. The only question is the extent to which the hydrogen shell will damp the instability. The division into two sequences is explained by a suggestion of Iben's, quoted by Paczynski, that an inner region where C has been changed into N by the CNO cycle will be separated by a sharp boundary from an outer part where C is essentially unaffected. If the mass loss comes from sufficiently deep regions, then it could change the C:N ratio. Paczynski gives other arguments (*Acta Astronomica*, 17, 375-6, 1967), but this summary suffices to exhibit his thinking.

Finally, Lindsey acknowledged that there might be other evolutionary paths by which a single star, for example, could reach the He-burning configuration, but that these would take a longer time and would result in a greater age for the star. This agrees well with her results suggesting that single WR stars may be slightly older than binaries.

Because the binary path seems well established, our primary attention is directed toward these alternative evolutionary paths. We ask whether such paths will lead to a WR object or only to a quasi-WR object. What are the basic characteristics of the model?

Fundamental to the model is the production of a He- or C-burning core and thin H-rich shell. It is this configuration which produces the instability which in turn produces the oscillations that provide the mechanical heating of the atmosphere. The characteristic of the WR spectrum that distinguishes it from, for example, the spectrum of the outer solar atmosphere is the intensity of the effect. The entire line spectrum of the atmosphere is involved. So it remains to be determined whether it is the quality or just the quantity of the mechanical heating that is distinctive. But if a He- or C-burning core surmounted by a thin H envelope is a sufficient condition for a WR spectrum, then anything that produces it will give the spectrum. For example, if a nova were a star which had a He- or C-burning core, and if the nova explosion could blow off enough of the outer H envelope, then we might expect a WR spectrum. Indeed, any star with such a core-configuration that could blow off the H envelope should act the same way. Then to what extent does the spectrum depend upon the remaining characteristics of the star, such as mass, radius, and gravity? The details of the energy supply and the accompanying aerodynamic motions are at this point mere conjecture.

This is a good point at which to close a survey of questions (3) and (4). In discussing the atmosphere we have given reasons for rejecting some alternatives as well as reasons for preferring others. Their consequences have not been worked out, but it seems to me that we have only a small range in possible atmospheric models. The discussion of a general model for WR objects is provocative, even though highly speculative. But this is what we need for the WR objects: something on which to hang our hat as a first approximation.

5. The Structure of Quasi-WR Objects and Its Effect on the Atmospheric Features

We have had no detailed discussion of question (5). I have commented over the years on the relation between chromospheres and normal atmospheres in various kinds of objects, but I shall not repeat them here. It is clear that there are many sources of stellar instability and that they all lead to the production of chromospheres and coronas of one kind or another. Whether the variations on the He- or C-burning core and on the H-rich envelope lead to the unique type of chromosphere-corona that produces the quasi-WR spectrum can only be settled by further investigation. In the meantime, the solar and WR features appear to represent two extremes in the range of mechanisms by which such outer atmospheres can be produced.

These points are now open for general discussion.

DISCUSSION

Thomas: I suggest we follow the outline of the summary, treating in succession questions (1) to (5). So we start with (1): taxonomy. We discuss the possible existence of a homogeneous class of WR objects or phenomena exhibiting a "pure" WR spectrum. We also consider the quasi-WR objects. The discussion should be primarily from an observational-empirical viewpoint; we should recognize, however, that we are likely to become involved with considerations on question (2) - those features common to both WR and quasi-WR spectra that imply that some atmospheric properties of the former are also present in the latter.

Underhill: I don't really see the point of separating the WR spectra into these two categories, WR and quasi-WR. In all these types of stars we are looking chiefly at emission lines. The excitation of emission lines is a spectroscopic phenomenon that occurs in a plasma of a certain density and a certain amount of excitation energy. We have concluded that we can not maintain this level of excitation by radiation alone; we have to have a source of mechanical energy. So from the physical viewpoint, all these objects belong together. The so-called classical WR stars are distinguished only by their distinctive and well grouped class of lines. The others show the same phenomena, but because of the level of excitation or something - it's hard to be specific when you haven't described the physical process - we are not so struck by their peculiarity.

Thomas: Precisely. The "pure" WR spectrum is well defined by a set of characteristics which are exceptional and which I believe require very precise conditions for their formation. The quasi-WR spectra have no such well-defined characteristics, nor do they imply, in consequence, such a unique set of atmospheric conditions. I think you are much too glib in passing off a spectrum composed exclusively of emission lines as "a spectroscopic phenomenon that occurs in a plasma of a certain density and level of excitation". Can you <u>really</u> tell us the density and excitation conditions required to produce the WR emission lines? I remind you of the long-standing controversy in the astronomical literature on the interpretation of the conditions underlying the production of <u>any</u> emission line. Only recently have we even begun to sort out the situations under which such-and-such a variety of emission line can be produced.

Underhill: The difference between your two general classes is not well defined. The various spectral types are well defined and easily identified, but the basic physical problems are the same for all of them. So even though we speak of 7 or 8 different spectral classes, we should realize that all these stars lie in one physical class - they are stars with spectroscopic phenomena that imply a mechanical source of heating in an extended atmosphere.

Thomas: Welcome to the ranks of those who support the broad general thesis of twenty years ago. But now we are trying to establish the details of the mechanical energy supply and the precise way in which it interacts with the atmosphere. On the basis of what we have learned about the interpretation of spectra produced by such atmospheres, this division seems helpful in establishing reference points for our analysis.

Hjellming: I think the two general classes are based more on our ignorance than on our knowledge of any given object. First we define a WR phenomenon; then if we know nothing more about the object than that it shows a WR spectrum, we call it a WR object; if we know something more about it - for example, that it has been a nova - we put it in another category.

Stecher: I would like to correct an omission in the category of quasi-WR phenomena. Every observation that Carruthers, Morton and I have made on supergiants earlier than B8 (and B8 is the latest type we have observed) shows the resonance lines in emission with a Doppler-shifted absorption edge. The velocities corresponding to this shift are of the order of 2000 km/sec or greater, twice that observed in WR stars. I interpret this as evidence that mass is leaving early type supergiants with very large velocities.

Thomas: Does this mean that you would include all supergiants earlier than B8 in the quasi-WR category?

Stecher: I think we have to include them. Except for the presence of forbidden lines and recombination lines, they look very similar to the WR stars in the rocket UV. Perhaps the higher velocity fields give lower densities, so that you can't populate the higher levels that give some of the WR lines.

Roman: I would like to defend the distinction between WR objects and quasi-WR objects. I don't know of any system of spectral classification in which you do not have both borderline cases and related objects that do not satisfy any firm set of criteria. That doesn't make the division of objects into spectral classes useless. I think you'll have to keep your eye on quasi-WR phenomena as an indication of what is going on in WR objects. But I think that if you lump them all together, you will become hopelessly confused in trying to define a WR object.

Sahade: I too agree with this separation into two groups. I would just like to add a fifth condition to the four defining group A: the time-constancy of the four other conditions.

With regard to Of stars, which have been included in group B, I think we should keep in mind that they might turn out to be very similar to the WR objects. Let me remind you of the characteristics they share in common. Those Of stars that are binaries seem to have O-type companions and seem to be less massive than their companions. You have already seen the similarity in spectral features: Stecher showed it for ζ Pup in the rocket UV; several years ago R. Wilson of Edinburgh showed that underlying the narrow emission features characteristic of spectra in the photographic region, are broad emission features similar to those found in WR stars. So it may well be that the Of stars are closely related to the WR stars in some way not yet clear to us.

West: I suggest we change the name "WR phenomenon" to "emission line phenomenon" because all these objects are special cases of the same mechanism.

Thomas: This would be difficult: after all the Sun is an emission line object, so you are probably suggesting we include most of the stars. A more specific objection is that many kinds of nebulae are emission line objects, and we do not believe, for example, that the emission lines in planetary nebulae are produced by the dissipation of mechanical energy, but rather by the reverse process, a degradation of an ultraviolet radiation field.

Stecher: The question is: What is the natural division? We have been discussing the spectral lines as the distinctive feature; but I think it may be the velocity field which provides the physical relationship between these objects. I think Miss Underhill has been implicitly assuming a large mass loss from a number of these objects, and this is what relates them physically.

Thomas: Velocity fields and mass loss are quantities derived from an analysis of spectral lines. Until you are sure your diagnostic methods are correct you cannot use them as taxonomic criteria. And a large part of our discussion has centered on the methods by which to analyze these spectra.

I suggest we now move on to discuss the homogeneity and uniqueness of these spectral subclasses with respect to the physical quantities characterizing WR stars. Each of these quantities must be derived by some kind of analysis of an observed quantity.

Smith: I have always thought the things that define what a star is and what it looks like are its basic properties: mass, initial composition, and age. Now in the present discussion, we may have to add as an additional parameter the binary character of the star, and therefore the properties of the companion and the separation may be important. Given all these properties, we should be able to predict the evolution and spectral characteristics of the star. So the simplest explanation of these objects would lie in a one-to-one relationship between the spectral properties we observe now and the initial parameters. If you claim that a great range of initial parameters can converge to produce one spectral subclass, the situation becomes extraordinarily complicated, and I then see no reason why we should have a large range of subclasses. You would be saying the WR subclass is some sort of random property accidentally generated from one of a series of initial parameters. It seems infinitely more likely, and is in agreement with observed correlations, that certain initial parameters will produce certain final products. So I would suppose that when we see a class that appears homogeneous in spectral properties, luminosity and distribution, this class does represent a stage in a unique class of objects (as defined by the initial parameters). Therefore I assert that if we define our subclasses properly, those properties I listed will lie within narrow limits in each subclass.

Thomas: Do I understand properly? You assert that luminosity, radius, mass, and some parameters characterizing the continuous radiation field and the binary character should be nearly constant within a given subclass?

Smith: Yes, within a reasonable standard deviation, which represents not just observational error but a genuine spread in physical properties. And some of these properties vary from one subclass to another.

Payne-Gaposchkin: I don't think I can let this point of view pass. There are a number of parameters mentioned here, but the one I shall talk about is mass. It would be a brave man who would make the statement that throughout the whole range of spectral classes there is even one class that has a unique mass. There are less than 60 stellar masses known, including both components of binaries, and you must have a binary to get a mass. Those binaries from which masses are determined can be roughly classified as close binaries and wide binaries. I don't think anyone will quarrel with the idea that the wide binaries such as ZZ Boo, WZ Oph, 70 Oph are best for determining masses. Now these systems all have main sequence components, and if there is any spectral class for which you can state that you can unequivocally assign a mass, it would be main-sequence luminosity class V. Here I wouldn't quarrel with Lindsey.

However a large number of masses are not determined from wide binaries on the main sequence. Those stars which have the very highest masses, e.g., V Pup, μ Scorp, UW Can Maj, are almost contact binaries. They are still main-sequence stars, but in terms of mass exchange, they are somewhat contaminated. Nonetheless, these stars do not contradict Lindsey's assertions. If we include luminosity class as well as spectral class, stars of similar spectra do seem to have similar masses. You must recognize, however, that the material is rather more uncertain because of uncertainty in the inclination of the orbit.

Now there are other stars, still on the main sequence and still contact binaries, for which the masses are notoriously different although the spectral classes are nearly the same. The W Ursa Majoris class contains a number of examples, e.g., W Urs Maj and U Peg. Then you have another set of still queerer pairs for which the masses are extremely well determined: Sirius and its companion, and Procyon and its companion; Z Herc consists of 2 subgiants of about the same mass but different spectral class and too bright for the mass-luminosity relation. There are still odder objects like 85 Peg, which has two components of apparently equal mass, one a main-sequence star, the other 3 magnitudes fainter and probably a subdwarf. All these stars form strong exceptions to Lindsey's suggestion of a specific mass for each spectral class. There is also a very large group of stars off the main sequence, ε Aurig, ζ Aurig and VV Ceph, for which masses are known. Both components of ζ Aurig seem to satisfy the mass-luminosity relation, but some of the others don't.

All this simply illustrates Strand's statement that stellar evolution is all very well until you begin to look at double stars. It is nice to think you can get out of the problem by saying all the problem children are contact binaries; but you can't use that escape for Sirius or for Procyon, which are wide visual binaries with periods of about 50 years. It is clear that when you speak of stars with masses that do not agree with the mass-luminosity relation, you are speaking in the majority of cases of close and contact binaries. And I think it is in this category that we must place the WR stars. If I might stretch a point, I think we can regard WR stars as resembling Algol stars in that the two components are physically so extremely different; one is on the main sequence, and the other is probably a subgiant. How many well-determined masses are there among the WR stars?

Underhill: Seven systems - possibly eight - of which none are well determined.

Smith: Two and one-half.

S. Gaposchkin: Only one star.

Payne-Gaposchkin: I am talking about M, not M sin i; if they are not eclipsing systems, you can only determine $M \sin^3 i$.

Underhill: There are 3 eclipsing systems, not counting CQ Ceph.

Thomas: Cecilia, it seems to me you are saying: (1) We only know one or two masses, so it is impossible to know whether Lindsey's assertion is correct, but (2) on the basis of my other experience, I, C.P.G., do not believe it.

Payne-Gaposchkin: From what we know about far more well-behaved stars than the WR's.

Thomas: Would you comment on the visual magnitudes from the same standpoint? You are destroying our picture of physical homogeneity within subclasses; I am asking you to destroy it point by point.

Payne-Gaposchkin: Without having the data before me, I would not like to comment. I have heard what Lindsey and Anne have said, but I haven't been looking at their data. The absolute visual magnitude depends critically on the WR stars in the Magellanic Clouds. But turning again to stars in general, I think that except on the main sequence it would be very dangerous to try to specify the absolute magnitude from the spectral class. This is particularly true for the supergiants.

Thomas: We find ourselves in a very interesting situation. My viewpoint is similar to Lindsey's. If we can't get homogeneity by progressing to smaller and smaller subclasses, we must discuss each star individually. This is grim. So let's consider the next point: the possibility that a temperature, or some other measure of the distribution of energy in the continuum is constant within a subclass.

Payne-Gaposchkin: Here I don't object. What evidence we have is sound and convincing. But there are mighty few stars, and you can always put a straight line through two points.

Underhill: I want to make a basic comment on the method of procedure. For absorption-line stars on the main sequence, we have some confidence in the idea that if the spectrum is excited by radiation, the mass and luminosity are related to certain identifiable spectral features. We have hypothesized that this is a fixed rule of nature, and that we know what the rule is. I am not so certain that we do know the rule, but perhaps we are close to it.

Payne-Gaposchkin: So you disagree with my statement that knowledge of the spectral class does not necessarily imply knowledge of the mass?

Underhill: That is the assumption on which we proceed. I agree with you that it is a bit doubtful, but it is the basic assumption that is made - for the main sequence and nowhere else. Now it will certainly not work for the WR stars or for the whole group of stars with emission lines. There we are still searching for the relationship between spectrum and physical characteristics such as total radiation field and mechanical energy. When we can isolate the basic physical processes, we shall be able to relate them to the one or two masses and one or two luminosities that we know.

Payne-Gaposchkin: What I really want to take issue with is the belief that because you have to know certain parameters in order to interpret the spectra in terms of a given theory, it is correct to assume that you do know them.

Thomas: Let us carefully distinguish two questions: The first is whether in principle and in practice there is a homogeneous set of physical parameters that characterize a subclass; the second is whether we actually know the values of these parameters for each subclass. I had thought you were saying "no" to the first question; your last comment seems to imply "possibly" to the first question but at the moment "no" to the second.

Payne-Gaposchkin: I don't think that even in principle there is a homogeneous set of parameters for non-main sequence stars. I put most of my emphasis on masses only because they are a definite thing to talk about. I think Nancy Roman knows more about masses than I do, and I wish she would say whether she agrees with me.

S. Gaposchkin: There are only 54 masses determined from eclipsing binaries; if we include the visual binaries, the figure rises to no more than 70. The majority of these lie on the main sequence.

Underhill: Everything we know about the visual absolute magnitudes of WR stars is contained in the following table, extracted from Lindsey Smith's thesis. The results are based on stars in the Large Magellanic Cloud that show no evidence of being double. You can see how few stars we have; you can

Class	Number	M_V	S.D.
WN3	2	-4.5	±0.1
WN4	5	-3.9	±0.3
WN5	2	-4.1	±0.05
WN7	4	-6.8	±1.0
WN8	3	-6.2	±0.4

guess whether a statistician would go along with conclusions drawn from them. In addition, we have γ Vel and one other WC8 in the Galaxy, which Lindsey suggests gives us a magnitude about -6.2. I have stated why I think -5 would be more appropriate. So if I put this all together, I have WC7 and WC8 about -6 mag or brighter and all the rest about -4.5 ± whatever your statistical sense tells you is appropriate. I hope you will agree that this material is not sufficient to permit any differentiation with respect to spectral subclasses.

Thomas: The question was not whether a given mass or luminosity represents a unique subclass, but whether a given subclass represents a unique mass and luminosity - within some range.

Underhill: You can't answer either question. The absolute visual magnitudes are in no way related to mass. For the WR stars, I don't need to know the subclass - the absolute visual magnitude is -4.5.

Thomas: Anne! If I know the spectral subclass, do I know the visual magnitude?

Underhill: Yes. Even if you don't know the subclass, you know the visual magnitude.

Payne-Gaposchkin: A standard deviation of ±1 magnitude means you don't know!

S. Gaposchkin: I have the best determined absolute luminosity: V444 Cyg (WN7), -2.7. How can you disagree with that?

Underhill: The light curve is ambiguous. I know how to interpret the light curve of two spherical, well-separated stars. I don't know how to interpret the light curve of V444 Cyg. All spectroscopic investigations of that system indicate that these stars are <u>not</u> well separated and they are <u>not</u> spherical. Therefore I do not believe your value.

Kuhi: I agree with Anne one hundred percent.

Sahade: The point is that the luminosity ratio was not determined from the light curve because it was impossible. It was determined from spectra of stars other than V444 Cyg. Kron worked out his light curve and had to use the luminosity ratio that was in the literature, but that value was completely unreliable.

Westerlund: I would like to offer the following explanation for the lack of agreement between the absolute magnitudes determined from binaries and from WR stars in the Magellanic Clouds. In the Magellanic Clouds, the Wolf-Rayet stars appear at the turn-off points of the HR diagrams of clusters and associations. The ages of these stars are about 5×10^6 years. During this time the massive stars have increased in luminosity by at least 1.5 magnitudes. From the binaries you derive a luminosity by assuming the star is still on the main sequence. This value must then turn out to be at least 1.5 magnitudes too low. I think everyone will agree to that. The WR stars are not much more over-luminous than other slightly evolved massive stars.

Payne-Gaposchkin: On homogeneity of subclasses, I think it is worthwhile to raise again the question of the binary character of WR stars. If one looks in the catalogues, one sees a large number of double-line spectroscopic binaries. There is also a large number of single-line spectroscopic binaries in which the second star is there, but is not bright enough to affect the integrated spectrum. Its presence can be detected only by a change in radial velocity that can't be ascribed to pulsations of a single star. If there are no absorption lines, it is very difficult to detect velocity variations in WR stars. I don't remember the proportion of double- to single-line spectroscopic binaries: Is it not possible that double-line spectroscopic binaries are in a minority and that many WR stars are members of single-line spectroscopic binary systems? This would make a big difference in all our discussions about WR objects.

Underhill: One-third of all WR stars might be members of spectroscopic binary systems; we don't know about the others.

S. Gaposchkin: I too believe that all WR stars may be members of binary systems.

Thomas: Now today we are trying to summarize areas that were discussed in preceding sessions and to raise new considerations that may have come up when you were digesting earlier discussions. If there are points on which we agree, it will be surprising but most welcome. If there are points on which we disagree, we should carry the discussion just far enough to focus on the issues and then drop it. Hopefully you will go home, solve the problem, publish a paper, and acknowledge this symposium for providing the choler that pushed you into doing it. Now some of you are outspoken in your disagreement; some of you don't come forth with your objections except during the coffee break; I have been asked to clarify two points raised over coffee.

The first point is Charlotte Pecker-Wimel's assertion that my criteria (1) to (4), by which I define the WR spectroscopic class, simply confuse the issue of the central stars of planetary nebulae. She asserts that the planetary nuclei with WR spectra satisfy all the criteria (1) to (4) and that the only way to distinguish them observationally is by adding another criterion specifying the range in absolute magnitude. But absolute magnitude is not an observational criterion. It is my understanding that our values for the luminosities of these stars depend on distance determinations by Seaton and others who warn that they are subject to large errors. Similarly, we believe that planetary nuclei have masses of about one solar mass, but again this is not an observational criterion. Now you will recall Sahade's suggestion that as a fifth criterion we specify the time-independence of criteria (1) to (4). If someone can add a further specific criterion, I shall be delighted. But I would have thought we had just had an excellent demonstration of the uncertainty in the absolute magnitudes of WR stars. Aller's discussion of his own and Lindsey's work on the nuclei of planetary nebulae covers about all we can say.

The second point raised over coffee concerns the definition of the chromosphere-corona. By this I mean that part of the atmosphere where the assumption of radiative equilibrium breaks down, where a mechanical supply of energy is required to maintain the state of the atmosphere. In the classical model of a stellar atmosphere, the temperature decreases monotonically outward; thus the chromosphere-corona is often thought of as that part of the atmosphere lying above a temperature minimum. But several years ago Cayrel suggested that the same mechanism that fixes the temperature of planetary nebulae at about 10^4°K would act to produce an outward increase in temperature even in a classical model; T_e would rise from the boundary temperature to about the effective temperature of the star. The explanation is that at low densities T_e is fixed by the spectral quality of the radiation rather than by the energy density of the radiation field as at high densities. So if we want it to connote a departure from the classical model, we have to define a chromosphere-corona in terms of the contribution from non-radiative (usually mechanical) energy sources.

Johnson: So your definition of a chromosphere-corona could include an envelope produced and heated by mass exchange from one component of a binary to the other?

Thomas: Yes. Now that we have discussed the question of WR objects and the homogeneity of their subclasses, I suggest we turn to question (2): Are there features common to both WR and quasi-WR spectra that are produced by the same mechanism in each case?

Sahade: It is not obvious that the same mechanism produces the instability in all your quasi-WR objects. And I would have strong doubts about including the Be stars as members because their instability seems to be gravitational, and this is ruled out for WR stars.

Thomas: I think you must distinguish carefully between the mechanism of the instability and the physical effect of that instability on the atmosphere or its spectral features. It seems to me we are now pretty well agreed that those objects exhibiting some variety of WR spectrum have in common a supply of mechanical energy to their atmospheres. In proposing my categories A and B, I hoped to suggest differences in the detailed properties of this energy supply. We might suppose that the classical WR objects have a common kind of instability which produces the particular size and form of mechanical energy supply responsible for the "pure" WR spectrum. The quasi-WR objects might have different kinds of instabilities, which lead to different properties of the atmospheric energy supply and thus to different spectral features. If the common spectral features imply common atmospheric conditions, their study may help to clarify the mechanisms which produce them. I think the chief criterion for a quasi-WR class is a functional one: how much will it contribute to our study of classical WR stars? We could, for example, try to limit the category to those objects that have a large momentum as well as a large energy supply to their atmospheres; but then we would be prejudging on a basis not wholly observational.

Pecker-Wimel: You must be cautious about including the central stars of planetary nebulae in your list of quasi-WR objects because some of them show only absorption lines, and some show only continuum.

Roman: I'd like to be sure that "novae" include old novae and dwarf novae as well as supernovae. And I think you should include symbiotic stars and possibly those stars that have been definitely identified as x-ray sources.

Payne-Gaposchkin: I would like to draw your attention to the similarities between novae and Wolf-Rayet spectra. First, they both have broad emission lines with violet-displaced components. There is a definite correlation between the violet displacement of the lines, presumably due to the ejection velocity, and the apparent spectral class of the star. Nova Pictoris, one of the slowest novae, had an expansion velocity of 75 km/sec and a spectrum of class F5 before maximum. Nova Persei, 1901, was one of the fastest with an expansion velocity of about 2700 km/sec and a B-spectrum before maximum. Various other novae fell in between. Also, the excitation of the spectrum associated with the absorption components increases as velocity increases; this convinces me that it is largely mechanical. My second point is that among novae, as among Wolf-Rayet stars, there appears to be a significant difference in chemical composition as determined from the bright lines. For example, Nova Persei showed the forbidden lines of Ne III, Ne IV, and Ne V in successive stages. Nova Aquilae, on the other hand, was strong in oxygen, especially O III. Nova Geminorum 1912 was very strong in nitrogen. Nova Pictoris was strong in the iron lines, all the way up to Fe VII, and oxygen lines were very inconspicuous. Does this really reflect a difference in chemical composition? I know of only one nova for which a curve-of-growth analysis has been attempted. This is Nova Herculis, for which I made the analysis myself, but the results cannot be taken very seriously. However, there does seem to be a high abundance of carbon (recollect the cyanogen spectrum of Nova Herculis) and also of oxygen. I believe Aller attempted a similar analysis which has remained unpublished. Thus the situation for novae is similar to that for WR stars: we have neon novae, oxygen novae, nitrogen novae, and iron novae. The problem is, however, more difficult for novae than for WR stars because the most conspicuous lines involve forbidden transitions. I would guess that the WR problem will be solved before the nova problem.

For these two reasons - the correlation of excitation with radial velocity and the apparent difference in chemical composition - I feel we are justified in including novae in the same general category as the Wolf-Rayet stars. The novae certainly have expanding outer regions; for many of them we can directly observe the expanding nebulosities.

Underhill: I think the novae spectrum gives every indication that it is excited by mechanical energy. In general the novae have lower expansion velocities than do the WR stars, although there is some overlap. It seems to me probable that the excitation of the spectrum is a local process, depending upon the density. If you talk to plasma physicists who try to excite the third, fourth, and fifth spectra of various ions in theta-pinch configurations, they tell you that the geometry and local density are critical in determining which spectra they get. So I am more than ever convinced that the apparent differences in WR spectra and in novae spectra are largely chance effects of density and collisional mechanisms. Until we have sorted out these things, I am dead against speaking of abundance differences.

Thomas: I would agree with almost everything you say, except when you refer to "chance occurrences" and to the spectrum being excited locally. The latter is a strong statement; it implies that the radiation field produced in the atmosphere has nothing to do with the excitation; it implies that the atmosphere is optically thin. Maybe so, but it has yet to be proved.

Stecher: In trying to decide between protons, electrons, and α particles as the exciting particles in collisions, we should note that because of conservation of spin, neither protons nor α particles can easily excite the triplets in C III, N IV and O V. Recent experiments have shown that the cross section for exciting the He triplets is down by a factor of at least 100 over that for exciting the singlets. Thus although you can ionize in this way, the spin conditions make it unlikely you will excite the upper triplet levels.

Underhill: A more important point is that protons and electrons are equally effective in exciting He when they have the same velocity, not the same energy. So their effectiveness depends on how they are being accelerated. If we have magnetic fields, the proton excitation may be important; if we have only thermal velocity fields, I agree that the electrons are the most important.

Kuhi: I'd like to add one point, to emphasize Anne's concern. Mrs. Gaposchkin mentioned Nova Aquilae; this nova has changed its spectrum quite dramatically over the years. It started with very strong N III lines, the λ4640 complex, and then gradually changed over to C III and IV, λ4650. This is a very important point because surely the composition didn't change.

Beals: Rather than your two categories of classical WR stars and quasi-WR stars, I think that I would prefer the following breakdown: (a) regular non-binary WR stars; (b) regular binary WR stars; (c) WR nuclei of planetary nebulae; (d) stars showing only He and Hα. It appears that group (a) is getting smaller all the time, but it is the one I would study were I starting out again. Group (c) seems to be a good deal smaller than groups (a) and (b), but this is hardly a reason for excluding it. I had never heard of group (d) before, but it looks highly interesting. In a print Miss Smith showed me, Hα is so strong and He II λ5411 so weak that I don't feel one could attribute it entirely to He. I think we must consider class (d), particularly in view of Miss Smith's discussion of the evolution of these stars.

Now I have two remarks that may possibly relate to the question of spectral features common to WR stars and other objects. One concerns the suggestion of a coronal heating mechanism, which I find very interesting; the second is another solar analogy related to flares. I understand that in some solar flares, atoms are ejected with velocities up to 1200 km/sec. Furthermore, flares are associated with solar prominences. So if these kinds of things can happen on a quiet star like the Sun, they might occur in a greatly magnified form on the WR stars. In complicated conditions like those responsible for WR emission, it is unlikely that only one mechanism is operating. I would suggest that the coronal and the flare mechanisms should be considered.

Hjellming: Following in the spirit of Beals' remark, I would like to raise the possibility of a classification scheme based on the possible forms of mechanical energy. From the solar data, we can make two very clear divisions. First, there are the hydrodynamical phenomena such as the corona and the solar wind, and second there are the more energetic phenomena related to cosmic rays. It is well known that there are roughly three classes of cosmic rays from the Sun. One class occurs in the presence of a strong flare and produces extremely energetic particles up into the bev range; this happens once every year or two. A second class, again associated with a flare, produces energies of tens or hundreds of mev and occurs once every few months. Finally there is a quiescent, virtually continuous, form of emission which produces particles of a few mev. In discussing the forms of mechanical energy in the WR stars, you could be concerned with either or both of these classes. Everyone who has mentioned a non-Maxwellian velocity distribution could equally well speak of a Maxwellian distribution of velocities associated with hydrodynamical phenomena plus a non-Maxwellian tail which everyone has called cosmic rays.

Underhill: All the more energetic classes of mechanical energy in the Sun have in common the presence of a magnetic field. So I think it is perhaps serious that we have omitted mentioning that magnetic fields are necessary for some types of mechanical excitation.

Thomas: We don't seem to have made much headway with question (2), and in a sense Beals' remarks carry us over to question (3): What are the distinguishing properties of the WR atmosphere that produce the unique WR spectrum? Kuhi has summarized our present knowledge of the WR spectrum, and Anne Underhill has commented on the diagnostics. I have mentioned three points that seemed to me to stand out in Kuhi's talk: the breadths of spectral lines, the possible relation between line width and ionization, and the complexity of the atmospheric situation as revealed by the study of binaries.

Sahade: As a bridge between this point and the discussion of quasi-WR spectra, we should not forget that novae and symbiotic stars may all be binaries, and the mechanism at work in them may be connected with their binary nature.

Deinzer: Kuhi suggested an atmospheric model consisting of two regions: a "turbulent" region in which the emission lines are supposed to be formed, and an outer expanding region. Would anyone care to make a guess at the mass of these regions? I am concerned with what happens to the mass which a WR star loses during its lifetime. It must be somewhere around a single star; in a binary system, it may go to the companion.

Thomas: This was the problem underlying an earlier discussion of the nebulosity associated with WR stars. The question was: Why, if indeed there is a general mass loss, do only some stars show nebulosity? Lindsey's answer was that the nebulosity you see consists not only of the mass ejected by the WR star, but chiefly of the matter swept up by this mass from the interstellar medium. The ejected mass has so much energy and momentum that it sweeps up much more material than is ejected. You wouldn't then expect to see nebulosity except in those regions of the Galaxy where there is plenty of interstellar material. A detailed interpretation of the nebulosity will depend strongly on a knowledge of the ejected mass, on the properties of the interstellar medium, and on the details of the interaction between the two. Also note such arguments as Schmidt-Kaler's in a preceding session, where he suggested that any ejection of matter could trigger the formation of a whole shell of stars. So with regard to mass loss and its effect on the environment, there is a great deal of work to be done. Don't forget that not everyone accepts this idea of mass loss from single stars; and even if you do accept it, you still have to know how great it is and how it produces the observable effects. This brings us back to all the diagnostic problems discussed earlier: how do you infer the velocity fields, the effect of electron scattering on line profiles, and so on. Lindsey, do you have a figure for the mass of the nebula?

Smith: My figures depend on all the things you mentioned. The mass of the nebula lies between 1 and many hundreds of solar masses, depending on the assumed mass loss, the density of the interstellar medium, and the interaction.

S. Gaposchkin: There is no mass loss except in the form of the so-called "jet" lines in β Lyr.

General chaos and screams of protest.

Underhill: Certainly there is mass loss. It's also easy to calculate the mass of the turbulent region of the atmosphere. Just take the volume of the shell contained between the photospheric radius of 5 solar radii and an outer radius of about a factor 7 larger; guess a density, and you have the mass. A reasonable density is 10^{11} particles/cm^3. Assume the shell is pure H, or the usual H:He mixture, and you get about 10^{25} gm.

Deinzer: And the mass in the expanding shell?

Underhill: Forget it - it's small. For an expansion velocity of about 10^3 km/sec, the upper limit on the mass loss is about 10^{-5} solar masses per year. Electron scattering may affect our estimate of the expansion velocity, but not by an order of magnitude.

Deinzer: This means the WR stars can't be very old.

Thomas: No, these estimates can be very bad; the star could even have thrown off its mass before entering the WR phase. Our interpretation of the so-called expansion velocity could be very wrong, considering the effects of electron scattering and the difficulties of separating the expansion velocity from the velocity gradients. The picture of a turbulent lower atmosphere surrounded by an expanding outer atmosphere is highly qualitative, especially in view of the size of the velocities and the lack of a physically consistent model relating them to the excitation state of the atmosphere.

Hummer: I'd like to express a personal prejudice that electron scattering will not play a major role in determining the appearance of the lines in a WR star. It would be difficult now to support this with facts, but this is the impression I get from having done a fair amount of work recently on electron scattering in line formation. An important point to remember is that the electrons may be in the same region as the atoms, and not on top, in a reversing layer of the kind usually considered in theoretical work on electron scattering in line formation. Because of the competition from absorption and line scattering, the effect of electron scattering is much weaker than if all of the electrons were isolated in a reversing layer.

Johnson: Would you be willing to draw a typical line profile and show us where in the model each part of the line is formed?

Hummer: I'd be willing to speculate on the kind of velocity fields associated with each kind of profile. The characteristic very broad flat-topped lines are formed by purely radial motion and occultation, with not much optical depth effect, probably because the velocity gradient is sufficiently large that even if the densities and path lengths are large, everything is optically thin. The big Gaussian lines are formed in turbulent regions in which the turbulence de-saturates the line by spreading the opacity over a sufficiently large spectral region that things can't become sufficiently optically thick for self-reversals to appear. The third kind of profile is the P Cygni type, which comes from an optically thick region in which the velocity in increasing outwards.

Wrubel: But you still can't explain Kuhi's eclipse data with this interpretation.

Hummer: That's right. I've been talking about simple geometries. If I understand what Len has been saying, I would have to look at the radiative transfer problem in a dumbbell.

Kuhi: Let's not worry about the eclipsing system - the cases with simple geometries should be solved first.

Nariai: May I ask if anyone here really thinks the turbulent velocity is of the order 1000 km/sec? It seems to me that this is what you are saying. I also recall your saying the temperature of the gas which produces the line is about 3×10^4°K.

Thomas: The empirical basis for the use of "turbulence" is that the velocity distribution in the atmosphere must have the same effect on the line profile as a random distribution of velocities along the line of sight over a volume element whose size is about the mean free path of a photon. Now whether this effect can be mimicked by a distribution such that the motions are all in one direction in one region of space and all in the opposite direction in another region of space, or whether we require random motions in a given region of space, depends very much upon the size and distribution of the opacity, and the opacity in turn depends upon the velocity, so we need self-consistent models. For years astronomers used the term "astronomical turbulence" to indicate random motions in one region of space. Collisions between turbulent elements were ignored or were tacitly assumed to be absent or ineffective. All these aspects of what one means by "turbulence" in astronomy are vague. If one uses "turbulence" in the aerodynamical sense of random motions in a given volume, then as I pointed out long ago this implies that $T_e \sim 10^7$°K in the WR stars. I also said I would prefer a more elaborate aerodynamic explanation, because 10^7°K seemed high even to me, but perhaps I was unduly pessimistic. In any event, the 3×10^4°K you quote refers to the region where the continuum is formed. It is interesting to note that the value of the electron temperature acceptable to the astronomical public seems to be increasing with time.

Hummer: The line opacity will be different in a turbulent situation than in one with velocity gradients, and therefore the relation of the source function to the emergent flux will be quite different in the two cases. Hopefully we can separate the two effects by looking at the line profiles.

Stecher: 10^7°K can be ruled out by the x-ray data. The x-ray flux at the Earth from γ Vel would be stupendous if the entire shell were at 10^7°K.

Thomas: You are probably right, but I warn you that you must consider the mechanism by which the x-rays are formed. I presume your remark is based on the assumption of blackbody emission at 10^7°K. This means the shell must be opaque in the x-ray region you are studying. Do you believe that? I remind you that in the early days when I argued for a high-temperature solar chromosphere, the same argument was made with respect to the hydrogen-Lyman continuum. When rocket observations indicated a radiation temperature of about 6×10^3°K for the Lyman continuum, the arguments were repeated. The point is that these arguments are wrong; the non-LTE effects introduce a big correction factor; so we do in fact predict the observed Lyman continuum from the high temperature chromosphere, I caution you to check the emission from your WR model on the basis of a correct theory before you determine T_e from the x-ray flux.

Underhill: Don't forget that in these observations you are integrating over the whole disk. Now suppose I make a schematic model with absolutely no physical basis. I postulate a magnetic field that looks like a porcupine, and this produces coronal streamers of the same geometry. By an unknown flare mechanism similar to that in the Sun, streams of particles with varying densities shoot out from each of these streamers. As observed from the Earth, this group of overgrown spicules has a wide distribution of velocities and will produce a Gaussian-shaped profile. The only problem is I can't explain how I get the spicules.

Thomas: Plus explaining all the other things I outlined a minute ago: optical depth effects, collisions, etc.

Underhill: Right. I've somehow got to control my spicules. But what we do know, without any doubt, is that there is a certain amount of low density material leaving the star as a spherically expanding shell. That is best shown by C III λ5696, which is excited by particular processes as I described in the last session. It has a weak absorption component. The width of the emission gives the expansion velocity. Then you can consider a completely unrelated He I line, whose absorption edge is formed in that part of the envelope between us and the shell, and the displacement of this edge gives you the same expansion velocity. To interpret the velocity field in the material forming the rest of the spectrum, you need the trained field of spicules.

Payne-Gaposchkin: There are two reasons why the material that produces the Gaussian profiles must be near to the star: the first is that none of these lines are forbidden, and the second is the evidence from eclipsing stars.

Schmidt-Kaler: I have the impression that the profiles are separated according to excitation: flat-topped lines, low excitation; Gaussian lines, high excitation. Is this correct?

Underhill: The separation is more by process of excitation than by level of excitation. The flat-topped profiles appear in lines that are excited by particular processes. They're the lines you see in emission in Of stars. I mentioned that C III λ5696 is excited by He II emission; so you need He II λ303 and a supply of C^+ ions, which implies a cool shell. In other stars N IV λ4058 is flat-topped. For this you need collisional excitation: He^+ ions colliding with N ions. He I λ5876 may be flat-topped and possibly λ3888; λ4471 is too blended to be sure. The He I spectrum is, I think, a simple recombination spectrum in an outer part of the expanding, low density atmosphere. I emphasize again that you see these flat tops in only one or two lines that can be excited in a low density gas, and that their widths all indicate the same velocity as that shown by the few absorption edges.

Schmidt-Kaler: This would seem to confirm Kuhi's model of an inner turbulent region and an outer expanding region: Flat-topped profiles are formed in the outer region and Gaussian profiles in the inner region.

Smith: Then it follows that the expansion velocities are greatest in the outer parts of the envelope, and that the narrow lines - those from the higher stages of ionization - are formed in the innermost regions. This would imply that the velocity increases and ionization decreases outward.

Sahade: In Part B, we noted the existence of two envelopes, at least in binaries. One lies around the WR star; it is thick, it is where most of the emission lines are formed, and it follows the motion of the WR star in its orbit. All the evidence suggests that the matter in such an envelope is being decelerated outward, implying that lines like those of He II are formed close to the surface of the star, and lines corresponding to higher degrees of ionization are formed further out. Finally we reach the large expanding envelope where the lines show dilution effects, and the radial velocities do not follow the orbital motion of the star but always show the same velocity.

Thomas: I too prefer this picture to Lindsey's. Note that we have an initial jump in T_e from the photosphere to the region of line emission, so initially at least the ionization must increase outward. This follows the behavior in the outer solar atmosphere where again we have a region - the chromosphere and lower corona - of high excitation which shows no evidence of expansion. The solar wind expansion occurs much farther out, and indeed in a recent speculative paper on x-ray emission from WR stars, Wallerstein and collaborators (*Ap. J.* 151, L121, 1968) suggest the possibility of a stellar wind. So all this accords with Sahade's suggestions.

Smith: Is there no evidence of an outward increase in velocity in the solar chromosphere-corona? Is it possible that the temperature increases and the velocity decreases to a certain point where the situation then reverses?

Thomas: There is no evidence of expansion in the solar chromosphere nor, indeed, in most of the corona. I think that for WR stars we can only try to extend the work of Wallerstein and his collaborators. But if the expansion arises from the same source as the solar wind, rather than from some internal ejection process such as is usually assumed for WR stars, we must know the value of T_e in the WR corona. Maybe we have simply been arguing about the wrong kind of ejection-expansion all these years.

Stecher: I have a point that serves as a transition between questions (2) and (3). For γ Vel we have two values of g corresponding to two values of radius, which depend upon the assumed distance. The range in log g is 1.5 to 2.5. Given a photospheric temperature of 4×10^4 °K, can you construct a stable model in hydrostatic equilibrium with a value of g in this range?

Underhill: A very strong no. With $T_e = 3\times10^4$ °K and log g = 4, you can barely hold a model in hydrostatic equilibrium, particularly if you include the radiation pressure in the lines.

Payne-Gaposchkin: But you can't make a stable model even for Rigel.

Underhill: Not one in hydrostatic equilibrium.

Stecher: It is losing mass. I see Doppler-shifted resonance lines both in absorption and in emission in all supergiants.

Payne-Gaposchkin: And there are very interesting changes in the velocities of the absorption components of Hα, which are superposed on the emission lines. Do I understand correctly that you cannot make models for any of these stars?

Underhill: There are some model atmospheres published at effective temperatures below 2×10^4°K with log g as low as 1. These are numerical calculations that I don't think have anything to do with supergiants.

Thomas: We turn now to question (4): What is the complete structure of a WR object, and how does the structure produce the atmospheric features?

J. Cox: I have one comment and one question which is essentially the question Paczynski asked in his paper. If one computes a period for radial pulsations in the fundamental mode for pure helium stars of one solar mass, the period turns out to be about 10 to 15 minutes. For pure helium stars of 10 solar masses, the period is between 1 and 2 hours. Now if the helium star has a hydrogen-rich envelope on top of the helium core, the period will be longer by some factor which will depend on the radius of the star. The point is that the period could be anywhere from a few minutes to perhaps a day. Now Paczynski's question is: Are light variations with periods ranging from a few minutes to a few hours observed in the WR stars?

Smith: Many stars are known to be erratically variable. If the periods are that small, the periodicity could easily be missed.

Kuhi: I think the real answer is that no one has looked for such short term variations, although it would be easy to do so. Merle Walker has detected a period of 71 sec in DQ Herc, and I have recently been looking for a period of 1.337 sec in pulsars. A moderate-size telescope of 50 or 60 inches could easily do it, but we would have to be very careful to distinguish between emission lines and continuum. Presumably the variations we expect are in the continuum, so we would have to go to narrow-band work, which means increasing the size of the telescope, but it is still a soluble problem.

Castor: Do you know from the observations how large a variation could exist?

Kuhi: HD50896, for example, varies 10 percent or so over about a day. I don't know what causes this particular variation. We could try to fit it as a binary, but it is not clear what is happening.

Nariai: I would like to point out that whatever the mechanism, the period must lie between 10^2 and 10^4 sec: 10^4 sec is the diameter of the envelope divided by the velocity; 10^2 sec is the energy contained in the envelope divided by the energy flux in the lines. So suppose we do find this period; we cannot then conclude that the variation is really due to the nuclear instability.

Thomas: You have apparently taken 1000 km/sec as the velocity, and although this is a bit high even for $T_e \sim 10^7$°K, that will only increase your upper limit on the period. What would you buy for these periods, John?

J. Cox: The period depends on the size of the envelope. 10^4 sec would be a reasonable order-of-magnitude estimate for a large envelope.

Castor: If the envelope were so large that the pulsation could be a progressive wave in the outer part, the period should be dominated by the inner part in which the pulsation is a standing wave. So the period shouldn't be very long.

Hjellming: Wouldn't one expect two different classes of optical evidence for pulsation? On the one hand, if the pulsation were spherically symmetric, we might expect some periodicity in luminosity. But if there are other kinds of pulsations, such as a non-radial pulsation, we might observe only statistical fluctuations in luminosity. I would think it would be the latter alternative that would give us most of the evidence about the real situation. We might also have harmonics of the radial pulsation.

J. Cox: I certainly agree that it might be hard to observe non-radial pulsations directly; the scale of the motions would be considerably smaller than the size of the star. But if the pulsations are generated by nuclear sources in the interior, I think it unlikely there would be harmonics of the radial pulsation.

Schmidt-Kaler: I believe Ross has made broad-band observations of about a half-dozen WR stars and has found no variation. So I think this underlines Kuhi's remark that narrow-band work must be used in the search for evidence for pulsations.

Underhill: For my 1968 *Annual Reviews* article I collected what evidence there was from broad-band work. The results are contradictory: One group will say a given star varied, others will say it didn't. Lindsey Smith has some evidence for variability from her narrow band observations.

Smith: Not much evidence: Most stars reproduce from one night to another to within 0.01 to 0.02 mag. So if variations are always there, they must be pretty small.

Wrubel: If thermal instability is responsible for some aspects of the WR phenomenon, the energy released during the thermal pulse may not go into the pulsational modes. Thermal instability produces a convective region immediately above the energy-producing shell. Perhaps energy is pumped directly into the turbulent zone. I would also remind you that if, as Lindsey suggests, you want to add an additional parameter called "binary character" to the physical properties of the star, you must really add several parameters. The results on mass exchange depend on the stage of evolution at which the mass exchange takes place. Thus several parameters such as mass and separation of the components are involved.

Thomas: Are there any more comments on the mass-exchange models? What about the problem of chemical differentiation, for example?

Wrubel: That aspect is the most speculative part.

Thomas: Well, if you don't want the differences between the WC and WN sequences to arise from excitation effects, you have a choice: Either you buy initial differences in composition or you buy something in the evolutionary process that produces these differences. And Paczynski's remarks on chemical differentiation are no more speculative than his arguments on the production of supernovae. Actually it is a bit harsh to refer to these suggestions as speculation. In a sense they are an extrapolation to the next stage of the models from that sequence which carries the evolving object up to the stage of mass ejection with He- or C-burning cores and the resulting instabilities.

Smith: It is on this basis that I prefer the alternative I mentioned of C-enrichment of the atmosphere. On the basis of the Kippenhahn-Weigert series of models we know we will get He-burning or C-burning models with a thin H envelope. We know they will be unstable, and we know they will develop thermal pulses. Thus it is likely we will get C-mixing in the atmosphere. Agreed we are arguing on the basis of extrapolation; I just think the distance we have extrapolated is a bit less here.

Schmidt-Kaler: I agree that these mass-exchange mechanisms seem quite well established. However, I think there may be two instability mechanisms working to produce the WR phenomenon. First, the WR binaries we see belong to the narrow-line, strong continuum class (Hiltner-Schild, WN-A). Thus you have not an enhancement of the WR characteristics but the reverse; the single stars seem to show the broader lines, hence enhancement of the WR characteristic. Second, all the WR stars that show the ring-like shells seem to be single with the possible exception of the weird star HD50896. A third rather weak piece of evidence is that the Of stars do not seem very far evolved. Fourth, the supergiants showing the same kind of high ejection velocities in the rocket UV are all single stars. So we seem to have strong evidence for the existence of a second mechanism which doesn't depend upon mass exchange.

Smith: In my summary, I said that another mechanism was probably needed for the single stars; and the statistics do indicate that the binaries are younger than the single stars. Sco OBI is a good example, so I think this is virtually established.

Underhill: I personally would like to see all these suggestions on instabilities more firmly established before deciding on the evolutionary history of any of these stars. We know we require a source of mechanical energy and that this in turn requires an instability. Several such instabilities have been proposed, but only proposed. We do not have a single definite calculation of how much energy any one of them actually contributes to the atmosphere.

Thomas: Yes, Anne, but from the types of instabilities suggested and from the kinds of velocity fields they are likely to produce, we now have something definite to calculate. The situation has improved enormously since 1938 when there was only a vague feeling that ejected shells had something to do with WR phenomena; there was then no thought of modifying the classical, radiation-dominated atmosphere. The situation has improved further since 1948, when the idea that we needed an atmosphere strongly influenced by mechanical supplies of energy and momentum was only an assertion, and we had no idea where to get it. Also since then, the arguments have been stimulated by the Paczynski-Kippenhahn alternative to differences in excitation or initial composition as the explanation of the WC-WN dichotomy. Even those objects which changed from WN to WC may be telling us something. All these are definite conceptual models that we can investigate; they are not just hypotheses about radiation pressure, magnetic fields, super-flares and super-spicules.

Beals: If there were any truth in flare models, you could not say there would be any one temperature at a given level in a WR star. There might be local patches where T_e is 10 times higher than in the surrounding areas. This could produce marked differences in the UV spectra of WR and O-stars.

Thomas: Beals has raised the question of the effect of inhomogeneities in the atmosphere. And again we might turn to the Sun as a guide. People are trying to explore the effects of inhomogeneities in the visual by studying observations such as those on granulation. They are also trying to differentiate between plage and non-plage regions by looking at the rich variety of rocket-UV lines. So maybe the results for the Sun will give us some insight into Beals' suggestion.

Pecker-Wimel: Wray has shown me satellite photographs of many supergiants and WR stars, so we are not limited to the single observation of γ Vel that West says is all he can get from a rocket.

Wray: All we have so far are very low dispersion observations. We did observe several WR stars: HD156385 and HD192163. The Apollo S-019 experiment should give us the kind of observations discussed here. There is a magnitude limit of about +9.5 m_V at λ1350 with a dispersion of 60 Å/mm.

Westerlund: I have one question with regard to Paczynski's theory. In his paper V he says that the companion should be a normal main-sequence star. I believe that Kippenhahn and Lindsey expect the same. But in the Galaxy there are at least 4 or 5 components classified as supergiants or evolved stars. There are three in the Magellanic Clouds, and in these three it is likely that the WR component is WN7; you can see how the luminosity increases with the luminosity of the supergiant. This does not seem to me to agree very well with the binary theory.

Kuhi: One might say something about the spectrum of the companion. Conti and I have started a program to look at the O-type companions, and so far we have not been able to find any differences whatsoever in chemical composition or in anything else, compared to ordinary O and B stars.

Westerlund: So you are suggesting that the classification is wrong in those cases. I have not classified any myself, but in Lindsey's catalogue of galactic WR stars, there are several binary systems with one component of luminosity class I. If the WR stage lasts only 4×10^5 years, that is hardly sufficient time for a normal star to accumulate mass, evolve up and off the main sequence, and become a supergiant. But the simplest explanation may be that the classification is wrong. I would only like to know whether those you observed were selected so that the companion was classified as Ia? There were only 4 or 5 of these out of our 50 or more binaries. The chances are that you have not yet observed any of them.

Smith: Let me ask a question. The time spent in the phases that we suggest are WR stars is 5×10^5 years, maybe as much as 10^6 if we include stages E and F. How massive does the secondary have to be on the main sequence to evolve in that time? We can get a pretty massive secondary, so if it evolves within 10^6 years we can get a supergiant with the WR star.

Underhill: I think it would be well to look at those classifications again. It is extremely difficult to classify stars when you have two spectra on top of one another. If you think the H-absorption lines look a little bit thin and a little bit sharp, you may say Ia. But they will obviously look a little bit thin and a little bit sharp just because they have the WR emission on top of them. So I think the most doubtful thing is the classifications.

Kuhi: Right.

Wrubel: I think Kippenhahn assumed that the material that left the WR star was immediately deposited on the other star. Actually the other star is well inside its critical lobe, and the material that comes off the pre-WR star can float around for a considerable time, possibly producing a spectrum quite contrary to what you would expect from an ordinary single star.

Thomas: In addition to the spectra of normal stars and of supergiant components of WR binaries and of WR stars, we now have the complicated spectrum produced by material that doesn't know whether it belongs to the WR star or to the supergiant.

We have a Visiting Fellow Program in JILA that provides a sabbatical year for people who will come, we hope, to pose interesting scientific problems to us. Wrubel will be a Visiting Fellow next year; he has just posed such a problem. We invite those of you who are interested to consider doing likewise. Thanks very much for being with us this week.

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