OPEN DISCUSSION

Chairman : P.S. Conti
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Conti: One of the main conclusions of the Wolf-Rayet symposium in Buenos Aires was that Wolf-Rayet stars are evolutionary products of massive objects. Some questions :

- Do hot helium-rich stars, that are not Wolf-Rayet stars, exist?
- What about the stability of helium rich stars of large mass? We know a helium rich star of ~40 $M_\odot$. Has the stability something to do with the wind?
- Ring nebulae and bubbles : this seems to be a much more common phenomenon than we thought of some years age.
- What is the origin of the subtypes? This is important to find a possible matching of scenarios to subtypes.

1. Do hot population I helium rich stars that are not Wolf-Rayet like exist?

Are there bright luminous stars that are helium rich but are not WR stars?

Bohannan: We have heard from Dr. Underhill the importance of studying the entire structure of a star before leaping to conclusions about its properties. In the case of Wolf-Rayet stars we do not see the underlying photosphere, rather an array of emission lines from an extended atmosphere. One of the stars in my butterfly collection of spectra of the emission line stars in the Large Magellanic Cloud, BE 202, may give us some hints about the generally unseen photospheres of Wolf-Rayet stars. With one exception the spectrum of BE 202 is very similar to spectral type B1, essentially equivalent to that effective temperature reported by Underhill earlier this afternoon. The difference is that lines of helium are strong, He I $\lambda 4471 > H\gamma\ \lambda 4340$, suggesting a He/H ratio of 5. Unlike other helium-rich stars in the Galaxy, BE 202 is more luminous ($M_V \sim -5$) and does not appear to be a binary. It is as though BE 202 lacks whatever mechanism drives a Wolf-Rayet star into a high density, fast extended atmosphere. From the moderate emission at Hα, the mass loss rate of BE 202 is reflective of its luminosity, rather

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than the high rate of 3 x 10^{-5} almost universally found for WR stars. What causes a star with apparently all of the properties of a WR star to fail to exhibit the proper phenomenon?

Henize: Conti's original question reminds me of the two very close short-period binaries studied by Ed Nather. These are helium-rich emission line stars (little or no H appears in the spectrum) and are nicely explained by Nather as a binary in which the hydrogen envelope has been peeled off due to the influence of a compact secondary. They probably have lower masses than the WR stars (I don't actually remember their masses) but it would seem that they represent an extension to the binary WR phenomenon. HDE 326823 is another example of a He-rich hot emission-line star. Again, only weak Hβ appears in the spectrum. It shows mainly broad (± 20 km/sec) He I emission and a few weak Fe II lines. But so far not enough observations have been made to determine whether it is a binary.

2. What is the stability of a helium-rich star of large mass?
According to theoreticians helium stars more massive than 10 $M_\odot$ are unstable. Is this involved in the wind? Has this instability somewhat to do with the fact that the wind is so different from that of O en Of stars?

Renzini: WC stars are either massive objects, with a convective core, or light PN nuclei, which are essentially hot C-O white dwarfs, surrounded by a thin skin mainly imposed of He and C. Well, in spite of having so different internal structures they exhibit very similar spectra. This suggests that the internal structure, and therefore the vibrational instability os massive stars, has little to do with the star being a WC. WC PN nuclei are not unstable against the so-called ε-mechanism which may operate in their more massive counterparts. However, both types of WC stars could be unstable against the κ-mechanism driven by the last ionization stages of carbon, which is a major constituent of their enevelopes.

Smith: Please clarify for me: do stars in the mass range giving planetary nebulae ignite C in the core and if so at which stage of their evolution?

Renzini: You mean in planetary nebulae nuclei?

Smith: Yes, in low mass stars in the mass range you were talking about.

Renzini: Carbon is produced in the core during the core helium

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burning phase and then later on the CO-core grows during the whole asymptotic branch phase. But C never burns. The stars which ignite carbon should become supernovae, these are the more massive ones. Once carbon is ignited in a degenerate core you get a tremendous thermonuclear runaway and what is produced is a supernova. In planetary nebulae nuclei hydrogen or helium burns in a shell, but never carbon.

Smith: You mean the concept of a pure-C C burning star has no real counterpart?

Renzini: Planetary nebulae never produce carbon burning stars.

Lundström: We know from photometry that most WR stars are variable with small amplitude, $0^{m}.02$-$0^{m}.05$. However, it has never been proved that these variations are periodic. The expected period of an unstable helium star is to my knowledge 30 minutes, but I don't know the expected amplitude. Could such variations be detected photometrically?

Renzini: There is another possible instability in stars where the envelope has a composition of, say 50% carbon, or when the carbon has a significant contribution. In cepheids the powering mechanism is the so-called kappa-mechanism, which is procuded by the opacity in the ionization zones of helium and hydrogen. In stars where the envelope contains say 40 to 50% carbon we expect the same sort of kappa-mechanism, now working on the ionization species of carbon (up to VI) so that the driving region can extend to a couple of million degrees. This can work in helium stars, so that they are variable, with magnitude differences of some hundredths of a magnitude.

Chiosi: A much more efficient mechanism which may work in this type of stars is the nuclear reaction instability. The theory of stellar instabilities states that all new regions where nuclear reactions start are vibrationally instable; they may show instabilities in the radial or non-radial mode. These instabilities in most cases never show up at the surface since the hydrogen rich envelope quenches all instabilities, generated in the central part of the star, and the only stability which shows up is then the one due to the ionization of the envelope. If the helium or hydrogen region is on top of a convective core, having nuclear reactions the instabilities probably will not be smoothed out by the outer regions. However it is hard to predict the kind of instabilities. No numerical verifications of this statement exist.

3. Ring nebulae and stellar wind bubbles

Are these physically two different things, or two manifestations of one kind mechanism or of several different mechanisms? One of the things that might be important is whether we see evidence of spherically symmetric or non spherically symmetric ejection. Paris Pismis would like to say something about this problem.

Pismis: I would like to recall the presentation of results during this Symposium on so-called ring nebulae which may have given rise to some confusion for those who are not actively engaged in the topic. We were shown photographs of new ring nebulae. I must say first that I had to exert my imagination to see ring structures in quite a few of these.

Secondly, the "ring nebulae" were divided into four sub-groups, namely amorphous H II regions, whell structured H II regions, stellar ejecta and stellar wind-blown bubbles. It is my belief that a classification carried out in much detail defeats its purpose. Moreover, one should not rely on the hypothetical properties (for example a "wind-blown" bubble) in classifying objects; thus I advocate towards dividing emission nebulae into two general groups : a) Nebulae essentially vestiges of star formation; b) Nebulae essentially caused by matter ejected from a star (or stars). Within this category I include ring nebulae, planetaries and other symmetric nebulae. Group b) is the relevant one to the topic of this symposium. The ejection may or may not have been isotropic. Previous work on the velocity field combined with the morphology has provided evidence that those symmetric nebulae that my group and I have studied have originated through ejection from the parent central star non-isotropically and often in puffs. These nebulae are NGC 6164-5 (symmetrical), NGC 2359 (double ring) and M1-67.

The proposed model for ejection of matter from the central star is as follows: (I quote from a brief paper of mine in Proceedings of Symposium No.83 on Mass Loss and Evolution of O Type Stars) where the model is concisely described and references given of previous papers on the subject.

"The following model which we propose may explain the velocity field and the main structural properties of NGC 2359 and NGC 6164-5. Ejection of matter started t years ago ($t = 1\text{-}2\times10^5$ for NGC 2359 and $4\text{-}5\times10^3$ for NGC 6164-5) from active regions, spots, located nearly at the extremities of a diameter on a fast rotating star. This direction of the ejecting regions is oblique to the axis of rotation. In the case of NGC 2359 the axis of rotation is close to the line of sight whereas that of NGC 6164-5 is close to the plane of the sky.

If the proposed model of non-spherical ejection is correct one should expect other nebulae ejected from the parent star to be observed at varying projection angles. Objects formed in this manner in general would show bi-symmetry and sometimes ring structure. It would be interesting to determine the velocity field of H II regions with axial symmetry to check the validity of our model.

The central stars of NGC 2359, of NGC 6164-5 and M1-67 are all losing mass at present. It is reasonable to expect that if the gas ejected from these stars in the past has been non-isotropic the present mass loss may also be taking place in a similar fashion, that is from localized regions on the star.

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It is difficult to image that active spots on a star located, as we suggest, nearly diametrically opposite on the star may be due to anything but magnetic phenomena. In fact in my paper on NGC 6164-5 I have suggested that the agent funneling the ejecta is likely to be a magnetic dipole along the direction of ejection, that is along a diameter oblique to the rotation axis of the star.

In the light of this suggestion we may ask whether the line profiles of the Wolf-Rayet and Of stars would not be consistent with a non-isotropic ejection of matter at the present time. It may be worthwhile to construct synthetic line profiles for rotating stars with active spots and compare them with observed profiles. Perhaps the variations of the spectral line profiles in some WR stars may find an explanation by this mechanism of mass ejection".

Kwitter: M1-67, which appears spherical and symmetric on over-exposed broad-band plates, actually exhibits discrete clumps and is not symmetric, when examined on (N II) and Hα photographs. Also some regions show evidence of multiple episodes of bulk ejection, like NGC 6164-5.

Pismis: This is a very good example of blobs. In this mechanism we expect that the mass should be ejected in blobs.

Underhill: The calculations of Rumpl reported at this conference show that to obtain lines of the observed WR shape you must contain most of the material in an equatorial band or wedge. It is conceivable that this band originates from magnetic structures anchored in spots. The spots would rotate with the star and they would change from time to time. Such a ring of stellar activity might well be the source of the occasional ejection which some nebulae suggest has occurred.

Melnick: Is there any evidence that WR stars are fast rotators? What do we know?

Conti: We have direct evidence only for those very few WN7 stars with absorption lines (e.g. HD 92740). In these cases the line widths do not appear large. We think now that HD 193077, which Massey and I thought might be one star with very broad absorption lines, is two stars. Hence the absorption and emission are not related. We have indirect evidence of rotation for these few stars such as HD 50896, with periodic line width changes, or periodic polarization, that the rotations are relatively slow, that is a few days.

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4. What are the differences and similarities between the O and WR stars nebulae?

Heckathorn: I would like to comment on the morphology of symmetrical nebulae around O stars as opposed to shell structures around Wolf-Rayet stars. I have looked at all of the O stars contained in the Parker, Gull and Kirschner Survey, and I find that, as a class, their associated nebulae are diffuse, and they are brighter in Hα+(NII) than in (OIII). This is consistent with the idea of a photoionized nebula, rather than the wind-blown bubble suggested by most of the Wolf-Rayet stars.

Kwitter: I have spectroscopy of NGC 7635, around BD+60°2522, which is an Of or Oef star, and I can say that spectroscopically, it looks just like an average low to moderate excitation H II region.

Henize: NGC 6164-5 and its Of central star present a fascinating set of nebulosities associated with a peculiar Of star. There is a bright inner nebulosity evidently ejected by the star, an extended H II region a degree in diameter (~20 pc) and with a dust rim, and a cavity within the H II region which is rimmed by a filamentary halo and is evidently a "stellar bubble" being blasted out by a stellar wind. However, in this case the H II region and its dust rim suggest that this is a very young O star (~30000 years in age) (Bruhweiler, Gull, Henize and Cannon, 1981, Ap.J. 251, 126) and that O-star H II rings are the result of the initial turning on of O stars rather than of their demise into some other stellar stage.

5. What is the origin of the different spectral subtypes?

van der Hucht: This morning Tony Moffat showed a nice relation between binary mass ratios and subtypes, suggesting a continuous transition from "late" to "early" subtypes caused by binary mass transfer, both for WN and WC stars. Last saturday Phil Massey showed that there is no good relation like this or perhaps in opposite direction. Are Tony's results too good to be true of is Phil too pessimistic? Could we have both gentlemen in the ring with Peter Conti as a fair arbiter?

Conti: I would say the truth lys somewhere in between. We need more data.

Underhill: To understand the meaning of the WR subtypes we have to look in the data presented by Mickey Leep at this conference. Basi-

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cally the types are defined by the apparent relative intensities of a few selected lines of the nitrogen ions, the carbon and the O ions. From star to star of a given type, the ratios of intensity remain fairly constant. However the total emission in the lines is very different from star to star. The amount of emission observed in any one line is a function of the electron temperature, the density and the volume of gas having then T_e and ρ as well as the relative abundance of the element and the velocity distribution. Clearly the line-emitting regions of WR stars vary greatly in volume from star to star, but some present similar relative volumes of the needed T_e, ρ and composition, so are put into the same subtype.

Massey: If you plot the mass ratios vs. subtypes as Tony Moffat did, but you include only the galactic double-lined systems, you don't see any correlation with type. Tony has included the WN3 stars in the LMC and SMC (but no latter types) and several SB 1's. I don't know quite how you find a mass ratio for an SB 1. Pair blending, or differences between the Galaxy and the Clouds, may explain the low mass ratios for his WN3's. Certainly Virpi Niemela's galactic WN3+O system has a reasonable mass ratio.

Moffat: Massey's presentation of the same relation does not include the MC WR stars which are really crucial to fill in the gaps in the subclasses of galactic WR stars. The dispersion about a straight line fit of m(WR)+m(OB) vs. spectral subclass is reasonable compared to observational errors of both quantities.

Concerning the possible perturbation of emission-line velocity amplitudes in close binaries, perhaps the best way to proceed is to look for correlations between mass ratio and log P(d), which is coupled mainly to the orbital separation. For short period systems (WNE,WC) one would expect m(WR)/m(OB) to be smaller in the mean. In the MC's, present evidence shows that this is probably not the case. Thus, the observed mass ratios cannot be too far off the truth.

Vanbeveren: If the line emitting region in WR stars, members of close binaries, is affected by binary geometrical effects (which may be true taking into account the radii of WR stars and their Roche radius) I wonder how far we may believe the WR binary mass ratios studied so far.

Moffat: We learn most of this from diagrams where is plotted the mass ratio versus the period, or the separation. In Magellanic Cloud stars of low mass ratio there is no apparent trend as you might expect if streaming motions or distortions were the source of the problem. In other words we should expect the mass ratio to be smaller for short period systems.

Niemela: Tony, would you like to comment on that WC binary you showed me earlier, which has C lines moving with high amplitude and O VI emission which does not move?

Moffat: There was a problem with the O VI lines in that there was a shift of over 200 km s^{-1} between two epoches. I have not yet found out what is going on.

Conti: The next question is : where do they go? What happens to WR stars? HD 50896 apparently has an occurrent period change in ~2000 years; this is a very short evolutionary time scale.

Renzini: WC or WN stars (at least some of them) will eventually explode as a supernova. The most massive ones, say with $M > 10\ M_\odot$ will not exhibit H in their envelope and will therefore appear as a SN I. Chevalier made computations for explosions (1979), and the results were that the lightcurve was some 5 magnitudes fainter than those of SN I or SN II. The speculation was that Cas A, ~ 3 kpc away, and never observed, was produced by a WR star. Problem: why have the astronomers of the 17th century not seen Cas A? If it were a normal SN (I or II) it would be impossible to miss it. A possible explanation is that Cas A was produced by the explosion of a compact star, possibly a Wolf-Rayet star. Therefore the study of slow moving filaments and knots of the SN remnant in Cas A may have something to say about the previous evolution of Wolf-Rayet stars. The other point on supernovae and Wolf-Rayet stars concerns the fact that the incorporation of mass loss rates into an evolutionary code is not easy since the mass loss rates are so uncertain. The way to perform evolutionary computations by parametrisation of the mass loss leads to the result that the outer layers are expelled or not, depending on the adopted parametrisation.

Yesterday Maeder showed a diagram giving the results of what happens if Bernat's mass loss rates for red supergiants are used. Stars brighter than $M_{bol} = -6$ lose their envelope during the red supergiant phase perhaps producing Wolf-Rayet stars. This corresponds to masses of ~10 $M_\odot$. If this were the case, it means that all stars peeling down to 10 $M_\odot$ will become WR stars and therefore do not produce type II SN, which are reasonable believed to be the product of the stars in the range 10 to 50 $M_\odot$. So one has to be very careful.

Firmani: I would like to make a comment on HD 50896. There is the aspect what will happen in 2000 years, but there is also the aspect what is happening now. There is the preliminary result of a period change on a time base of ~5 years, which is very short. The noise of the data is quite large. On the other hand, a change of 180 seconds per year is a very high rate. This means that the structure of the binary is little different from the usual conception we have of these binary systems with low mass companions (possibly compact companions) orbiting in a strong stellar wind. I would emphasize that it should be interesting that theoreticians consider this problem. The soft X-rays can provide more information about these binaries. The peculiar interaction of the collapsed object, X-rays, UV, modifies the ionization structure of the wind near the compact object; the acceleration mechanism can change the accretion and the structure of the wind. We see also peculiarities in the optical spectrum that cannot be explained (e.g. in He λ 4686). X-rays can give a lot of information about the structure of the wind and about the structure of the colliding atmosphere.

Stenholm: Just a comment on Cas A. There are indications that Flamsteed observed Cas A but that the star never became much brighter than the sixth or fifth magnitude or so.

Mendéz: Concerning the period of HD 50896, although our data are also noisy, from data covering the interval 1971-1979 we have no clear indication that the period is changing.

Kwitter: I remember a provocative suggestion made by Bob Parker at an AAS meeting a few years ago regarding NGC 6888 and HD 192163. He noted that the enriched composition and morphology of the nebula were similar to the quasi-stationary flocculi in Cas A, and speculated that perhaps HD 192163 is a pre-supernova.

Conti: There is also a suggestion that Wolf-Rayet stars could peel down to nothing. Wouldn't that be awful? I would like more to believe that something exciting happens!