GENERAL DISCUSSION

Plavec: Concerning the shape of the critical Roche lobes when radiation pressure is included: for a purely gravitational case, you may indeed assume the attractive force emanating from a mass point. However, the radiation pressure felt by the other star emanates from the surface of its companion. In order to evaluate it, you must carefully integrate all incoming flux at a given point. You are dealing here with continuous radiation pressure. Naturally, part of the surface will be in the shadow cone and there will be no external radiation pressure there. If you want to study the effect of radiation pressure on gas motions in the circumstellar space, I think you must consider selective radiation pressure in individual lines which may be very different for different species and stages of ionization. Then it will be also necessary to include the interactions between individual particles.

Niemela: I would like to remark that the Roche-model is useful only when the force-field is mainly gravitational. But when e.g. radiation pressure has to be taken into account, the equipotential surfaces will change.

Bolton (to Willis): How well is the absolute calibration of your UV photometry determined? What effect does this uncertainty have on your effective temperature determination?

Willis: The absolute calibration of S2/68 was believed to be better than 20 percent in the absolute photometric sense. A comparison with S2/68 and other calibrated UV data (OAO-2, ANS, etc) shows that the calibration may be better than this, say 5-10 percent. I would not think this would introduce uncertainties of more than 5000 K.

Bohannan: The velocity gradients presented by Moffat and Niemela were steeper in WN7-8 than in WN5-6. What does this observation indicate about the extended atmosphere?

Castor: If the atmosphere is very dense, all lines may have the same or similar widths, while in less dense atmospheres the range in width is more. This effect goes in the right direction, if I understand your (Bohannan) comment correctly.

Moffat: Answer to Bohannan and Underhill concerning ionization/excitation structure of WN, Of envelopes : We assume that the violet shifted P-Cygni absorption edge of the optically observed He I lines (especially at λ 3889) represents the terminal wind velocity for each star. But since the ter-

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minal velocity of WR winds varies from one star to another by up to a factor of ~ 2, we normalized our plot of velocity of absorption edges versus ionization/excitation parameter to RV/RV_{HeI} in order to facilitate a comparison for different stars. Perhaps this is an oversimplification. The change of velocity with excitation/ionization potential is less steep for WN5/WN6 stars than WN7/WN8 stars possibly because most of the observed absorption edges in the WN5/WN6 stars are formed in the outer envelope close to the terminal velocity.

Moffat: Question to A. Willis : You mentioned that it is plausible that the very luminous WN7/WN8 stars can and should evolve downwards in the HRD to the less luminous WR stars (WN4,5,6 and all WC). But if WR-mass loss is responsible for this, it cannot be of the normally observed wind type - we require a more violent, rapid process to account for the clear gap of stars in the HRD between the WN7/WN8's and the rest.

Willis: An evolution from WN7-WN would indeed suggest that we should see intermediate objects. The fact that we don't may suggest a more violent, short time effect in the mass loss which could cause the transition from the high to low luminosity WR stars. The evolution models do seem to show that if the mass loss rate is high enough, during the hydrogen burning phase a single star can drop in luminosity significantly. I don't think we can rule out mass loss rates high enough to ignore the Of-WN5,WN6 possibility. What are the mass loss rates for WN7,8 stars?

Sreenivasan: In answer to Willis' comment about the evolutionary picture of these WR stars, I would like to remind him of the HR diagram that Conti showed wherein there was argument regarding the location of the observed points. The mass loss rate can of course be altered to produce an evolutionary track with different time scales etc., but the observers have control over ensuring that the points are in the "right" places. If the observers could ensure that the effective temperatures and magnitudes are consistent with all the criteria used to check them, the model-makers could do their share of producing reasonable evolutionary scenarios.

Chiosi: I wish to comment briefly on the problem of the N-enhancement and its abundance in binary systems where it seems normal. If during the first mass exchange CNO processed material (N-rich) falls to the surface of the companion, an inverse μ-gradient is built up. However, owing to the Rayleigh-Taylor instability such a gradient is destroyed on a very short time scale, diluting the N-rich material to quite a normal abundance.

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