POSTER PAPERS - SESSIONS 1 and 2.

Chairman : A. WILLIS.

Willis:

I thought I would give a brief overview of a selection of the poster papers and hopefully inspire some informal discussion. We have seen today many HR diagrams and evolutionary tracks and it seems to me that one must ask which tracks to use and what do we believe regarding the physics that goes into generating these models. The poster paper by Nasi on synthetic HR diagrams for luminous stars uses various model evolutionary schemes involving mass loss and other effects and tries to reproduce the HR diagrams that are observed now. They conclude that one can only reconcile the observed HR diagrams with models incorporating overshooting and a CNO opacity bump. I am sure the stellar evolution pundits here will comment on this.

Maeder:

There has been a debate about the CNO opacity bump which started with the Carson opacity model. Recently there has been a joint paper from the Los Alamos and Carson groups concluding that there is no opacity bump. The origin of this bump is in some trick in the Carson model, so that the current status from atomic physics is that there is no bump, and that its assertion is hypothetical. There is another thing that is not hypothetical and that is the change in the C12 alpha-gamma O16 rate. Usually in stellar evolution when one is changing the energy production rate there is no great influence on the lifetime, but in this case during the He burning phase,traditionally one was converting He to C and a bit of O was formed. Now with this new rate you convert He to C and then most of C is converted to O. Moreover as the rate is larger, the core is larger so that the He burning lifetime is greater and this can explain the larger number of stars outside the MS. So I would say in summary that the present status is that the opacity bump does not exist, but there is some effect in the cross sections that considerably increases the number of stars outside the MS.

Chiosi:

The synthetic HR diagrams produced by Nasi are meant to test how closely current models for massive stars may reproduce the observational HR diagrams. Usually the comparison is made counting stars in different spectral groups and comparing those stellar counts with theoretical lifetimes. Broadly speaking, the groupings of spectral type correspond to MS, post MS and WR stages and/or stars. This may give only rough indications, while the synthetic HR diagrams are a more sophisticated

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tool. There are of course problems with completeness, Sp:BC:Teff scales, non hydrostatic effects on stellar radii etc.; nevertheless it seems to me that the simulations in question strongly indicate that models evolved at constant mass as well as in presence of mass loss hardly can match the observational data. The scheme with mass loss and overshooting is much better and indeed the closest to reality. Now comes the problem of the revised reaction rate. It is true that the novel rate increases the lifetime of the core helium burning phase as compared to the core hydrogen burning phase, but it is also true that adopting the mass loss rates that everybody believes in we are producing models which preferentially locate stars at the left part of the HR diagram, which have probably to be identified with WR objects. So there is a group of stars (A to M) which is not easily accounted for by any type of models. In this context comes the suggestion that CNO opacity is still underestimated. The recent paper by the Los Alamos group is not conclusive in this respect. In fact, suppose that the Los Alamos and Carson groups are reconciled as far as the past divergence in their CNO calculation is concerned, this does not imply that real CNO opacities may not be slightly higher than usually estimated. This is the spirit of our suggestion.

Willis:

Roberta mentioned the new techniques of IR surveys of late supergiant stars, with recent results for the Carina region almost doubling their numbers. This is clearly a powerful new technique and the results have serious implication for say the number ratio of WR/RSG.

Humphreys:

I think it is very impressive that Jack MacConnel has been able to get very good IR spectra demonstrating the very powerful use of the IR CaII triplet with CCD spectra on such faint objects.

Willis:

The paper by Graham and Humphreys using the IR CaII triplet to disentangle foreground stars and real members in NGC 300 and thereby get correct statistics gives a (J-H) vs (H-K) diagram in which is stated evidence for a composition gradient, and I was not sure how this was arrived at.

Humphreys:

The evidence for the composition gradient comes from the HII regions which have to be taken into account. You can see in the 2-colour diagram that some of the stars do have compositions more like LMC values than galactic values.

McGregor:

I would like to ask if you have classified the spectra in the red to determine their types, whether they are K or M?

Humphreys:

Yes the types are given in the paper.

Zinnecker:

Was there any attempt to measure CO in these objects?

Humphreys:

No.

Willis:

Roberta's review included some mention of instabilities and mechanical energy deposition in supergiant stars and two posters show observational data on HR 8752 and HD 217476 which may provide evidence for such effects. David Stickland pointed out to me that these papers are on the same star. The first paper by Zsoldos suggests variability with a 400 day period caused by pulsations, whilst the second by Smolinski et al. suggests a binary system with a period of 620 days. There appears to a be common envelope with at least three shells with considerable line activity. Obviously we need to consider the cause of such variability, and this star (and others) is clearly going to be providing the kind of observational material that de Jager would like for his models of pulsation induced by mechanical energy deposition. I know Stickland has observed this star and may wish to comment.

Stickland:

The only question I have at the moment concerns the periodicities we find in this star. Dave Lambert has been studying this star and finds a period of about one year in the lines. We have periods of 400 and 600 days, and I wonder if we are dealing with a semi-irregular pulsation or whether it is really a binary.

Smolinski:

Well, our observations cover at least 15 years at high dispersion and there is no doubt that the velocities of the lines are changing. In addition there is a common envelope which sometimes has the same velocity as the star, sometimes is not seen and at other times is split into several components. If the star and the envelope velocities are the same at some times they will be superimposed, so we really only see one line. The mistake of others is not to use high dispersion and be unable to distinguish the star and envelope components. For this reason it is very difficult to obtain the RV. With our data of over 200 high dispersion spectra we are able to distinguish the stars component lines and according to our present results it seems clear that it is a binary. There may be additional evidence for pulsations. However, I think perhaps that the optical light variations may result from increased line blanketing when multiple components are seen, which may explain some of the photometric variations being interpreted as due to pulsations.

de Jager:

I would like to know how sure we are about the binary hypothesis - did you try to calculate the elements of the system?

Smolinski:

At the moment we have some elements but these are only preliminary.

Viotti:

There is also radio emission from HR 8752, is that not so?

Smolinski:

We are not sure of the origin of the radio emission but I think it comes from the HII region in which this star is just inside, which is excited by a hot B star nearby.

Willis:

We heard in Rolf's talk this morning about mass loss in OB and WR stars with radiation pressure being highlighted as the mechanism for initiating and driving the mass loss in hot stars - there is a poster by de Jager, van der Hucht & Nieuwenhuijzen which studies all the rates available in the literature and on looking at correlations with stellar parameters concludes that the mass loss rates are a sole function of L and T(eff) for all types except WR and C stars where the observed rates appear too high by a factor of ten or so. I think we know the reason why the rates seem too high for the WR stars, since the radiative

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luminosities for these stars are taken using older values of T(eff) of say 30000 K rather than the more recently deduced higher values of say 100000 K. The higher values based on 1985 results for temperatures will give higher L and thus higher predicted mass loss rates, which are likely to come into agreement with observations. How the C stars can be explained I do not know, and we have to ask what is the cause of the discrepancy in this case.

Lamers:

I am not sure that their analysis shows that mass loss depends on temperature and L alone, the paper assumes such a dependence and then it fits. Whether this is true remains to be seen. If you take the example of the Be stars, which are known to be rapid rotators, and which have the same L and T(eff) as normal B stars with considerably lower mass loss rates, there we know that there is another effect. In this case either the mass loss is due to something else or it is enhanced by some other mechanism than radiation pressure.

de Jager:

I agree of course. Our intention was to test possible correlations of mass loss rates and stellar parameters in order to see if a fairly simple parametrisation could be found which could be used in stellar evolution models. This appears to be the case for all normal types of stars (O to M).

Chiosi:

Did you compare the observed mass loss rates with the predictions of the Reimers formula in the domain of intermediate mass loss? I remember the rates predicted by Reimers are somewhat lower than observed in the range of the bright supergiants.

de Jager:

We made some rough comparisons but not systematically. There are in the literature about a dozen formulas which describe the mass loss in dependence on the radius, mass and L, but these refer only to parts of the HR diagram. Reimers formula relates to the cool stars and not the hot stars. Other formulae apply only to hot stars. But I did not really make a solid comparison. This is something we should do.

US Voice:

I believe there was a paper by Wayne Waldron in ApJ letters last

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year which found a similar relationship. Also it is not clear to me that the relationship demonstrates that radiation pressure is in fact the significant mechanism in late type stars.

de Jager:

We did not discuss the mechanism of mass loss but with regard to the late type stars I am sure that mechanical turbulence plays the significant role. Radiation pressure is unimportant for stars later than B-type.

Moffat:

I do not think it is quite fair to say that these are "1985" WR temperatures of 100000K. These values have been around for a while, it's just that some people have not accepted them. The Russians for instance have published these values in 1975.

Willis:

That is certainly a fair comment. In talking about mass loss in cool stars, and in addressing activity on the supergiant region of the HR diagram - if there is sudden and extensive mass loss in these cool stars we might expect to see dust forming in the shed material. Dave Stickland has a poster on IRAS observations of cool hypergiants more luminous than M(Bol) = -8. Apparently there is direct evidence for dust emission around stars later than G-type, but not around the earlier F hypergiants.

Humphreys:

The circumstellar dust feature around F, G, K and M supergiants has been known since the early 1970's. One of the problems we are all concerned with is what direction those stars are going in the HR diagram - are they approaching the supergiant region or on their way out of it. Do you have any intuition on that subject?

Stickland:

No, I have no ideas about that at all. I think that the fact that the brighest F stars do not show dust emission maybe means that they are moving from left to right, and one will see the dust produced later on when they go through some period of mass loss. On the other hand, maybe they have dispersed it and they are going backwards. I just don't know. The important thing is to extend the survey because there are rather a limited range of absolute mags here, and in act, if you look a little bit later to 89 Her you do see the dust, but it is hot dust and also it is a peculiar star. Its IR excess is rather similar to Eps Aur, with a temperature of about 700 K. What I was really trying to do (apart from get a ticket to this conference!) was to see if the amount of dust emission is greater in the more luminous stars, and the present sample does not show that. The fact that the sample is limited is historical: the IRAS Point Source catalogue came up at RAL a few months ago, and there was not another in the UK. I thought 'what should I do with this', so I looked at my favourite star, HR 8752, but it did not seem to be very exciting in the IR. Then I looked at the other "official" hypergiants and they showed a rather strange collection of results, so I extended it using your paper of 1978. Finally I remembered the horrible BC's for M stars, and so rapidly threw in some of those too. I ended up totally confused, and maybe you are confused as well.

Lamers:

I want to add a word of warning for those who use the IRAS Point Source Catalogue, particularly in the two long wavelength bands of 60 and 100 microns. A large number of the fluxes quoted in the catalogue are not very reliable. If you look at the real tracings of the data you can see that many of the Point Source data are in fact due to some background. So one has to be very wary of the 60 and 100 microns data.

Stickland:

I quite agree and reiterate that warning. In fact most of the stars in my paper have only 12 and 25 micron data, a few have longer wavelength observations.

Willis:

I omitted reference to another paper dealing with variability in these cool supergiants, which refers to the star mu Cep and its light variations which seems to come up with two periods of 4700 days and 873 days and the author believes these are due to multi-mode pulsations. Here again we may be dealing with observational evidence for mechanical energy deposition.

de Jager:

I do not think that there may be any difference between a star like this and the Sun where we have now discovered many hundreds of periods by refined observation. I guess that this kind of seismology applied to these supergiants may really give us profound information on

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the internal constitution of these stars.

Willis:

I would draw attention to the important paper by Hummer, Bohannan and Abbott on the recalibration of T(eff) for hot stars taking into account the recognition that wind blanketing is so important for properly determining the photospheric parameters. Their analysis of Zeta Pup provides the solution to the long standing temperature question, and most excitingly I think, the He and N abundance enhancements. My question to the authors is that when you say that the derived abundances are consistent with CNO burning products, have you looked into the question of getting that stuff out into the atmosphere from the nuclear burning regions?

Hummer:

Of course not!

Willis:

Thank you David. The point I am getting at is that if we believe the WR stars are chemically evolved in which we have stripped off all the outer atmosphere during the H-burning phase, then one can automatically expose the interior burning products. Here clearly one still has a very large H atmosphere content, and not therefore necessarily peeled down to interior regions, so there may be a problem.

Bohannan:

I think the next step is to look at a variety of Of stars. When you look at Zeta Pup you realise it is slighly more evolved than other O stars.

Kudritzki:

I want to make a general comment. Evolutionary tracks taking into account mass loss and turbulent mixing in general produce this type of chemical enrichment in the photosphere. Whether the agreement is really quantitative in this place in the HR diagram, depends on the mass loss rates before. We cannot be sure of a steady mass loss rate for example. Qualitatively there does appear to be good agreement.

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Maeder:

A general comment. For a long time in stellar evolution only the HR diagram was used as a constraint on models. Now it is clear that the C/N and O/N ratios are also very strong constraints on models and must be taken into account in order to give a good description of stellar evolution.

Bohannan:

One of the things that stellar astrophysicists get critisised about is that they just hack away trying to get better and better stellar parameters. For instance is changing the temperature of Zeta Pup 4000K significant? The answer is that it is. That change makes a big difference when you consider the amount of ionising radiation and M(Bol), so one would be quite wrong to say that a change of only ten percent in T(eff) is insignificant. It does have profound effects in the most luminous and most massive stars in the Galaxy.

Willis:

We also have discussion opportunities for todays review talks.

Conti:

I would like Alan Sandage to respond to Roberta's 5-minute comments after his talk - what is your reaction to that attack.

Sandage:

I dit not see it as an attack. The difference between Roberta Humphreys and me about the calibration of the red supergiants is not as great as it might seem. Because it all depends on the absorption question and I am not concerned about three tenths of a magnitude in M33. What I am concerned about is whether the absolute magnitudes of the RSG's is a function at all of the absolute magnitude of the parent galaxy. I think from what Roberta showed this afternoon, we are in essential agreement because she puts for M101 a value of -9 and if that was put on her diagram it would be the highest point and essentially the brightest galaxy and the other points slope down through -8 to something like -6.7 for WLM. I think we both agree that the absorption problem in galaxies like the SMC or even fainter like Sextans A, Sextans B, IC 1613 and especially WLM which is -13.5 is not present. So the question of the internal absorption is not present fainter than -16 to any great extent. I would like to stress that if you use the apparent distance modulus and the apparent magnitude, if there is a veil of absorption no matter how strong, if it is constant you get the right absolute magnitude. So the only difference between us is what the differential absorption is between the RSG's and the Cepheids. If the absorption is essentially the same for the RSG and Cepheids on the average no matter what the value, then if you use the apparent magnitude and apparent modulus you get the correct absolute magnitude no matter what the absorption. So we might disagree by a few tenths of a magnitude for M33, but the whole question of the use of the RSG's as distance indicators comes down to where you enter the calibration. I think we both have a slope - my slope is two tenths, that is the absolute magnitude of the RSG's is 0.2 times the absolute mag of the parent Galaxy, which if I read correctly is very close to that shown in Roberta's data. So I really do not think there is any fundamental disagreement.

Conti:

I would like to ask something else that was concerning me. There is a lot of belief that there is a difference between RSG and BSG evolution, in the sense that some of the most luminous blue ones do not get over to become red ones. What I find curious then is why the red ones, since they are from lower mass stars, should have a luminosity dependance on the galaxy. If that is really correct it is telling us that the whole luminosity limit argument has some dependence on some other property of the galaxy.

Humphreys:

Alan and I do agree at the lower luminosity end. It does turn down, but I think that is primarily a statistical effect. It is only turning down significantly when we get to the very low luminosity galaxies and I think we are seeing the same statistical effect we see in the BSG's - we just do not have enough massive progenitors and eventually it has an effect even on the M supergiants luminosities. We saw that in my plot comparing M(V) and M(Bol) and you notice how there is a dependence in M(Bol) but M(V) flattens out much more. I explain this as a metallicity effect compensating in these dwarf irregulars - they have lower metallicity and therefore smaller Bolometric corrections. I am still worried about the calibration of the M supergiants in the more luminous galaxies, say more luminous than -20 or -21. I do not know what is going to be the final answer in M31, we haven't surveyed the whole galaxy, but we have yet to find a M supergiant as bright as -8. What I really worry about for M101 is not the -9 calibration, but the calibration of M(Bol). Coming out from the IR photometry we get M(Bol) as -10.5 or pushing -11 for the M SG's. That is in strong diagreement with what we understood about the effects of mass loss on stellar evolution. I emphasise that the distance modulus that Alan has produced for M101 is an apparent value modulus. I still disagree about the effects of absorption. What is the true distance modulus of M101?

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Sandage:

We do disagree on the modulus of M81. If Roberta had put M81 at 28.8 then the RSG's in M81 would have values of about -9, and that would then have increased the dependence of the absolute magnitude on the parent galaxy. Now M81 is surely not at the same distance as NGC 2403. We know that for several reasons. We have 27 novae that have been observed since 1950, and the difference of the apparent modulus of the novae of M81 and M31 is 4.5, and the distance modulus to M31 is 24.3, so we get 28.8. Now Roberta used 27.5. So there is a fundamental difference in the parameters and we shall just have to agree to disagree until someone really determines the distance moduli to these galaxies.

US Voice:

When you start using galaxies of earlier Hubble type should you be using the luminosity of the whole galaxy which includes a bulge component, or should you be scaling more to the disc in which case there may be a de-coupling of the differential effect of the stars luminosities and the luminosity of the active part of the galaxy.

Sandage:

That's a very good point. I think you can wiggle the abscissa by say a magnitude that way.

Lamers:

I have a similar question. Suppose that in some galaxies for some reason there is more circumstellar reddening around the late type supergiants. Could that solve the problem or enhance it?

Humphreys:

That's why we turn to the Infrared. Some of the stars on my diagram of M101 in the IR colour-colour diagram are in the upper right hand corner and they look like they must have extensive circumstellar dust shells. So we have to worry about the contribution to the reddening of these dust shells, so that's why the use of IR data can help in correcting for these effects. But these stars probably would not be picked up in surveys for the brightest red stars. They are visually fainter, although intrinsically they are extremely luminous.

Massey:

A comment for Roberta about M31. We have UBV CCD of NGC 206 and

Wendy Freedman and I have got spectra of one of the blue stars there and John Hutchings and I have got IUE UV spectra of it. It is a pretty normal late O supergiant. We also have frames looking for WR stars and it seems to be full of them. Even though it's only one small region, if it is representative of all of the area I do not believe the answer for the lack of very luminous supergiants to be the lack of suitable progenitors - they do seem to be there, at least in NGC 206.

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