POSTER PAPERS - SESSIONS 5 and 6.

Chairman: P. CONTI.

P. Conti.

We have a large number of papers dealing with stars and associations in nearby galaxies. In particular there are papers dealing with the stellar content of giant HII regions, for instance 30 Doradus and NGC 3603 and we have already seen other results on this. In the case of 30 Dor we have seen that it is made up of normal stars although at the extreme upper mass range of say up to 200 solar masses.

J. Graham.

I was impressed by the paper on CCD observations of young stellar associations in the MC's by Lortet et al. which states" it is clear that a number of multiple systems, at any rate those with a separation of 0.5 arcsec or less, are still unrecognised" and this made me feel that is just like R136a - we have to learn a lot more about stellar multiplicity in the MC's. We just cannot assume that every object we see is single.

M.C. Lortet.

I would add that among the multiple systems discovered is one of the candles for distance determination in the LMC. A significant fraction of these candles might be found to be composite, though we did not yet observe them.

A. Sandage

The question of whether low mass stars ($M < 2\ M_{o}$) are formed along with the O stars in associations and HII regions is, of course, central for the problem of how much time it will take for a given region of a galaxy to convert all its gas to stars and therefore whether galaxy evolution has taken place yet in a major way along the Hubble sequence from Sc to Sa types.

But the question cannot be answered by looking for faint MS stars accompanying O star formation because, in any given cluster of a given age, there is only 7 magnitudes of main sequence. Fainter than 7 magnitudes below the brightest O star, the MS terminates due to not enough time for the lower mass stars to contract, first along the Hyashi track and then along the nearly horizontal radiative track to the MS. Hence, if low mass objects are indeed present they would not be MS

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objects but rather T-Tauri stars, cocoons or other pre-stellar objects, not seen at optical wavelengths. Merle Walker's 1950's study of NGC 2264 is the prototype. Hence, mere brute force methods of faint searches for red stars in giant HII regions will not answer this crucial point of whether bimodal (Eggen's term) star formation exists, or whether there is a low-mass cut-off, say at 3 M_o as Silk expects. If it is correct that no such low mass stars accompany each O star formed (in numbers given by the "general" luminosity function) then M33 can last as a star-producing galaxy for at least one more Hubble time. But if, in M33 a number G, K and M main sequence dwarfs are produced for each O star, M33 will tie up its remaining gas in such stars (given its present SFR) in only 10^9 more years. If this seems impossible, the conclusion via this argument would be that stars with mass > 2 M_o accompany each O star formed -i.e. there is a low mass cut-off to the formation LF for massive stars.

Conti:

I am worried a little about the background given by the galaxy itself.

Appenzeller:

I have two points. Firstly nobody mentioned T Tauri stars. Two of my colleagues have proposals in for ESO to try to detect these objects in 30 Dor and it should be possible to do this. Secondly, to what extent is it possible to learn about the low mass content of 30 Dor from its integrated spectrum.

Rosa:

From the integrated spectrum it is almost impossible to disentangle the contribution from low mass stars and the red supergiants. Spectra taken in the 8000A region can be used to look for the signatures of RSG's. Also one can try to use 7 colour narrow band photometry excluding the nebula emission lines simulating Strömgren colours going into the near infrared.

This can hopefully be used to search for the low mass stars and whether these are on the MS and possibly have been formed earlier, or whether those are near-IR objects. I hope this can be done.

Moffat.

On the basis of what I saw in the Chu et al. paper the luminosity function is rising rapidly down the 17th magnitude and then it drops. This is a clear case of the incompleteness problem and I think that as it stands now, there is just no indication that the numbers drop off for faint stars. There is no indication of a seven magnitude difference at all, we are only going down to about 3 magnitudes below the turnoff.

Conti.

One of the posters sits a little bit by itself in character, and this is the one by Zinnecker on how to make a superstar. The idea here is that in the central core of a dense cluster you will begin to have star-star interactions and you can eventualy have coalescence and thus build up that way. Also it seems that you can make runaways at the same time as you make the very massive stars since you need three-body interactions.

Appenzeller:

I should point out that he is only dealing with stars up to 200 solar masses and not up to thousands of solar masses that have been postulated.

Zinnecker:

I think that 200 M_{o} is "super" enough! I would like to add that in relation to the dense clusters like 30 Dor and NGC 3603 one has to be aware that N-body evolutionary effects may be potentially very important.

Conti:

A number of papers have dealt whit the evolution of single stars and binaries. One of these papers said something about V729 Cyg.

Sybesma

You are referring to my paper on the influence of overshooting on the evolution of binaries. The problem with V729 Cyg is the nature of the smaller star, whether it is an Of star or a He star accreting from the more massive component showing up as a WR star with a strong hydrogen component in the spectrum. If we take evolutionary tracks including overshooting and you consider the masses which are determined by Conti and Bohannan, and Massey and Conti, you have to conclude that it has to be a He star, that is a leftover He core which was an overshooting core. So if the model proposed by J.M. Vreux in A and A this year is correct, the smaller star is not an Of star.

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Conti:

I was very amused at this because I have also seen Vreux's paper where from his better H-alpha data for this system, he finds in fact what is now suggested theoretically, that the lower mass star, which is very luminous but shows H, is in fact a helium burning star, with an accretion of hydrogen such that it masquarades as an Of star. The irony of this is that when Bruce and I wrote that paper the title was "BD+40 4220 , a star on its way to become a Wolf-Rayet", the irony is that it seems it's already been there and it's now on its way back! It's nice that there is now theoretical confirmation of this.

Sybesma:

I would also stress that the theory confirms that the He star is accreting in the wind and not as the result of Roche lobe overflow. The system is too large for Roche lobe overflow given the considered mass range.

Conti.

In fact, if this is correct, then this is the first evidence that I know that we really have wind accretion.

Vanbeveren:

Some eight years ago a couple of papers appeared in the literature critically discussing the physical meaning of the Roche equipotentials for massive binaries and especially the influence of radiative forces which are extremely important in massive stars. The papers on massive binary evolution presented in this poster session are all using the classical Roche lobes. I wonder what is the meaning of the results, in particular the results of accretion. Do they have to be considered as "interesting" theoretical results" without any real application?

Conti.

I have heard this type of worry expressed at several of our recent conferences as Danny has done today, and on each occasion nobody really responds! Moving on, I think there may be some comments on the poster dealing with "Observed dynamical properties of star clusters in the LMC".

Kontizas.

The very important thing that has come out of the observed dynamical study of the populous young star clusters in the LMC is that they are found to be very extended. In other words the young disk clusters are found very much larger than the disk clusters in the SMC and both are larger than corresponding clusters in our Galaxy.

Graham.

I expect this difference is also telling us about the large burst of star formation at intermediate ages (say 10^8 years) in both Magellanic Clouds.

Conti.

I would draw attention to the important paper on metallicity effects on absolute magnitude determination by Dubois and this calls our attention to the fact that using H-gamma calibration, which is one of the methods used to determine the distance of the Magellanic-Clouds, the line strength will depend a little bit on the metallicity and thus the calibration that is derived from stars in our Galaxy may not be strictly applicable, particularly for the SMC. The stars we are concerned with are B and A supergiants. This paper thus raises the issue of whether the spectral classification in fact has a dependence on metal abundance.

Kudritzki.

I think the problem is not only the one you mention but also normally in these supergiant stars the hydrogen lines may be affected by the stellar wind, so what you normally calculate is photospheric absorption lines which will now be partially filled in by emission from the wind. If for example one takes the case of the SMC the lower metal abundance will give a less strong stellar wind and thus one would really be seeing mainly just the photospheric lines.

Feast:

The problem encountered by Dubois appears to be related to the abnormally strong H lines found in some early type stars in the Clouds (but not in all early type stars) first by Fehrenbach and later by Humphreys. The effect in these stars is so strong that they were first thought to be foreground objects. Do any of the theorists present have any ideas on how to explain this interesting effect?

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Kudritzki.

I think the explanation I gave can work for these stars. The galactic objects will have the more filled in H-lines because they would have stronger winds. In the SMC the winds are smaller and so would be the filling in. In my thesis on AI supergiant non-LTE model atmospheres 12 years ago I found the strange effect that the Balmer jump and the H lines became stronger if one slightly increases the helium abundance. This is due to the increase of mean atomic weight, which causes an increase of the density in the photosphere. In consequence one has more free electrons and more hydrogen atoms around unit optical depth, although the H abundance is reduced. Now according to what we heard on stellar evolution, in the B and A supergiants phase an enrichment of helium in the photosphere is reasonable and could explain the effect. I would like to see the acquisition (which is now feasable) of high quality CASPEC spectra of these objects and a detailed atmosphere analysis. This should help to clear up the problem.

Conti:

There are several posters dealing with star formation which I am sure will inspire comments.

Graham:

I would like to comment on Pennington's paper about NGC 5128. You will remember that it is the closest of the giant radio galaxies that we can see in detail, which is not available for other examples of this type. The paper shows very interestingly that star formation is occuring across the whole of the dark lanes and I would emphasise that in places like radio galaxies one will find star formation occuring in rather bizarre circumstances, which will not be found in normal galaxies. I would also mention in connection with star formation in 5128 that we also find these loose chains of blue supergiants stars which are about 50 kpc out from the center of the galaxy round to the direction of the radio lobes. It is clear that these stars must have been made out there and somehow they are connected with some sort of compression of material by whatever it is that is powering the radio lobes.

Zinnecker:

Concerning star formation in Cen A; this galaxy is often thought to be a merger. Does your work add support for the merger hypothesis?

Pennington:

Cen A seems to be the result of a merger between a large gas

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cloud and an elliptical galaxy. My observations suggest that the cloud is still augmenting the dust lane. The importance of Cen A is that it seems to present a simple example of star formation in that HII regions first form downstream from the infall region and later form across the entire height of the lane. Star formation, or at least the HII regions, dies in one rotation. This is a much more simple picture than that present in spiral galaxies.

Graham:

The evidence for a merger rather favours the merger of a whole galaxy with NGC 5128 rather than just a gas cloud, because its very hard (and I don't know how you would do it) to produce a beautiful system of ripples or shells that we have round 5128 without having a stellar component for the infall.

Pennington:

But those ripples are not the same colour as the O-stellar component, they are slightly redder than the O stars. I dont know how we would produce the ripples, but if that were the case why are there ripples in about 123 other elliptical galaxies many of which show no evidence for mergers in terms of dust or star formation. You see them in apparently very normal elliptical galaxies not just in anomalous cases like Cen A.

Graham:

They are normal elliptical galaxies - merged!

Kaufman:

I would like to comment on the oscillating star formation. You showed that under certain conditions, self-sustained oscillations develop in the star formation rate and you plotted this as a function of a dimensionless time variable. For comparing your results with starbursts in dwarf galaxies one would like to convert to time in years to know what the physical variables are involved in converting from dimensionless to dimensional time, so that one can do this for different environments.

Bodifee:

The dimensionless time can be converted to a time variable expressed in years by multiplying by a factor that is essentially the mean lifetime of stars triggering further star formation. If it is assumed that only massive stars are able to induce star formation then the factor should be approximately 10^7 y.

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Conti:

We also have time for further discussion of the review papers.

Walborn:

I think it is important to pursue the discussion of the relationship between WN and WC stars and the population of the most luminous HII regions because we seem to have a disagreement between the evolutionary scenarios and the observational data. From the observations established first by Roberts many years ago, the WN stars appear more concentrated in clusters than WC stars and it is observed that the stellar content of the luminous HII regions are completely dominated by WN stars. My understanding of the models is that the most luminous stars should get to the WC phase, whereas the data suggest that the most massive WN stars do not get to become WC stars. So I think there may be a fundamental problem between the observational morphology and the expectations from models.

Vanbeveren:

A question to the observers. Is it more diffuclt to recognise a WC star as association member? Is there any selection effect problem?

Walborn:

No, in fact it should be easier since the emission lines are stronger.

Massey:

I think I should point out that the number of WR stars has approximately doubled since the earlier results referred to. Stenholm and Lundström have published papers giving lists of association members and I cannot remember them having any difference between the WN and WC stars in this respect. Certainly in M33 the OB associations show no difference between the number of WN and WC's found.

McGregor:

WC9 stars are usually heavily reddened so we may not be picking them up in the Magellanic Cloud.

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Conti:

OK, the suggestion is that we are missing all the WC9 stars in the LMC. I suppose that is possible but there are effectively no WC8, WC7 or WC6's. I do not think those have been missed on the basis of your argument. I suppose it is just possible that the WC9's have been missed.

Niemela:

In our galaxy we have WC stars which have blown away their HII regions. Neutral H radio maps show a huge hole surrounding the WC stars, so we do not see them within the HII region.

Conti:

My personal belief is that it seems to me that the most massive stars may look like WN stars during a good fraction of the H-burning lifetime. Some of the stars Nolan showed the other day clearly have H present yet we identify them as WR stars. What I do not know, and I don't think there is any quantitative work on this, is whether those stars would show up in the integrated light or not. This goes back to the question of what is a standard WR star - there is a whole range of emission line strengths in both WN and WC stars. The very luminous stars, for the most part, have rather weaker lines than a star like HD 50896 or V444 Cyg which are clearly more highly evolved.

de Loore:

If you consider stars of all masses, then we have claimed that you have first formation of lower mass stars in an association and afterwards, say twenty million years later, bursts of more massive star formation occurs. It all depends on what time you are looking at the association. In my opinion you can have all ratios of WC/WN and WR/O-star depending on the moment of observation.

Sreenivasan:

Is it definitely established thah WN-WC is an evolutionary sequence?

Conti:

Oh no, I thought I'd stressed that in my talk. I think you can say the following: the WC because of their composition are more highly evolved than WN stars.

Sreenivasan:

That's fair enough, but does every WC have to go through a WN stage?

Conti:

Nobody knows the answer to that.

Morgan:

Are the evolutionary models capable of explaining WC stars with an absolute visual magnitude of -1, such as the couple of stars I have found in 30 Dor?

Conti:

Apparently David has found one WC star in the LMC with an apparent magnitude of about 18 but you don't have the reddening yet. It seems there are three possible explanations out of this: (i) it is anomalously heavily reddened which is not unprecedented for a WC star in our Galaxy (ii) it is the central star of a Planetary nebula and the nebula has not yet been detected, and the third (iii) is that we had all better go back to our drawing boards!

Maeder:

I would like to comment on a possible simple way to explain the lack of WR stars in some HII regions. Firstly, remember that in most cases the central conditions of stars "ignore" what happens at the surface and the central evolution inexorably leads the star to a supernova explosion. The observational status of the star may be WNL, WNE, WC or WO according to the mass loss rates involved. Remember that there is some considerable scatter in mass loss rate for all the kinds of stages considered. For relatively low mass loss rate (say 10^{-5} M_o/y in the WR stage) the star may reach the SN explosion when its surface conditions make it still a WN star, while for larger mass loss (say 10^{-4} M_o/y in the WR phase) the peeling-off of the outer layers will have largely preceded the WC stage.

Walborn:

I think that may well be the answer of the whole problem. There may be some mechanism whereby these most massive stars become SN after the WN stage, and this is not included currently in models. I think

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what we observe in the Carina nebula is possibly direct evidence for that. It seems to be possible that Eta Car has already been a narrow-lined WN star.

Renzini:

I do not think Andre's explanation is going to give the answer for the discrepancy between the theory and the data. The point is that if you have a lower mass above which stars can become WR stars you will also have, according to all the parametrisation of mass loss used to date, another mass limit above which WN stars evolve into WC stars. That is so in all evolutionary scenarios so far proposed - the most massive stars become WN and then WC, at intermediate mass range you have only WN stars and the SN takes place during the WN phase. This is in disagreement with the observational evidence in other galaxies, according to which the youngest associations do not have WN or at least in the proportion that they are found in the solar neighbourhood, where it is 1:1.

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