

6. Outstanding Problems in Research on Peculiar Red Giant Stars

Panel Discussion

Johnson: Before beginning our closing panel discussion, we, the SOC, cordially thank our invited speakers, panelists, and all of you for your attendance and participation. It's been great to have you here! Now I call upon our panel and its chairperson, Al Glassgold (NYU).

Glassgold: Thanks. I believe this panel discussion will be a very useful way to summarize and end this excellent conference. Our panelists are, from the left, Bengt Gustafsson, Uppsala; George Wallerstein, Washington; Alvio Renzini, Bologna; Peter Wood, Mt. Stromlo; Ben Zuckerman, UCLA. We will begin with George Wallerstein.

Wallerstein: The commonly accepted doctrine (fortunately not yet dogma) is that cool stars evolve along the AGB through a sequence of types M-MS-S-SC-C. Along this sequence the abundance of ^{12}C increases by the mixing of helium-burning products to the stellar surface. At type SC the C/O ratio passes through unity. While this sort of sequence may well be followed by some stars I would like to point out some apparent inconsistencies as possible guidance for both discussion here and future research.

1. Among the carbon stars there are two groups that do not fit the sequence -- these are the early R stars (Dominy 1984) and the ^{13}C -rich carbon stars (often called type J) which constitute 13% of the sample of Lambert et al. (1986). The former do not fit because their luminosities are too low for the AGB, while the latter have converted ^{12}C to ^{13}C via hydrogen burning, presumably in a shell. Furthermore, the J stars often do not show s-process enhancements, thus indicating that they have not

passed through the S star phase but rather evolved directly from M to C.

2. The oxygen isotope ratios of the SC stars do not fit the sequence. Their $^{16}\text{O}/^{17}\text{O}$ ratios range from 75 to 1200 while the range for MS and S stars is 550 to 3000, and the range for the low- ^{13}C carbon stars is 550 to 4000. Surprisingly, the $^{16}\text{O}/^{17}\text{O}$ range for Barium stars is 100 to 500, much like the SC stars. Furthermore the Ba stars have enhanced carbon, though with carbon still less than oxygen, and the coolest Ba star (HD 121447) has also been classified as the hottest SC star. Are the SC stars binaries that are about to dump on their companions? If so, we are observing them at a remarkable time because several SC stars show technetium, which indicates that they are still in the shell flashing stage. In any case, a search for binarism in SC stars would be worthwhile (Wallerstein 1988 and references therein).

3. A simple evolutionary sequence of M-MS-S-SC-C along the AGB should be reflected in the periods of the long period variables (LPV's) of those spectral types. However, if you eliminate the M-type LPV's of short period, high velocity, and small mass, there is no such correlation except for a small tendency for the C-type LPV's to have periods near 450 days. In fact, the discovery of the OH/IR stars shows that oxygen-rich LPV's are found with periods as great as 1000 days. They were not previously known because they are hidden in their own dust. Thus many AGB stars evolve to be stars of extreme radius without becoming carbon stars. Perhaps they are of higher mass than those which become carbon stars.

With these anomalies in mind, as well as others that have been discussed (or presented in poster papers) at this meeting, we should conclude that evolutionary sequences certainly depend on initial mass and are likely to be affected by both mass loss and mixing. While mass loss

probably does not affect the evolution of the deeper layers of the star (until all the hydrogen is gone), mixing can affect the subsequent evolution so bifurcations or even trifurcations in evolutionary sequences may take place due to convective events that are very difficult to model theoretically (Renzini 1987).

Wood: I think George partially answered his own question when he said there's a range of masses involved in the M, S, C stars, and that the luminosity or period where the transition from M to C star occurs is a function of the mass of the star. If you have a very massive star, it may never become a carbon star. The less massive it is, the lower its luminosity when it becomes a carbon star. In the Magellanic Clouds, where we also see this M-MS-S-SC-C sequence in a single cluster, we know that it does occur. How frequently it occurs is, I guess, another matter.

Little-Marenin: Is there a continuous sequence in period between the optical miras (which tend to have $P \lesssim 500$ days) and the OH/IR stars (many of which have much longer periods)? Is there a gap?

Wallerstein: I'm not sure. The dusty transition objects, like UX Cyg, WX Ser, and VX Sgr, which are rather obscured, were discovered among the OH sources but are not as heavily obscured as the pure OH-IR stars, which are usually discovered by the OH line, and which are found later in the IR and are virtually completely invisible. I think there's a continuity there.

Lloyd Evans: This regards the R stars, especially the early ones. Can one exclude the possibility that ^{13}C -rich stars evolve from early R \rightarrow late R(J) \rightarrow N(J) stars (i.e., evolution up the AGB from the red giant clump as a carbon star) on the basis of detailed abundances (e.g., N star abundance discussed by Lambert *et al.*)?

Lambert: I would like to go back to the start of this discussion and the problems with the M-S-C sequence. I think it's important to keep in mind what I would call normal stars in that sequence. Not all stars in that sequence may necessarily go through each step; it's possible they get so much carbon that they miss the S-star step, for example. Those are what we call normal, and I don't think there is any doubt it occurs roughly as we've outlined. Then there is a group of peculiar stars which we are discussing in this conference: the R stars, J-type N stars, cool hydrogen-deficient carbon stars, and R Cor Bor stars.

Let's say it takes X to produce an R-type carbon star. We don't know what X is, maybe a core flash. The R stars evolve, and they must account for some of the J-type N stars, so you would expect similarities in abundance between ^{13}C -rich cool carbon stars and the R stars. There are some; they are both ^{13}C -rich. The 4 or 5 N stars we looked at carefully are very ^{13}C -rich while the J-type R stars are moderately ^{13}C -rich. Neither group shows s-process enhancements. It's very hard to fit the J-type carbon stars into the M,S,C sequence because they're not s-process enriched. You can change carbon into nitrogen and fiddle with ^{13}C and the oxygen isotopes, but it's very hard to obliterate the s-process elements once you've made them.

The nitrogen abundances are of course a problem, and I pointed out in my talk that nitrogen abundances are a problem for the M-S-C sequence itself. Our carbon-star nitrogen abundances are not as enhanced as that sequence would lead one to expect. In our paper we acknowledged that and discussed ways in which it might be resolved; my hunch would be that we should push the effective temperatures up. I'd justify that, but then I'd have to write it down! (Laughter) The nitrogen doesn't agree with the K or M giant stars; it's a mystery. My gut feeling is that the nitrogen

abundances we got for the carbon stars are systematically in error by some amount. That includes the J-type carbon stars, so I wouldn't refuse linking the J-type and early R stars on the basis of our nitrogen abundances. Also, there are undoubtedly objects that fit neither the M-S-C sequence or R-J-type sequence; maybe there's another sequence.

Stencel: George, you mentioned that the miras in 47 Tuc and other globular clusters share similar periods -- suggesting non-evolution of period. What is the evidence that mira periods do evolve?

Wallerstein: For miras in any rich globular cluster, the periods are similar. Of course, 47 Tuc is the best example, but there are others as well. If the cluster has two mira stars, the periods are similar.

Bowen: Concerning the question about whether there is a discontinuity between mira variables and OH/IR sources (at ~ 500 day periods); without wishing to claim too much accuracy for the theoretical envelope modeling, I simply point out that in the mira models there is a completely natural and continuous change from models showing the characteristics of a (somewhat dusty) optical mira to one with characteristics of an OH/IR source including very rapid mass loss and optically thick circumstellar dust shroud, for periods $\sim 500^+$ days. The result certainly suggests that this really is a continuous change, and that these objects are of the same kind, distinguished only by the natural changes in behavior that accompany the period increase.

Augason: We have observed ten bright, non-mira M and ten non-mira S stars. These are non-mira M and S stars with the same periods. The stars with longer periods are cooler. If luminosities are determined with the period-luminosity relationship, both the M and S stars form an AGB. No evolutionary sequence is apparent.

Feast: Regarding 47 Tuc. It's true that the miras in a cluster are concentrated around the same period, but one has got to remember that there are also low amplitude variables with shorter periods in that same cluster. So there is a sequence, and the large amplitude ones must be concentrated if one believes in evolution in the classical way. But the range is small, and that probably is consistent unless you can argue there are too many miras, which I don't think anyone has ever tried to do, and you would agree that there are not too many there.

Wood: It's about right. The number of stars in the cluster and the number of miras in the period range in which they exist is not inconsistent with evolution over a range from 90 to 320 days.

Feast: With the large amplitude stars, one is just isolating a very narrow region in the period luminosity relation.

Willson: The small range of periods of miras in globular clusters does not imply a lack of evolution in P for longer period, more massive stars. As is most clearly seen in the $\log M - \log L$ ("Wood-Cahn") diagram, when a low mass star turns on as a mira, it already has a mass close to its final (WD) mass, so it does not need to change period very much. However, higher-mass stars must pass through a large range in P in going from where they turn on to their final (WD) masses.

Glassgold: We'll ask Alvio to make one or two short points.

Renzini: I have three points to touch on. (1) Is it worth recomputing synthetic AGB models (a la Iben & Truran 1978 or Renzini and Voli 1981)? (2) Why in the LMC and SMC are there no AGB stars brighter than $M_{bol} \sim -6.5$? Or, equivalently, what happens to the ~ 3 to $\sim 8 M_{\odot}$ sun stars as they reach the thermally pulsing AGB stage? (3) What is the origin and evolution of R CrB stars? For this panel discussion I have singled out

these three questions that to my taste are now particularly hot in the current theoretical research related to Peculiar Red Giants.

Concerning the first question, I have been frequently asked to update those calculations, taking into account the tremendous observational discoveries that have been made since earlier computations were completed. The old synthetic AGB models are in fact obsolete for a number of reasons, some of which have been mentioned by Lattanzio in his excellent review at this conference of the more recent theoretical work. I would now like to show you what I believe are the principal difficulties that for the time being tend to hamper rapid progress in this direction.

Back in 1978–1981 we used a very simple expression for the amount of mass dredged up after each thermal pulse. The whole dredge-up process was in fact described by only one very simple expression in which the dredge-up mass was given as a function of the core mass only -- what we used to call the "dredge-up law". The whole thing was contained in just one punched card! We now know that the real situation is enormously more complicated, and that besides its dependence on the core mass, (M_{core}), the dredge-up law must depend also on the total stellar mass, (M_{tot}), on metallicity, (Z), on the pulse number, (N_p), on the mixing-length parameter (α), and last but not least on the code and the physical assumptions concerning the boundaries of the convective regions. A "minimum" exploration of this parameter space may require computing evolutionary sequences for at least, say, 5 values of M_{core} , 5 of M_{tot} , 3 of Z , 30 pulses, 3 values of α , and perhaps 3 "codes" for a total of 20,000 thermal pulses. Each thermal pulse requires the calculation of about 2,000 models, and each model takes about 10 seconds of CPU on a Cray computer, for a total of about 10 years of CPU time! (Laughter). So when people ask about another generation of synthetic AGBs, this is the kind of computational effort which is required.

One may argue that technology is advancing very fast and that perhaps I am a bit pessimistic in estimating the number of cases that should be computed, but certainly the calculation is very massive if one wants to do it properly. I have mixed feelings as to whether at this stage we should really embark in such a large computational effort. The reason is that the major uncertainty here comes from the way convective boundaries are handled. No doubt we now have an insufficient understanding of the physics of such boundaries, a situation that is hardly surprising, when one considers that we have to pick up the boundary between vanishing mixing and no mixing at all. It is worse than predicting the weather, as we don't have meteorological stations inside the stars! Therefore, I am skeptical that in the next few years there could be a decisive advancement of our physical understanding of convection boundaries. So, my impression is that for perhaps a long time we may better go the other way around, and use our ability of reproducing, e.g., the solar system distribution of s-process elements, to infer something about the dredge-up law.

The answer to the second question is perhaps easier to get. Magellanic Cloud studies have revealed that there are no AGB stars brighter than $M_{\text{bol}} \sim -6.5$ (whether none at all or just very few is presently an observationally unsettled question). This may imply that in AGB stars the core mass does not grow beyond $\sim 0.85 M_{\odot}$ (which corresponds to $M_{\text{bol}} \sim -6.5$), and the thermally pulsing (TP) AGB phase aborts. Why? Several possible explanations have been suggested, but no one has so far worked out a solution in convincing quantitative terms. A couple of years ago Wood and Faulkner reported convergence problems in their thermally pulsing AGB models with $M_{\text{core}} > \sim 0.85 M_{\odot}$. (Iben found similar convergence problems in AGB models of core mass larger than those which led him to discover the third dredge-up process.) When reading the Wood &

Faulkner paper, I was immediately struck by the coincidence of the two reported values of the core mass: virtually no TP-AGB stars in the Clouds with $M_{\text{core}} > \sim 0.85 M_{\odot}$ and bad convergence problems in TP-AGB models with $M_{\text{core}} > \sim 0.85 M_{\odot}$. Perhaps there was link between the two aspects.

If the coincidence is not merely fortuitous, what may happen can be described (for short) as a dramatic increase of the radiation pressure at the base of the hydrogen-rich envelope, as the energy released by one helium-shell flash leaks out of the intershell region. Such an increase in radiation pressure inflates the envelope from below, and as the excess energy is radiated away from the star the envelope recollapses, shocks may be generated, and heavy mass ejection may result. If this scenario is correct, a few thermal pulses may suffice to eject the whole envelope. It is worth emphasizing that such a TP-driven envelope ejection would operate only in the more massive AGB stars, those in which M_{core} grows above $0.85 M_{\odot}$, or in which M_{core} is already larger than this limit when stars initiate the TP-phase. In practice, this would apply in stars initially more massive than, say, $3 M_{\odot}$, which represent a rather small minority.

This scenario can be tested both observationally and theoretically. From the observational point of view, a counterpart to these hypothetical objects could be searched among OH-IR sources with $M_{\text{bol}} < \sim -6.5$, with a very variable mass loss rate over timescales of order $\sim 10,000$ years being a possible signature. Such a variability could manifest itself as a complex density stratification of the circumstellar envelope. From the theoretical point of view, the switch from a quasi-static condition of the envelope to its fully hydrodynamical behavior could be followed in detail using an appropriate code, such as, e.g., the KEPLER code used by Woosley and Weaver to follow the quasi-static evolution of massive stars as well as their final supernova explosion. Progress in these directions may be considerably faster than for updating the dredge-up law.

Stencel: There is an interesting coincidence between the $0.85 M_{\odot}$ core-mass limit for pulse-driven envelope ejection, and the estimated core mass for R CrB stars discussed earlier today. Could the R CrB stars be examples of initially "massive" stars which shed a lot of mass in an ejection? Gillett *et al.* (1986) estimated an upper limit of a few solar masses for the huge IR shell of R CrB itself.

Renzini: No, I disagree.

Wood: I would like to comment on the apparent lack of very luminous AGB stars in the Magellanic Clouds with $M_{\text{bol}} < -6$. Such AGB stars definitely exist (Wood, Bessell and Fox 1983, *Ap. J.*, 272, 99; Hughes and Wood 1987, *Proc. Astr. Soc. Australia*, 7, 147) but appear to be fewer in number than expected given the number of Cepheids in similar areas of the Clouds (Wood, Bessell and Paltoglou 1985, *Ap. J.*, 290, 477; Reid and Mould 1985, *Ap. J.*, 299, 236). A possible explanation for the lack of such stars is a radiation-pressure ejection mechanism which comes into play during helium shell flashes (Wood and Faulkner 1986, *Ap. J.*, 307, 659). Basically, at the peak of a helium shell flash, the luminosity escaping from the core of the star exceeds the Eddington limit there and the star has no option but to undergo a hydrodynamic envelope ejection. One way of overcoming this possibility is to let convection carry a significant fraction of the energy flux. A feature of stellar models of this type is that, the more massive the star, the larger is the fraction of the energy carried by convection. Hence, it may be possible to explain the AGB stars observed to have $M_{\text{bol}} < -6$ as the most massive of the AGB stars. It is worth noting that Richer, Olander and Westerlund (1979, *Ap. J.*, 230, 724) found that the most luminous carbon stars in the LMC were J stars with high $^{13}\text{C}/^{12}\text{C}$ ratios, which indicates that envelope convection in these stars is penetrating right down to the H-burning shell during the

interflash phase of AGB evolution. Such stars might be expected to carry a significant amount of convective flux at the bottom of their hydrogen-rich envelopes during helium shell flashes, and thereby avoid the radiation-pressure ejection mechanism (as well as undergoing dredge-up of carbon).

Glassgold: We will now hear from Bengt Gustafsson.

Gustafsson: I would like to comment on an old question. How good are classical models of photospheres, chromospheres, and envelopes? Could they be any better? By "classical" I mean that plane-parallel or spherical symmetry is assumed, together with static or stationary conditions and LTE. Furthermore, no back-reaction from upper layers (like the circumstellar envelope) or lower ones (like the chromosphere or photosphere) are considered, nor are magnetic fields. How good are these models? Are they internally consistent? Are they physically correct or at least reasonable? Are they in agreement with the observations? I claim "no", and I am not the first one to give that answer.

There have been numerous warnings and arguments from leading theorists. Also, the observational evidence is now rapidly growing, not the least for red-giant stars. This is not so astonishing - we knew long ago that they are more or less irregularly variable and quite tenuous. Signs of departures from LTE, of varying, non-spherical chromospheres, of giant prominences, of non-spherical and clumpy time-varying mass flows, of sometimes large-scale motions in shells, and of magnetic fields of significance for envelope dynamics are accumulating (see Gustafsson 1988, in Modeling the Stellar Environment: How and Why? (Proc. 4th IAP Meeting, in honor of J.C. Pecker, ed. P.J. Delache)).

At this meeting we have got more indications of interesting photospheric velocity variations in Arcturus (Irwin et al.), light

variations in Betelgeuse (Joris), of non-steady, non-spherically-symmetric outflows from long-period variables and other red giants (Heske), of episodic mass ejections and clumpiness of outflows (Olofsson). We have heard about theoretical indications of non-periodic response to periodic shocks (Bowen) and noticed Muchmore's demonstration that molecular formation may be well out of equilibrium in shock treated regions, which could seriously affect the structures in very complicated ways (see also Sharp's constructive calculations that show how complicated and delicate the chemical equilibrium may actually be). There are indeed complex couplings between molecular cooling, dust formation, and dynamics and atmospheric structure in general, as has been suggested earlier by Ayres, Kneer, Muchmore, Stencel, and others. Some of this is reflected in the (admittedly only partly astrophysical) quite different double solutions found to the classical model-photosphere problem for M stars by Scholz, for carbon stars by our group, and even for the sun by Nordlund.

Those who think that these phenomena only matter for the outermost stellar layers should look closely at the recent films of the solar photosphere obtained by Scharmer. From all this one may well get the impression that stellar photospheres, chromospheres, and outer envelopes may be nothing but a bunch of overlapping, transient, erratic phenomena in beautiful interplay. Maybe the long-period variables are the simplest of all these objects because the large-scale radial pulsations force them to be regularly structured.

What could we do in this difficult situation? My main point is that we could do much better, for several reasons.

(1) We have seen an enormous increase in computing power and in the efficiency in numerical methods in recent years. More is to come. My impression is that the theorists in our field have not exploited these

resources to the full extent. Also, now is the time to start planning for the use of future, and more efficient computers. Much more detailed modelling of stellar atmospheres and circumstellar envelopes is already possible - e.g. along some of lines beautifully sketched by Bessell and Bowen in their talks. Also numerical model experiments, studying basic physical processes and interaction between them, should be made.

(2) The rapid development of spatial interferometry, combined with spectropolarimetry, opens up new and very promising possibilities to study structures, such as departures from spherical symmetry. Also, the development of CCD-like detectors for the infrared makes high-resolution IR spectroscopy possible for many more stars than those studied so far.

(3) The semi-empirical modelling of the atmospheres and circumstellar envelopes could be attempted much more systematically than has been done until now. For example, as was shown by Tsuji, one could use the high-resolution infrared FTS spectra already obtained for deriving new and very interesting information on atmosphere and shell structures.

(4) The classical modelling is still important as a starting point for any interpretation of observations. The hard work done by Alexander, Augason, and Johnson, by Jorgensen, by the Australian group, and by others to compile or calculate molecular line data, or to identify new opacity sources, is of particular importance and will be of long-lasting value.

The question I posed at the start cannot be answered without discussing for what the models are to be used. For a general, overall understanding? As a background for exploring specific physical processes? For interpreting spectra, e.g., in terms of chemical abundances? As regards the abundance analyses, the richness of lines in the spectra, while being a nuisance in certain respects, often also allows the possibility of finding many abundance criteria for a given chemical

element. One should exploit this and systematically search for those criteria least sensitive to the uncertainties in structure and velocity field. At least, one ought to combine any abundance analysis with a careful discussion of the effects of structural uncertainties (including thermal inhomogeneties) on the results obtained. Sometimes one might find that abundances, or abundance ratios, are astonishingly insensitive to the structural uncertainties; see, e.g., our discussion of CNO abundances in N stars (Lambert et al. 1986, Ap. J. Suppl. 62, 373).

The use of high resolution spectroscopy in abundance determinations is in my opinion still absolutely vital for high accuracy work - these results for suitable sets of apparently bright stars may then be used for calibrating lower-resolution criteria, suitable for exploration of more distant objects. Sometimes, this program is not possible to carry out, e.g., because the extragalactic stars are systematically different from the galactic ones. There, synthetic spectroscopy is helpful and necessary for a direct calibration of the lower-resolution criteria, but that should then be tested cautiously for the set of nearby stars with well known properties.

Obviously, many things can be done to improve our understanding of AGB star atmospheres and circumstellar envelopes. The major problem seems to be the lack of (wo)man power. How do we solve that?

Most important is not to make these stars less interesting, than they really are. They are very fascinating non-linear systems, where the complexity gradually grows - the number of degrees of freedom increases - from the deep photosphere to the interstellar medium. Yet, they are simple enough to be understood, accessible enough to be well observed, plentiful enough to be studied statistically.

The outer stellar layers are in fact appropriate testing grounds for current ideas about the growth of structures in non-equilibrium thermodynamics. I claim that this is of interest, not only to astrophysicists, but to scholars in many other fields, including cosmology. If cosmology has any bearing on the real world it must deal with the formation of complex structures in similar situations. "Black holes have no hair", but stars have a lot - and we should be proud of that and learn from them. In particular, don't shave them by misusing Occam's razor!

Glassgold: Peter Wood is now going to make a few comments.

Wood: I will make some comments on mass loss.

1. There seems to be some concern here about the use of the term "superwind" with regard to rapid mass loss. The term "superwind" was coined by Alvio Renzini, who noted that the average mass-loss rate required for the production of planetary nebulae ($\sim 2 \times 10^{-5} M_{\odot} \text{ yr}^{-1}$), obtained by dividing the canonical mass for a planetary nebula by a typical planetary nebula lifetime, was much greater than that expected for red giants according to the Reimers mass loss formula $\dot{M} \propto LR/GM$. A question which arises is: does the "superwind" represent some type of mass-loss process different from that which occurs in, say, the optically visible Mira variables? I think the answer is probably "no", although schemes such as a switch in pulsation mode have been proposed in the past in an attempt to get a discontinuous increase in the mass loss rate (e.g. Jones *et al.* 1983, *Ap. J.*, 273, 669). More recent estimates of mass-loss rates of pulsating red giants (Mira variables, IRC sources, OH/IR stars) obtained from observations of microwave emission from circumstellar CO, combined with luminosities derived from a period-luminosity relation, show that the mass loss rate experienced by red giant variables increases

dramatically with luminosity at the rate of a factor of 10 per ~ 0.3 mag increase in M_{bol} , at least until the radiation-pressure-driven wind limit is reached (Wood 1987, in Stellar Pulsation, ed. A.N. Cox, W.M. Sparks, and S.G. Starrfield (Springer-Verlag), p. 250). The increase in \dot{M} with M_{bol} appears continuous, but it is much more rapid than predicted by the Reimers law. Thus there is probably no need to invoke some special mass loss mechanism in order to produce mass loss rates at "superwind" rates.

2. Another problem raised at this Colloquium is the observation of detached, cool shells around carbon stars indicating that a shell of matter was ejected typically a few thousand years ago. One possible way of doing this is with a helium shell flash. Detailed shell-flash calculations show that at a helium shell flash, the luminosity of an AGB star rises above the maximum interflash luminosity by ~ 0.5 mag for a few hundred years and then falls below the quiescent value by ~ 0.5 magnitude over a few thousand years. If the dependence of mass-loss rate on luminosity mentioned above for long-period variables applies, then the mass-loss rate during the shell-flash cycle might be expected to vary by a factor of 10 up and then down from the mass loss rate applying during interflash evolution. This may explain the detached shells observed by IRAS around many optically visible carbon stars. Given the typical maximum mass-loss rates for red giants of $\sim 3 \times 10^{-5} M_{\odot} \text{ yr}^{-1}$ and that the duration of the luminosity peak at a helium shell flash is ~ 600 years (Wood and Zarro 1981, Ap. J., 247, 247), and that a much smaller mass loss rate exists outside the flash peak, a shell of mass $\sim 0.02 M_{\odot}$ would be ejected at a helium shell flash. This amount of shell mass is more than sufficient to be seen by IRAS according to the models presented by Sun Kwok presented at this symposium. These models predict typical shell lifetimes of $\sim 3,000$ years and, with a typical interflash time of $\sim 50,000$

years, we might expect roughly 2-10% of helium-shell-flashing stars to have detached circumstellar shells visible to IRAS at any given time.

Another explanation for the shells around carbon stars is that they are produced by some kind of sporadic ejection event. Such an event has been observed in the carbon long-period variable HV2379 in the LMC (Bessell and Wood 1983, MNRAS, 202, 31p). This star was observed to eject a shell of mass $M > 10^{-6} M_{\odot}$ in a manner very similar to that in R CrB stars.

Glassgold: I think we should discuss this particular point now. Ben, do you want to go?

Zuckerman: Yes. The question I want to ask Peter concerns the relative percentage of the carbon stars with 60 micron excesses. They are referred to in a paper by Van der Veen and Habing (A & A 194, 125 1988). When I looked at a preprint of their paper last year, I thought that possibly these 60-micron excess carbon stars could result from thermal pulsations as they suggested, and even though I don't know as much about pulsating stars as you do, I could at least do the calculation on the time scales. With the numbers I published (1987, Proceedings of the Fifth Cambridge Workshop on Cool Stars, Stellar Systems, and the Sun, ed. R. Stencel and J.L. Linsky), I thought the percentages worked out very well. In other words, the percentage of carbon stars that show 60-micron excesses divided by the total number of carbon stars seems to fit in quite reasonably with the ratio of shell-flash to inter-pulse time. I personally feel that this is the explanation for the 60-micron excesses in carbon stars and the M stars too. Remember, Susan Kleinmann mentioned that more than 50% of the stars with 60-micron excesses in a certain box in the IRAS color-color plane that she showed were M stars. So there's no reason to suspect that the same thing won't be going on in them also.

Little-Marenin: Would you expect to see episodic ejections (i.e., multiple shells) around the two miras with decreasing P which are expected to come out of a helium-shell flashing episode (for example, R Hya)? Have these been searched for?

Wood: It really depends on where you are on that particular diagram. If you are down at 200 days, your mass loss rate has gone up from 10^{-7} to $10^{-6} M_{\odot}/\text{yr}$; that's still not a terribly high mass loss rate that you might be getting to. If it's above 350 days, it should have a shell.

Willson: Over the past several years, it appears that our ideas and those of Wood have been converging -- and not only toward the Mira pulsation mode as the fundamental (F). Consider the thermal mass loss. In the original Wood/Cahn scheme, \dot{M} increases gently until the F-mode onset leads to an ejection of a planetary nebula. In my original picture, I argued that the onset of the Mira F-mode pulsation brought about a large increase in \dot{M} . However, Ostlie and Cox pointed out that the growth rates become very large for small t_{KH}/P : up to 100%/cycle for $t_{\text{KH}}/P \sim 30$. This should be associated with large \dot{M} . On the $\log M - \log L$ plot the line to $t_{\text{KH}}/P = 30$ falls near where Wood and Cahn had "PN ejection". So I would propose that \dot{M} increases smoothly but also very rapidly for these stars with $t_{\text{KH}}/P \lesssim 30$. A rapid rate of increase of \dot{M} at the end of the AGB is consistent with the interpretation of Bedijn, Baud and Habing, and their collaborators from interpreting IRAS spectra.

Kwok: I'd like to comment on two topics Peter mentioned. One is the slow mass-loss rate versus the so-called superwind, and the other is sudden ejection versus continuous ejection in changing the mass-loss rate. When Reimers proposed a formula based on late K and early M stars, we (at Univ. of Minn.) knew as early as 1970 that the mass-loss rate for many stars would be extremely high. For example, Merrill had demonstrated there is a

continuous range of the optical depth of the silicate shell. As showed in my talk yesterday, the optical depth at 10 microns range over 3 orders of magnitude, so we knew there was a range of a factor of one thousand in mass-loss rate; in modern terms, \dot{M} ranged between 10^{-7} and $10^{-4} M_{\odot}/\text{yr}$. For years I have been skeptical about episodic ejection. The only discontinuity, as far as I see today, is still the onset of a total mass-loss mechanism because of the change in characteristics of the central star. It will go from a very extended star like an AGB star to a compact star like a white dwarf, and that will lead to a discontinuity as far as the mass-loss mechanism is concerned. Otherwise, as far as I can tell, and as supported by models, you can model it by a relatively smooth transition from 10^{-7} and $10^{-4} M_{\odot}/\text{yr}$. I don't see any empirical evidence for any sudden ejection at any stage.

Glassgold: I think we better have a final word from Ben Zuckerman.

Zuckerman: I don't have a final word, but I want to raise a point about an apparent disagreement between observations and a long standing fundamental aspect of the theory of post-AGB star evolution. I think this dates all way back to Paczynski's work in the early 1970's. Let me show you what it is that is worrying me, and then maybe someone can very quickly clear this problem up.

We've had several discussions of evolution on the AGB and stellar pulsations in this meeting and many, many times over the last two decades. When a star leaves the AGB, according to theory, it runs at essentially constant luminosity toward very high temperatures ($\sim 10^5$ K, see, e.g. the figures in Iben's article at this conference), and only then does the luminosity start to decline. The potential problem is that there are a variety of arguments from the optical, the radio, and the infrared that show for many stars there is a drop in luminosity at temperatures much

lower than 10^5 K. If in fact there is an observational disagreement with this fundamental aspect of the theory that we've had now for over a decade, then I'm worried.

Let me show you a list of observations that I've put together. A couple in the optical are based on Peter Wood's papers (e.g. *Ap. J.* 307, 659, 1986) and private remarks to me at this meeting. Observations of low-temperature planetary nebulae with $T = 20,000$ K in both the Milky Way and the Magellanic Clouds seem to lie a factor of 3 or more below the AGB luminosity limit. Now Peter, wanting to avoid a contradiction with the theory, has explanations for the apparent discrepancy between the theory and observations for both the Milky Way and the Magellanic Clouds. The Milky Way discrepancy is explained away by an uncertain distance scale; observers just don't know how far away the planetaries are. In the Magellanic Clouds, Peter has suggested that dust absorption is the culprit. At any rate, he is clearly worried about this problem, and he has had to think of ways to place the burden on the observations because as far as I can tell, the theory is quite fundamental.

These are optical observations. Relevant observations in the infrared and radio have also accumulated over the last few years, including some by people in the audience. One of them has to do with an apparent decline in mass-loss rate which shows up in observations of post-AGB stars in a variety of ways. One way is the shape of the IRAS flux data. The typical circumstellar envelope around an AGB star with mass loss produces an IRAS spectrum with monotonically smaller fluxes as one goes from 12 to $100 \mu\text{m}$. On the other hand, the spectrum of a star with a declining luminosity has a peak IRAS flux in either the 25 or $60 \mu\text{m}$ band.

Now a declining mass-loss rate could be due to one or more of three possible physical mechanisms. First is the cessation of pulsations, since as stars ascend the AGB, pulsations distend their atmospheres and promote mass loss according to what Dr. Bowen has told us. Another possibility is that dust grains stop forming as the temperature increases so that radiation pressure on these grains is insufficient to overcome gravity and lead to mass ejection. The third possibility, which is the most disturbing of the three, is that the luminosity of the central star declines. If radiation pressure drives mass loss, then a decline in the central star's luminosity will lead to declining mass loss. I say this is the most disturbing of the three possibilities, because this is the one that disagrees with what I think is pretty basic theoretical modelling.

Now we have three choices, so we could just say, well, for this observation of declining mass loss let's choose either option one or two as the explanation, but for the other observation that I would like to call to your attention there isn't any such choice. I think that declining luminosity is the only one that makes any sense. The problem arises when we take the ratio of the momentum in the wind deduced from CO to the bolometric luminosity, L , of the star. You can deduce $\dot{M}v_{\infty}$ momentum in the wind, from observations of CO rotational emission, as we've heard about a number of times through the meeting. You can deduce the star's radiative momentum by measuring the flux at all wavelengths, generally from IRAS observations. Now, if there are no serious errors in modelling the CO emissions, then this ratio is meaningful because both L and \dot{M} depend on the distance to the star squared, and therefore the uncertain distance doesn't enter in. Eight objects detected by IRAS, several of which were studied by the French and their UCLA collaborators, have effective temperature less than 20,000 K and IRAS spectra consistent with declining

mass-loss rates. For these 8 objects, $\dot{M}_{\infty}c/L$ lies between 3 and 20. So the impression one has, if one believes radiation pressure drives these CO emitting envelopes, is that the luminosity has dropped by about a order of magnitude in many of these stars. I think there's a good chance that we're not making any stupid errors in our analysis since for NGC 7027, which is very hot, one would expect a drop in luminosity and indeed the momentum in its wind deduced using CO is about an order of magnitude lower than the momentum that is currently being carried by the radiation from its central star.

Glassgold: Let me just ask a technical question on the last way of making this argument. Are those CO measurements from bipolar outflows? Could there be some problems in the estimates of the rate in which the momentum is being sent out in the wind?

Zuckerman: I don't think so, because NGC 7027 gives a similar result. Also, all 8 of these objects have the unusual IRAS spectrum which is consistent with a declining mass-loss rate. You don't find these large ratios of $\dot{M}vc/L$ from CO emission for stars that don't also have the funny IRAS spectra that peak at 25 or 60 μm . In other words, if you model a star with an ordinary IRAS spectrum, you will find that in fact $\dot{M}v \lesssim L/c$. Finally, as Omont mentioned earlier in this meeting, if you model the OH-IR stars which have funny IRAS spectra, if anything, you get too low a mass-loss rate from CO. The error seems to be such that CO gives you a lower mass-loss rate than is really there, so I think that the chance of seriously overestimating a mass loss rate from CO is not very great.

Omont: I do agree with your point, of course. Nevertheless, I would like to put forth some caveats. The first one concerns the analysis of the CO data. I mentioned the case of OH-IR stars; those are very cold ones -- the most extreme ones. Probably the computed CO mass-loss rate is

too small by an order of magnitude. But these are special objects which are not completely understood, and in that case, probably because you have some complex radiative transfer, you might achieve some much larger radiation pressure on grains and have some other consequences on heating the gas. So maybe it is not true that the CO mass is simply related to the real mass. Also, it is possible that the momentum could exceed the theoretically possible limit if the optical thickness is larger than 1; then $\dot{M}v > L/c$. Nevertheless, for some of these objects, especially for the few IRAS planetary nebulae, I don't see any way to avoid this decline in the luminosity before reaching 30,000 K.

Zuckerman: I think Alain is being very conservative. Let me just comment on one point that he raised -- on the large optical thickness in the infrared dust continuum. It's true in principle that if the envelope is very optically thick, you might get optical depth values of a few, but I don't think they'll be as high as 20. But, more important, some of the IRAS sources, which have ordinary spectra that decrease from 12 to 100 microns, are certainly very optically thick in the dust continuum and CO; they have enormous mass-loss rates -- just as big, I think, as the objects with funny IRAS spectra which seem to show $\dot{M}v > L/c$. It seems to me that this phenomenon, $\dot{M}v > L/c$, should show up in these objects which have normal IRAS spectra if it were simply a matter of large optical depths in the IR, but it never does. One always finds $\dot{M}v \leq L/c$ in objects with normal IRAS spectra, none of these order-of-magnitude discrepancies.

Glassgold: Let's accept the evidence and then ask what it means.

Zuckerman: Yes, but I want to make the argument as iron-clad as possible.

Renzini: I have several comments. The first is that evolution during the post-AGB phase takes place at constant luminosity insofar as hydrogen-burning models are concerned, while if the stars leave the AGB

right at the time of a thermal pulse, then there is a decrease in luminosity during the transition from the AGB to the PN stage. This, however, is probably less than a factor of 10 (cf. Iben 1984, Ap. J. 277, 333). On the other hand, if I remember correctly, it is not infrequently the case that carbon stars as well as oxygen-rich stars exhibit a momentum flux in excess of that of the stellar luminosity. I remember Knapp et al. providing evidence for this, so right back on the AGB so there seems to be the same kind of situation you have mentioned.

Zuckerman: No, no! The discussion in that old (1982) paper by Knapp et al. has been superseded many times, and right now the situation is that with the best data and best analysis, the ordinary AGB stars do not have excess momentum; they lie within a factor of 2 of L/c . It is only these transition objects which seem to show us, time and time again, the much greater excess momentum.

Renzini: On the other hand, the ejection itself of the envelope in some cases might not necessarily be due to radiation pressure. The ejection might be due to processes which have nothing to do with your idea.

Zuckerman: Well, as Sun Kwok commented earlier, I think he believes that basically you start out with radiation pressure on dust grains and you end up with radiation pressure in resonance lines or something similar. This leads to these high velocity winds coming off the hot central stars, and there isn't any special thing that throws off a lot of mass. I personally agree with him. So unless you can show very clearly that something that does not have to do with radiation pressure is operating at just the critical moment here, I don't think it is a way out of the problem.

Renzini: There does seem to be a lot of confusion over the years about the semantics of sudden mass ejection and superwinds, a confusion that

dies hard. (Laughter.) Anyway, for example, I would find it very difficult to distinguish operationally between a discontinuous transition from a "regular wind" to a "superwind" regime, and a continuous increase in mass loss rate by, say a factor of 10 which takes place while the luminosity increases by only 0.5 mag. Back to the momentum problem, I would like to mention the possibility that the final ejection may not necessarily be an individual, unique, dramatic event, but may well consist of a series of less dramatic events, each removing perhaps a small fraction of the envelope, until the whole ejection is accomplished. This kind of scenario would indeed apply to the thermal-pulse mechanism proposed by Wood for getting rid of the envelope in the more massive AGB stars. In such a mechanism mass ejection is due to processes which are not entirely radiative. So, I think we should leave open the possibility that (some) PNe may be ejected by processes other than radiation pressure, and Zuckerman's momentum argument can be interpreted in support of this possibility.

Wallerstein: I would like to comment on another aspect of mass ejection from massive stars which hasn't been mentioned here. It's suggested by Iben's evolutionary tracks. There exist quite a number of hot subdwarf stars with effective temperatures of 30-50,000 K. Some have hydrogen-rich envelopes, and some are helium stars. They don't have planetary nebulae around them at all!

Iben: They are merged white dwarfs!

Parasarathy: IRAS observations show no evidence for the presence of dust envelopes around subdwarf OB stars.

Wallerstein: I think that's correct. Nothing else is visible, and with a central star of 30-50,000 K you can very easily see emission lines.

Renzini: The question is that the time for the envelope to disperse may be far shorter than the time for the star to fade. So we can have a bright "planetary nebula nucleus" without the "planetary nebula"!

Glassgold: Did you get the answer to your question, Ben?

Zuckerman: Well, I just want to make a comment about non-radiation pressure mechanisms in these objects. In IRAS 2128 +50, for example, the authors of a paper (Astr. & Ap. 198, L1, 1988) which includes two members of the audience state that their estimate of the distance to this cool PN corresponds to a luminosity that is less than the minimum luminosity a star can have and still be on the AGB according to the theoretical models. If you want to say there is no decline in luminosity, then you've got to increase the distance to IRAS 2128+50 and other bright transition objects. I feel that as we go through this list (and we have done this), you will find that it is very difficult to move all these stars to the distances required to get luminosities of a few times $10^4 L_{\odot}$. Independent distance estimates place the star close by, and therefore it doesn't have the luminosity that it had on the AGB, independent of CO.

Iben: I think Alvio's point is that there may be no relationship whatsoever between \dot{M}_v and L/c . Any mass loss occurred at some point earlier than the observed L/c .

Zuckerman: No, what I'm saying is that this independent estimate of luminosity has nothing whatsoever to do with the CO. It is based solely on distance estimates.

Iben: The fact remains that your major argument is based on making estimates of \dot{M}_v and saying that this implies a value of L/c which is less than that anticipated on the AGB. The only direct evidence for luminosity is the thing you just now quoted.

Zuckerman: OK. If one goes through the objects in question, there will be a real problem in placing them at the appropriate distances. At any rate, one has to show there is some other mechanism that can throw off large amounts of molecular gas.

Iben: Tuchman, Sack, and Barkat (1979; *Ap. J.* 234, 217) a decade or so ago found a hydrodynamical ejection mechanism for planetary nebula. I don't know why Peter doesn't defend that because he did the same thing.

Wood: I don't believe it! (Laughter)

Zuckerman: I don't know if anyone believes these mechanisms anymore. There must be some reason why they've fallen into disfavor. One would have to look at it in detail.

Iben: It's tough to do!

Renzini: Yes. That's the only reason.

Glassgold: Probably we should stop this discussion. We're getting close to the 4:00 o'clock deadline. We have time for one brief comment.

Renzini: Let me just say a few words about my third point: R CrB stars. Just ten years ago I suggested a possible way for producing hydrogen deficient giants from single stars, and I still think that this scenario is in every respect far superior to the one reviewed this morning by Schonberner. In brief, when a final thermal pulse takes place in a post-AGB star where the hydrogen shell has already ceased burning, then the tiny residual hydrogen envelope can be ingested into the convective shell, at the base of which helium is burning. Hydrogen is then carried into the hot interior where it burns quite quickly, and the released energy may cause the expansion of the former intershell region, which is mostly composed of helium and carbon, which are indeed the dominant species at the surface of R CrB stars.

I am reasonably convinced that such a scenario can explain every major characteristic of R CrB stars -- such as dimensions, timescales, surface compositions, luminosities, and kinematics. Quite naturally the timescale of the R CrB "loop" comes out to be nearly 1/10 the "fading time" of the previous planetary nebula stage. (This follows from the identical ratio in the available hydrogen fuel in the two stages.) The lifetime of the R CrB stage is therefore extremely sensitive to the final mass of these stars, and may range from just a few years (as e.g. in V605 Aql) for $M_f = \sim 1 M_{\odot}$, up to perhaps as much as 10,000 years for $M_f = \sim 0.55 M_{\odot}$. Concerning surface abundances, beyond He and C, the presence of trace H, N, and occasionally s-process elements (as in U Aqr) is also a natural property of this model. In your place I would have no problem at all in choosing the best R CrB model! (Laughter)

Glassgold: I don't think we're going to be able to discuss this until the next meeting. Thanks to all the participants and to our hosts for an excellent time! (Loud applause.)