CONCLUDING REMARKS

N. Dallaporta
SISSA, Trieste, Italy

Since I am here, I have been asking myself why on earth have I been chosen by the Organizing Committee to this final and quite uneasy job to formulate some concluding remarks to the enormous complexity of this conference. And the only reason I can find, is that I am the least competent person to do so among all the people gathered in this room, as I have never directly put my hands, nor very deeply speculated, on any of the items which have been here discussed. I feel however somewhat encouraged by the belief that the secret wisdom which has dictated my choice was precisely to select an outsider whose function should essentially consist to look at things from a certain distance, with no a priori opinions related to so many unsettled questions. And therefore I shall mostly try to keep myself in such a role asking in advance for forgiveness for all the shortcomings to which my lack of specific differentiated knowledge may have induced me to commit.

I wish moreover to stress that what I present is not a "summary" of the conference, a too much impressive word for my capacities, but only some "concluding remarks" as they were termed in the earlier announcements of the meeting. And according to this definition of my task, I shall use the privilege to concentrate only on some main basic points of interest: mass loss in itself, and effects of mass loss on evolution for large, intermediate, and low mass stars; to my regret, I am obliged to leave by side everything related to the last two sessions as there has been no time gap between their ending and now, in order for me to prepare anything.

According to this program, I think I would summarize the present situation concerning the phenomenon of mass loss by underlining the main phases which have successively characterized its evolution. Almost since the beginning, it was surmized, perhaps a little arbitrarily, that two completely different mechanisms had to be considered as responsible in the two cases of early and late type stars. And, while for the

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red giants and supergiants, following the suggestion offered by the solar wind mechanism, almost all investigations did rely on the verification of the formula introduced by Reimers, with a single value for the arbitrary constant, there appeared to be an increasing general consensus towards the interpretation of mass loss from early type stars as due to the radiation pressure effects as in the line driven wind theory, especially in the form given to it by Castor, Abbot and Klein; the main reason for this consensus being that this theory appeared to predict not only the right order of magnitude for the mass current observed, but also because this current did depend only on the total luminosity of the star according to a rather low power, 1.1, (Barlow and Cohen). I think that it is to this phase that Dr. Conti did refer when he presented his brief summary of current beliefs about winds.

It is not much earlier than one or two years ago that a new set of data on mass loss from early type stars due to Conti and Garmany, de Loore, Lamers and others, has acted a bit as an earthquake in the apparently settled situation. The reason was that while up to that moment most data obtained did refer to stars already evolved, the new data were extended also to main sequence objects, and did show that in this case, for the same absolute luminosity, the rate of mass loss appeared to be much lower than for the evolved ones, and did gradually increase along the transition from OV to OI, then to Of and likely also to WR stars. This discovery has acted as a strong stimulus towards the development of two main lines of new approaches to the problem: a) from one side, the belief that the line driven wind theory, even when one agrees that radiation forces do in fact modulate the flow, is unable by itself to determine the value of it; and in order to achieve this aim, the opportunity of reconsidering some older theories intended either to complete, or to almost substitute the line driven wind mechanism; such as the approach of Cannon and Thomas, which considers that any perturbation on the velocity field must necessarily lead to a general instability of the star surface and place the origin of matter flux in a subatmospheric non thermal kinetic energy storage; and the elaboration of models such as the warm corona one of Lamers and Rogerson, or the small hot corona model of Hearn, and Cassinelli et al.; or the more recent statistical theory proposed by Andriesse and worked out as a direct application of stochastic variations in the outer layers of the stars. These different approaches mostly share the common tendency to reject the dichotomy assumed since the beginning as to the physical cause responsible for mass loss for early and late type stars, and to consider it as a general phenomenon valid in all cases. b) From the other side, next to the theoretical stimulus, the new data has been used as the source for a phenomenological analysis, as done by Chiosi, intended to derive directly from the data a parametrized formula such as to yield the right value of $\dot{M}$ as a pure function of the main parameters of the stars, in whichever evolutive situation they may be found. A clue to the success of these two lines of approach could be considered the fact that one of the formulae of Chiosi, deduced by him only to fit the Conti and Garmany data, almost coincides with the formula of Andriesse theory.

However, the optimism which this last coincidence would naturally tend to inspire has been gradually damped by some aspects of a natural development on which now attention does focalize. This is the increase, both in number and quality, of the data on mass loss rates. And this is at the origin of two difficulties of different kind which have been focussed since the first day of this conference.

1) Both Conti and Lamers in their introductory reports have presented sets of refined data concerning mass loss rates, which both put into evidence the dependence of it from the luminosity class, but disagree between each other as to the dispersion of this dependence; the variation of it for stars with same luminosity being a factor 100 according to Conti, and only a factor 10 according to Lamers. Moreover, by a new phenomenological analysis, but using now more refined data, Lamers finds a dependence of $\dot{M}$ on stellar parameters which does not coincide any more with Andriesse's formula, thus spoiling the precedent agreement between data and theory. However, doubts have been raised about the procedure for the mass determination, while other speakers have stressed the agreement of their findings with Andriesse's work. I would therefore be inclined to conclude, concerning this issue, that the present situation has turned more involved and unclear as never before.

2) The second difficulty connected with the improved quality and quantity of data has been focussed since the beginning by Conti and stressed later by other speakers; and refers essentially to the as yet unexplained occasional disagreements of mass loss rates obtained for the same star by different methods of observation. The spreads are sometimes so large that a mean value does not appear to have any real sense, so one is rather inclined to foresee the necessity of a general revision of the main assumptions which underline the different methods; Conti has expressed some of them in a list of unresolved issues: a) Are spherical symmetry and homogeneity justified in any way? b) What is the source of X-ray emission? c) What the role of rotation, turbulence, magnetic fields? d) Is an unstable flow a source of variability? The consideration of any of these points offers such new possibilities of interpretation and analysis that one is very easily induced to turn a bit skeptical about present achievements. The very important implications of asymmetry and variability have been particularly stressed by different spea-

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kers.

Even leaving aside for lack of time the mention of many interesting results on both early and late type stars, I think I should not end this part of my remarks without stressing at least some few particular points which to my knowledge may be considered as bringing rather new data to the domain of mass loss rates. One is the evidence of winds going on from the nuclei of planetary nebulae, as found by Perinotto, and of some O subwarfs, (but not all, and this is a new mystery similar to Conti's enigma) as found by Viotti. The other are observations, of abnormally small mass loss rates for Of stars as observed by Panagia, and for a WR star as reported by Van der Hucht. If no special reasons are found to convalidate and explain such exceptions, may be that many general ideas already acquired concerning luminosity class effects on $\dot{M}$ have to be revised or completed.

If the general scenario appears to have evolved towards an increasingly involved and not easily understandable situation, what have now been the outcomes on the other line of advance I have earlier mentioned, that is the multifaced theoretical side of the problem? In this conference, we have heard excellent reviews concerning the present status of theories, both relevant for early type and late type stars; and these two sets have been completed by a very incisive presentation of Andriesse's work, which appears particularly appealing among all other attempts for its being so tightly grounded on basic thermodynamics. Next, we have witnessed the presentation of some rather new attempts of interpretation of the mass loss phenomenon, adding new possibilities of understanding to an already so crowded theoretical field. I would mention, on this behalf, the links of supersonic winds with the origin of cosmic rays, presented by Montmerle, some very detailed interpretations of line profiles, as those of Kunacz and Hamann, the localization of the energizing mechanism in the model of Leroy, the importance of multiple scattering as emphasized by Panagia, and last but not least the evidence for a narrow component of the wind studied by Heinrich and for a two component wind model presented by Lamers. However, as interesting these several attempts may appear, I think that the really central part of the theoretical sessions has been the debate arisen after the two main speaches of Hearn and Thomas and which may be synthetized as a clash between two apparently opposite outlooks to the whole problem of mass loss, which I may call, to be brief, the Lamers outlook, considering that $\dot{M}$ must fundamentally depend on the main stellar parameters, and the Thomas outlook for which $\dot{M}$ has some individuality depending only on subphotospheric energizing mechanisms, with not much relation to basic stellar quantities. Both views of course rely on some important experimental evidence which may be focussed on these two extreme aspects: for Lamers, variability which in fluences so much the line profiles, does not necessarily affect the value of $\dot{M}$, which changes much less than the structure of the envelope; for Thomas, stars with quite similar main stellar parameters, but different line behaviour, have completely different $\dot{M}$. Now are these two positions really incompatible? and must we really choose between a theory depending only on stellar parameters, and a theory grounded only on surface phenomena? I, as an outsider to the field, am rather personally inclined to share the tendency expressed, if I am not wrong, by Iben and by Conti, of an intermediate position. Why should the phenomenon not depend on both the main stellar parameters, and such further effects as rotation, convection, turbulence or asymmetry? Is it not likely that complex physical facts should be function of more degrees of freedom than assumed up to now by any of the schemes? And are surface phenomena not at least partially also dependent on the main stellar parameters?

If now we wish to analyze the situation concerning evolution with mass loss of large mass stars in the light of the preceding considerations, we may here also distinguish different phases of the problem. In the first stage which has practically lasted up to now, most groups working in the field, whatever the precise formula assumed by them, have used a mass loss rate dependent only on a low power of the luminosity, for the whole early type stage. As for large mass stars the evolution from the main sequence occurs at practically constant luminosity, this means that in all the works quoted $\dot{M}$ turned out to be practically constant up to the red zone. Therefore, the main effects of mass loss on the evolution resulted in being quite similar in all cases and could be summarized in the following main points:

1) Mass loss reduces or makes disappear the intermediate semiconvection or full convection zone when mass conserving evolution is treated with the Schwarzschild criterion for semiconvection, with the result that the evolutive tracks mimick a Ledoux criterion type evolution of a mass conserving star; that is, they have a loop and turn back from the red to the blue.

2) This turning back may be considered as a competition between the normal core evolution and its reaction on the envelope pushing the star towards the red, and the peeling off of the outer envelope due to mass loss whose effect is to make appear the helium underlying layers, pushing back the star from red to blue. This last effect increases with luminosity, so that the turning back is also a function of the total mass of the star, and, the larger the mass, the nearer is the turning back to the main sequence. In any case very large mass stars may never reach the red, or even not move very far away from the main sequence, which fact could explain why there are not very large red supergiants and very long period cepheids.

3) The helium may ignite only when the star is back to the blue after its possible excursion into the red.

4) On the whole, in order to fit the data, it appeared in all cases that large values for $\dot{M}$ should be chosen.

Now, the phenomenological new formulae obtained by Chiosi and Lamers have in some sense imposed as a first urgent problem the revision of all the evolutionary tracks, using a loss rate changing with evolution phase, and much lower on the main sequence, so that on the whole one could expect that the effects of loss should be less important as up to now surmized. And in fact this has turned out to be the case, according to the results now obtained by several authors and partly presented during the conference. And these results are such as partly to raise or stress some difficulties, and partly to lead to new outlooks, especially concerning the latest stages of evolution.

For what concerns first the difficulties, I would initially mention the apparent contrast between the new results, for which the H-burning region in the HR diagram is much more extended towards the right, and the upper limit in the composite diagram of Humphreys for the supergiants, which appeared to be much better interpreted by the older tracks. However, the difficulty has been by-passed in a paper of Chiosi and Greggio, who construct artificially a composite HR diagram by assuming stochastic birth formation of stars according to a given mass dependence law, and, by populating accordingly the isochrones, do demonstrate that even by assuming a small mass loss rate, the probability of observing stars for a not very densely populated diagram, in a region beyond the upper border of the Humphreys diagram, is practically zero, and that therefore this upper border should appear even in cases of no mass loss at all.

As a second point, a more severe difficulty already focussed by Lamers when he did present his parametrized formula for $\dot{M}$, has now emerged also from the use of the new evolutionary tracks in order to experimentally determine the mass of the stars; it can be shown in fact that the new values thus deduced for the mass lead to a new slope for the mass loss rate as a function of the evolutionary state, which again does not coincide any more with the theoretical formula of Andriesse.

If now however we leave by side any theoretical question connected to the real physical cause of the phenomenon of mass loss in itself, and just try to use it in any possible parametrized form as a tool to refine the agreement between theoretical evolutionary and observed data, can we conclude that this pursued agreement can lead to impose constraints on the rate and the behaviour of $\dot{M}$? The answer is yes, according to the results presented by Chiosi in his summary, the most sensible indicator being the location of the He burning phase in the HR diagram, which can, by changing $\dot{M}$, be moved from left to right and made to coincide with the most crowded area.

If now we accept this kind of best fitting in order to calibrate on it the later phases of evolution, what has been said concerning the correspondence between the theoretical tracks and the observed stages of large mass stars? Not much, if I am not wrong, concerning the problem of the Of stars, whose physical evolutionary state has been once much debated. Are they still in a late H-burning phase, or are they placed on the returning track after the red phase, in a He-burning situation?

Much more attention instead has been shed on the Wolf-Rayet stars; concerning both the meaning of their sequence and their dependence on composition. That they should form a unique sequence, in the sense that for single stars any star moves from WN_9 - to WN_3 and then from WC_9 to WC_3 has been denied by Niemela; and this is not at all in contrast with the scenario outlined tentatively by Chiosi, according to which larger mass stars, when partially spoiled of their H envelope should go through the sequence WN_9 to WN_5, while lower mass stars almost entirely spoiled from H (although Conti has stated that there are exceptions to such a rule) should run the sequence WN_3 to WN_5. A further important result has been presented by Maeder showing that the ratio N_{RG}/N_{WR} (RG = Red Giant) strongly decreases with increasing mass, thus justifying the absence of very large mass red supergiants.

Much attention has also been given to the problem of the effect of chemical composition on the behaviour of evolutionary tracks, which have been studied by Hellings, with again most emphasis on the Wolf-Rayet stage. The main findings on this subject have been brought by Bisiacchi, Maeder and Van beveren; all of whom, with different methods, using data from the Galaxy and Magellanic clouds, arrive at the same conclusion that both the ratios N_{WR}/N_{RG} and N_{WC}/N_{WN} are strongly dependent on composition, in the sense that both increase with increasing metallicity. I think I should add, in order to be more complete, that the russian school has adopted a quite different outlook on the whole subject, based on the idea that mass loss does not practically alter the conservative evolution, but I am too short of time to enter on further details.

Let us now turn to the third main item: the effects of mass loss on the evolution of low and intermediate mass stars, much more complex, owing to the very involved phases which are foreseen for the last end of objects smaller than about 4 initial solar masses. In the frame of the mass conserving theory, in all cases in which the ejection of the outer envelope due to instability and leading as final stage to the planetary nebula state occurs before degenerate carbon ignition takes over in the centre of the core, the ascent along the asymptotic branch is intermingled with different phases due to the superposition of two main different phenomena. It appears nowadays as very likely that during this ascent the outer envelope for a given star becomes more and more unstable, so it may be surmised that it goes through three different thresholds with increasing luminosity: a) The envelope becomes lightly unstable, so the star probably becomes an irregular or semiregular red variable. b) The perturbation becomes stronger, the star becomes a Mira variable. c) The instability is such that the star finally ejects its envelope. On the whole, the less important the mass of the envelope is, the more acute are the instabilities, so only low-mass stars are expected to go through the last stages. However, during the same ascent (and this occurs up to somewhat 8 $M_{\odot}$), the He-burning shell breaks into thermal pulses, the third dredge-up mechanism of Iben sets on, and the mixing with the envelope, bringing on the surface the s elements and carbon, gradually transforms the star into an MS, S, SC and C star. The two effects put together have as a consequence an almost inextricable mixture of all possibilities between different types of pulsation and composition. If now, to all this, one adds furthermore mass loss, this will drastically change the mass limits in which the multiple phenomenology of variable and carbon stars will manifest itself, and the whole picture will be quantitatively strongly shifted in the HR diagram. There are of course many unknowns in this very complicated scenario. What elements have been brought by the conference towards the solution of the different problems?

It must be understood that the complication of the phenomenology stems from two practically independent physical phenomena, occuring during the same evolutionary phase of AGB, that is the instability of the envelope and the instability of the He burning shell. It is quite natural that in a first stage these two series of facts are studied independently, leaving to later developments their correlation in the various cases, and this separation has in fact been kept in this symposium; to such a point that yesterday's two sessions, rather than entitled to the evolution of low and intermediate mass stars, could have been termed: "instability and ejection of the envelope" and "consequences of the dredge-up mechanism".

I think that in both cases extremely interesting results have been presented. Concerning the morning's session, Renzini's talk was focussed on two fundamental points; the precision of the constraints that population II phenomenology imposes to the rate of mass loss - a further test having been brought by Tornambè's contribution, - and the necessity of a phase of strong wind next to the normal Reimers type wind, to explain the planetary ejection. On behalf of this same phenomenon, we have been presented with the Kwok interacting shell wind model, and the very interesting possibility, successfully explored by Kovetz, of a satisfactory interpretation for most of the properties of the planetary nebula phase, obtained with a moderate continuous mass loss; while the preparation to this planetary ejection has been very strongly focussed by Willson to be the Mira pulsating phase, with a drastic request for an extra mass loss due to pulsation, which to my mind could very well be the forerunner of Renzini's strong supplementary wind. So I think that on the whole this conference has brought a definite advance in the understanding of the ejection phase, with the complementary evidence on the importance of the winds brought by Weidemann with his systematic study of the remnants.

Quite apart from any other contribution, the very interesting paper by Zinnecker on accretion during the T Tau phase, may be considered as a hint to foresee in a future conference a session on mass loss with reversed sign.

Referring now only to the papers connected directly with the mass loss, I would say that the afternoon session has been mostly focussed, following the pioneering works in this field of Iben, and more recently, of Renzini and Voli, on the combined effects of Iben's third dredge-up mechanism with mass loss to yield the anomalous compositions of the C and S stars. Iben has mostly shown how these combined effects can be tested by the ratio of white dwarfs to supernova final states, and by the enhancement of carbon and nitrogen in the envelope of the planetaries, while Wood has put into evidence the strong dependence from the composition of the minimum mass, below which the dredge-up mechanism no longer works and no carbon stars are formed, which decreases as Z decreases, a fact which is confirmed by the strong excess of C stars in the Magellanic clouds in respect to the Galaxy.

At this point, while I again apologize for my inability to improvise an the last sessions whose almost continuous run has ended only a few minutes ago, I am only left with some general conclusion for these remarks. The most usual one would be to say that everything was all right, very well organized and that the conference has been a great success. That it has been a success, I have no doubts personally, judging by the extreme interest with which I have followed almost all talks and discussions; a circumstance which is by no means very frequent, according to my experience. But just to say it has been a success would be much too commonplace; it is more than a success: it is a paradox in itself. Because, on the one hand, it has turned out that we do not understand anything of stellar winds, and that we haven't the least idea of which their real physical cause is: on the other hand, it has also turned out that a lot of observed facts are explained and connected to each other by the pure assumption of a mass loss rate parametrized according to the simplest formulae (even if there are some exceptions which do not enter in the rule). How must we behave in such a contradictory situation? Dr. Andriesse has suggested to me an analogy, which has been confirmed by the intense participation of so many scientists to this symposium, and that I can formulate as a final sentence to this talk. We accept that the question of the origin of stars differs from the question of their properties: we have to accept that the question of the origin of stellar winds differs from the question of their properties.