8. CONCLUDING REMARKS

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If you permit me, I will not try to summarize the papers we have heard during these sessions today. They were beautifully presented and each of them described a separate topic, in many cases only loosely connected with the other papers of the session, a circumstance which I think is an unavoidable consequence of the glorious width of the overall subject of this Joint Discussion. Instead, if you permit me, I would like to discuss a specific way of taking a general look at the present state of today's subject.

May I start with an item right from my home university. The Princeton University Press has, during the last few years, published two astronomical books, one by Dr Struve and one by a theoretician. The first book is entitled *Stellar Evolution* and the second book (mine) is entitled *Structure and Evolution of the Stars*. On the basis of these titles, you might have expected to find very similar contents in the two books. But one look into them makes it entirely clear that they have nothing—or hardly anything—to do with each other. The theoretical book describes all stars as perfect spheres in perfect equilibrium—for the simple reason that we theoreticians have not been able until very recently to handle anything more complicated than these highly idealized stellar models. In the other book, by Dr Struve, spherical stars in simple equilibrium do occur, in the chapter which seems to me pretty well the least interesting in the whole book. The rest of the book is full of stars as, I suspect, they really are: varied, complicated and fascinating.

May I then go briefly over a list of omissions which have kept our theoretical work still so far removed from real stars. This gap has been vigorously emphasized several times today. May I discuss this list in terms of 'perturbations' to our idealized theoretical stars.

There are three major classes of perturbations of spherical equilibrium stars, which are still *spherical* but not in equilibrium: pulsations, mass ejection by single stars, and explosions. What is the present state of observation and theory in these three spherically symmetrical types of non-equilibrium phenomena? The pulsations are in by far the finest shape. Observationally, as we all know, the material is gigantic and beautifully systematized, and during the last six or so years the theory, which Eddington had started, has taken a magnificent jump. Starting with the work by Shevakin, we have learned the basic energizing mechanism for Cepheids, and recently even the non-linear theory has been driven forward, with the result of giving us the first tentative theoretical light curves.

The second class of spherical perturbations, mass ejection, is still in rather worse shape. The original impetus to the interest in mass ejection came largely from the theoretical side; our Russian colleagues very early got interested in the possibility of this phenomenon in certain types of stars. By now, I think, we are all highly suspicious that mass ejection plays a serious role in the advanced evolution phases of a star. However, neither observations nor the actual physical theory of this process are in a conclusive state at all.

Then we come to the third class of spherical perturbations: explosions. Here we are in just the most tenuous contact with nature. We heard earlier today that as to spherical explosions novae are to be discounted owing to their binary nature. So there are left super-novae and maybe the quasi-stellar radio sources, in case they turn out to be single stars. And certainly on both those subjects the observations are still fantastically limited and theory consists of a large number of stimulating speculations none of which, I think, could yet be designated as concrete theory.

So much for the present situation of these three types of spherically symmetrical 'perturbations', for which theory and reality are still far from well linked. But now to two, I think, overwhelmingly important classes of *non-spherical* perturbations—both central themes of Dr Struve's research throughout his life: number one, rotation, and number two, close binaries. Both of these problems have been treated many years ago in the linearized theoretical form by Dr Chandrasekhar and others. However, the astronomically more significant cases and practically all observations refer to rotating stars or close binaries where the non-sphericity is so strong that the linear theory is probably not catching the main points, with one exception: the linear theory seems to have given the key characteristics conclusively for the phenomenon of apsidal motion.

Where do we stand with the first of these non-spherical perturbations, the rotation? Observationally, beautiful material has existed for a long time for normal type stars and recently fascinating material has been added regarding special classes of stars like the metallic line A stars. On the theoretical side what is required is the ability to solve the full set of internal equilibrium equations for a two-dimensional case (the third dimension being inactive as long as we restrict ourselves to axially symmetrical cases). Strong rotational perturbations, with or without magnetic fields, give us theoreticians a wonderful area for widening our techniques for one-dimensional problems to those appropriate for two-dimensional ones. Those of you who happen to have attended one of the sessions of the Commission for Stellar Interior will have noted that for the first time the beginning of extensive theoretical work on fast rotating stars was presented there. Though the models investigated are still highly idealized in many regards, I personally feel very optimistic that this work—done in part with the help of the big computers—will result in the near future in major progress in our understanding of fast rotating stars.

Now to the second and last field of non-spherical perturbations, the close binaries. In this field the observational data are overwhelming in their volume and nearly equally overwhelming, for the theoreticians, in their confusion. It takes men like Struve who not only are at home with observations but who also understand what is essential for the development of theory and who are ready to spend the effort and the time, to summarize the available observational material in such a manner as to make it intellectually accessible to theoreticians. On the theoretical side, how might we sort out the problems that we should attack? I think, roughly speaking, you can group the theoretical problems in two areas. The first area concentrates on the interior proper of close doubles (including true contact binaries such as the very common W Ursae Majoris stars may be) and would ignore photospheric and chromospheric phenomena. Even with these restrictions this is a big enough problem as it stands; you cannot avoid full three-dimensionality and you will have to take into account Eddington's circulations to obtain a complete solution of the full set of equilibrium conditions. In spite of its difficulties, I think this problem may get into our theoretical reach in the next few years and may very well, I feel, present us with quite startling new phenomena. For example, it is not at all clear to me that, if the two components of a binary have a substantial common envelope, the Eddington circulations will not carry great fluxes of energy from the one half of the dumbbell to the other half. The discovery of such a phenomenon would shed new light on the very disturbing observations regarding the mass-luminosity relation in these extremely close binaries.

The second area is obviously concerned with what mass motions might occur just outside the photosphere of such a close double. For the case of one of the two components filling its potential lobe, I think the dynamical analyses of which we have heard some interesting examples today are having already a most promising beginning. On the other hand, in the cases where gas streams are spectroscopically observed but neither of the two components seems to fill its potential lobe convincingly we seem to be still seriously far away from a theoretical understanding. However, it might just be that these cases are the pathological extremes which we have to study to gain clues for an understanding of mass ejection from any star. This last area

https://doi.org/10.1017/S0251107X00019520 Published online by Cambridge University Press

may turn out to be a hard one and not so close to solution, but an extremely rewarding one in the long run.

Altogether, looking at the entire field we have discussed today, I think the way Struve chose his work and executed it has very direct messages to us. Certainly there is a very clear message to us theoreticians: stop lazily enjoying simple spherical equilibrium configurations and put more energy into the study of the multi-dimensional problems that nature actually presents. And to the observers I would feel Struve's work implies two requests: first, do not fall in love with an astronomical object just because it is more peculiar than anything else. Even from whole classes of perturbed stars it is hard enough to gain clues for general theoretical work; an over-pathological single object will rarely give such a clue. Occasionally it is essential that we concentrate a massive observational effort on a single object, but I feel only when we are sure that the chosen object is a good example—like β Lyrae—of a whole well-defined and interesting class of objects. And finally the second message to observers that I would like to draw out of Dr Struve's work is this: at least a few astronomers have to act on occasion as the all too rare and all too vital links between the pure observers and the theoreticians, by sorting out of the bewildering maze of observed data those facts and relations they deem most fundamental and physically significant; only such summaries, based on the knowledge of all the observations but written with the courage to select the essential, can make the whole world of observation effectively accessible to the theoretician.

https://doi.org/10.1017/S0251107X00019520 Published online by Cambridge University Press