perature gradients. And when one investigates the outward increase in temperature accompanying such a process, he finds there exist very definite regions of stable radiative dissipation, together with essentially « jumps » in temperature, corresponding to radiative instability. Thus, one must couple to this problem posed by Liepmann a solution of the radiative transfer equation, indeed the solution under a non-thermodynamic-equilibrium kind of situation. This stresses again the point I think is the most important, in discussion of aerodynamical problems in astrophysics. Essentially, laboratory aerodynamics deals with mechanial transfer problems; the astrophysics of static stars, with radiative transfer problems. When one treats aerodynamic motions in an astrophysical environment, he must combine the radiative and mass transfer problems, and the coupling introduces many problems outside the experience of both aerodynamicist and astrophysicist. Our greatest need is to develop some sort of feel for this new range of problems, so that we are not too quick to simply take over a solution from aerodynamics to an astrophysical situation.

In this last sense, there falls very close to the random noise problem another class of problems, just touched on briefly, that of the ejection of material into the stellar atmospheres in « jets ». The spicules form one example; possibly some of the eruptive prominences form another. The problem has hardly been mentioned in the symposium, so should not be dwelt on at length. I would only mention it as a possibly simple example of a place to study the coupling between radiation and velocity field just mentioned, from stability considerations. One might look into the problem of a supersonic jet, where ambient conditions correspond to high enough energy that radiative loss becomes a significant problem during the compression phase of the jet motion. This might provide a place to develop some kind of physical feeling for the difference between the wholly aerodynamic treatment of a well-known problem, and the astrophysical perturbation.

Thus, to me, in maybe an oversimplified way, two kinds of problems stand out. First, how to extend the range of astrophysical information that may either inspire or check an aerodynamic theoretical approach—and here we deal with the subject of sophisticated interpretation of line profiles. Second, how to develop our feeling for the change in aerodynamic solution coming from the introduction of a significant radiative energy loss — or the methodology of studying coupling between velocity and radiation fields.

— R. Lüst:

I feel somewhat in the situation that I have attended a very nice party in the evening, then I am sent a guest book and have to write in something very nice. But already some people before me have done the same. Of course, this summary will be a very subjective one, since probably everybody likes to pick the points in which he was most interested.

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I think it was really quite good for this present meeting that the magnetic-hydrodynamic phenomena were not brought into the discussion too much, since I think this is still the field where our knowledge is highly limited. But on the other side. I think the groundwork is laid for the direction of co-operation between the aerodynamicists and astronomers that really already exists in other than in this field. We may hope that in the near future we can enlarge it quite a bit. Then I would like to remark on the problem of the corona. Since now in the laboratories we have a good possibility to simulate the main features of the corona; for instance, radiation process and things like this, I think that it is really necessary that we put specific questions to the experimental physicists.

I would like just to summarize a few points that I think we have made some progress on. The first one was already mentioned—I think this was the mass-loss problem or the out-flow problem from stars. I do not refer to the sun, but to the stars in general; here new observational data have been presented by Deutsch on M giants — we can see there that circumstellar envelopes imply large outflow in the outward direction. On the other side, there are certainly quite a number of other stars where one knows that a mass flow is occurring, but these stars had not been really discussed in great detail in this meeting. For instance, I would just like to mention the B stars, which played a large role in the Cambridge meeting in England; for instance, expansion of the H II regions and things like this which are certainly connected to the mass-loss, but this point has not been discussed here. I think it was already mentioned by Liepmann that after some discussions there was some kind of agreement that there is now at least a first approximation to a good hydrodynamical model; namely, that we can describe the outflow by the hydrodynamical outflow in gravitational fields, and this is a very essential contribution from the aerodynamicist.

I would emphasize two aspect. Namely, first, that shock-waves might be really important for describing or fixing the boundary problems. I think this was the first heavy discussion between Deutsch and Parker — what are the right boundary conditions? Solutions did come from the aerodynamicists, that we have to take into account eventually the shock-waves. And the second point was the very clear presentation of this picture by Germain, who pointed out that we have just a one-parameter family of solutions, not a 2-parameter family.

The next important point in connection with the mass-loss problem, and I think this was also a step forward, that we could reach some kind of agreement in the solar wind picture; namely, that for the sun we should have hydrodynamic outflow, and that this should give us the so-called solar wind or the corpuscular stream in the solar neighborhood. If one compares the situation with ten years ago, there just the opposite assumption had been made for all these kinds of problems; that we have inflow. Really the same equations have been used, as McCrea pointed out; but now I think we have more and more evidence that we have really an outflow. There were really two points — first, that we got this description, and then that we could reach an agreement on the understanding of what should be the density for this solar wind in the neighborhood of the earth's orbit. On the latter, the evidence came from three points. First, for the first time there were some indications from the satellite observations on the density in the solar wind. Then secondly, from the comet observations. Third, from the geomagnetic observations in the polar regions of the earth. These three kinds of evidence point to a density somewhat smaller than 10^3 particles per cm^3. The average velocity for the undisturbed wind is about 500 km/s. But the important question which has been raised in this connection — and I think it is also true for the other stars, especially noting the observations by Deutsch for the M giants — is what really is the region around the star which is one way or another more or less still connected to the star? How far does this region go into the interstellar space? For this question it is important to see how one may have to place the shock-wave, if one is just treating the problem on a wholly hydrodynamical basis. For this some figures have been given, depending on the pressure in the interstellar space. But I wonder if we would get in this way the right answer, since probably some other kind of boundary conditions might be even more important; and also since these extimates which have been presented here have been based on a steady stream of outflow of corpuscles leaving the sun or the other stars. Especially, I would think for the sun it might be really important to discuss the feature of steadiness, even while taking into account the magnetic field.

Also I should mention one other point which has been discussed already by Liepmann. In the problem of outflow, a point which has not been really discussed is the rotation of stars, which might play an important role, and in this connection also the magnetic fields of the star itself. Parker touched this point very shortly. It did not come into the discussion, what the best picture would be if the magnetic field would be really carried away to a large distance and twisted. Should one see it at a large distance — for instance, in the earth's orbit, or not? This would raise the possibility that the outflow starts at larger distances than have been discussed until now. These kinds of problems have not been discussed, and I think one should keep them in mind. One should note that already some observations on the interplanetary field have been mentioned, and the situation seems to be very difficult if one compares these first observations with the theoretical picture which he might expect for a solar wind. However, it is probably too early to really draw definite conclusions.

Now this brings us to the question, what is really the energy source for this solar wind. What is the temperature in the outer region of the star. Especially for the sun, what is the temperature in the solar corona? We heard

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yesterday that there is disagreement between the electron temperature and the ion temperature. Until now it is not clear if there really is a difference in the electron temperature compared to the ion temperature — this would be one possibility, as has been mentioned by SEATON; but I think this question really depends very much on the exact computation of the relaxation time. The second possibility would be that there would be large motions in the corona. Finally it was mentioned that maybe they have not used temperature at all, and this then leads us directly again to the solar wind or the outflow of matter. Now I think it was clear from the picture of the hydrodynamical outflow that one needs quite high temperatures in this outer solar region to get such an extremely high outflow, and therefore the question has been raised, where is really the source responsible for heating up this outer region? There the idea is that this might be the hydrogen convection zone, and we have discussed several possibilities for heating up the chromosphere and the corona. Here I would like also to mention, as has been pointed out by Mrs. BÖHM-VITENSE, that we cannot expect in every star a hydrogen convection zone. For other stars — for B stars on the Main Sequence — one should not expect to have a hydrogen convection zone — only a very shallow one for the giants and super-giants. But on the other side, from the observational evidence, we got the impression that these hot stars, expecially giants and super-giants, exhibit higher velocities, and therefore the question is in which way we can explain this kind of high velocities in these kinds of stars. There I think no reasonable answer has been yet found. It was mentioned that the rotation in these kinds of stars may be responsible for this kind of thing, but I think it is too early in the thinking to be sure.

Now going down into the solar hydrogen convection zone, it was explained that what the astronomers are doing is completely wrong and one cannot trust it at all — especially in using the mixing-length theory. But I think we did reach some kind of agreement, that the theory was not so bad, to a factor $2 \div 5$. If one does not worry about a factor like this, then this kind of mixing-length theory is probably good enough. Of course, it was recognized that some work has been done to improve these theories on the hydrogen convection zone by really looking into the more detailed problem — discussing and treating the stability problem — this has been done by the Princeton group and by BÖHM. There we learned also one other important thing from the studies on the hydrogen convection zone, and this is that one may expect that the temperature fluctuations are not correlated over the whole wave-number scale; that there might be a correlation only for a small wave-number scale, but not for the larger wave numbers. So this might play a role for the interpretation of observations. This brings me now to the observations; where we really learned quite a number of new details, expecially due to the improvement in space resolution, from the balloon pictures, and from the pictures

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by LEIGHTON. May I just mention one number which looked to me somewhat new in these observations, and this is the lifetime of the granules. It is somewhat larger now, due to the new observations, than one has assumed earlier from the old observations. Also the question had been raised if one could really speak of a well-defined lifetime for the granules at all.

Then further, I would like to mention one additional point which was completely new, observations by LEIGHTON that one has some kind of oscillations in the velocity fields with period of about 5 minutes. Interpreted correctly, this might be of importance. This brings me back to one point which has been mentioned already by LIEPMANN, when he spoke about the cepheids and the mechanism of the cepheids. He mentioned that probably the mechanism for the cepheids is such that positive and negative damping just cancelled, and he mentioned also the possibility that one has random kinds of excitation. It might be that this is the mechanism responsible for this kind of oscillation, since we have this random noise at the top of the hydrogen convection zone, and the observed period for this oscillation is very near to the frequency of the solar atmosphere.

Now I do not want to say too much about the active sun. This field has been touched only just at the beginning, and due to new observations on the magnetic fields, we have now a better detailed picture of the magnetic field. For these larger regions, the magnetic field strength is on the order of 50 gauss, and not as one could see it one the Babcock diagrams, only of one gauss. This refers to the calcium regions. Also, we had some discussions on the motions within sunspots. Here it was good to learn that apparently the observations are becoming better and better, so that probably in the next few years there might be some hope to decide this question, if the motion is really along the field or if there is some discrepancy, as there would be with some kind of motion across the field.

I would like to mention very shortly the problem on the collision-free shock-waves. I think this problem really only at this stage permits comment from two sides. Namely, that one has to do experiments and also has to try to get a real theory. But I think that also for the sun this kind of problem is really of importance.

Finally, in discussing the problem of generating waves by the hydrogen convection zone—we discussed just the propagation of these waves, but we did not really discuss in too much detail the dissipation phenomena in the solar atmosphere. I think this is still an important problem, where much work has been done, since probably the kind of dissipation mechanism which has been discussed until now has been just dissipation by shock-waves, and also some dissipation by ambi-polar diffusion. The possibilities of dissipation mechanism will be probably much larger if one is really studying the plasma in more detail. Since we know already from experiments that we have many

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more instabilities than we first thought of—the plasma always behaves more or less unstably—I would think that this kind of phenomena, which one is just at the beginning of understanding, might have played quite an essential role in understanding phenomena on the active sun. Also, there the point which was mentioned by PETSCHEK—the question of conductivity, a reduction of conductivity might be really quite important. Until now I think we have always been simplifying the problem to the extreme, by assuming infinite conductivity, and I believe it is fair to say that probably all the problems on the active sun—especially flares and other things—can only really be understood if one takes into account finite conductivity and tries not to make this very simple assumption of infinite conductivity. But if one then really wants to take into account finite conductivity, I think it is really essential to understand what is the mechanism for the conductivity, and what is the value for the conductivity, which one should take into account.

Discussion:

— A. UNDERHILL:

THOMAS has noted that at this conference little discussion has centered around information about velocities that may be determined from line-profiles. This is clearly because of the difficulties of separating the true physical information from the assumptions involved in the interpretative processes. Thus, I think astrophysicists must continue to examine the unfolding processes by which they derive information. They must develop methods which determine the physical results in a manner in which we may have confidence. As a result of this conference I feel that we may be able to make some progress in this difficult field. For some time, not too much work has been going on in this field, perhaps because people did not realize its necessity. Furthermore, I believe that if now at this very moment we iterated the conference, and started again, that a better appreciation of the meaning of the results presented, particularly the observational results which were presented in the first few days, would result.

— A. J. DEUTSCH:

I should like to say that as soon as I get back to Pasadena I feel inspired to press forward a program of observing the outer envelopes of stars. Largely as a result of our deliberations here over the past week, I feel that, with the collaboration of the aerodynamicists, the time is now right for us to investigate those appendages of the stars that must be analogous to the solar chromosphere and the solar corona. These heretofore have received very little attention. There have, of course, been good reasons for this; these objects are not

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