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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easy to observe in integrated light. But now I think we see our way clear to specific observational tests for investigating what stars do indeed have extended atmospheres of this kind, and to discover in what respects they resemble, and in what respects they differ, from the familiar example of the sun.

Several other speakers have referred earlier to the fact that our operations upon stars are relatively coarse. We have to use the means at our disposal, and unhappily they are pretty gross. So that we shall never, for example, be able to get the beautiful, detailed information about stratification that the solar observers have extracted from years of patient research; or about the very complicated and fascinating velocity fields which we have heard discussed, actually starting down in the layers of the sun which we cannot see, penetrating into the photosphere, and extending right into the reversing layer where the absorption lines are formed, the chromosphere, and the corona. We never, in the foreseeable future, will be able to get detailed information of this kind about the stars. And therefore I feel it will be necessary for us to exploit very fully those observations that we can obtain by more or less conventional means. In addition, we must certainly apply the new techniques of space and satellite spectroscopy, whenever these become available, for helping us to acquire more data about the radiation in the vacuum ultraviolet, which arises in stellar chromospheres and coronas. I think that this opportunity may not be too far off. Third, we must be guided by theoretical considerations suggested to us by the aerodynamicists. And fourth, we shall have to lean heavily upon analogies drawn with the sun.... I suspect that, as a result of the last couple of weeks discussion here, many of you share my feeling that the sun is in very good hands indeed, and that before very much longer we may hope to have a quite complete and detailed picture of the brightness and velocity fields in the accessible layers of the sun. This should act as a very secure guide to the theoretical people, and particularly the aerodynamicists, in formulating the physical laws that govern the outer structures of the sun. Perhaps we will also gain enough insight into these matters that then, with really much less detailed information to go on, we can apply the same kinds of arguments to the stars.

We shall have to proceed cautiously in any case. You remember that there was a table on this blackboard a few days ago, where we listed the relevant parameters of the flows that we observe in the expanding atmospheres of stars. These parameters are the thermal velocities, the escape velocity, the flow velocity, and the density. We found that these parameters range over several orders of magnitude; and that not only do the parameters themselves span several orders of magnitude, but that their ratios also span several orders of magnitude. Apparently nature is able to build the outer envelope of a star according to any of a number of different plans; and I suspect that in the case of the stars it will be necessary for us to lean rather heavily upon the kind of

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theoretical considerations that you people have suggested in order to decide which of all possible plans nature has decided upon. Even our vocabulary seems to be rather inadequate for the job. I am a little reluctant to speak of the corona of an M giant when most of the spectrum lines I observe arising from this structure are lines from Fe I. I cannot see any lines with excitation potentials above a few tenths of a volt; in some cases I cannot even see anything above .01 V. Maybe it is a corona; but it is a very different kind of structure from the suns's corona, upon which we have concentrated much of our attention. It is quite clear that in extending the theoretical arguments that are immediately suggested to us by the solar corona, we want to keep in mind the possibility that in different stars the relevant parameters can span a very wide range indeed.

There were a few problems which were put forward in connection with steady outflow from a star, which were discussed only very briefly because of the limitations of time. Perhaps it would be appropriate to mention these again. First of all, there is the question of the appropriate boundary conditions to use in discussing such flows. Arguments have been given to show that the solar corona, for example, at a temperature of one or two millions degrees, cannot be in hydrostatic equilibrium with the interstellar medium and that it must therefore expand. If one deduces the consequences of this, he finds that it must expand with a speed of the order 500 km/s, in apparent agreement with some observations. But is it obvious that the temperature is the independent variable, that it must be a million degrees, and that the velocity must accommodate itself to satisfy this condition at the inner boundary, while the pressure goes to the interstellar value at the outer boundary? I do not know, but certainly there are alternative formulations. We require a general physical discussion of the way in which it is appropriate to tie together the near and far boundary conditions. The latter may be specified as the terminal velocity, and the terminal pressure, or the temperature and the density separately. The former must no doubt be the fluxes of energy, mass, and momentum coming through the photosphere of the star. Having specified these two end states, can we then establish that there is a unique path the gas will follow? The aerodynamicists have given me some reason to believe that indeed there is and that these problems can be solved.

My colleagues in observational spectroscopy, and I, will have to do a lot of careful work to establish whether the consequences of theories of this kind are compatible with the observations. There has seemed to be general agreement that we can understand the flows at best only by supposing that there is always a high temperature region closely surrounding the reversing layers of the stars. There is some residual doubt whether in fact there is always room inside the shell we observe for an envelope of this kind, which would remain virtually invisible. The question has to be investigated in a quantitative way. There is also a little more information we may get about the geometry of the structure by considering the binary systems, where we have not one but two lines of sight through the flow. And there will be fascinating questions, which were referred too briefly, having to do with other possible observational consequences of the existence of extremely large shock fronts around stars. What is the nature of the dissipative process in the shock front? Is it possible that this process has anything to do with the generation of non-thermal radio noise? What is its importance in the modulation of cosmic rays in the neighborhood of the sun, and in establishing the nature of the cosmic radiation throughout the galaxy? What happens, if anything, when these shocks collide with each other? For, if stars are to have enveloping shock fronts around them with diameters of some hundreds or thousands of astronomical units, space then is not so terribly empty any more, and at any one time in the galaxy there will be some collisions occurring. Does this lead to observational consequences? No doubt we shall have to look rather carefully into the stability of whatever kinds of flows we arrive at by a straightforward solution of the momentum equation; are all such flows stable against mechanical and thermal perturbations?

I wonder whether it will be possible for us to extend considerations of this kind so that we can make progress in discussing some of the more exotic kinds of stars that were not mentioned here at all, or only very briefly in passing. These are clearly suffering the same kind of fate as normal stars, but according to a much more elaborate pattern. There are stars, you know, in which we see not just the ordinary set of lines produced in the reversing layers, and the displaced set of zero-volt lines produced in an expanding shell; but in which we may see—and this is no exaggeration—up to 6 or 8 sharply defined components of the same line. Each has its own radial velocity, which remains virtually constant over a period of years. What can the hydrodynamics of this process be? We must be looking at a series of shells, one within the other, shells that are remarkably stable. How does it happen that we observe the atoms which have one velocity, which presumably lie in one shell; and the atoms which have another velocity, which lie in another shell; but nothing in between? It is quite clear that this year we do not observe in a shell the same atoms that we observed last year . They have moved out to some place else; we are observing a level in the gas. Why is it that we cannot see the atoms between the levels?

What about all the problems having to do with catastrophic mass-loss? How about the nova phenomenon, which we agreed to refer to only very casually here? Or the phenomenon which produces the planetary nebulae? You remember that the problem was posed as to what would happen if, in an infinite layer of uniform gas, one were to set impulsively in motion a certain slab of gas and ask for the subsequent motion of the gas both in front of the slab and behind the slab. In particular it would be fascinating to know whether, and in what circumstances, the consequence of such an impulsive disturbance will be to detach a slab of gas from the rest of the gas. Or, if not literally this, then at least to produce a very deep density minimum. This might well correspond to the kind of thing that we have in the planetary nebulae, or in the novae.

There are some of the problems that we did not have time to discuss.

Finally, I want to say what very good news it is that LIEPMANN and, presumably, some of the other aerodynamicists are now, I think, more interested than ever in attempting to simulate astronomical phenomena in the laboratory. He did say that he thought it would be some time before he could incorporate an arbitrary gravitational field in his experiments. On the other hand, I think it was he who, at the beginning of his talk, indicated that the effect of the gravitational field—at least in the problem of spherical outflow—is just that of a nozzle. So possibly with a sufficiently clever geometry in his experiments, he will find that he can simulate the gravitation field too. It seems to me that, with so many avenues of exploration open to us, in the course of the next few years, we may confidently expect some pretty exciting advances in the study of aerodynamic phenomena in stellar atmospheses.

— B. E. J. PAGEL:

I would remark on the widths of the lines in the corona. In discussing the discrepancy there between the electron temperature and the kinetic temperature—it was mentioned that one possible explanation was macroscopic motions. I would just like to show that a macroscopic motion necessary to produce this effect seems to be perfectly compatible with figures given by PARKER in the theory of the solar wind. This might be a means of putting the two effects together. If you consider the limb of the sun, and a region of the corona where you see through a few scale heights—actually you see quite a distance because the densities do not vary rapidly along the line of sight—contributions to the observed intensity will come from a horizontal distance equal to an appreciable fraction of the solar radius. So, if material streams outwards at say 50 km/s, then one will see a component of velocity dispersion along the line of sight of the order of perhaps one-half of this, or perhaps a little less—say 20 km/s.

Now the thermal velocity of iron atoms at a million degrees is roughly 15 km/s, so it is clear that this velocity dispersion is of the same order as the thermal velocity, and is enough to raise the temperature that you would deduce from the line-widths to 2 or $2\frac{1}{2}$ million degrees.

— G. K. BATCHELOR:

I have a few remarks which are not actually summarizing but are personal comments on what I think is worth remembering in the conference.

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I think that as in the case of the three preceding meetings, the fluid-mechanics people have given the astrophysicists the distinct and correct impression that we know little about turbulence. Practically no speaker in the sessions was able to say anything about turbulence at all, without somebody else questioning his remarks, or saying, well, it may apply somewhere else but it does not apply here, or adding that some more qualifications have to be made. Turbulence is a messy subject, which intrinsically does not allow very precise and specific statements, and it is also a subject about which we know very little by comparison with, say, gas dynamics or magnetohydrodynamics; in fact, we know practically nothing. Two particular aspects of the turbulence problem were raised in the discussions of the last four days which I think are interesting. One is the aspect touched by LIEPMANN in his summary; namely, the concept of fairly definite patterns of motion in what we usually call fully developed turbulence. It may not be true of all steady fields of turbulence that these ordered patterns exist, but it is certainly true of some. Thermal turbulence is probably one of them; likewise turbulence generated in the space between rotating cylinders, and probably jets and wakes—in these cases one can recognize the existence of certain large-scale structures which qualitatively resemble what one would find from stability analysis for the mean velocity profile which exists in the turbulent flow. Now I do not think the concept of stability of fully developed turbulent flow is yet wholly precise. What we have to think about—I suggest—is the sense in which these ordered large-scale features represent some kind of finite-amplitude disturbance which exists in the statistically-steady turbulent state. It is natural to think of these ordered motions as representing one single mode which has been selected by a process of selective amplifications of the kind we know about in linear stability of laminar flows. But I don't think that notion by itself makes sense, at any rate not if taken in a simple or straightforward way, because it leads to the idea of a turbulent state with the energy partly distributed over a continuous wave-number spectrum and partly in a single spectral line representing the particular mode that has been selected by the instability. However, we believe that owing to the non-linear effects, which are undoubtedly important, the energy which is confined in a single line would very quickly be spread over a whole continuous range of wave numbers by the interaction with the continuous part of the spectrum. Possibly the position is that there is a peak in the spectrum rather than a single line, a peak whose width is determined by processes which are at this moment unclear to us. However, some of the information presented during the meeting about the granulation in the convection zone of the sun does force one, I think, to recognize this as a definite and interesting problem. When I first saw the photographs of the solar granulation published by SCHWARZSCHILD (*Ap. J.* 1954, **130**, 345), I was struck by the apparent regularity of the cells—they look approximately like the Benard cells, but yet they are existing at a Rayleigh number which is far above the critical value and are existing presumably under fully developed turbulent conditions. They are not perfectly regular, but they have considerable regularity and to understand how that regularity can exist in that situation is an interesting problem.

A second aspect of the turbulence problem which has not been mentioned today, but which did strike me as something to be thought about further, is the failure of the similarity theory to predict correctly the distribution of mean-square temperature fluctuations in thermal turbulence. Here, I am referring to thermal turbulence of the simplest kind, generated by a heated lower boundary, and with rigid boundaries. As MALKUS pointed out in his talk on Friday, such experiments as have been made suggest that the root-mean-square temperature fluctuation falls off with height as something like the reciprocal of the distance from the lower boundary. The simple similarity arguments which suppose that only the rate of transfer of heat across a horizontal boundary is relevant produce a variation of the mean-squared temperature fluctuation as height to the minus $\frac{1}{3}$ power, and are thus in conflict with experiment. Malkus' theory is or is not right—the theory is difficult to understand and requires a lengthy discussion. But leaving that aside for the moment, it is interesting to ask why the similarity theory is not right. Malkus' suggestion is that it is incorrect because it takes no account of the effect of the viscosity and conduction in the region which is not near the lower rigid boundary. He may be right—it is hard to think of any other reasons why the similarity arguments should fail. If he is right, and if viscosity and conductivity are important in their effect in the interior of this flow, then that raises interesting questions and requires some revision of our thinking about turbulent flows of this kind. These are two aspects of the turbulence problem that I at any rate would like to carry away in my head for further thought.

Finally, if I try to characterize this meeting by a few words, it would be something like this. You remember that previous meetings have been summed up in a few words and I suppose it is always handy to have a label. The first one was largely devoted to turbulence and magnetic fields in the galaxy. The second, for the most part, to shock-waves and turbulence. The third to magneto-gas-dynamics. This one is perhaps not as easily summarized, so I would not suggest that the phrase that I am going to use is appropriate for everybody, but for me the problem thrown up most clearly is that of turbulent convection. Everyone will have his own particular label, but that is the one which seems to me to characterize this meeting in contrast to the three preceding ones.

— E. SCHATZMAN:

I think that we should add the problem of the influence of the radiation field in aerodynamics that has been stressed already by THOMAS. I think that

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is very important to astrophysics, and it does not yet enter heavily in the laboratory experiment of aerodynamicists. When we introduce a radiation field into the equation of aerodynamics, we get into trouble with the equations because they become incredibly complicated. We have tried once or twice a linearized approach to the motions, including the radiation field and even then things are horribly complicated.

Closing remarks to the meeting by M. Minnaert.

I should like to present a few general impressions from this interesting meeting of scientists working in two different fields. It has often been noticed that progress in science is made just on such meeting points, and I think this specially applies to astronomy in combination with other sciences. Astronomy in the last 50 years passed successively through the age of optics, then of atomic physics, then of nuclear physics, and now again of aerodynamics and electro-magnetism. At first sight I get the impression that the contribution of aerodynamics to the development of astrophysics has been less spectacular, less sudden perhaps than the progress made at the moment when Bohr's atomic theory was discovered and applied by SAHA to stellar atmospheres; or at the moment when principles of nuclear physics were discovered, and fusion processes were found to be the main source of energy in cosmical bodies and the source of evolution. But this might well be a perspective effect. In the time when you are living, you always have the impression that things are going slowly; and when you are looking back, then you see that really in that period in a short time a considerable progress was achieved.

Such concepts as convection, shock-waves, magnetohydrodynamic waves are so fundamental now in astrophysics that we could not miss them. They are part of our vocabulary, they play such an important role that looking backwards, perhaps in 10 or 20 years, we may also have the impression that this was really considerable progress in a short time.

It is clear *a priori* that the contribution of aerodynamics to astronomy must be very important; for after all, with very few exceptions, our universe is composed of gases. It is an aero-universe, and more specifically it is not only an aero-universe but an aero-dynamic universe. Everywhere we find that there are motions in stellar and interstellar space, even in these layers of the sun and the stars where you would have thought that there was at least radiative equilibrium. We find that inside that same layer there is « microturbulence »; and in deeper layers there are motions of convection; and then there are the chromosphere and the corona with their shock-waves. Everywhere there are motions; the whole universe is really an aerodynamic universe. It is clear by adding to astronomy the knowledge which we can get from aero-