or not. In the discussion on these shocks there is one outstanding experimental result apparent: in the course of these four symposia *i.e.* in the last 12 years, one has reached a point where practically every figure in the variables of state that has been quoted in this symposium is not completely out of reach of laboratory experiments. Laboratory experiments on gas motion in the solar corona with its temperature of some 10^6 °K would have looked perfectly ridiculous in Paris in 1949; today the temperatures are within reach. In general the possibility of investigation of astronomical or astrophysical phenomena in the laboratory has increased enormously. Furthermore the interplay between shock-waves and turbulence, and its relation to astrophysics, is becoming closer. This is particularly evident from Petschek's discussion. Unfortunafely, we cannot produce in the laboratory the size and the gravitational fields; this is something which I think even most of us today would consider as necessarily left to the astrophysicists proper. Even in the prevailing satellite craze, I do not think anyone yet thinks of building a satellite big enough to show there significant effects.

— R. N. THOMAS:

Let me turn to look at the Symposium from the standpoint of the astronomer, in terms of the background that LIEPAMNN laid. Really there are two viewpoints that must be considered. From the standpoint of an astronomer anxious to find a ready-made analytical approach; what kind of structures do there exist in aerodynamics, relative to the problems found in astrophysics, that we can take over, use and apply? In essence, LIEPMANN has given a survey to answer just this viewpoint. Then the astronomer might ask, what is the viewpoint of the aerodynamicist? Why should he be interested in such things, other than as a kind of altruistic consultant? It would seem that the astrophysicist's hope of attracting the aerodynamicist lies in the possibility of enlarging the domain of the arodynamicist's experience. Now LIEPMANN has just given a discouraging comment on this last—by stating that more and more we can do in the laboratory everything that the astronomer can do, with maybe the exception of gravitational fields. However, I would point out two other aspects. One is from the standpoint of time-scales; in astrophysics, one can get steady-state phenomena departing rather widely from local thermodynamic equilibrium; and he can do this at quite low densities, so that collisions do not predominate everywhere as the important rate process. Second, and correlated, radiative phenomena have a much greater importance in the astrophysical situations; the coupling between velocity and radiative fields in determining the thermodynamic state of the medium becomes very important. (There is, of course, a third aspect, very large dimension in the astrophysical case, which is of importance both in hydromagnetic and in radiation problems. I want here, however, to emphasize the other two points.) So maybe these

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aspects offer an interesting extension of the region of aerodynamical experience, sufficient to characterize the astrophysical domain as having unique properties.

Now, if I want to look at these problems from the standpoint of both the astronomer and the aerodynamicist, it is necessary to keep in mind that both somehow have to get in the mood of the other's viewpoint. From the standpoint of the astronomer, he must somehow buy the aerodynamic terminology and approach. For example, for years we have been talking about « astronomical turbulence », and have been asking is it a question of semantics, whether or not we agree that this is a good terminology, or is there something misleading in buying such an « expropriated » term. I suggest that when the proceedings of this symposium come out, the astronomer reads again the comments that CLAUSER and LIEPMANN have made on this point, and searches, his soul a bit. This example seems to me something very worthwhile taking to heart. If we could somehow get into the habit of thinking on the basis of existing background, then one has a better chance of using the aerodynamic concepts to solve astronomical problems.

From the standpoint of the aerodynamicists, I would pick up the point that LIEPMANN made a few minutes ago, that it would be nice if the astronomers would give him a table of velocities, temperatures, and so on. I would just point out one thing—that to get these quantities that would be put into a table, one requires oftentimes almost to solve just those problems which one asks for the help of the aerodynamicists on and for which they ask such a table as a starting point. Two examples on this: one, the point that SEATON raised yesterday—with respect to the corona. If one says there exists an electron temperature, and a kinetic temperature, which are different by a factor of two, and asks for interpretation of this, then one wants to be sure that his interpretation is precisely what he says. As SEATON pointed out, there were essentially three, possibly more, alternative explanations on this: — One, we just have wrong collision cross-sections, and so deduce the wrong electron temperatures. Thus, a table pointing out that there exists a difference in electron temperature and kinetic temperature is already an assumption. Second, maybe this difference really exists. Third, maybe we have a « turbulence »; and fourth, maybe we have a non-Maxwellian velocity distribution. Now all of these things possibly are what should go under the column as uncertainties. But you see the uncertainties reflect what it is that I am talking about. I have to discuss each of the alternarives from the standpoint of asking their likelihood.

The second point which I think is something very worthwhile keeping in mind is when you say that you are impressed very much by the sun as a source of information. A few years ago some astronomers would have said we need Mach 3 turbulence in the solar chromosphere to explain the observations of the change in intensity of the spectral emission as a function of height, which was interpreted as the density gradient in the atmosphere. Astronomers would have furnished you tables of density, temperature, composition as a function of height, and asked an interpretation of how this Mach 3 turbulence exists. Now, applying some ideas of non-equilibrium thermodynamics, we come up with a quite different structure of the same atmospheric region. It seems to satisfy hydrostatic equilibrium. The aerodynamic problem seems to be, what kind of a non-radiative energy source that provides negligible momentum transfer can exist? And we give you a quite different table of densities and temperatures. The aerodynamic problem, as it existed a few years back, was a question of a momentum supply, with no energetic coupling to the ambient temperature; the current problem, a source with strong energetic coupling, but no momentum supply.

This emphasis on looking carefully at the ultimate basis for the kind of astronomical information you want, is what PECKER and I tried to stress in the opening summary paper—which essentially fell flat. The point is, we observe spectral line-profiles—that's your table of directly observed quantities; temperatures, densities, velocities—those are inferred, their values are a function of the inferential procedure, which often already has in it an assumed aerodynamic solution, at least conceptually. An example is the existence of Mach 3 turbulence that doesn't couple energetically with the ambient atmosphere. In certain simple cases, we can get information from line-shifts alone, and do not require in a first approximation the analysis of the profile; but these are exceptional cases.

With these cautions in mind, let me summarize what it seems to me the aerodynamicists have said about the problems the astronomers have posed, and add a few comments from the above directions. LIEPMANN has broken down the problems which stood out for him simply as problems; let me recast the approach slightly, in terms of the astronomical material upon which the problem rests.

The question of non-thermal velocity fields that are described as random at a particular point in the atmosphere. – The material discussed mainly refers to a small-scale motion, and the results can be broken down into those coming from total absorption in the line, and those coming from an analysis of line-profiles. These velocities are what the astronomers call « microturbulence ».

Based on Anne Underhill's summary of results from total absorption it seems to me the aerodynamicists say the following: « We would indeed be very much surprised if you did not find existing in the stellar atmosphere small-scale motions, whose velocities are smaller than the local sound velocity. It appears that your results do indeed give such subsonic velocities, so we find nothing surprising. It is not clear to us from your methodology that what you call « microturbulence » is wholly a velocity, rather than some

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neglected effect in your analysis; but since your derived values are upper limits on any velocity, we find no cause for alarm. » In addition to the remarks PECKER and I already presented, I would only add that I personally still find it difficult to see why compressibility and dissipation effects are obviously so negligible as a number of the astronomers seem to believe; for these « microturbulent » velocities extend to $M \sim \frac{1}{3}$—nearly 1. Schatzman's talk in Part III-B emphasizes this same remark.

If, now, we turn to material from analysis of line-profiles, then it seems to me that in this symposium we have talked surprisingly little about many of the problems which astronomers have discussed so extensively in the literature and which are necessary background for any aerodynamic synthesis: the questions of the depth-dependence of the small-scale motions, and their possible anisotropy. These points have been mentioned in passing, several times; but nothing like the desired compendium of knowledge has been presented for the aerodynamicists to see, and possibly find problems in.

Thus, all that has really been said about these small-scale random motions is that their magnitude is not surprising; but nothing else, because of lack of presented material. I question whether this underemphasis really represents the astronomical situation relative to material or to interest — or does it?

We find an even greater absence of discussion of material relating to large-scale random motions; indeed, relating to large-scale motions of any sort, except systematic motions whose properties exhibited here rest mainly on line-shifts, or correlated properties of the (optical) continuum. This underlies my remark that all the rather elaborate considerations PECKER and I put out in the introductory paper were really rather beside the point, as regards the eventual emphasis of the symposium as it has evolved. I do not mean to imply that we in any sense repeal the paper; PECKER comments that he thinks it will be a good paper in 1974. That is, by then we may, at one of these symposia, be basing our conclusions upon material derived from line-profiles analysed according to the methodology critically discussed there. I am sure the aerodynamicists were aware of the interchange among the several groups of astronomers, as to how elaborate a methodology is necessary for use in discussing current astrophysical data on spectral line-profiles. I would simply like to stress that getting such material, and analysing it, is really the prerequisite for that detailed picture of the aerodynamics of a stellar atmosphere which we all desire. Turn then to the problems which have come up from the more restricted kind of data, which essentially deals with systematic velocity fields.

Systematic large-scale fields. – Again, let me categorize these slightly differently than did LIEPMANN, emphasizing different aspects. Three problems stand out: *A*) the « orifice » problem, or continuous mass flow from the star regarded as the diverging nozzle problem in aerodynamics; *B*) two sets of problems connected with the He-He ionization zones; *C*) the problem of propagation of a compression wave in a region of decreasing density.

Problem *A*) has already been discussed, and summarized. I would only comment on this from the standpoint of extending the range of aerodynamical experience — namely, the one thing not covered by simply applying the classic aerodynamic literature to this is the question of how do we take into account the radiation field, so far as excitation of electronic degrees of freedom, the energy leak, and an energy dissipation term are concerned. This essentially enters both through the specific heat ratio, γ, and through problems of internal excitation, which again relate to γ. While maybe I can solve the problem by talking simply about a range of γ-values, very probably the way that I have to go at it, is to start talking about coupling to the velocity field — and studying the solution as a function of position rather than the solution simply as a function of a constant γ-value.

Problem *B*), the He-He ionization zone, has really two sets of problems associated with it: the ionization zone as a source of convective motion for all stars having such a zone, and the ionization zone as the combined thermal valve giving a phase change in, and as an energy source maintaining, the pulsation in the pulsating variable stars. The first point is of especial interest with respect to the preparation of a list of observed phenomena for the aerodynamicist. For, if I abstract correctly the discussion centered on the presentation by Mrs. Böhm-Vitense, Spiegel, and Malkus — if presently existing theories hold at all, they hold only up to a point significantly below where we have direct observational material bearing on the problem. The quasi-mixing length theory gives reasonable results, if at all, only up to optical depth (in the continuum) about 1, thus below the region observed in spectral lines. In the transition zone, where we have some possibilities of data — observations of weak lines, center-limb contrast of granulation—the theory cannot be expected to give good results. Questions of compressibility certainly enter here—recalling the discussion of the other day, I would note that the Mach number becomes about $\frac{1}{3}$ at the beginning of the transition zone, so compressibility cannot be neglected. Thus, the questions of the distinction between random noise, and eddy-type turbulence, which have been touched on several times during the symposium and by Liepmann a few minutes ago, enter. To my mind, the approach started a few years ago by Moyal and Uberoi still offers the best direction to start getting some kind of a picture.

The second problem on the ionization zone, connected with the pulsation, touches both on the solution to the pulsation problem in the interior, where we do not have direct observations, and in the atmosphere, where we do. If I look at the situation in terms of the piston problem, which Whitney has summarized, then I personally still prefer to attack the general problem as two coupled problems. One, is the solution of the interior problem, with,

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if Ledoux's suggestion is correct, the ionization zone fixing the energy source maintaining the pulsation. The solution to this problem provides the initial value conditions for treating the piston problem—viz. provides amplitude and phase relation between pressure and radiation flux. Then, a study — as **Whitney** has summarized — of the compression waves moving outward into the atmosphere provides both the energy dissipation in the upper atmosphere and the observational material for checking the theory. Again, it is the radiative flux term that provides the unique astrophysical element, over the straight aerodynamic solution of a compression wave in an atmosphere of decreasing density. Then one must, of course, return to the solution in the interior, to make sure the energy supply from the ionization zone does indeed match the energy dissipation computed for the running wave, so it is an iterative process.

It is not clear to me which aspect of this solution **Liepmann** referred to, when he said he looked for great progress within the next four years; it seems to me it was the problem of the interior. But let me stress that we have a detailed observational test only for the part referring to the atmosphere. And, there, we must begin to look into the problem of the detailed interpretation of a line-profile in an atmosphere with sizeable systematic velocity fields, which couple with the thermodynamic state of the atmosphere, both directly and through the radiation field. So, we require two kinds of work — on the aerodynamical problem and on the analytical problem of interpreting spectral line-profiles — they are, of course, coupled. It will be interesting to see which is the closer, Liepmann's estimate of four years on part of the aerodynamic problem, or Pecker's of 1974 on the interpretive problem on line-profiles.

Finally, problem *C*), that of a compression wave in an atmosphere of decreasing density, grades directly into the second problem of *B*). Indeed, there are two problems—an individual wave propagating outward, and a statistical array of waves. The former, when it embraces the whole atmosphere, is just the cepheid problem discussed. The other is the ensemble of waves, a brief approach to which was outlined by **Whitney** and **Krook**, and which **Liepmann** thought offered a good hope of rapid extension of present ideas. I would like only to elaborate on one point raised by **Liepmann**. He emphasized that it would be very nice to treat this problem as a whole, to begin with the ionization zone, solve the convection problem, produce the random noise waves, follow them up into the atmosphere and investigate their energy dissipation, thus fix the temperature distribution in the atmosphere. I would like to emphasize that one is led into another class of stability problems in such thinking. For, it is not sufficient to consider only the local mechanical dissipation of energy — one must ask how the energy gets away from the point of dissipation. While in the laboratory, this is largely by conduction; astrophysically, it is wholly by optical radiation except near very steep temperature 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.