Part V.

Summaries.

Chairman: F. H. Clauser

— H. Liepmann:

I have been asked to give a short survey summing up the general impressions of the meeting by an aerodynamicist—or better, by a non-astrophysicist, Since my remarks have to be made with little time for preparation and on the basis of limited notes from the meeting, my presentation is quite subjective and I have to apologize for my omission of some important points.

First of all in comparing the present Symposium with the previous ones in 1949, 1953 and 1957 I am impressed with the very much improved balance in the topics and interpretation at the present Symposium. The three previous meetings were each concerned almost entirely with a single group of fluid mechanics phenomena: Turbulence, shock-waves and magnetohydrodynamics in this order. In the present Symposium all three of these seem to have found their place and the preoccupation with a single one of them has passed. In former Symposia I felt often completely frustrated by my inability to distinguish between an observational result and a highly opinionated interpretation. Frequently an astrophysical problem which sounded quite interesting for a fluid mechanicist was presented, only to be immediately torn to pieces in a discussion or in a subsequent talk. In the present symposium I have not felt this way at all and I do believe that the preparation of survey talks helped very much toward the presentation of a more balanced and stable astrophysical picture. I hope that the astrophysicists feel similarly about the survey given by non-astrophysicists.

A few quite fascinating problems for fluid mechanics have been brought out in this conference. First of all the problem of cepheids was excellently introduced in Ledoux's lecture and it looks to me that the problem of pulsating stars is a rather well-defined and challenging problem. Observed is quite a stable and very exactly periodic oscillation of a gaseous system. Off hand there are two possible models which show such a behavior.

One may think of a linear oscillator with slight positive damping excited by random forces. A typical case of this type is the Brownian motion of a torsional balance which exhibits an oscillation in its natural frequency with

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a constant mean amplitude with more or less pronounced beats. The second model is a non-linear oscillator for which positive and negative damping cancels at a certain amplitude and which thus oscillates with constant amplitude in a limit cycle.

This latter model seems by far the more likely in view of the observations which show apparently a very constant amplitude and no beats whatsoever. This problem look to me very promising indeed and I feel that much progress can be made in the next few years. I understand that CARRIER is already actively working on such a model. I am sorry indeed that SEDOV was unable to join us here since he has been working with models of pulsating stars for some time with more emphasis on the actual motion in the stars. The oscillator model should serve to clear up the mode of energy transfer to the oscillation and it should then be possible to connect the two approaches to give a reasonably complete theory of cepheids.

The second well-defined fluid mechanical problem discussed at this Symposium was brought up in DEUTSCH's paper in connection with the efflux of matter from stars, in particular the solar wind. Reduced to the bare essentials we deal here with the spherical symmetrical sourcelike flow of a compressible fluid in a central gravitational field. The streamlines of source flow are everywhere divergent. The gravitational field has an effect upon the motion which is equivalent to the appearance of a throat in the streamline pattern of a force-free fluid. Hence the motion can be reduced to the well-known problem of flow in a convergent-divergent channel. CLAUSER and GERMAIN have discussed this problem at length and GERMAIN has presented it in a very elegant and simple form with which one can easily discuss all possible flows of this general type.

The model used is obviously over-simplified: Neglected are rotation, magnetic field effects, and the fluid is considered non-viscous. Thus shock-waves appear as sharp discontinuities; and this is a suitable idealization only if the shock thickness is small compared to its radius of curvature. In any case the model can serve as the basis for more refined calculations and for an overall representation of the simplest possible steady flow from a star.

The fluid mechanics of the convectional heat transfer—the convection zone—I find still a fascinating subject. I am very sorry for having worried so many people with my derogatory remarks about mixing-length theory, being unable at the time to supply any substitute. What I mean was mainly to say that a theory which was not really well-founded should be treated with caution. I also cautioned about the application of the Kolmogoroff turbulence theory to gases with high random velocities. Now, on the convection problems; first of all I feel that there is still quite a bit of possibility of experimentation. From the viewpoint of purely fluid mechanics, I do not consider the problem of convection and turbulent convection to be solved. In the work of MALKUS,

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there are certainly some good and possibly even deep ideas; but I am not prepared at this time to analyse it completely, simply because I am still not able to understand it fully. But here we have a problem that is really difficult, and I believe that one cannot estimate the years necessary for its complete solution. In any case it could stand quite a bit of experimental and theoretical work.

I would like to add one point which I don't believe was stressed sufficiently here: In general, a sharp division was made between laminar instability and convection cells and turbulent flows. There are other cases of turbulent motion which are similar to convection zones, notably the flow between rotating cylinders. Here the centrifugal force takes the place of the buoyancy, and the instability here appears in the form of the Taylor vortices which are the equivalent of the Benard cells in convection. In flow between rotating cylinders it is known from the experiments of MacPhail, Pai and Coles that it is also possible to have a *turbulent* basic flow and superimposed on that, cellular structure. So, that an instability motion with a definite pattern is superimposed on a fluid which is already turbulent, and behaves like a fluid with different transport parameters. A similar motion has been observed very recently in the vortex shedding in the wake of a cylinder. Here Roshko found vortex shedding on top of a turbulent wake. I feel certain that something like that is possible with convection cells. Hence there is a good possibility that the granular structure as observed by Schwarzschild, and by Rösch at the Pic du Midi, are not quite regular, not quite as you think Benard cells should look, but not as irregular as the intermittent boundary between a turbulent layer and the outer flow discussed by Clauser. The Benard cells are superimposed on a turbulent medium quite in line with Goldstein's remarks, that the observations show a superimposed pulsating motion. So, it is not simply the problem of distinguishing between laminar instability or turbulent motion and turbulent instability. A particular laminar flow can be unstable to very definite types of structure—to waves, to vortices, to cells. These superimposed structures can grow, and may then exist in a stable form in certain cases. Eventually the flow may become turbulent, and the turbulent flow may also exhibit a large-scale structure, which we sometimes used to call the superstructure. The flow made up of cells superimposed on a turbulent motion will have a continuous power spectrum as well but with a clearly different low frequency component. The scale of the superstructure differs from the scale of the turbulence in much the same way as the scales of the laminar instability modes differ from the mean-free-path. This whole field of, say, « turbulent instability » can use much work.

Connected with this problem there is an equally interesting and to a certain extent not quite as difficult a problem, the excitation of the sound and shock-wave field which is supposed to heat the corona. Actually, of course, one should solve the whole problem of heat transfer in the sun in one step: The actual velocity field can be represented by a superposition of a sound field, a convective cell field and a turbulent field, possibly a field of hydromagnetic waves has to be added as well, and this *one* model should lead to the temperature distribution including the corona. Such an approach is probably too difficult, so one makes a « step » at the convection zone and only from there on discusses the sound field. I would like here to emphasize a remark made earlier by CLAUSER, about the difference between sound waves and turbulence, in the terminology which we are used to. You can do this in several ways: Mathematically a velocity field can be made up of a field which has zero divergence and finite curl, and one which has zero curl and finite divergence. The former you call turbulence; and the latter, sound. This is not quite complete, because you can also have temperature spottiness and hence an entropy field. Turbulence exists, as you all know, in an incompressible fluid. The equations of an incompressible fluid are elliptic, or, in certain cases, parabolic, which means that if you disturb it at one point, you produce not a wave but a diffusion pattern. Sound is governed by hyperbolic equations, and if you disturb one point, waves propagate outward. Consequently, a random sound field is really a stochastic field of waves in the strict sense of the word, but in a turbulent field, we deal with the stochastic field of diffusing elements. I am not sufficiently familiar with the observations on the sun. Observations of a random sound field and a random turbulent field in the laboratory differ in a characteristic way which can be observed in photography of a fast-moving shell. Here one sees the boundary layer and also a sound field (cf. Fig. 1 in Part IV-A: discussion). There is a characteristic difference in appearance. In the boundary layer, the structure looks more spotlike and round and the sound field looks more like entangled spaghetti. And this is, of course, typical of the two cases: hyperbolic equations tend to give you fronts—mixed up fronts of sound waves; while parabolic or elliptic regions give you more or less circular structures around the point of diffusion. The point of disturbance will diffuse out. It is not always as clear as that. In these pictures of the sound radiation from the boundary layer of a fast-moving bullet, it is obvious. You can distinguish the two fields exactly. And it is clear then that diffusion fronts will exist even if the motion is incompressible: *i.e.*, in the limit of the incompressible approximation, the sound field will vanish but the diffusion field will remain. I think I will take a chance here of boring the audience with a model I have used very often, to illustrate the coupling between turbulence and sound. I had a great deal of conceptional trouble at the time I got interested in the problem and I found one model which helped me: First of all, note that the pressure in an incompressible fluid is nothing but a constraint; the pressure p is introduced to keep the condition $\nabla \cdot V = 0$ satisfied, p enters as a Lagrangian multiplier in deriving the

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equation of motion to take care of the auxiliary condition of zero compressibility, exactly like any constraint in mechanics. In terms of the motion of an ordinary pendulum—and this is the analogy I am going to draw—the force exerted by an inextensible string, the constraint which keeps the pendulum on a spherical surface, is exactly equivalent to the pressure in incompressible motion. Consider the motion of this pendulum in the analogy as the incompressible turbulence. If one admits compressibility, one relaxes the condition that the string is inextensible. As the pendulum moves back and forth it sets up oscillations in the string; the coupling between the two is exactly like the coupling between turbulence and sound-waves. Namely, for small Mach numbers—for small energies in the turbulent motion—you can see immediately how you would compute this: compute first the pendulum motion, taking the string inextensible and the fluctuating force, acting on the string. The oscillations set up in the string by these forces are the analogue to the acoustic radiation. This is the exact equivalent of the Lighthill theory. He computes the pressure fluctuations neglecting compressibility effects and then applies these to a compressible medium. It is evident that such a procedure must break down if the energies in these two motions become of the same order. In this case the turbulence and the sound have to be treated as a strongly coupled system. This already complicates the pendulum problem; to discuss a three dimensional continumm on this basis is really quite unpleasant. In any case if the energies in the turbulent motion become large, the Lighthill theory must cease to be correct. *E.g.* the 8th power law ceases to be valid—indeed it must or at $M = 3$ no turbulence would be left anymore. In any fluid with high energy random motion, a mixture of these two modes—sound and turbulence—must be found; but in different ratios depending upon the energy of the motion. On the other hand, from observations of, say, the solar atmosphere it may be difficult to tell the difference. One will not be able, as far as I can see, simply from observing the Doppler distribution in a given direction to distinguish between what is sound and what is turbulence, from one observation at least. In the laboratory it can be done by checking on the phase relation between the fluctuation in velocity and the fluctuation in density, or between the fluctuation in density and fluctuation in temperature. Since sound is essentially an isentropic process, even when it becomes pretty strong, a very definite phase relation between pressure and density exists and therefore between pressure and velocity and therefore between density and velocity and so on. Hence this phase relation has to be used to discriminate the modes. Consequently if Doppler observations yield velocities which are of the order of, or more than, the velocity of sound, the existence of hypersonic or supersonic turbulence does not follow, but it may mean the existence of a random array of sound and shock-waves.

One other feature of the solar observation was not—I believe—adequately discussed: The very sharp demarkation zone between the umbra and the penumbra in a sunspot. If one looks at fluid mechanics in general, the chance of having a sharp transition zone exists in a few cases only; one is a shock-wave, and the other is a vortex sheet, which can be quite sharp. Now it is true, as PETSCHEK pointed out, that radiation may exaggerate the sharpness of such transitions. *I.e.* an exponential dependence on the temperature may make a smooth but steep transition look very sharp. But still, there is the problem that I think one should come up with a reasonable idea why does the umbra-penumbra boundary look so sharp. And from the pictures I have seen, it is really quite surprisingly sharp. In general, I have the feeling that while it may be too early to get a complete model of the sunspot, from the observations it may be possible to at least draw a kind of kinematic map which combines the magnetic field, the velocity field and the density field in such a way that one is left without any real strong contradictions in the magneto-hydrodynamic behavior of the fluid. And I think we are almost up to that point; I think the observations are such that within the next year or so one should really be able at least to draw something like a reasonably complete streamline and magnetic line diagram as a basis for more refined models.

Similar thoughts came to me during the discussion of flares: Here too I think that one may be able to take a set of observations and try to form an overall aerodynamic model not so detailed that one is capable of describing the mechanics of the phenomena, but at least to tie different features together in an overall way, free of obvious contradictions. I feel that here progress will be made; *e.g.*, in the symposium we discussed whether the photosphere below a flare is undisturbed. From the contributions here, mainly by PARKER, we came to the conclusion that we just aren't sure. No large disturbance is observed but it is possible that the disturbance dies out too fast, so that when the flare is past the surface is already quiet. But it may be possible to add here to the observational facts.

The whole problem of magnetohydrodynamics of rapidly changing and slowly changing phenomena on the sun is one I think fascinating for anyone working in fluid mechanics. Needed here from the astronomers is a short list of observational facts. We have gotten part of these already here; namely, a sort of map on which the observations, devoid as much as possible from any speculations, are put down and the implications for other phenomena are only indicated. CARRIER made a strong point concerning such information and suggested that the astronomers give him a sort of « zero-order-time-table » in which it is plainly stated what is observed, what is deduced, and what is the limiting error, and no speculation. This time table is to serve as a sort of reference for the aerodynamicist to start thinking, and then go to an astrophysicist to argue.

Now lastly I would like to add a few words on the problem of collision-free

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shocks. We can leave aside for the moment the question whether such shocks are really needed to describe the observations. In any case there is no doubt that we have to be able to understand them. We have to be able to understand whether collision-free shocks exist; and if so, what is their general structure? Now it looks to me that this is a problem that should be solved within the next two years or so. The models, as you have seen, are still somewhat contradictory. None of the models have yet led to a solution in the sense that you can present it with you hands tied behind your back. And they still involve a lot of argumentation and loose ends. Especially since terrestrial experiment here are very difficult. There is really only one set of experiments—Patrick's experiments—and I have the feeling that the theories and the experiments do not agree, but they lean on each other, and that if you pull one out, the other falls. This is not exactly the way a theory and experiment eventually should behave. But there is no doubt that this problem leads into some really fascinating fundamental questions about the general nature of the plasma equations. Also one is forced to consider boundary conditions, which plasma physicists from my experience do very reluctantly. The general equations are usually discussed in an infinite domain. In the actual case one is forced to deal with at least the dimensions of the system, and one is probably going to be forced to set up the shock-producing mechanism in a little more detail; one has to keep in mind that in dealing with ordinary shock-waves one is lucky indeed that one can treat them completely locally, remote from their origin and any end conditions. This is only true because the equations of motion in aerodynamics have the nice feature of having these narrow zones like boundary-layers and shock-waves which couple one equilibrium state to another equilibrium state. This is by no means true for a shock-wave, or what should become a shock-wave, in a very viscous gas: a piston moving into a very viscous gas takes some time to produce a shock-wave, and consequently if something else interfers no shock may be formed at all. Whether a collision-free shock exists is, of course, intimately tied up with the possibility of defining an entropy function of state. In passing through a shock from one equilibrium state to another one cannot satisfy conservation of momentum, energy, and mass without increasing the entropy; hence one must be able to define an entropy in a complete shock theory. I feel that this ultimately should not be a « sort » of entropy, but it should be an entropy that can be connected up with the entropy of thermodynamics. If this is not done, I am sure someone will be able to produce a cyclic process with greater than Carnot efficiency. These are questions which I am quite sure can be settled, and the necessary experiments can be done within a limited amount of time. They are not very large-scale plasma experiments—one is not trying for fusion. But we will surely find many very interesting phenomena regardless of whether the collision-free shock turns out to be a useful element in astrophysical discussion 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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