PART IV.

Considerations on Localized Velocity Fields in Stellar Atmospheres: Prototype — The Solar Atmosphere.

B. - Consideration of Convective Instability form the Viewpoint of Physics.

Discussion.

Chairman: G. K. BATCHELOR

— R. N. THOMAS:

Do I understand correctly that your n_0 should give a lower limit to the observed size of granulation, if your considerations are applicable to that problem? If I am correct, would you say how to compute this quantity?

— W. V. R. MALKUS:

You are referring to two different parts of this study. The first development refers to the question of the asymptotic consequences of the spatial structure as n_0 approaches infinity. So as far as spatial structure is concerned, we do not define or compute n_0. The second part—done extremely briefly, and only verbally—was to identify n_0 with the smallest scale of motion that is marginally unstable on the mean field. That's a stability problem. You have to know the mean field or deduce the optimum mean field to determine n_0: Such stability problems have been solved. In principle they can be solved for the solar atmosphere. BÖHM has solved one recently in a particular form for the polytropic atmosphere. I think others can be done. They will establish a smallest scale, and they will then also tell us something about the energy transfer we can expect due to motion. Note that you will not see the smallest scale. It will be a lower limit. The final spectrum has almost all of its energy in the big motions; practically none in the small motions.

— R. N. THOMAS:

Is it correct to say that n_0 is still the smallest thing that I should see and if I see something smaller than that, I should be unhappy?

https://doi.org/10.1017/S0074180900104607 Published online by Cambridge University Press

— W. V. R. MALKUS:

I think so—unless what you see is isotropic. This n_0 is the smallest motion responsible for a transfer of heat.

— E. SPIEGEL:

I would like to reply to Thomas' question. This theory says that n_0 is the smallest scale that transfers heat. We can, in principle, still see fluctuations (either granules or velocity) at scales much smaller. The n_0 is only the smallest scale for which there is a velocity-temperature correlation; there is no reason so far why there should not be velocity and temperature fluctuations at smaller scales.

— W. V. R. MALKUS:

In the laboratory these smaller scales are not observed. I do not know the full story here. I should guess it tells us something about the strength of non-linear transport down the spectrum. But if we take this n_0 and compare it with the laboratory flows—LAUFER has looked into the shear flows, TOWNSEND has looked into the convection flow—there are no observable motions above the background noise smaller than this n_0; but there may be smaller scales in more complicated turbulent processes.

In the central regions of the shear flow, LAUFER draws a picture looking like this for the energy spectrum and then he draws $\overline{UV}$ correlations near but not at the center of the flow and it looks like this:

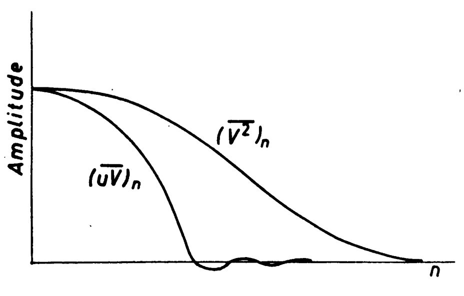

Fig. 1.

However, this does not establish that the energy spectrum for the whole flow goes to higher wave numbers than the transport spectrum in such a flow as this; the stress of necessity vanishes in the mid-regions. Due to external constraints there can be no torque on the fluid as a whole. What we must compare is the smallest scale of motion in the mean flow, which exists near

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the boundary of the region—in the so-called « laminar » sub-layer—and determines n_0. Then we ask if that is smaller or larger than any other spectral component in the flow. LAUFER has some lovely graphs of those data—the boundary is very sharp as you know. Its dimensions are roughly the smallest scale of motion responsible for momentum transport. LAUFER studies this region right to the middle of the boundary layer. He measures the horizontal Fourier components of the motion. He discovers no motion which is smaller in dimensions than this spacing Z_0/n_0.

There is no motion in the flow whose scale is smaller than the dimensions of the boundary of the region and that is the essence of this theory. The smallest motion in this flow transports momentum.

— G. K. BATCHELOR:

One question which bothers me, and possibly the astrophysicists, is that the rigid boundary plays a dominant role in Malkus' whole theory. The effects of conduction and viscosity that he deduces by his arguments are due to the presence of the rigid boundary. One cannot help wondering what would happen if there was no rigid boundary present. In an astrophysical situation, such as a stellar atmosphere, one might idealize the problem by introducing artificially a rigid boundary as a lower boundary; or, one might, I suppose, choose to think about an atmosphere that extends over many scale heights and try to do without any rigid lower boundary. I do not know if anybody has actually worked out the details of such an approach—a conversation which I had with SPIEGEL suggests that it is being looked at. In that case I do not quite see how the smallest scale of motion—that represented by n_0 in Malkus' argument—could come in and I do not see the connection between the physical processes in those two cases of convection with and without a lower rigid boundary.

— L. BIERMANN:

A point of interest underlying Malkus' discussion is how general are the assumptions underlying the formulae used in astrophysics, as discussed in the last section. I looked into these questions in 1935, and reached two general conclusions. First, the scale most effective in transporting heat is the largest one compatible with exterior conditions. Second, the velocity behaves in such a way that for fixed temperature difference, the heat flow is a maximum. This is of some interest, because in irreversible thermodynamics, the contrary is done. But it seems that in problems of this sort, the turbulence and possibly other mechanisms adjust themselves in such a way that, when you fix the boundary conditions, the production of entropy is maximized. So I would

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ask whether, if one applied such considerations as that presented by MALKUS, you would not end up with essentially the astrophysical formulae—to within a correction of a factor 2 or π, etc.—or whether you get something widely different.

— E. SPIEGEL:

There are two problems which have come up on which I would like to comment. The first is that of turbulent convection in a layer whose thickness is much less than the scale height of density. This is the problem discussed by MALKUS. The second problem is that of applying theories of turbulent convection to astrophysical situations. In the discussion yesterday I tried to allude to one of the difficulties of this second problem, that we do not have walls bounding astrophysical convection. However, we do have what might be called soft walls; that is, we have stable layers bounding the unstable ones, and this seems to give some boundary-like behavior to the motions. I cannot discuss these problems in detail in a short time, so I will summarize the Princeton work in general qualitative terms.

Let me begin by discussing our work on turbulent convection in a layer whose thickness is much less than one scale height. In the absence of motion the temperature profile is linear, but the advent of convective motion distorts the profile—cf. Fig. 2 in Malkus' talk. Convective heat transfer causes distortion of the profile so that the temperature gradient, hence heat conduction, is small in the body of the fluid and large near the boundaries.

Let us now imagine the velocity and temperatures fields to be expanded in some suitable set of orthogonal modes satisfying the boundary conditions. If we study the dynamics of one of these modes of the system, we find it convenient to speak of three processes acting. The first is the buoyancy force which results from the mean (ensemble average) temperature profile. The term in the equations describing this force may be regarded as linear once you specify the temperature profile. The second process is the non-linear interaction of the mode with all the others separately. And finally, there is the effect of the viscous forces.

We are interested in statistically steady convection, and therefore look for a statistical balance of these three forces. The main difference with Malkus' theory lies here. He does not treat the non-linear interaction explicitly, but instead has an ingenious way of seeking the net result of the non-linearity by applying integral constants. But, in the work by myself, LEDOUX and SCHWARZSCHILD the non-linear interactions are explicitly considered and are made to balance against the quasi-linear buoyancy term and the viscous term. The procedure outlined is naturally carried out by iteration. We make a guess

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at the mean temperature profile, and compute all linear inputs for all modes. Then we approximate the non-linear interactions and solve the balance equation for the relative amplitudes of the modes. It is then possible to compute a correction to the profile and to repeat the process, though to date we have completed only the first iteration.

The immediate question then is, how do you represent the non-linear interaction? Our feeling has been that if there is a tendency for the fluid to reach a preferred steady state—perhaps in the sense MALKUS has mentioned—the particular form of the non-linear term should not be important so long as the essential physics is contained. We have tried to use the best possible form for the non-linear term available from turbulence theory which may be reasonably tractable. In my opinion, the best representation is contained in the recent work of KRAICHNAN; but at the moment this is a more difficult representation than we are prepared to cope with. We have, therefore, in the current formulation of the work used the ideas suggested in Heisenberg's heuristic theory of turbulence.

One new point that comes up is that the interaction terms have not been approximated before in the case of anisotropic turbulence. To handle this difficulty we have made the specific assumption that the anisotropy of the motion is that of the most unstable mode for any scale. The most unstable mode is the one which derives energy most effectively from the buoyancy forces.

This in a rough way summarizes the physics which we have put into the problem. We have actually made the application only for small values of $\sigma R/8\pi^4$ where $\sigma = \nu/\varkappa$, $R = g\alpha\beta d^4/\varkappa\nu$ and the definitions of symbols are those used by MALKUS. Let me remind you that R, the Rayleigh number, measures the ratio of buoyancy force to viscous force and that σ, the Prandtl number, is about 10^{-5} in the photosphere since $\varkappa$ is determined by radiative processes. This approximation simplifies one of the main difficulties in the convective processes—a difficulty which we are now trying to deal with and which I would like to discuss briefly at this point.

In studying the convective motions we have expanded the velocity and temperature fields in terms of a complete set of orthogonal functions. In the approximations we have considered, these functions are eigenfunctions of the linearized equations and each pair of such functions for temperature and velocity we call a mode of the system. For every wave number there are two possible eigenmodes, one with temperature and vertical velocity in phase and one with them out of phase. Only the in-phase motions are unstable in the sense that they derive energy from the buoyancy and viscosity and owe their existence, if any, to non-linear interactions. In the general turbulent situation, when we make a representation of the velocity and temperature fields, we must include both kinds of mode. In general then, the correlation between vertical velocity and temperature is not unity, but some smaller value which depends on the relative amplitudes of the two kinds of modes.

We have then a mechanism by which the turbulence can lose its energy, besides that of the cascade processes contained in the ideas of KOLMOGOROF and HEISENBERG. Motion can be induced in a large scale by the buoyancy force; this motion will have temperature and vertical velocity in phase. But the non-linear term can then act to convert this motion in part to one with an arbitrary phase—we would consider this a mixture of in-phase and out-of-phase modes. That part of the motion in out-of-phase modes is then damped by buoyancy force and lost back into gravitational potential energy. Whether this mechanism of randomizing the phases is more important dynamically than the usual cascade process for setting the form of the power spectra depends on the parameters of the system, So far we have considered the low Prandtl number case where randomizing of phases is not important, but we plan to go on to consider more general situations.

This must suffice as summary of the physics of our approach, since time does not permit a discussion of the details. In a qualitative way the results agree with those discussed by MALKUS, but there is one significant difference. We do not get a cut-off in the heat transport spectrum.

In MALKUS' paper there is an explicit assumption that the smallest scale of motion is the marginally-stable motion on the mean field. Our results on this give the classical power spectrum of turbulence for the velocities as a function of k. There is k_0, which is the largest permitted scale; since you have a finite system, you cannot have wave numbers going down to zero for the motions satisfying the boundary conditions. k_0 is the smallest wave number that can occur. And so we get a spectrum which starts as k^{-7}, but very quickly makes a transition to the $k^{-\frac{5}{3}}$ law; ultimately, in the dissipation region, it goes as the familiar k^{-7} law. The spectrum for temperature fluctuations, in this low Prandtl number case, drops off initially like k^{-11}. At $k = 2k_0$ the value of the temperature spectrum is quite low but the velocity spectrum has appreciable amplitude. This result seems relevant to the solar photosphere where it seems that the granulation, which measures temperature fluctuation, does not go down to the small scale on which we expect to find velocity fluctuations.

Let me turn now to the problem of convection in stars. Mrs. BÖHM's talk showed that we need to add something to our present models of stellar convection zones; in her theory it would be a value for the mixing length. The possibilities available at present seem to be to try to apply either Malkus' approach or the one I have just discussed. In either case we have to determine the appropriate complete set of functions to use—in the Princeton scheme these would be the normal modes of the linearized equations of motion. We also need to know the growth rates for each mode; that is, if the time-dependence is like $\exp[nt]$, we want to find $n(k)$ for all k. From the classical case, *i.e.* that studied by RAYLEIGH, we have the following simple picture:

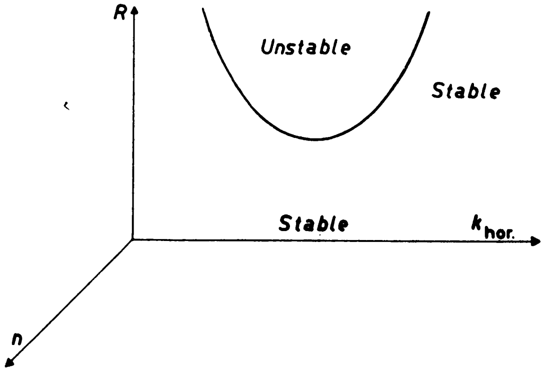

Fig. 2.

In the R-k plane we have shown the critical curve for instability of a fundamental mode. The growth rate, n, for unstable modes is given by a surface which is above the plane inside the critical curve and below outside. The suggestion then is to find this surface for atmospheres varying in density from top to bottom and bounded by stable layers of gas. Much work by many astrophysicists has gone into this determination, but it is far from complete. Time does not permit the discussion of that work. Let me close by saying that it is our hope to couple such work with the non-linear procedure I have outlined above.

— G. K. BATCHELOR:

Sometimes it helps to have a greatly simplified view of a problem. I want to say a few words about what I think is the essence of the calculation of SPIEGEL'S. My remarks concern the situation which I understand is relevant for stellar atmospheres—namely, the situation in which the Prandtl number is very small because the conductivity includes the effect of radiation and is very large—very much larger than the kinematic viscosity. We wish to obtain information about the mean-square temperature fluctuations and mean-square velocity fluctuations, for given values of temperature gradient or temperature drop or something of the kind. For those simple averaged quantities one can give a physical argument which I think is essentially right despite the very simplified character. The level surfaces of mean temperature are horizontal planes, and temperature fluctuations are produced by convection carrying these surfaces upwards or downwards against the action of conduction (which includes radiation), which would tend to keep them horizontal. Since the

Prandtl number is small compared with one, and the Reynolds number is large compared with one, the motions are uninhibited and take place freely and actively, whereas the temperature fluctuations tend to be suppressed. So on the whole, there is not much departure from the situation in which the level surfaces of temperature are horizontal planes despite the fact that there are big up and down motions. That general picture enables one to extract from the equation of motion and the equation for the temperature field estimates of the general magnitude of the velocity fluctuations and the temperature fluctuations. First of all, for the equation of motion, one would have as representing the order of magnitude of the acceleration V^2/L, where L is the scale of the motion of the energy-containing eddies. This will be balanced by—or supplied by if you like—buoyancy forces. Viscous forces we may neglect in view of the small value of the viscosity. Buoyancy forces will have as their general order of magnitude $g(\alpha\,\delta T/T)$. Then there will also be an equation for the temperature, T, which is just the heat conduction equation with allowance for the effect of convection. The total derivative of the temperature can be represented by $V\beta$, where β is the vertical gradient of mean temperature, fluctuations in temperature gradient being negligible, since temperature fluctuations are inhibited by conductivity,

$$\frac{V^2}{L} \sim \frac{g\alpha\,\delta T}{T},$$

$$\beta V \sim \frac{K\,\delta T}{L^2}.$$

Here one has a pair of equations from which to determine V and δT in terms of L and β:

$$V \sim \frac{\alpha g \beta}{KT} L^3, \qquad \delta T \sim \frac{\alpha g \beta^2}{K^2 T} L^5.$$

This I think gives the general order of magnitude of these quantities δT and V representing the root-mean-square temperature fluctuation once one knows, by observation or any other means, the scale of the whole motion and the temperature gradient against which the motion takes place.

— E. Böhm-Vitense:

This is really a remark answering the question of Biermann whether, according to the investigation of Malkus, we would expect the numerical results to be changed by more than a factor of 2 or 3. Now the theory on which our calculations were based was criticized very hard yesterday. It seems the main criticism was against the philosophy on which the theory was based, rather than against the numerical values that have been obtained—at least this was the impression I got from various discussions. Now, I am not interested in the philosophy—what we need are numbers and so I shall just talk about the numbers. If we can expect that this principal of maximum energy flux, which was derived by MALKUS, applies also to stellar atmospheres—I think then we shall derive numerically results that are in good agreement with the ones we obtain; because, we just chose our scale length of the motions in the way that we got a maximum energy flux. If we correlate the scale length to the wavelengths with maximum instability, according to the calculations of BÖHM and RICHTER, we find, within the limits of errors, of course, the same length that was introduced into the calculations to obtain the numerical results as I pointed out yesterday; wavelengths of maximum instability can be estimated to be larger than 300 km and the length used was of the order of twice this length. So, I think the numerical results can still be expected to be correct within a factor 2 or 3.

— E. SPIEGEL:

A factor 2 or 3 in what—the temperature or the energy transport?

— E. BÖHM-VITENSE:

I was thinking about the convective energy flux in the critical layers, that means in the upper layers of the convection zone.

— A. J. DEUTSCH:

Do I understand correctly that these considerations all refer to the regions not observed, in terms of velocity fluctuations? The part of the atmosphere where the absorption lines are formed is the radiatively stable part, but there were suggestions in Spiegel's comments that the tail of the curve may indeed be responsible for what we have called micro-turbulence. Is there a reason to expect that perhaps it is just these small eddies that will be observed in the spectral lines?

— E. BÖHM-VITENSE:

I would say that not just the tail of the spectrum would overshoot into the stable region. I would think that also the main wave numbers could easily overshoot. If there is anything true to the picture that the material is moving with a certain velocity because of the buoyancy force, then this material will rise to the upper boundary of the unstable layer with a certain surplus temperature—so there is no way to stop them; they will just go on up.

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— E. Spiegel:

What I was really only trying to say was, that there is a micro-turbulence in the velocity field, which we think provides the energy for the acoustic noise generation.

— A. Underhill:

Is it then a reasonable generalization of what has been said to state that your calculations indicate that irregular velocity fields are carried upward across the border of the convective zone, but that very few temperature inhomogeneities are carried across by these turbulent motions?

— E. Spiegel:

Let me be more specific about these non-linear terms. In the dynamic equation, they are the $U \cdot \Delta U$ terms, and their size relative to the viscous terms is the Reynold's number. For a large Reynold's number, as in stellar atmospheres, we expect a large range in the scales of motion because of relatively small viscous damping. In the energy equation, the non-linear terms are the $U \cdot \Delta T$ terms, and their size relative to the heat-conduction terms is called the Peclet number. For small Peclet numbers—such as occur in stellar atmospheres because of the great efficiency of radiative conductivity—there is relatively large thermal damping so that the amplitude of small-scale temperature fluctuations is relatively small. Therefore we might expect fairly large amounts of kinetic energy in small scales, even though there would not be appreciably large fluctuations in temperature on the same scale. In particular, the heat transfer due to convective motions would be small, so that you could indeed have velocities without any effects whatsoever showing up in the thermal structure. Radiative equilibrium for model-atmosphere theory for hot stars could be quite good and still there would be fairly reasonable velocity fluctuations. I would like to suggest this in answer to the question raised by a number of people; I remember de Jager raised it—namely, where do the observed motions in the hot stars, in the early stars, arise? The motions could arise in convectively unstable zones in those stars in which the instability is due to the first or second stage of helium. They could be those motions in which velocity fluctuations are appreciable, but which would not show up in our model-atmosphere calculations. And I think it is the answer to Miss Underhill's question.

The question of Deutsch was how did the remarks I was making in regard to the application of these processes to the variable density atmosphere relate with what we might expect to see in microturbulence? In the scheme I pro-

https://doi.org/10.1017/S0074180900104607 Published online by Cambridge University Press

posed, we first look at the linear equations for the variable density atmosphere and calculate the eigenfunctions to use as our complete set in which to discuss the turbulence. This is convenient because of the simple form the equations take when you use the right set of modes. These modes have the general property that they are decaying exponentially in the stable layers, they get large amplitudes in the transitions, and then it turns out they drop off exponentially below. The drop-off distance is $1/k_h$, where k_h is, in fact, the horizontal wave number of the motion. The most relevant modes, the ones that would do the most convecting, are those that are the most unstable in the sense that I defined earlier. I need not redefine it, I think the term is suggestive enough. And the most unstable ones, one has at the moment from the calculations, appear to be those for which k_h is on the order of the scale height near the top. Therefore, it turns out that the most dominant modes of the spectrum are those modes that are also related to the scale height. But, as pointed out earlier by WHITNEY, the scale height is also of the order of the free mean path of a photon. Therefore, already the smaller scales of motion which will be generated, if they exist at all appreciably, will be by definition microturbulence since they are on a scale which is smaller than the mean free path of the photons. Only the largest modes could be seen as visibly excited. Moreover, it is a suspicion that the temperature scale would drop off steeply and that, therefore, one would see effects in the temperature field only from these largest modes but that there would be an appreciable tail of velocity spectrum which would contribute to the observed micro-turbulence through the line-profile. The point being, as Mrs. BÖHM stressed, the largest scales will be the most efficient in getting up to the stable region. They will have the strongest amplitude in this region. So that, therefore, these will be the ones that will be most related to the observed granules.

— A. UNSÖLD:

Let me give an astrophysicist's summary. First, note that we have dealt so far, explicitly, mostly with problems involving two boundaries. What I have learned this morning from the talks by MALKUS and especially SPIEGEL, is that, having a limited atmosphere heated from below, then we get a distribution of the energy over different wave numbers k. We begin with one smallest k, the scale being determined by the thickness of the atmosphere. Then, SPIEGEL said, we get a fairly steep slope extending over a fairly small range of wave numbers, less than a factor of 2. These modes are evidently driven directly by putting in thermal energy from below. The following modes with larger k are driven mechanically by the modes with greatest wavelengths, and follow essentially the well-known Kolmogoroff-Heisenberg theory of isotropic turbulence with a slope proportional to $k^{-\frac{5}{3}}$. Finally, when viscosity

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becomes predominant, one obtains, a steep slope proportional to k^{-7}. So what is new to us is essentially what happens at the small k; what follows at larger k's is essentially driven by the longest waves. And we have learned that for the heat transfer, only this range of small wave numbers is important, which is rather obvious. Trying to link up these considerations with what we have heard before in astrophysics—chiefly the lecture by Mrs. BÖHM-VITENSE—it seems to me first that the equations which BATCHELOR wrote down were not so different from the old-fashioned mixing-length theory of Prandtl. The latter essentially just uses an average over the group of « small k » which is responsible for the heat transfer. To these, « driven modes » one might attach more or less a Kolmogoroff-Heisenberg spectrum which is produced by the dynamical pressure only. Here, the big whirls are divided up into smaller whirls and so on—purely by means of the $(U \cdot \nabla)U$ terms in the hydrodynamical equations. It seems to me that the heat, which is finally produced through viscosity, is astrophysically in general not very important as long as we have velocities considerably below the velocity of sound, that is,—astrophysically speaking—as long as we are in the photosphere and not in the chromosphere.

The next point is: « What do we need more in astrophysics? » Summarizing a small colloquium which SPIEGEL, BIERMANN, and MALKUS and I had this afternoon, the chief point seems to me that we should find out how to pass from the problem with two rigid walls (mathematically simple because one can easily use the methods of Fourier analysis) to an atmosphere in which the scale-height as well as the degree of instability varies considerably with height. SPIEGEL correctly remarked that here the methods of Fourier analysis cannot be applied any more because one has no scale to begin with. Should it not be possible to find out as a function of scale-height and degree of instability, with a certain approximation, something like a variable characteristic length? Following that, as a function of depth, one might visualize on which scale the modes doing the heat exchange, and the KOLMOGOROFF tail attached to them (only of secondary importance in astrophysics) go on. *I.e.*, one should try to find an approximation where the fundamental wavelength or the fundamental k_0 becomes a function of depth. That would, of course, not be a Fourier analysis in the strict sense, but something like an approximation familiar in optics where one also takes the wavelength as a function of the variable refractive index and follows a wave along a curved ray using within small intervals a plane wave solution. And the aerodynamical problem would be now to find some similar methods of attack considering the fundamental wavelength as a function of depth. Some people have considered whether this wavelength might be connected with the most unstable wavelengths; but that doesn't seem to find general acceptance. And so I should like to ask the hydrodynamics people whether our problem might be approached as kind of adaptable k-wall problem?

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Then, finally we understand for the later type stars to a certain extent how the whole mechanics is driven thermodynamically and how the energy is transferred. For the hot supergiants, where we observe large motions, I think, we should not go too hastily over the problem. We have there very large motions; no doubt about the observations. But I think it is an open question, at least at present—what is the driving machine? SPIEGEL indicated his idea that it might perhaps be the helium convective zone. But recent calculations by Mrs. BÖHM about convection in hot stars indicate that in these hot stars the radiative energy transfer is so predominant that no turbulence element can keep any appreciable temperature difference. And so, the mechanism of an ionization convection zone doesn't work simply because all the « valves of the engine » are out of order. Another suggestion which BIERMANN reported and which has been worked out to a certain extent by KIPPENHAHN in Munich, is that the motions in the atmospheres of hot supergiants might be connected with their rotation. A rotating star must, in connection with the nuclear energy generation have meridional currents; and these in turn would lead to rather high velocities in the atmosphere.

— H. LIEPMANN:

I would like to point out here that if the mixing-length approach is used in ordinary compressible boundary layer theory one obtains 21 different theoretical results—all different. Also, I am always a little shaken with the astrophysical applications of theories of incompressible turbulence, *i.e.*, neglecting coupling with the sound waves and coupling with magnetic fields.

— R. N. THOMAS:

It seems to me this last is just the point. I thought you would comment on only the Kolmogoroff-Heisenberg interaction having been admitted, rather than also the compressibility dissipation, which UNSÖLD believes negligible.

— A. UNSÖLD:

No, I didn't talk about sound waves because they become important only if you approach the velocity of sound. Such motions are unimportant in the photosphere—and I propose to deal only with that—but are predominant in the higher chromosphere.

— R. N. THOMAS:

Forget the chromosphere-photosphere division and concentrate on the aerodynamics. My reference was to the coupling, through the non-linear terms

$U \cdot \nabla U$, of the « eddy-turbulence » and « random noise » in the sense of the discussion by MOYAL and by UBEROI a few years ago, to which CLAUSER has implicitly referred in his general remarks in the Part I discussion. It was always my impression that compressibility effects could not be neglected when the Mach number exceeded about $\frac{1}{4}$; note that it is $\frac{1}{4} \div \frac{1}{3}$ in these lower solar photospheric regions you are discussing. If you want to be more general, and associate the observed « microturbulence » in other stars with the tail of the curve discussed by SPIEGEL, then I recall from Underhill's data that velocities run up to Mach 0.5 ÷ 0.8, and in certain cases through Mach 1. So how can I confine attention only to the Kolmogoroff-Heisenberg kind of interaction, and neglect the compressibility?

— E. SPIEGEL:

I think the important thing is the coupling to the pressure field—that is a kind of acoustic term. And that I have proposed to try to get around by choosing the right eigenmodes for the system which will allow for all the complexities of a pressure field of a compressible motion. But as for the non-linear coupling, I think that one can neglect it. The compressible flow produces a distorted pressure field which is a standing pressure field—and that has its effects on how the amplitude of the velocity distributes itself. This is not the same as the acoustic term generated through the non-linear term essentially. If you take it in the wave number space—the amplitude of the velocity field is still kept to the wave number. So you don't have compressibility generated through the non-linear term even though you may at least in the work term try to allow for pressure effects.

— H. LIEPMANN:

I get also slightly worried that the existence of the Kolmogoroff spectrum is here taken for granted in spite of the scarcity of convincing experimental evidence. I feel perfectly fine as long as you say we have large-scale motions that have a tail of isotropic turbulence; but to go into too much of the details is likely to be wrong.

— G. K. BATCHELOR:

I thought that recent observations made by people in British Columbia provided extreme agreement with the $k^{-\frac{5}{3}}$ law for the energy spectrum.

— F. H. CLAUSER:

The other day we were told that in this region in which the convection takes place, it was very important to get the right answer, otherwise big errors could

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be made in stellar structure. I thought that as a result the heat transfer across that region was a sensitive thing; and now people say factors of 2 or 3 make no difference at all. I am lost.

— E. Böhm-Vitense:

We have done the calculation for $l = H$ and then we have done the same calculation for $l = 2H$, to see the effect. And now in the layer where we have reached the adiabatic gradient already, let us say for pressure $\log p_g = 8$, we find that the temperature varies from $\log T = 4.45$, in the one case, to 4.57 in the other case. So those are the size differences which occur.

— J.-C. Pecker:

But the differences that occur in the mass of the star, due to different values of the ratio l/H, when you carry the integration inward, are very large—the change in radius is also very clear.

— E. Böhm-Vitense:

This may well be—I just should draw your attention to one calculation that was done by Schwarzschild, who checked which characteristic length one should take in order to obtain the observed position of the sun in the H-R diagram. He found that if you do the kind of calculation we have done, you get the right answer for the characteristic length $l = \frac{3}{2}H$. This, of course, does not mean anything with respect to the method, but it does show that with these calculations you can at least get agreement with the observations.

— A. J. Deutsch:

At the Liège meeting last year, a paper was presented by Temesvary, who considered the effects upon red giant models of changes in the ratio l/H. The effect is sometimes enormous; at a given temperature, it can alter the luminosity of the star by a factor of 50. These results threw grave doubts on many of the conclusions that have been drawn from H-R diagrams about the abundances of metals in old stars. In some contexts, therefore, I think it must be very important to know the effect of these factors.

— A. Underhill:

The point is, that in some spectral types the convective zone has an effect only on the upper atmosphere; but in others it affects the structure of the star as a whole. Particularly with cool stars, the question of convection is critical

https://doi.org/10.1017/S0074180900104607 Published online by Cambridge University Press

for the internal structure of the whole star. The atmosphere convection has a different effect from the internal convection which has a major effect upon the size of the star. This is the point. As I remember it, the statement by Mrs. Böhm referred to the upper atmosphere convection. Its effects are not so great because we are really saying nothing about energy generation. We are not disturbing the rate of energy generation, we are merely modifying the manner in which energy can escape from the star.

— L. Biermann:

The paper at the Liège meeting mentioned by Deutsch was the one where our group had explored the sensitivity of the solutions that you get for the evolution curve—as a function of the parameter expressing the ratio between the mixing length and the scale-height. We found first that disregarding the convection altogether does not lead anywhere; second, when you use mixing-length theory, then one finds for this particular kind of problem—for the stars which have moved away from the main sequence by increasing their radius by a factor of about 10 or 20—that taking for the said parameter the value one, or two, respectively, makes a considerable difference in the solution. This difference is so large that indeed (as was pointed out) it is impossible to derive, by comparison with the observed color magnitude diagram for instance, reliable values for the chemical composition which also enters there quite sensitively.

— W. V. R. Malkus:

In the last session I mentioned an experiment in progress to study the effects of the penetration phenomena. The observations indicated very sharp limits on the convective motions, a limit which was well above the point of the maximum density. In other words, there was definite penetration into the stable layer. I believe we can analyse aspects of this problem, and I do not think they lie completely outside the scope of the analysis presented this morning. Perhaps we can get results that will be valid in those regions of the star which are inaccessible to us, experimentally.

I have two suspicions about this work we see outlined on the board. One notes that the whole spectral structure of the tail has been put in by assertion; and though the assertion may be sound in astrophysical settings, it is not possible to test it in the laboratory. To speculate further on the untestable hypotheses I think is unwarranted. Ledoux and Schwarzschild have the intuition that the hypothesis is sound, but their confidence is based on the assumption that the fluid can find no other way than this cascade mechanism to dissipate its energy. However, I think Spiegel mentioned, and is now exploring, the

https://doi.org/10.1017/S0074180900104607 Published online by Cambridge University Press

possibility that the energy available for release at a particular wave number can be either dissipated by cascading down the wave number spectrum, or it is possible for the motions and temperature fields to get out of phase at this wave number and prevent energy release. That is, U and T can be large and not exactly correlated. Hence, this large amount of dissipation is not required. All the observations that are made in the laboratory, and in the atmosphere where very large-scale convection processes occur, indicate that the correlation between velocity and temperature is quite small, even in those scales of motion which are the most responsible for the transfer of heat. This indicates phase blockage as we call it, inhibiting the release of energy from the mean field; and I offer this thought as caution in accepting the Heisenberg-like dissipation mechanism.

— C. DE JAEGER:

My first remark concerns temperature fluctuations and the inhomogeneous photospheric model. I think that all the work done in previous years on the inhomogeneous photosphere has to be revised and that nothing can be stated actually on the values of the temperature fluctuations as derived from line-profiles. We should remember that our opinions on the structure of the solar photosphere have considerably changed, especially those on the outer layers. Compared to previous results we know that the temperature in the outer part of the sun is not so low as BÖHM suggested in 1953; the temperature may be somewhere between 4 000 to 4 500, and we know that the temperature increases toward the chromosphere already near $\tau = 0.01$ or 0.001. If further Lab's measurements of the continuous solar radiation are correct, the temperature in the whole solar photosphere has to be somewhat increased. Turning now to the problem of micro- and macroturbulence which has been discussed several times in the course of the meeting, I would make a short remark. In the stellar photospheres, we are dealing with a velocity field. The energy of this velocity field has a certain distribution with wavelength and we do not know what the distribution function is—it may have one peak or various peaks; it may even be just one frequency that is active. Now astronomers have in effect developed two methods for studying this velocity field. These methods effectively consist in filtering out a certain part of the energy of the velocity field—one filter is called microturbulence and is effective only for wavelengths λ such that the product of the absorption coefficient $\varkappa$ (cm^{-1}) and λ is small compared with unity. The other filter is called macroturbulence, it refers to the region where $\varkappa\lambda \gg 1$. So we only get a part of the velocity spectrum, but we want to know all of it. If the situation is, as is suggested by the observations, that the main part of the energy of the field is fed into elements with characteristic lengths of the order of the scale height of the atmosphere, then we are in a bad situation. We know that the relation between the absorption coefficient $\varkappa$ and the scale height H is in nearly all cases such that $\varkappa H$ is of the order of unity, both for the continuous and the line spectrum—so that the main energy is neither in the region $\varkappa\lambda \ll 1$ nor $\varkappa\lambda \gg 1$. That means that especially when studying micro-turbulence we always get a rather small part of the true energy of the spectrum, and this may perhaps have some relation—I come to the next point that I wanted to discuss—to the anisotropy of the turbulence field. WADDELL and SUEMOTO have found that the turbulent velocity field might have a certain anisotropy; but it is not quite certain to me that there this anisotropy is really in the velocity. It might fairly well be that it is the scales that are not isotropic. So, when approaching the limb, the effective scale of our elements becomes larger and that might have for effect that we get a greater part of the turbulent spectrum through our « filter », than by looking straight inwards to the sun. We should bear in mind that in a free atmosphere with a density gradient anisotropic scales might well occur in the turbulent motion field. *E.g.*, it has been found in the high terrestrial atmosphere near the 100 km level that the vertical scale of the motions is of the order of the atmospheric scale height, 6 km, while the horizontal scale is of the order of 150 km. Of course, the terrestrial atmosphere is not directly comparable to a stellar one, but we should keep this case in mind.

— E. BÖHM-VITENSE:

I would like to ask DE JAGER—I do not quite understand your first point, because in the line-profile investigations, you always include small scale *and* large scale motions; so if you find a change in line-profiles, you could not explain it as due only to the transfer of the velocity field from macro- to micro-turbulence.

— C. DE JAGER:

Of course, the whole velocity spectrum contributes to the detailed line-profile, but in my feeling nobody has worked out a reliable way to determine the *detailed spectrum* from the line-profile (which is widened by so many different and badly known causes). The only direct ways to study the motion field are either by considering the position of the flat part of the curve of growth (« micro-turbulence », $\varkappa\lambda \ll 1$) or by considering the line widening which does not influence the equivalent width (« macro-turbulence », $\varkappa\lambda \gg 1$).