Part IV.

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

A. - Convection and Granulation.

Discussion.

Chairman: W. H. McCrea

— W. V. R. Malkus:

Consider the variations of the gradient that would be computed just from radiation theory. There would be some subadiabatic region, an adiabatic region, and then again a subadiabatic region, in the absence of heat transport by motion. This adiabatic region, of course, would be called a convective zone and there would be penetrations into the regions beyond—both above and below. Now, the astrophysicist, as I understand it, has in the past often assumed that wherever one computed instability using radiative transport alone, he could then recompute the atmospheric structure assuming that in this region convection carried all of the excess heat flux, and that it really stayed at an adiabatic gradient. The convective region has really gotten bigger when one has made this assumption. Now, clearly, that extreme is never quite realized. We've seen here how one reduces it a little bit by assuming that one must have a finite difference between the adiabatic gradient and that achieved through the convection process. But one might anticipate that the actual gradient would be rather closer to this extreme than it was to the initial picture; that is, that the convection both lengthens the region in which convection occurs and greatly reduces the superadiabatic gradient. Now, how much of a departure from adiabatic actually exists apparently is important to the astrophysicist, because he wants to compute temperatures in the interior of stars, and he has to do it by some theoretical computation that carries him below the gradients observed at the surface. Even this small difference, I'm told by Schwarschild, can make a difference in the interior temperature of the star. I am no authority on how important that difference is. In fact, at first glance, thermodynamicists might wonder why one couldn't get perfectly satisfactory stars just by integrating in and whenever you got to a superadiabatic region, calling it adiabatic, assuming there is convection there, till you get off the adiabatic region again, and radiation can carry the

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entire heat flux. But we're told this isn't true, and, in addition, one wants to know more about the dynamics of the motion in these regions. Perhaps one wants to know how much beyond these regions convection can penetrate due to inertial features. In fact, as penetration occurred into the stable region above, one might expect smaller scales of motion to disappear rapidly. This is important because it is all we see of the sun. We only see the region where convective elements are penetrating into the stable layer (and, at best, a little bit below that). It is, unfortunately, in just this penetration region here that we must look more carefully at the dynamics, and can't accept very simple explanations of a constant mixing length or a mixing length depending only on local scale height. The region in question extends roughly one scale height, and in that region the convection goes from highly correlated velocity and temperature fields which transfer lots of heat, to velocity and temperature fields that are just left over after the penetration and have no correlation having been turned back by the stabilizing layer.

Now, I don't pretend to be able to deal even roughly with the problem in this complicated situation but I wanted to describe briefly a much simpler situation in which one can explore penetrative convection. It is oversimplified but if one wants to explore the dynamics of penetration of a convective motion into a stable layer, one may get some insight through certain laboratory experiments. We can see how a system of this sort can have its convective region altered by the penetration process, we can perhaps test hypotheses regarding the nature of penetrative convection in such controlled laboratory experiments, and then with some confidence in these hypotheses, apply them to the sun. Rather than heating from below, the experiment I'd like to describe involves cooling from below. Take a layer of ordinary distilled water and put it on a block of ice, or have a lower surface which has a temperature of 0 °C, then an upper surface which has some temperature—assume the simplest case, 100°; boiling at the top and freezing at the botton. Now, in this case, the temperature gradient is roughly linear in the absence of motion. However, since the density reaches a maximum at 4°, there is a reversal of density and this whole lower layer is potentially instable. When the dimensions of that region are such that the Rayleigh number is comparable to 10^3, convective motions start in such a layer, cooled from below. What can it do? Well, if convection starts near the base, it will soon hit the stable region; there will be a certain penetration—alterations of the field. The convection carries heat, as it must release potential energy; then the gradients at the boundary must sharpen to carry the additional heat. If they sharpen in this lower boundary region they must sharpen throughout the entire stable region, and the 4° water will occupy a much larger portion of the flow. Then we have convection which has altered the dimensions of the region in which instability occurs, and increased the heat flux. One of the things one wants to see is how far the motions press beyond the point of maximum density. The only controllable parameter is the spacing between the two surfaces. One would like to explore, as much as possible, the dynamics of this type of convection which can alter its own boundary conditions.

Another facet of this experiment is that the stable region is a stratosphere of sorts and can have wave-like motions in it driven by the convection at its base. I can only cite two achievements in this study so far. One was the stability problem. If one deals with a density profile that is parabolic, one has a Rayleigh problem with a single non-constant coefficient. We can solve this problem. It leads to eigenfunctions which are large in the unstable region as you might expect, and drop off in an exponential way in the stable region. The other result concerns the first experiments with very crude temperature measuring equipment. We observed the changes in gradients anticipated above—and the level to which the convection penetrates was at 8 °C to $8\frac{1}{2}$ °C. This penetration is well beyond the point of maximum density.

I believe this type of problem offers some hope of understanding aspects of the aerodynamics of the penetration in that region where we may expect simple, heuristic theories like mixing-length arguments to cause us some difficulty.

— E. Böhm-Vitense:

I think that in astrophysics the question of the upper transition region is not quite as serious as was pointed out by Malkus. I do agree that the calculations with mixing-length theory are wrong at this point, for one reason: In our theory we always assume that the values at the point in question are mean values over a region extending from half the mixing-length below and from half the mixing-length above the point in question. If we then calculate the convective energy transport as being proportional to the difference between the actual temperature gradient and the adiabatic one, we will, of course, get convective energy transport zero, at the transition point to the stable layer, which is, of course, not true because we have moving matter through this point. But on the other hand, if we just calculate from the observation the amount of convective energy transport which we have in this region—or we can take our model and start calculating the amount of energy transport—it comes out to be just about 5% of the whole energy transport. And this modifies the temperature gradient only very little. Therefore I don't think that the calculated stratification of this transition region is much influenced by the assumptions we have made.

— L. Biermann:

What is the Reynold's number associated with these motions, these convective motions, in this experiment? Is it large compared with 10^3, or is it small? Or to put it otherwise, is the convection stationary or non-stationary?

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— W. V. R. MALKUS:

There seem to be two types of convection in the experimental situation. It may help to describe them relative to an experimental plot of the dependence of heat flow on Rayleigh number. I plot the log of the Rayleigh number as abcissa, and as the ordinate the log of the Rayleigh number times the Nusselt number, which for the astrophysicists would be the ratio of the effective coefficient of heat transport over the actual coefficient of heat transport- If there were no motion, the plot would be a straight line, which would cor. respond to pure conduction. Now, in the experimental situation, after reaching a certain critical value, one departs from the first linear curve and goes to another curve, which over the range in which one can plot it is very close to a straight line. Generally the data are such that you can lay a ruler right along it. This has a slope of $\frac{1}{4}$, corresponding to a heat flux law which is proportional to the mean gradient in the flow, the thermometric conductivity and to $\frac{1}{4}$ power of the ratio of Rayleigh number to some critical Rayleigh number. This is the region that has often been called unsteady cellular convection. There are many scales of motion, but it still has a quasi-cellular character, and it proceeds to a Rayleigh number of about 10^6, about 1000 times the critical Rayleigh number. At this point the curve, experimentally, has a very sharp break again. I will discuss some of the theories about these results tomorrow. It breaks to a curve whose slope is a $\frac{1}{3}$ power. This is a region which we have come to call fully turbulent convection. The motion is quite disordered. You can get 10^{10} Rayleigh numbers in a small bottle of acetone. Hence, I was shocked to hear that the Rayleigh number in the sun is only 10^{10}. In any event, between 10^6 and 10^{10}, and beyond to the best of my knowledge, one has what one would call fully turbulent convection. It is interesting to note, that when you have a $\frac{1}{3}$ power law, the heat flux becomes independent of the spacing of the bounding surfaces. The intermediate region acts as a short-circuit to the flux of heat, the concentrations of the gradient are all confined to the boundary region. Now, may I answer the question? This corresponds in the first instance to just cellular convection and we must then ask about the Rayleigh number of the evolved field. Now strangely enough, in the experiment, we cannot control the effective Rayleigh number because the dimensions of the unstable region are changing. We can control the heat flux, which is another possible experimental parameter, and let the fluid pick its own Rayleigh number. From the dimensions achieved in this first experiment, the depth of the layer was of the order of 10 cm when the total depth was about 20 cm. This yields a Rayleigh number of about 10^7. So the most evolved form of the convection we were looking at was in this region, but by changing the basic parameter, supposedly you can cover both these regions either with quasi-cellular or fully turbulent motion.

— K. H. Böhm:

It should be added that the Rayleigh number which has been given here, 10^{10}, refers to the thickness of the layer which corresponds only to the most unstable part of the convection zone, assuming a thickness of 500 km for this part. Compute the Rayleigh number for the whole convection zone, you get a number which is much larger. It has usually been assumed that it is correct to compute the Rayleigh number only for the very unstable part of the convection zone, because one believes that the coupling between this layer and deeper-lying layers of the convection zone is small.

— E. Spiegel:

In answering the question whether one should look for a mixing length, and continue to apply mixing-length ideas or seek a more elaborate theory, one has very little choice but to try to test the validity of these notions in connection with laboratory experiments on convection, since we cannot hope to do better on the sun observationally. For this reason I would like to mention the connection of the mixing-length ideas with convection theory, and the laboratory results. In the solar convection studies, the mixing length has been taken to be nearly the scale height. But if one looks at the expression for the scale height, one finds that it is roughly proportional to the distance from the surface of the atmosphere. In particular, for the polytropic model it is exactly proportional to the distance from the edge of the star. This is an amusing coincidence with the kind of mixing-length assumption made in the ordinary boundary layer theory, and one might surmise that, if an application of these ideas is made to the laboratory situation, then the natural choice would be to make the mixing length proportional to the distance from the boundary. It is possible then to write a single expression for the closed system relating the temperature gradient to the mixing length. Then one can put in the hypothesis that l be proportioned to z. One finds that, away from the immediate neighborhood of the boundary—what Townsend in his experiments has called the boundary sublayer—the dependence of T goes into a $z^{-\frac{5}{3}}$ power law. This is not the same answer as one derives from dimensional analysis. The dimensional analysis has been applied by Priestley, and he finds a $z^{-\frac{1}{3}}$ law, while the experiments by Townsend give a z^{-1} law. So there seems to be at least in this sublayer a difference in the dependence on z between the experimental and the mixing-length calculations. One might think that this would suggest trying another kind of mixing-length hypothesis, but I wouldn't know what to suggest at this point. So I think the question is then raised that perhaps near the boundary, in the transition zone discussed by Malkus, we cannot hope for a precise representation; although one feels very strongly that in the deeper regions the representation by the mixing length would be fairly adequate. The only question in my mind then would be the difference in opinion between Mrs. BÖHM-VITENSE and MALKUS on the importance of the transition region. I believe myself that the thickness of the transition layer is of importance for the following reason.

When you get into the deeper regions you are essentially in an adiabatic gradient. This is the one you integrate in to the center of the star. Any small error in the gradient could show up as a large error in the temperature derived at the center of the star. However, the adiabatic gradient you get to depends on the thickness of the transition layer. So in that sense I would have thought that the transition layer, at least in thickness, was important. If this is the case, then it is of some importance what the dependence in the sublayer is. It is also clear that the thickness of the layer, in any mixing-length theory, will always be of the order of a few mixing-lengths. Therefore it could never be thinner than a mixing length. I cannot imagine how you could get a structure smaller than a mixing length. So, in that case, in using a mixing length theory, you are essentially putting a lower limit to the thickness of the transition layer by the very nature of the approach used. These are the few ideas I have about trying to test the layer, and I hope Mrs. BÖHM-VITENSE will have a correction for it.

— E. BÖHM-VITENSE:

It seems to me that the main disagreement is in what we call the transition layer. I didn't regard this whole very unstable region as a transition layer. If I talk about a transition layer, I just mean the very upper part of it, only those layers where I get disagreement between the mean value of any physical parameter (taken over one scale height) and the local value at the point which I am just regarding. If you take a point about ½ scale height below the boundary layer, then the difference between this mean value and the value which you obtain at the point in question is not very large. To check this, for instance, you can calculate the ΔT's by following the upward moving gas starting ½ scale height below the point considered, up to the point, and then calculate the ΔT which you obtain by following the downward moving gas starting ½ scale height above the point, and then take the mean of these two ΔT's. This you can compare with the ΔT obtained from the relations used in our theory. In the region somewhat below the boundary, you will find agreement within 20 or 30%. But in the very high layers you will find disagreement, and this is the layer in which I think our theory is certainly wrong. This region, I called the transition layer. An error in this region really does not affect very much the adiabatic which the temperature and pressure follows in the very deep regions. An error in the temperature values for the very unstable region, of course, would.

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— W. H. McCrea:

Would you tell us what this means in terms of optical depths?

— E. Böhm-Vitense:

Optical depth is not a good scale in the convection zone. For the optical depth you would reach values of several hundreds already, when the pressure has only increased by about 50% from the boundary of the convective layer. One should introduce the geometrical depth. I would guess that the region to which I referred as the transition region is about 100 or 150 km thick, but that is just a guess. That is, below $\tau = 0.8$, which is the upper boundary of the unstable layer.

— H. Liepmann:

I'm afraid I have to make a quite negative statement. I think nobody in aerodynamics believes in mixing-length theory anymore, and hasn't for at least the last ten years, I do not know enough about convection zones, and I like to leave these to somebody more qualified. In aerodynamic shear turbulence the mixing-length theory had in early time one advantage; namely, to put all the factors of ignorance in a length, and it was believed one could imagine a length easier than something else, say like apparent shear. Using this approach, after a while one begins to take the length seriously, and then, of course, one gets into difficulties.

Prandtl introduced the mixing length by analogy with the mean-free-path of gases. Now a fluid in turbulent motion is anything but a gas. No particle is ever without interaction with its surroundings; turbulent motion is much more analogous to a liquid. If one attempts a viscosity theory of liquids on the basis of a mean-free-path argument, one gets in exactly the same difficulty. So if you like the mixing length, keep it, but do not take it too seriously; *i.e.* if you get lengths small compared with some characteristic length don't worry about it, and if the mixing length goes to zero or infinity it is also no cause for alarm. But any result which you can get from the mixing-length theory, you can get in all cases which I know of, *e.g.* in boundary-layer theory, jets, etc., without the mixing-length concept, from much more general considerations of similarity. I think that eventually one will be able to get rid of this ill-defined auxiliary length and develop the theory more straightforwardly. In boundary-layer theory these days, and I think Clauser would be the expert on this point, one uses *e.g.* more general asymptotic considerations, which are essentially similarity considerations. And I think that eventually we will do that here too. I am not prepared to make any suggestions in detail at this time.

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Just as a last fly in the ointment: I was a little worried by Spiegel's remark that dimensional analysis gives something else than is observed. This would be against the laws of nature, I think. Dimensional analysis must be right if you've got all the right factors.

— E. SPIEGEL:

I agree that dimensional analysis, done right, can't be wrong. But as it has been done in convection problems, that is, as it has been done by PRIESTLEY, it has given an entirely different power law than Townsend's experiments produced. TOWNSEND worried about this very seriously as you can imagine, and has, as far I know, not been able to discover the cause of the discrepancy. So, I don't know why there is a difference, it's probably dimensional analysis not properly applied; or there may be a factor missing. And I think one amusing factor is that Malkus' theory does give the right tendency towards the boundary.

— W. V. R. MALKUS:

The phrase « dimensional analysis » seems very convincing; you can't have anything wrong. Usually you can't have anything. You find that if you use a complete dimensional analysis you have learned practically nothing. Invariably any use of dimensional analysis and similarity arguments that leads to more than trivial results is also based on some physical assertion about the nature of the flow. So when you say dimensional analysis or similarity arguments can't be wrong, they can't be wrong if your physical assertions are correct. Tomorrow I would like to talk to you about the classical assertions concerning these flows; for instance, assuming that viscous processes are unimportant far from boundaries, one can then show how to apply these same similarity arguments to the convection problem, where they lead to incorrect results. This then requires a reinterpretation, a reassessment of the assertions about the mechanisms which underlie the similarity argument. In doing that we will have to construct new assertions, in keeping with the observations. So I wish to add to Liepmann's comment; dimensional analysis can't be wrong if you say nothing wrong about the physics. But if you make a false assertion, you say that viscosity and conductivity are unimportant somewhere—which might, or might not, be a false assertion—or you assert that the flow depends only on a distance from a boundary, these assertions then lead to results in a quite general way without specifically describing the mechanism. If you don't get experimental results agreeing with these, obviously you are only assessing the validity of your assertions. The general similarity arguments concerning these flows are all constructed in terms of non-dimensional numbers. For example for laboratory-like convection the quantity R (RAYLEIGH) and σ (PRANDTL) are the only non-dimensional numbers. That's all you need to know to specify the flow. If you hold $\nu/\varkappa$ fixed and R fixed, all you learn from the general equations is that the flows will be identical. For sheer flow the corresponding number is the Reynold's number; if you hold it fixed, and keep the same geometric arrangement, you find the flows will be the same. But you don't know what the flows are. Additions to these results, such as the logarithmic velocity laws, are based upon additional physical assertions. It is these assertions, we must assess carefully, particularly when we go to more general situations in a stellar atmosphere where there are more parameters and more physical variables are important.

— F. H. Clauser:

I might say a bit on what we know about boundary layers, and interpret that somewhat in the light of Malkus' remarks, which I think would have a certain tie-in with what we know about turbulent boundary layers. If we have flow over a surface and a boundary layer occurs, then there is a layer next to the wall in which viscosity plays a very significant role. If we divide the boundary layer into two regions, an outer region and an inner region, then the experimental results are, that this outer portion, which is fully turbulent, is completely similar as far as profiles and structures of the large eddies are concerned to every other turbulent profile under the same conditions of zero pressure-gradient along the plate. This outer structure, properly taken, is divorced from the wall. Its structure as regards the velocity profile, the big eddies, the energy-bearing eddies, the shear-bearing eddies, and so on are concerned is completely independent of Reynold's number; that is, completely independent of viscosity. If you had some magic way to turn up or turn down the viscosity in this region, you would find no change in the characteristics as far as the large eddies are concerned in this region. Now the boundary layer as a whole does show an effect of Reynold's number, of viscosity, but this is because when you try and fit this outer layer onto the inner layer, a major portion of the velocity jump, and the same is true of the temperature jump, occurs in this laminar sublayer, which is only a minute function of the total layer thicknes.

The Reynold's number dependence occurs primarily because of the insulating layer, insulating as far as heat conduction is concerned, insulating as far as shear transfer is concerned, which occurs.

Now if we were to apply this to Malkus' results, it seems to me that we would have in this turbulent region a transfer taking place, in which every layer that is fully turbulent is similar to every other layer, and we would have relatively slight gradients within them. The transfer in this region is probably very great, but you do have regions in the two boundaries which would differ

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depending on the boundary conditions that you meet. I guess that if we put a solid wall on top, and a solid wall on the bottom, again we will have two laminar sublayers, one on top and one on the bottom, and a major portion of the temperature drop will occur in these two laminar layers, one on top and one on the bottom. If I understand Malkus' thought, this is essentially in agreement with observation. Now then, if we free either of these boundaries from a solid wall, as he has done, there is no constraint that zero velocity must occur at a given place; and my guess is that again, if you were to make observations, you would find that there would be a sharp layer, with turbulence inside and non-turbulent flow outside. There is remarkable similarity between the picture that you see when you look at the turbulent wake of a bullet or the turbulent boundary layer of a bullet, and what you see in this picture of granulation in the sun. If you were to free both boundaries, as apparently you do free them on the sun, my guess is that you would apparently have on the lower edge, a sharp but wiggly boundary; and that consequently this layer in between would probably have very sharp edges top and bottom, a turbulent region in between. Above you would have laminar flow, and below you could have laminar flow. If you watched, with time you would find that these protrusions would in fact go in and come out, with a certain massaging motion. It's almost as though you could put a rubber membrane here, and massage it from below, as far as the upper flow is concerned, and the same is true of the lower flow, but you would have this highly turbulent, highly chaotic vortical motion, taking place within the layer.

This last portion is speculation. I have no direct experience with such convection, but I've seen this kind of thing happen with jet jump, and other things so often, that it wouldn't surprise me a bit if this picture would look good. Now, if this is true, I wonder what observational consequences this might have. If, in fact, the upper and the lower edges of this convective layer had a sharp boundary, sharp as far as a turbulent change is concerned, you would not see it if you looked at it straight on. You very well might see it if you looked at it edgewise, with enough resolution. My guess is, from the numbers you've used so far, that you have far from enough resolution, because at present you are just able to see with some clarity the big eddies; and to see this you would have to be able to see the smaller eddies that take place. Otherwise this boundary would just be fuzzed out.

— E. Böhm-Vitense:

Where would you expect this boundary to occur? Would you expect it where the motion in the fluid is decelerated or where the boundary of the unstable layer occurs? Note that in our model of the solar atmosphere there is a very smooth transition between convective heat transport and practically no convective heat transport at all. Where would you expect a transition region between turbulence and non-turbulence?

(*Ed. note*: there followed a confused discussion in which no more information was added than in Clauser's remarks above. The following interchange acts to clarify a bit this attempt to work back and forth between laboratory cases involving solid boundaries and the astronomical case of a free boundary.)

— R. B. LEIGHTON:

I'd like to ask whether it is really clear that one can extend or apply the laboratory situation results to the sun, because there might very well be other parameters that are important. I take it that this very thin boundary sublayer, whatever it is, is one in which viscosity, molecular viscosity, is the thing that determines the flow. Can we really expect viscosity to play a significant role on the sun?

— F. H. CLAUSER:

The laminar sublayer is associated only with a fixed boundary; here, you have both boundaries free—you have no laminar sublayer.

— R. B. LEIGHTON:

Well, the thing that I am worried about, would it be literally viscosity that would define the thickness of the boundary between these two types of flow on the sun? Also, may not the fact that the sun has cell sizes that are comparable to the scale height make a great difference in the type of flow that we have? Will the compressibility, and perhaps other things, play an important role on the sun, whereas they are of negligible importance in the laboratory?

— F. H. CLAUSER:

I haven't made myself clear. In the sun I do not anticipate any laminar sublayer. The laminar sublayer—I brought that in only because I wanted to explain at first what I really know, and that is this case of the boundary layer in which one edge is free and one edge is fixed. Now then, I think that the case that applies in your convective layer with both edges free, would more properly be that of a jet emerging into the atmosphere from an orifice, which has thus all edges free. There, we have no laminar sublayer at all, just a sharp wiggling boundary on both sides.

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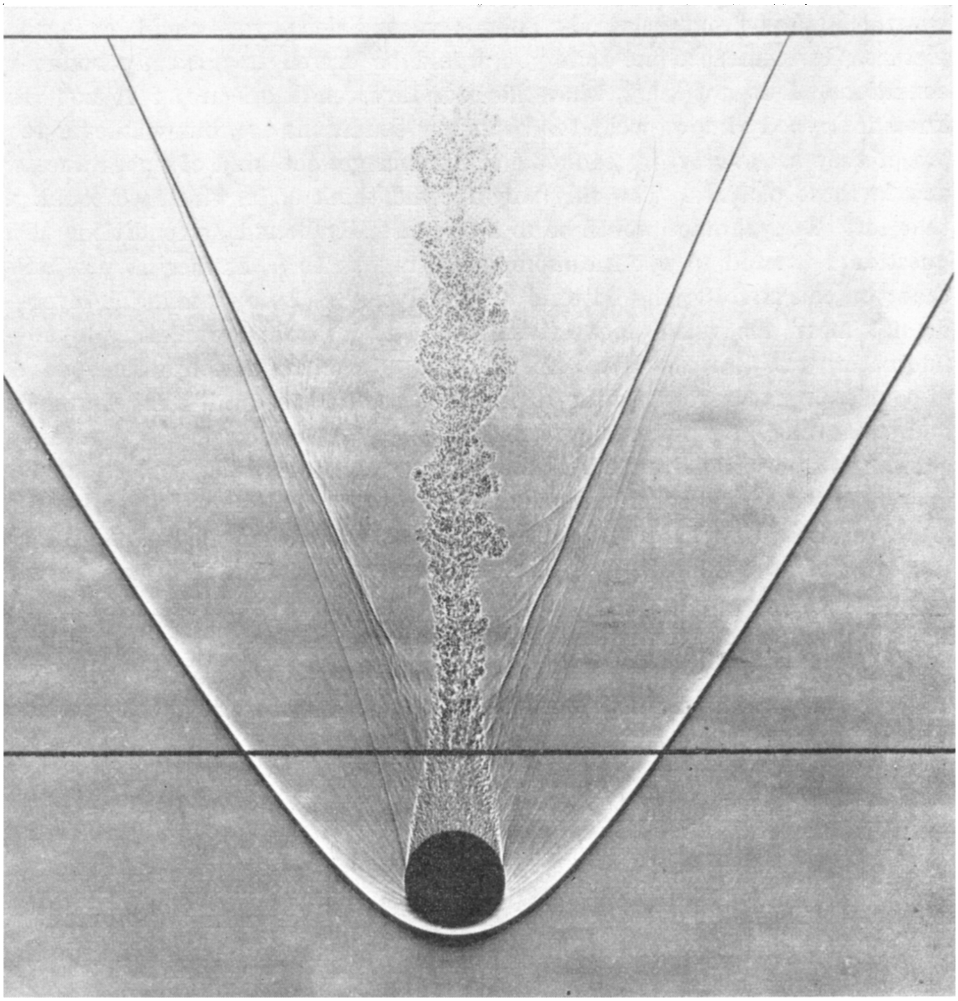

Fig. 1.

(*Ed. note*: see accompanying photograph of a sphere in flight; the turbulent wake corresponds to either the ballistic or jet models mentioned by CLAUSER.)

— S. GOLDSTEIN:

I would raise quite another problem. I am thinking about the granulation on the films we saw yesterday. It appeared that these were certainly motions due to instability. The ordinary Rayleigh theory, for example, for the instability of a thermal layer does not produce a fluctuating phenomenon such as we saw nor, I think, would a fully developed turbulent flow produce the quasiperiodic fluctuating pattern I saw. I could think of no mechanism whatever by which, if you put in steady boundary conditions, you would get such a picture. It seemed to me that you would be driven to unsteady boundary conditions. I do not know what the boundary conditions are; I do not even know if anybody knows what the boundary conditions are, but whatever they are, if they are steady, we cannot I think ever get the kind of appearance we saw in those pictures. The only way I could think of in which we could get that sort of appearance would be to have unsteady boundary conditions. The question I wanted to ask the astrophysicists was this: Is there a possibility that you can have, at the bottom of what I may call the granulating layer—I do not mean the whole convective layer, but just the granulating layer—a fluctuating temperature with something like the right period? The periods, of course, do not have to be the same; when the calculation is done, harmonics and subharmonics soon will appear. But, in a crude way, if the overturn is about the same as the period of the temperature fluctuation, you will get an instability which will contribute the right kind of fluctuating appearance. That is a lot more, and I am talking now purely of the convective part of the process, not of anything else. The temperature variation does not have to be very large, but perhaps it may be large enough to go through the critical Rayleigh number for the granulating layer. My question is, is such a temperature fluctuation possible? Such a model is interesting in its own right. There are a number of these fluctuating things in nature where you get intermittent instabilities and intermittent turbulence.

— W. V. R. MALKUS:

I want to report, as a geophysicist to the aerodynamicists, some experiments, which have not been very familiar to the aerodynamicist, because his concern has primarily been with shearing flow. This problem of interpreting turbulence only in terms of Clauser's wind-tunnel has a certain danger. Most of us in geophysics and astrophysics come across turbulent flows whose basic energy source is thermal. There are good laboratory experiments, which have been performed, regarding thermal turbulence. I think the aerodynamicists will see in them much of the character they see in their shear turbulences, and the astrophysicists may see in them examples of processes he observes in nature. Now, in direct answer to the question raised by GOLDSTEIN, consider an experiment done between two rigid plates held at fixed temperatures. As CLAUSER anticipated, sharp boundary regions are formed, and we'll explore how they differ from boundary regions one might expect in shearing flows tomorrow. This flow where the Rayleigh number is between 10^3 and 10^6, is a quasi-steady, aperiodic motion, with cells that form and persist for only a short time. The characteristic lifetime of a cell is equal to the dimensions

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of the cell divided by its velocity. That is exactly the same sort of lifetime as we get for solar convection, too. The characteristic scale of motion even in the fully turbulent situation is comparable to the vertical dimensions of the system. So, for example, a layer like this has motions in it, whose dimensions are roughly the dimensions of the entire system, and it is these motions that have the largest amplitude even though there are a tremendous number of other spectral components. If you look from the top, or from the side, or from anywhere you can into the system, you see motions which are aperiodic, whose characteristic scale is the dimension of the system, whose period is thus $4d/V$. I suppose we may be a little incautious in calling them turbulence, so I use the phrase, and I hope it will be acceptable, thermal turbulence. We are in the rest frame of these motions in contrast to shear flows. No one runs along with their instruments keeping up with the mean flow in shearing flow, and so you don't see the evolution of individual elements advected with the fluid. This will make it look different from turbulent shear flow. Still accept it, though, as an example of turbulence, and that the properties of such a flow are so similar to the ones we see in the sun that many of us for many years now have thought there was a very intimate connection. Certainly Mrs. Böhm-Vitense has, in even mentioning that there is such a number as this, suggested a similarity and I believe that tomorrow we can provide some convincing evidence that there must be.

— J. Tuominen:

Clauser said that in the laboratory, the higher resolution we have, the smaller eddies we can see. Has not this come in connection to the slides shown yesterday by Severny? He has so small a resolution that he could not see the granules, but he only saw larger areas of the sun. If we consider a part of the sun, then he found areas with different velocities, upwards and downwards. These areas are much bigger than the granules. Now, if we have a higher resolution, then we see the granules. Perhaps, if we had still higher resolution, we could see still smaller eddies on the sun.

— L. Biermann:

I understood Goldstein to state that if one has strictly stationary boundary conditions, he couldn't see how you would have unstationary conditions in the layer in question. Now suppose that you have the case of a thermal instability, a superadiabatic gradient. Inevitably, no matter how, if you get motions with sufficiently high Reynold's and Rayleigh numbers, you would expect non-stationary features just from the ordinary reasoning of the theory of turbulence. Then necessarily you would get non-stationary features, just of the kind you observe. I'm not aware of any real problem in this area.

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— S. GOLDSTEIN:

I'm sorry, perhaps I didn't explain very well. The « period » of the intermittency is the so-called lifetime of the appearance, and is not, so far as I can see, explained on any physical theory that has yet been given.

— W. H. MCCREA:

Don't we have the same intermittency in our own weather?

— S. GOLDSTEIN:

There are two answers. The first is we do not have the same kind of « periodic » intermittency, and the second is that we still have a job to do in meteorology. In detail, for example, there certainly are in meteorology, theories of cloud formation, but what is seen is quite different from this kind of intermittency.

— H. PETSCHECK:

If I understand the question correctly, it could be explained by a superposition of different periods. You see, you have periods of the granules, and then superposed on them another period, which you say would have to come from boundary conditions. Now in the sun, as you go down, all of the conditions change, scale height and so on. So that the characteristic frequency for a slightly lower layer is very probably different from the one of the layer that you see. If the motions from there are superposed on the ones which you see, I think one gets exactly the effect that you're looking for.

— C. A. WHITNEY:

Let me summarize how this situation on the upper part of the convective zone looks to me, then comment particularly on the region above the convectively unstable layers, above optical depth unity, in terms of some specific calculations. Some of these thoughts have come from interchange with KROOK and THOMAS.

Below some depth in the solar atmosphere, there is a region that cannot be static. Radiative transfer processes are insufficient to carry all the energy flux from the solar interior, so convective motions set in. Just above the unstable regions the atmosphere is in radiative equilibrium, and if isolated would be static. However, in Clauser's words, it is being massaged from below, so it cannot be static. Because in this interaction region, all apparently agree, the mixing-length representation of the convective zone breaks down, a detailed picture of the interaction region is difficult. However, I think we can make some comments on the kinematics. Whatever the model, in the penetration region, it will include pressure and temperature fluctuations, which will in turn produce perturbations travelling up into the stable regions. An explicit formulation of this situation by KROOK starts by imagining a plane at some depth, writing all the significant variables—pressure, velocity, etc.—as random, or quasi-random functions of space and time on this plane and then using this plane to define the boundary conditions for the flow in the upper region. In other words the lower region is eliminated, and its effect is simulated by the plane of fluctuations. KROOK has discussed the effects of this type of boundary conditions on the flow above, although there has been very little explicit work done on this model. It is quite obvious that there will be a variety of modes of motion generated.

A point which KROOK particularly emphasizes is that the system must be treated as a whole. We must look for steady-state solutions and must recognize that these regions will be acting on each other. There is a sequence which we might in principle go through. Having solved for the structure of the radiative region under the influence of the convection zone, we then go back and rederive the structure of the convective zone as it is influenced by the modified radiative zone. This process should be repeated to convergence. There are reactions in both directions which may well turn out to be significant.

In the region above this fluctuation plane, the gas is stable against convection, so we might offhand expect the motions to be predominantly of the curl-free or compressive type. There will, however, also be a divergence-free type or gravity wave. Both types will exist, but one's feeling is that perhaps most of the potential energy associated with the wave motion will be bound up in compression rather than gravitational potential. In this situation, when you have waves of both types, it is impossible to weigh what we should expect in the way of phase relations between one quantity and another. It is impossible to say, for example, whether we should expect the rising elements of this region to be hotter or colder than the descending ones.

I might conclude by outlining two ways of looking at the granulation. These are extreme models and clearly the situation lies somewhere in between. A complete treatment along the above lines should provide, among other things, a picture of the granulation. Lacking such a complete treatment, one might look at two extreme models of granulation. One way is simply to forget about the temperature fluctuations in the convective region, and regard the convective motions as equivalent to pistons which produce pressure perturbations. Thus, as above, both acoustic and gravity waves will be produced. We might say that what we see in granulation is the field of acoustic waves generated by the convective zone. A second extreme model is to conceive of the convective motions—below the idealized plane referred to above at the top of the convective zone—imposing temperature variations, the overlying layer remaining unaffected. In the simplest terms, we would consider the granulation to result from looking down through the overlying atmosphere to the hot and cold gas in the convective zone. We know this is incorrect because the higher temperature associated with the rising element will affect the temperature distribution in the stable region, so that we should modify this simple picture by introducing temperature variations in the stable region.

I would like to summarize some numerical work we have done on the basis of the first oversimplified picture. We took the initial value approach, putting a piston in the solar atmosphere at about optical depth unity, and gave the piston a period of five minutes and a velocity amplitude of one km/s. We wrote the continuity and momentum equations in standard form, including the gravitational acceleration, and restricting ourselves to one-dimensional motion. Since a proper solution of the energy equation including radiation transfer terms is exceedingly laborious, we made the following simplifying assumption. Each atmospheric element was taken to be optically thin and immersed in a radiation bath at a constant temperature. We integrated the equations numerically and obtained the following results.

The temperature, density, and velocity amplitudes of the wave increased rapidly as the wave moved up into the region of decreasing density. The phase relation between the temperature and density within the wave was quite different from that within an adiabatic wave, because the energy loss term is very important under these conditions. In fact, the maximum of the temperature profile within the wave corresponded to the forward portion of the density profile, so that the regions of maximum temperature and maximum rate of compression coincided. The wave gave up its energy to the radiation bath, and by the time the wave had travelled two hundred kilometers its total energy has decreased by about 25%.

The width of the high temperature front of the wave was about 100 km. From this solution of the one-dimensional equations we might construct the following three-dimensional model of granulation. Imagine that the top of the convective zone be replaced by an array of pistons and that each produces a high-temperature region moving up through the atmosphere as described above. If the dimensions and separations of the pistons are about 1000 km, the appearance of an atmosphere disturbed in such a manner will be consistent with the observational features of granulation. Also the concept that we are actually observing the temperature fluctuations within the convection is consistent with observations.

Unfortunately the bulk of the continuum radiation which we observe from the sun is emitted from that limbo region of transition between the stable and unstable layers of the atmosphere, so it is difficult to separate the effects of these regions by observations in the continuum.

— M. MINNAERT:

We heard this morning some quite interesting theories of turbulence. I would like, if possible, to connect these considerations with the astronomical phenomena discussed in the first days of the symposium. The aerodynamicists have warned us that we should not use the term turbulence loosely. So consider for a moment how far the phenomena on the sun may be designated by the term turbulence, real aerodynamical turbulence.

If we review the observational facts they amount to these. In the lower photosphere, we observe temperature differences. Unfortunately, we are not able to measure velocities in this layer, but we see these local temperature differences varying in time—this is granulation. In the higher photosphere, the region where the lines are formed, we observe in the first place local velocity shifts, directly observed, these are the wiggly lines; and in the second place, we have a certain number of spectrophotometric observations from Fraunhofer lines, curves of growth, etc., which also show that there are velocity differences. Only the first are directly observable macroscopic motions, while the second are microscopic.

And now I should like to ask in the first place about the macroscopically directly visible velocities and the probably connected temperature differences of the granules. Can we call this real aerodynamical macroturbulence as astronomers are used to calling it? Is it not necessary, for example, to have vorticity in order to be able to speak about turbulence? What are the conditions which a velocity field should satisfy in order to be called by that name? One may say that it is only a question of terminology; but as soon as you use the term aerodynamical turbulence, that means that the turbulence spectrum will have a certain number of properties which astrophysicists would like to apply. How far is this allowed?

The second thing is, how far are we allowed to speak about microturbulence in the granular layer? I should think that if there is real macroturbulence, then just because of the turbulence spectrum, one may *a priori* expect that there will also be many *minor* turbulent elements, and that also from the aerodynamical point of view microturbulence looks probable.

The same questions have to be put for the higher photosphere, though the answer may be different there. It should be ascertained whether random waves would give the same spectral phenomena as real turbulence.

— R. N. THOMAS:

I would like to put a couple of numbers on the board relating to what WHITNEY has said. As I mentioned earlier in this symposium, we tried some time ago to calculate the heating of the chromosphere by aerodynamic dissipation of the energy of a spicule on the assumption it was a supersonic jet

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but gave up because we didn't have any real knowledge of the thermal state of the medium we were trying to work with or the thermodynamic properties of the spicule system. We have spent the last several years trying to get better information on these unknown properties of both medium and spicules, as well as to develop the analytic structure for treating such an aerodynamic system coupling with a radiation field. I would stress the importance of radiative stability in computing the aerodynamic configuration of such an assumed supersonic jet, maybe coming back to this point later in the symposium.

To make decisions on several models discussed by WHITNEY the same knowledge of properties of the medium must be made. So let me make several points. First, I would like to ask what these observed brightness differences in the granulation mean in terms of the distribution with height of the temperature fluctuations. Now this is a numerical calculation that DE JAGER and PECKER suggested a long time ago; so far as I know nobody has done it in detail. Always one says that an observed brightness fluctuation corresponds to a certain temperature fluctuation, not specifying where in the atmosphere this fluctuation occurs. Let us assume a 5% brightness fluctuation. To a first approximation, we can estimate distribution at the disk center and center-limb variation by considering the fluctuations over a spherically-symmetric surface. We find $\Delta T_e \sim 50°$ at depths everywhere below $\tau \sim 0.3$ suffices to produce this 5% contrast at the center of the disk. The same is essentially true at $\mu = 0.6$. At $\mu \sim 0.2$, the contrast would drop to $\sim 1.5\%$ for the same ΔT_e or require ΔT_e to extend upward to $\tau \sim 0.1$ to give the same contrast; at $\mu \sim 0.1$, the contrast would be undetectable. If we wish to confine ΔT_e to regions below the $\tau = 0.3$ level, and to detect a granule at $\mu = 0.1$ (assuming a contrast of 1 to 2% is necessary for detection), then we require $\Delta T_e \sim 100$ or $200°$ at $\tau \sim 0.3$. To hold the contrast to 5% at the center of the disk, we require, however, ΔT_e to decrease rapidly downward. For example, if we set $\Delta T_e = 200°$ over the interval 0.33 in $\log \tau$ centered at $\tau = 0.46$, $\Delta T_e = 50°$ over the same interval centered at $\tau = 1.00$, and $\Delta T_e = 0$ elsewhere, we find contrasts: 5% at $\mu = 1$, 8% at $\mu = 0.6$ and 0.2, and 2% at $\mu = 0.1$. Changing ΔT_e to 150° in the interval centered at $\tau = 0.46$, keeping it at 50° around $\tau = 1$ and zero elsewhere, we find contrasts: 4% at $\mu = 1$, 6% at $\mu = 0.6$ and 0.2, and 1.5% at $\mu = 0.1$.

This is pure numerology. I take an observed intensity distribution and ask, what temperature distribution is compatible with this? I stress this because these observations relate to the regions above the level of convective instability. We are in the region where penetration occurs, in the region where whatever is going to heat the chromosphere is starting from. So this is the aerodynamic boundary condition that one would like to get out.

Let me emphasize that these several alternatives give a different behavior of the granule intensity contrast as we go to the limb. This is a question which

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must be solved observationally, but to the best of my knowledge, the data do not yet exist. I certainly hope to stand corrected on this. This question is relevant to many problems. The computation of line profiles; the interpretation of the effects mentioned by MINNAERT, the boundary conditions for the things WHITNEY has talked about, and, lastly, I want to know what this does to the low chromosphere.

A second point is that an empirical analysis of the structure of the atmosphere shows the absence of momentum input to a height of some (1000÷1500) km above the level $\tau = .01$. The atmosphere is in hydrostatic equilibrium under the normal solar gravity value, to an accuracy of some few percent. The temperature rises by about 5000°, but there is no momentum input by whatever the mechanism which causes the temperature rise. This is a strong requirement on any kind of aerodynamic theory of the energy input mechanism. We must have an energy source, but it cannot be a momentum source.

— G. ELSTE:

Was the geometrical effect of shielding taken into account in these calculations? The hot and cool regions will screen each other.

— R. N. THOMAS:

All I have really done is use your contribution function method, and a spherically-symmetric distribution of temperature fluctuations. I assume local thermodynamic equilibrium in the continuum, and ask what results from assumed fluctuations in the source-function.

— G. ELSTE:

On the picture WHITNEY roughly sketched, the granulation would consist of bright regions with adjacent darker regions. But the granulation does not *look* this way. The granulation looks like bright patches surrounded by narrow, dark regions.

— R. LÜST:

I would also like to make a remark on this one-dimensional problem, if you want to compare in detail this calculation with observations. It is my experience in connection with two-dimensional computations, including a vertical magnetic field, that the geometrical factor, what you are losing in the sidewards direction, is quite severe; therefore the amplitude increase is not as large as one would expect from the one-dimensional computation. I think one should therefore be somewhat careful in directly applying the calculations to observational data.

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— E. Böhm-Vitense:

I would like to ask how much the density is increased in this wave, because if the increase is not appreciable I don't think you would see this wave.

— E. Schatzman:

I think that when we go from the plane problem to the non-plane problem, we have the following difficulty. For the plane problem, the velocity field is irrotational, but for non-plane waves the motion is not in general rotational-free *e.g.* for waves coming from points distributed on a given layer. I think that it would be interesting to see how at some distance from the source, the waves coming from different portions of the surface will interfere with each other, and will produce a chaotic velocity field which could turn out to be something between a shearing field and compression waves.

— C. A. Whitney:

The velocity semi-amplitude increased from 1.5 km/s at the piston to 2.5 km/s at a height of about 200 km above the piston. The density amplitude had reached a factor 1.5 by the time the wave had gone several hundred km. The amplitudes of all perturbations increased with height, although the total energy of the wave decreased.

In answer to the other questions I must agree with those people completely that when we start talking about geometry, these calculations are inadequate. My point in mentioning it was merely to demonstrate some physical effects which had not been mentioned this morning.

— H. Liepmann:

The random piston problem has been partially treated by Phillips. It has not been treated yet for the case of a variable density atmosphere with an energy correction in, and Phillip's treatment was a linearized one, but I think the complete treatment can be made. All you have to do is give the space-time correlation of the fluctuations in the plane, and then you can solve the wave equation as an initial value problem with stochastic variables in it. My feeling is that the linearized two-dimensional problem including the density variation should be the next step.

— *General discussion*:

Relative merits of proceeding with any linearized treatment as opposed to a non-linearized treatment. Agreement that linearized problem might give reliable results for the lower parts of the atmosphere.

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— E. Böhm-Vitense:

If the density variation is only a factor 2, I don't think you would see the feature described by Whitney. The optical thickness of this region would be only about 0.1. Also I do not see why you think that my picture of this morning, which viewed granulation as matter rising from below and circulating through the stable layer, would not be able to represent the observations?

— C. A. Whitney:

My apologies, I tried to give the impression that both pictures are possible, in terms of present knowledge. Until we get the type of data referred to by Thomas, there seems to be no way of choosing between them. On your comment about the optical thickness of the high temperature region, I find from my computations that the increment of emergent intensity produced by the wave is 4% at the center of the sun, and 12% at $\mu = 0.2$, when the wave lies at $\tau \sim 0.3$

— L. Biermann:

Three points: a general comment on the use of the mixing length in astrophysics; on instability in early-type stars following the work of Kiepenhahn; and the possibility of observation of the type of oscillation mentioned by Leighton yesterday.

The mixing-length theory as presented by Mrs. Böhm-Vitense is mainly used for two purposes: one, to interrelate the several data of observation—size, velocity, lifetimes, and contrast of granulation; the other, to deal with the internal structure of stars and their evolution. Regarding the first, I think it reasonable to say that within the factor two or so associated with the application of this type of theory, there is reasonable agreement between theory and observation. Regarding the second, consider two methods of integration of a stellar model. If we neglect convection and just use the theory of radiative transfer, starting from the theory of stellar atmospheres, we obtain one curve in the $\log T$, $\log P$ plane. If we make allowance for convection in the way mentioned this morning, we get another curve, giving a lower temperature for the same pressure. These give quite different models for the sun and for the stellar interior. For the sun, it happens that it is not easy to say definitely which model is more correct. It turns out that what we know about stellar evolution from star clusters can only be understood, for their particular part of the H-R diagram, by using the mixing-length theory just in the form it was presented this morning. I think that this one fact shows that there is in astrophysics, entirely apart from anything that was mentioned this morning, something which indicates that the application of the mixing-length theory is not so far off as one might have guessed. To get to the radiative solution from the convective, you need a mistake in the mixing-length theory —in the dimensionless quantities that enter—by one power of ten or more. Therefore, for most purposes in astrophysics, we would be happy if any error introduced by the mixing-length theory would be less than a factor 2 or so. There are, of course, special questions in which this is not true, but I would simply make the point that our restrictions on the use of the theory are by no means as severe as they are in the discussion of experimental evidence in the laboratory.

The second point concerns KIEPENHAHN's work on circulation in a rotating early-type star. It can be shown quite generally that a star rotating without meridional circulation is in a singular state. An old theorem of von Zeipel shows this for radiative equilibrium; I discussed the case of convective equilibrium at the Stockholm conference a few years ago. The speed of the circulation depends upon the stellar structure; only for very thin convective layers—essentially pure radiative equilibrium—should one expect large circulation velocities near the surface. KIEPENHAHN has attempted to work out numerical results. For the hot supergiants, in which according to the mixing-length theory one should not expect extensive hydrogen convection zones, KIEPENHAHN obtains velocities of the order 1 km/s. We know from observations that such stars have atmospheric turbulent velocities of some km/s, and this proposal is to link them with the meridional circulation.

The connecting argument is that these circulations would be dynamically unstable according to the criterion of REYNOLDS concerning the instability of shearing flow. It can easily be shown that the Reynolds number associated with these motions is exceedingly large, so one should really expect instability of the dynamical variety, not thermal. This is the root of the idea of KIEPENHAHN for accounting for the observed turbulence in this type of stars.

The third remark is short; LEIGHTON mentioned what appeared to be pulsation with a period of about five minutes in addition to a decay, and I just want to point out that this is rather near to the fundamental period of oscillation in the sun's atmosphere. This quantity can be brought into the form $P = a/g$, where a is the velocity of sound and g is the gravitational acceleration. This period is obviously a minimum in the photosphere, and in this case it is not far from the observed value. It might be worth-while to inquire into the meaning of this.

— K. H. BÖHM:

You said that some of the results of the mixing-length theory are in agreement with observations, and you quoted among other things the size of the granules. I am not quite sure that one can predict the size of the granules

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from the mixing-length theory in a convincing manner. We heard this morning that the scale height, which is used as mixing length, is, in a polytropic atmosphere, always proportional to, and of an order of magnitude equal to the distance from the top of the atmosphere. So, depending on your detailed assumptions, you can get elements of almost any size at the surface of the star. In the sun, the ratio of the local scale height to the distance from the surface is about 0.8 in the upper and 0.4 in the lower parts of the convection zone.

— L. BIERMANN:

Let me just refer to a recent detailed discussion of this point in *Zs. f. Ap.* by some of our people.

— J. WADDELL:

I should like to mention some work which PIERCE and I did on the analysis of limb-darkening, as I think it bears on the discussion we have been having. In solar observations we go from the intensity, $I_\nu(\mu)$, observed on the disk to the source-function, $S(\tau_\nu)$, and then finally we can go to the monochromatic radiation flux, $F(\tau_\nu)$. Studies I have made concerning the errors involved in each of these steps indicate that at $\tau = 10$ one can magnify these errors in the first step by a factor of a 100. When one gets to the monochromatic flux, however, the error of the flux is only a factor of 10 greater than the errors in the observed intensity. The reason for the large error is that the function $S(\tau)$ is effectively the inverse Laplace transform of $I(\mu)$, a risky numerical procedure; on the other hand, the error in the radiative flux $F(\tau)$ is small because it is an integral over $S(\tau)$.

We have computed $F_\lambda(\tau_\lambda)$ for optical depths as deep as 8 to 10. The value near the Balmer discontinuity is a little uncertain, and we can only go down to a wavelength of 3100 Å, so we know nothing about the ultraviolet. About 30% of the graph is incomplete. However, the uncertainty of the final values of the integrated flux is about 10%. It appears that to within this limit, the radiative flux is conserved down to an optical depth of ten.

— A. UNDERHILL:

This implies that convective transport is not important above $\tau = 10$. I would like to ask Mrs. BÖHM whether this is consistent with her work on the convection zone using the mixing-length theory?

— E. BÖHM-VITENSE:

I cannot answer decisively but you can altogether neglect convective flux down to $\tau = 2$. The convective flux increases rather smoothly with τ and I don't think these results are inconsistent with theory. I would not think we can draw any conclusions from this, however.

— K. H. BÖHM:

I would just like to say that we are aware that it is a slightly dangerous procedure to derive temperature inhomogeneities from line profiles. The point of view which we would take now is the following one. We are inclined to believe with the British-French group that there are temperature inhomogeneities of the order of $\pm 260°$, as has been given at equal optical depth. We would say that they must certainly have an influence on the line profiles, and it is good fortune that if we take into account these temperature inhomogeneities, some of the discrepancy in the theory of the center-to-limb variation of line wings are reduced. But, on the other hand, we are aware that in principle we should have to take a better source function too. So we don't think this is real independent evidence for the *magnitude* of the temperature fluctuation. We just consider it as one argument in addition to the evidence already indicated by direct observations.

There is one other point I would like to mention, which is independent of what I have just said. I think a few numbers which have not been quoted so far could be mentioned in order to state the hydrodynamical problem of the convective zone more clearly. These numbers will show how radically different the situation is from laboratory convection. In the solar convection zone, having a thickness of about 60 000 km, the density varies by a factor 10^4. The conductivity by radiation varies by a factor of at least 10^3; it varies very rapidly near the upper boundary of the convective zone, and then the variations are much less rapid. Finally a point which I think must have some bearing on the calculation of the currents in the convection zone is that the quantity

$$\frac{\mathrm{d}T}{\mathrm{d}z} - \left(\frac{\mathrm{d}T}{\mathrm{d}z}\right)_{\text{adiabatic}}$$

varies by a factor of at least 10^2 if we compute the structure of the convective zone using the mixing-length theory which has been quoted. If we use just a radiative model this quantity varies by a factor 10^4 or more within the hydrogen convective zone.

— W. H. McCREA:

As regards this zone, a few years ago I tried the effect of temperature differences of about 1 000° to calculate the continuous spectrum of the sun. You can get wonderful agreement with the figures by putting this in! I think ELSTE mentioned this afternoon that when you look obliquely you see through

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one temperature to another, and that effects the limb darkening, and it seemed to me at the time you'd get good agreement by taking into account this effect. BÖHM asked about the lines, but I wanted to point out that there are interesting effects in the continuum as well.

— J.-C. PECKER:

I have two points to make. The first is to mention work by Mrs. ROUNTREE-LESH measuring the mean-square velocity from curve-of-growth analysis. She found for Ti II a value which I think is the extreme value which has been so far found in the sun, a value of 4 km per s. The problem is how to reconcile this value with others. It seems to me that the only way to reconcile them is to make use of the curve-of-growth theory, with *consideration of non-LTE source-function.* My second point is this. I want to report on work by Mlle. CURY, M. LEFEVRE, and myself in Meudon and Istanbul. This work tried to make use of both equivalent widths and central intensities of lines and of their variation from center-to-limb. We tried to correlate these phenomena with the inhomogeneous model proposed by BÖHM. These results can be seen on Fig. 2. At the center of the disk of the sun, one of the Ti II multiplets gives a source-function represented by curve *A*, one point from each line. If I follow each line from center to limb, I should find again the curve *A*, if no other effect comes in. Actually, the observational results of LEFEVRE were quite different, the lines behaving as shown by curves *B*.

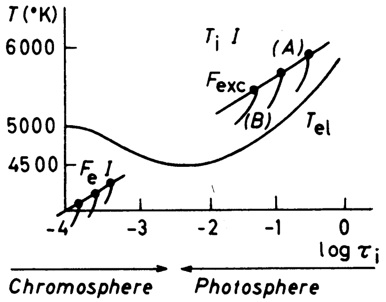

Fig. 2.

Similar measurements have been made at Instanbul on a few multiplets of Fe I, and there the effect was also seen but much smaller. Now, to show how to interpret this, I just want to draw a very quick picture of the results without going into details. If we use a three-column model, as proposed by BÖHM, the optical depth $\tau = 1$ in the line is generally much higher in the cold column than the optical depth $\tau = 1$ in the hot column. So if you look from a certain direction, not normal to the surface, what you actually see is influenced very much by the size of the elements. If their size was very large at the limb, you would see the same proportion of cold and hot as at the center. But if *h* was small enough, you would see only the cold columns. Some computations

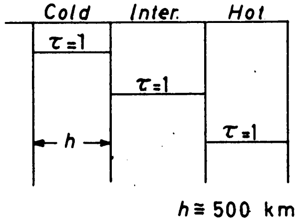

Fig. 3.

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have been made using several values for h, and, as suggested in this discussion by ELSTE, account has been taken of the fact that you penetrate from one column to another. The result is quite striking; it is possible to fit the observed behavior using a value h of the order of 500 km. Of course, the result depends upon the temperature differences assumed in Böhm's model. If one had been using another model with smaller temperature inhomogeneities, one would have gone to smaller values for h. This is just an example of what could be done to investigate not only the inhomogeneities, but the size of the elements, from center-to-limb variation, taking into consideration possible departure from LTE.

The question I want to ask now is « What is really the true temperature difference between hot and cold columns? » On this point I just want to mention two things briefly: 1) Measurements by SERVAJEAN at Pic du Midi which agree entirely with the conclusion given by Miss MÜLLER, especially the fact that the correlation between velocity and brigthness seems to be very poor. 2) I think that RÖSCH will agree with me that the value that has been given by the so-called French-British school could be too high for a very definite reason: There are actually large scale fluctuations and small scale fluctuations. Some methods of measurement may give large fluctuations of temperature when, around the mean value which is what really counts, you would measure much smaller values. This is the reason why I am inclined to believe the value given by RÖSCH and SCHWARZSCHILD is correct.

— G. ELSTE:

How did you convert the equivalent width of the Ti lines into temperatures? Did you use the linear approximation $S = a + b\tau$ for the source-function?

— J.-C. PECKER:

No, we used the actual source-function, derived from central intensities.

— G. ELSTE:

Did you assume LTE and then compute the excitation temperature?

— J.-C. PECKER:

Yes, for the first approximation, but then we iterated the solution.

— J. RÖSCH:

I would like to mention several points in connection with things which have been said. First, the question of the value of $\Delta T/T$ seems an important

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piece of data. I wish to mention the difficulties in deriving correct values of $\Delta T/T$. Once you have taken a picture, the simplest way is to make a microphotometer tracing through the field. Then you get a curve like this:

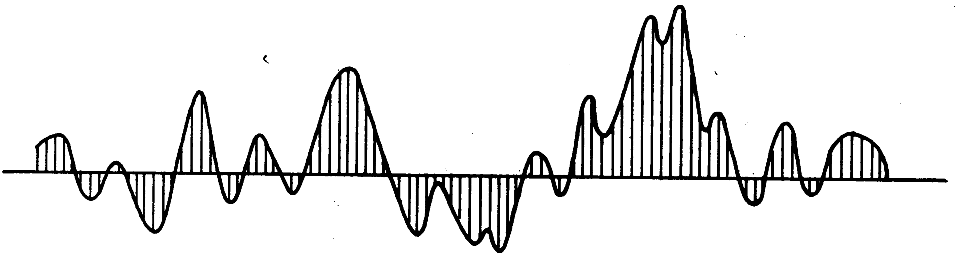

Fig. 4.

Then you take an average curve and compute a r.m.s. deviation from this curve. Doing this you generally find a rather small number. There is another longer way which may give more significant results. You make many such curves or use an isophotometer and make a map of isophotes of the granules looking like this:

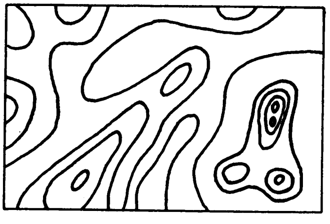

Fig. 5.

You may then draw the profiles of individual granules. What you then find is profiles of granules looking like this:

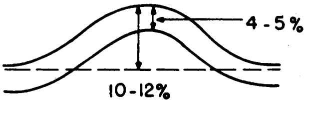

Fig. 6.

We have done this for approximately 60 granules on a picture which was not one of our best. We found differences between maximum and minimum intensity of about 11% of the average—with a displacement of the various granules by about 4%. On the same solar field also we made a r.m.s. estimate and found 3.5%. After this you must correct for the effect of limited resolving power. The scattered light is not very important. We correct these values by (11÷12)% and find a factor of two must be applied, giving an average $\Delta I/I$ of—say—24% total amplitude for a given granule. The maximum fluctuation, not for one granule, but from the darkest to the brightest part of the film is, when corrected, about (30÷32)%. You see that the result is widely different from 3.5%; and if you measure the $\Delta I/I$ starting only from the r.m.s. you must perform a mathematical analysis to derive from this the total amplitude $\Delta I/I$. We avoid this mathematical step by going to the trouble of making isophotal maps, and I think the result is probably better.

From the present values, derived at about λ 6 000 Å, one may compute a total amplitude $\Delta T \simeq 350°$. This was done with our 23 cm objective and the picture was not one of our best. We are ready to do this with a larger objective now. I expect to find steeper sides on the granules but not a bigger $\Delta I/I$.

I would like to comment on the work of SERVAJEAN, who does not find a close correlation between brightness and outward motion, If you consider that there are granules which seem to explode, there appears to be some dark matter just in the middle of a ring of bright matter.

Why shouldn't this dark matter in the middle also be an *ascending* column. so that if you enter it into the analysis it will diminish the correlation? Another point concerns what THOMAS has said about the observations near the limb; he said the observations must decide between the various possible curves showing variation of $\Delta I/I$ across the disk. I am afraid it will be difficult to decide near the limb because the distribution seems to be rather different. You see a rather smaller number of granules and only the brightest points are visible—separated more widely. It will be hard to define $\Delta I/I$ and the interpretation will be difficult.

The last point concerns Clauser's comment this morning about the appearance of these motions at the limit of a turbulent layer. He said that if this is turbulence one must see smaller and smaller elements. It seems to me that we can now say that we see bright regions separated by definite dark areas and we do not have a continuous phenomenon. We must try to interpret the size of these things. We may, with better resolving power, find things inside these areas but the fact remains that at least one definite scale exists and we must try to interpret it.

— B. E. J. PAGEL:

This is really in the nature of a short question on the observational side, but I think it is a fairly important one in connection with the various types of motion that have been discussed. SEVERNY mentioned yesterday that there

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are several scales of motion, and I am not quite clear which motions are on which scales. First, there seems to be the 10^4 km scale, rather persistent fields of motion—several hours—which might perhaps be connected with meridional movements. I think this has been suggested in the past and BIERMANN brought it up again this afternoon. Then there are the « wiggles » which, if I understand correctly, are on a scale somewhat larger than the classical granulation—about 3 000 km, I think. I don't know if there is any evidence on the lifetimes of these wiggles. I should presume—I'd like to be corrected if I am wrong—that the oscillations observed apparently have a lifetime of 20 minutes. Finally we have granulation, which is less than 1400 km in scale, with lifetimes of the order of 8 minutes. Down to here we seem to have distinct phenomena which are not affected by the resolution of the equipment. Perhaps below here, limited resolving power comes in. I would be glad to know if this picture is consistent with the observational material.

— R. B. LEIGHTON:

With respect to the large scale structure, which I would call greater than 10^4 km; the lifetimes of several hours refer only to horizontal motions, as far as our own observations are concerned. These are things that we think must be the divergent streaming along the surface of matter which must have come up from underneath. We do not see it coming up for some reason that is, I think, connected with the observational technique—I'm not quite sure. At any rate, the several-hour lifetime is associated with *horizontal* motion. I understand that SEVERNY has found large scale motions, with a vertical component of somewhat smaller velocity amplitude than we find, which also have lifetimes of several hours. We have no information about that. I think it is probably true that the « wiggles » and our oscillations in the vertical motions have essentially the same scale. However, as far as our observations are concerned, I would designate the scale not as 3 000 km, but as *greater than* 3 000 km, this being the *lower limit* imposed by our resolving power. I think it is significant that over a very wide range of wave numbers there is a single frequency that the sun picks out. Concerning the granules we have as yet no information.

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