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CONVECTION IN STARS*

MARTIN SCHWARZSCHILD

Princeton University Observatory

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If you permit me, I would like to begin this discussion with a short historic review and start the review by talking about an uncle of mine. This uncle of mine, Robert Emden, wrote a book in 1907. This book was the first basic text on the stellar interior. For the purposes of this book he used the working hypothesis that convection and nothing else was the mechanism which in every star brings the energy that is produced in the deep interior out to the surface. On the basis of that working hypothesis he constructed his famous family of polytropic models. About the same year that my uncle wrote this book my father found that radiation at the high temperatures inside the stars might very well also be an extremely efficient mechanism to carry energy out from the center to the surface (K. Schwarzschild 1906). Indeed, he developed the concept of radiative equilibrium and applied it to the atmospheric layers of the sun. He found right then good arguments that the atmosphere of the sun is likely to be in radiative rather than convective equilibrium. He also developed the general criterion by which you decide whether a layer in a star is in a convective state or in a radiative state. Finally, he suggested that if you went below the atmosphere farther down into the sun, you might find yourself leaving the radiative layers and come into convection, his main argument, I think, being the observational fact of the solar granulation.

Twenty years later Eddington (1926) wrote the second basic book on the stellar interior. He applied radiative equilibrium in every star from the center to the surface. No convection whatsoever in his picture! It was in the early 1930's that the question of radiative energy transfer versus convective energy transport was attacked in detail. The first move was by Unsöld (1930) and Siedentopf (1932, 1933, 1935), who investigated the outer layers of the sun just below its atmosphere, i.e., the layers responsible for the observable granulation, and decided that the granulation could be understood in terms of convective motions caused by an instability arising from the ionization of hydrogen. The next step in this investigation was made by Biermann (1932), who found that quite likely the cores of many stars would be convective if the nuclear energy sources were highly concentrated toward the center of the star. Taking up this suggestion, Cowling (1935) developed his well-known model, which consists of a convective

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core and a radiative envelope. Finally, still in the thirties, Biermann (1935) and Cowling (1936) discovered that in some types of stars an appreciable portion of the envelope (not just a very thin hydrogen-ionization zone) is quite likely in convective equilibrium. They found that the question of whether or not a star has such a deep outer convection zone depends on a rather delicate condition right in the atmosphere of the star.

May I now summarize our present picture regarding convective regions in various types of stars. As is often the case, the more we learn about the actual state of a complex physical entity such as a star, the messier the picture gets. But in the case of the convective regions within stars the situation is not yet so bad. As to convective cores, the present picture runs as follows. If the main energy source of a star is gravitational contraction or the slow burning of hydrogen into helium by the proton-proton reaction, then there is no convective core, but if the main energy source is the violent burning of hydrogen by the carbon cycle or the subsequent high-temperature reactions like helium-burning, then the star will have a convective core. That means that all the luminous stars with violent energy sources have convective cores, the faint ones have no convective core. As to convective envelopes, the present picture is similarly simple. The early-type stars, earlier than type F, have atmospheric boundary conditions permitting the whole outer portion of the star to be quiescent, i.e., to be in radiative equilibrium, while the later types of stars have boundary conditions enforcing convective envelopes. Indeed, in the extreme cases of the late M dwarfs the convective envelope reaches so far inward that the entire star becomes convective.

After having reached this picture, those of you who tend to be critical of the early speculative phases of astrophysical theories might perhaps say: Look at the comparison of our present picture with that of Emden, or that of Eddington! Wasn't their work entirely in vain? Did they not start the theory of the stellar interior ineffectively early so that they had to make some wild guesses and could not possibly hope to obtain reliable results? I feel that such a criticism would be rather wrong. I think it is quite clear that Eddington would have had great difficulties if Ritter and Lane and Emden had not developed the polytropic models (particularly since Eddington, it seems, hated numerical integrations, while Emden loved them). Again, if Eddington had not done his courageous work without having all the physics available to make unique answers possible, the nuclear physicists, when they were ready, would not have had available amazingly precise temperatures and densities for many stars so that they could not have securely isolated the relevant types of nuclear processes without which our present picture could not have been developed. So it seems to me that this development is a beautiful example of those bright occasions when theoreticians enter a new field before all logically necessary data are at hand, proceed on the basis of some clear working hypothesis, and in this manner gain results essential for the later derivation of a more definitive theory.

This far I have talked entirely in a non-quantitative vein. Obviously, to come to definite statements as to the extent of convection in the stellar interior, it was necessary to determine how effective convection is as an energy-transport mechanism in the stellar interior, and to do this in a quantitative, numerical form. To achieve this step, even today, the only applicable theory, however approximate, appears to be the one developed by Prandtl and called the "mixing-length theory." It was Biermann (1932) who first applied Prandtl's mixing-length theory to the stellar interior. To use the mixing-length theory and get numerical answers for the efficiency of the energy transport by convection, you have to know the mixing length. Biermann, however, soon found, very much to his happiness, that if you consider only the deeper interior where the temperatures are really high and where therefore even the slightest motion carries an enormous amount of energy, it does not make any real difference what mixing length you assume within wide limits. The convective efficiency is so high that quite certainly it will establish very nearly the adiabatic temperature gradient. So, to all intents and purposes in the

deep interior, you just stop worrying about the correct characteristics and sizes of the convective elements, and you accept the temperature gradient to be adiabatic for any convective layer.

The situation turned out nowhere near so happy when astronomers started to study the outer convection zone near the surface of a star. They found that in the subphotospheric layers the temperatures and densities were not high enough to make the convection very effective. In consequence, the temperature gradient will not be close to the adiabatic one, and you have to know the effective mixing length fairly accurately to get the actual temperature gradient in these layers. The situation became even more nasty when it was found that the radius of a stellar model can depend very sensitively on just this subphotospheric temperature gradient.

Now you may say: Professor Prandtl developed the mixing-length theory on the basis of laboratory experiments; could you not find out from the same experiments what determines the size of the turbulent elements (which is practically the same as the mixing length)? In the laboratory it turns out that whenever you take a vessel containing a fluid and heat it from below so that you get heat convection, the average size of the moving elements is determined by the depth of the convecting layer. So the first thought of Unsöld and Biermann was that in the outer layer of a star the depth of the convection zone would determine the size of the convective elements. This seemed to fit the observations on the sun gloriously (by the accidental compensation of two mistakes, as we now realize). The depth of the convection zone in the sun was at that time estimated to be a few hundred kilometers and the granules—the visible tops of the hot, rising convection elements—were observed to have diameters of the order of 1000 km. So the depth of the layer and the size of the granules seemed to fit satisfactorily, and everybody was happy.

Only as the theory was further developed did it become entirely clear that the depth of the solar convection zone had been greatly underestimated, that it was much closer to 100000 km than to 1000 km, and that, therefore, something was completely wrong in the comparison between the depth of the convection zone and the size of the granules. It seemed to us at that time (Richardson and Schwarzschild 1950) that the trouble might well come from one very big difference between the solar convection zone and the laboratory experiments. In the laboratory the density of the fluid used is practically constant throughout the convective layer. In the sun, however, the density increases by many orders of magnitude from the top to the bottom of the convection zone. This basic difference has the following consequence. If in the sun a convective element had a size equal to the depth of the layer—as suggested by the laboratory experiments—its internal motions would have to have a very peculiar and unlikely pattern; the slightest motions of the dense gases near the bottom would necessitate (according to the continuity equation) enormously fast motions of the thin gases near the top. Instead, it appears much more likely that the typical flow pattern of a convective element will extend only over that depth in which the density varies only moderately, say by one factor e , so that the speed of the motions do not have to vary too seriously throughout the element. If this is correct the size of the convective elements should be governed by the depth in which the density varies by a factor e , i.e., the density scale height.

The question then is: Does this new picture for the size of the turbulent elements fit the solar observations? At the time that these speculations were made, our picture of the outer layers of the sun was obviously still quite uncertain, and I estimated that the density scale height in the relevant layers might be as low as 150 km, suggesting diameters of granules of the same order of magnitude. The observations at that time, however, gave granule sizes of the order of 1000 km. So there was now a discrepancy in the opposite direction from before. After some experimenting we became more and more suspicious that the trouble lay in the insufficient resolution of the available photographs of the surface of the sun. The best resolution then attained corresponded to about 700

km on the sun, clearly inadequate for our problem and equally clearly hard to improve upon, since atmospheric seeing—not instrumental faults—was the main trouble. So it seemed that if one really wanted to settle the problem of the sizes of the granules, one would have to try to get photographs of the surface of the sun from very high altitudes, where the seeing does not limit the definition. It was then that we started wondering about solar photographs from stratospheric balloons.

It is a bewildering experience if one suddenly has the choice of either stopping a line of research that one has been following or of going at it with an entirely untried new technique. The choice—between the thrill of the new and the fear of failure—is not easy. Nor is it made easier by the realization that these new techniques require large public funds; others may have the responsibility of assigning you the funds, but you are left with the responsibility of making the new experiment succeed. Whichever way you eventually decide, you can count on an awful lot of advice in such a situation, from the extreme of those who tell you that your experiments just won't work to the other extreme of those who call you a reactionary if you don't try.

As you know, in Princeton we did go into Project Stratoscope. This was made possible mainly by the sponsorship of the Office of Naval Research and the National Science Foundation. As you may also know, we did fly a 12-inch telescope at 80000 feet on a total of six flights in two seasons, during which photographs of the solar surface of high definition and in sufficient number for our purposes were obtained. The credit for these photographs should go to an extremely capable and energetic group of engineers in a number of firms with which we had contracts, to my young colleagues in the balloon group in Princeton, and to Professor Spitzer, without whose enthusiasm and persistent help I would not have had the courage to go into this undertaking.

From the photographs which we did obtain we derived two numbers, if you want to pare it all down to the barest essentials; one is the average diameter of the granules, and the other is the average lifetime of the granules. The average diameter (defined as twice the distance over which the autocorrelation of the photospheric intensity fluctuations drops to half) was published nearly two years ago after the early flights (Schwarzschild 1959). It turned out to be about 700 km. It was, of course, embarrassing for me that this value came out closer to the 1000 km that the ground observers had estimated earlier than to the 150 km that I had enthusiastically predicted. But perhaps the more important point is that the new value is based on plates with a resolution limit of about 300 km, while the earlier value had to be based on plates with a resolution limit probably near 700 km. The second main number we obtained is the average lifetime of a granule (defined as twice the time interval in which the autocorrelation of the photospheric intensity fluctuations drops to half). It came out to be about 8 minutes (Bahng and Schwarzschild 1960).

How do these two numbers now fit the picture of the mixing-length theory if we assume that the mixing length (or average size of the elements) is determined by the density scale height? A new value for this scale height had, in the meantime, become available through a detailed analysis of the solar subphotospheric layers by Mrs. Böhm-Vitense (Vitense 1953). The new value is 300 km, about twice what I had assumed earlier. Now picture a convective element of 700 km (observed granule size) in horizontal diameter and 300 km (density scale height) in depth, with hot gases rising up in the middle and cool gases flowing down all around the circumference, and you have a flow pattern of a geometry quite commonly encountered in laboratory convection experiments—a satisfactory status.

Then, regarding the lifetimes of granules, laboratory experiments indicate that in violent non-stationary convection the average lifetime of the convection elements is of the order of the turnover time, i.e., the time which the gases from the bottom of the element take to reach the top and vice versa. If you accept for the granules our new dimensions and use the spectroscopically observed velocities, you get for the turnover

time of a typical granule about 5 minutes. This value is in satisfactory agreement with the observed 8 minutes, in view of the uncertainties involved.

To summarize, the two new observed values in no sense prove that the mixing-length theory combined with the density scale height is correct. They seem to me, however, very encouraging for the idea of using the mixing-length theory combined with the density scale height for our purposes until a more accurate theory for convective energy transport has been developed.

If we follow this idea, quite some work on stellar models, particularly in the red giant region, may have to be done over again. Not that the mixing-length theory had not been applied to these models, but the mixing length had been connected with the pressure scale height instead of with the density scale height. Until recently we had assumed that these two heights would be reasonably equal. New integrations have, however, shown that they can be very different, particularly in red giant envelopes. I am fairly convinced that in these cases the density scale height and not the pressure scale height is the correct choice, since it is the former which (through the continuity equation) affects the geometry of the flow, just as the depth of the vessel does in the laboratory.

So much for the problem of convection as an energy-transport mechanism. There are some other consequences of the convection in the outer layers of stars that seem to have important consequences. First, there is the effect of the convective motions on the spectroscopic lines which we observe in the spectra of the stars, and, second, there is the effect of the convection in the photospheric layers on the higher layers, i.e., the chromospheres and the coronas of the stars. To investigate these effects, you have to learn quite a bit more about the character of the convection than the mixing-length theory in its present form will give.

One method that in recent years has gained appreciably in popularity, and I think with good reason, is the so-called laminar-mode method. This method proceeds in the following way. Imagine that you know the average structure of the convective layer, that is, the average density and the average temperature for every level from top to bottom of the convective zone. Now ask: If there were no other motions, how would a single convective element look? To answer this question, you can simplify the equations enormously by considering only the linear terms in them. In this manner you get the whole set of laminar modes for any particular convective situation. Then imagine that the true convective motions can be described by a superposition of the laminar modes. This does not say that you ignore the effects of the interactions of all these modes on one another. It only says that you believe that the over-all field of motion can be described as a superposition of the laminar modes. I would like to describe two applications of this method.

Recently Dr. Ledoux and Dr. Spiegel and myself (1961) applied this method to try to get an idea about the distribution of sizes of convective elements on the sun. Thus far I had discussed only the average size of the granules. But it appears important for the other problems I mentioned to find what the distribution of element sizes should be. We applied the laminar-mode method in combination with Heisenberg's estimate of the interactions between the modes in a not conclusive but, I hope, constructive fashion and obtained the distribution of element sizes in terms of the usual turbulence spectrum. The one result from this investigation that may be of immediate interest is a striking difference between the turbulence spectrum for the convective velocities and that for the temperature fluctuations. The two spectra have their maxima at the same element size (presumably determined by the density scale height). Toward smaller sizes, however, the velocity spectrum declines slowly, as in the well-known Kolmogoroff spectrum, whereas the temperature spectrum drops steeply. This means that the smaller elements contain appreciable velocities but show very little temperature fluctuation. Physically the reason is clear, as emphasized by Spiegel (1957); small elements in the solar photosphere are so transparent that they cannot hold on to any temperature excess over the

average surrounding but will lose their initial temperature excess by radiation in a time short compared to their lifetime. In this process, however, they do not lose their velocities.

Observationally, this phenomenon has the following consequence. From our rough calculations it appears that convective elements in the solar photosphere with diameters smaller than 300 km have negligible temperature fluctuations. Now the temperature determines the surface brightness. Accordingly, on photographs of the solar surface, however high the resolution, you should not expect to see elements smaller than the smallest shown on our balloon photographs, which have a resolution limit of just about 300 km. I must admit, though, that this type of prediction is a dangerous one to make, since one is clearly suspect of underestimating the possible value of further improvements over that one has just accomplished.

Regarding the solar convective velocities, on the other hand, our calculations suggest that if you want to observe these velocities by Doppler shift measurement of granules and if you want to get more than half the mean-square velocity, you have to have a resolution better than 300 km—a tough spectroscopic experiment indeed, even from balloons.

Next I would like to report on another application of the method of laminar modes. This application was carried out by Dr. Unno (1957). He was interested in the problem of how the convective motions in the subphotospheric layers of the sun affect the absorption lines in the solar spectrum. Formally the convective instability stops at a level with an optical depth of about 1. Convection rules below and radiative equilibrium above this level. The absorption lines in the solar spectrum are formed in the stable upper layer. This layer is terribly thin, and it is clearly fictitious to imagine that this layer, just because it would be quiescent if left by itself, can lie there quietly while enormously fast turbulent motions occur below. The question, then, is how far the convective motion from below penetrates into the officially stable layer above. In this question Dr. Unno felt—and I am convinced he was right—that good insight could be gained by studying the flow patterns of the dominant laminar modes for a convectively unstable layer bounded above by a stable layer. He carried through the analysis and found that for the case of the sun a penetration of the convective motions into the stable layer up to a height between 100–300 km could be expected.

Next, Dr. Unno (1959*a*)—following a method suggested by Dr. Goldberg—analyzed certain solar lines and got, so to say, an empirical determination of the average convective velocity as a function of height in the atmosphere, which you can compare now with the theoretical expectations. The results are shown in Figure 1. At the bottom of the figure, height is indicated in kilometers, with the subphotospheric layers at the left, the photosphere in the middle, and the lower chromosphere to the right. Optical depths are shown on a separate scale below. The dots in the middle of the figure represent Dr. Unno's empirical determinations of the average velocity as a function of height in the reversing layer. They show clearly—as had been known before—that the convective motions of the unstable layer below optical depth 1 penetrate into the stable layers above. The indicated penetration height is of the order of 100 km, in satisfactory agreement with the theoretical estimates. I have drawn a curve through the empirical points and extended it downward (to the left in the figure), following the theoretical analysis of Mrs. Böhm-Vitense. The upper empirical and lower theoretical data could be fitted together without doing violence to either. I rather think that this combined curve may be a not-so-bad approximation to the actual convective velocities in the sun.

But then, in the higher layers shown at the right of the figure, something new and wild must be happening. The crosses, which represent Dr. Unno's determinations for the lower chromosphere (1959*b*), indicate relatively high velocities, and, furthermore, we know from eclipse data that the velocities in the upper chromosphere are still higher. If these velocities are motions of a convective type, we must be completely misunderstand-

ing the situation. But there is a good chance that these motions are something quite different, namely, progressive waves, either simply hydrodynamic ones (sound waves) or hydromagnetic ones (Alfvén waves). Indeed, Alfvén, Biermann, and myself suggested earlier that the turbulence in the convective region should produce progressive waves and that a portion of these waves should travel outward and could become the main energy source for keeping the chromosphere and the corona hot.

It is therefore extremely tempting to draw a curve through these chromospheric points as I have done in Figure 1, quite independently from the convection curve, and to believe that this curve represents progressive waves going outward. That the velocity of the waves should increase outward follows automatically, because progressive waves when they pass through thinner and thinner gases will increase in their velocity amplitudes. If this interpretation is right, then in the photosphere of the sun the main velocity

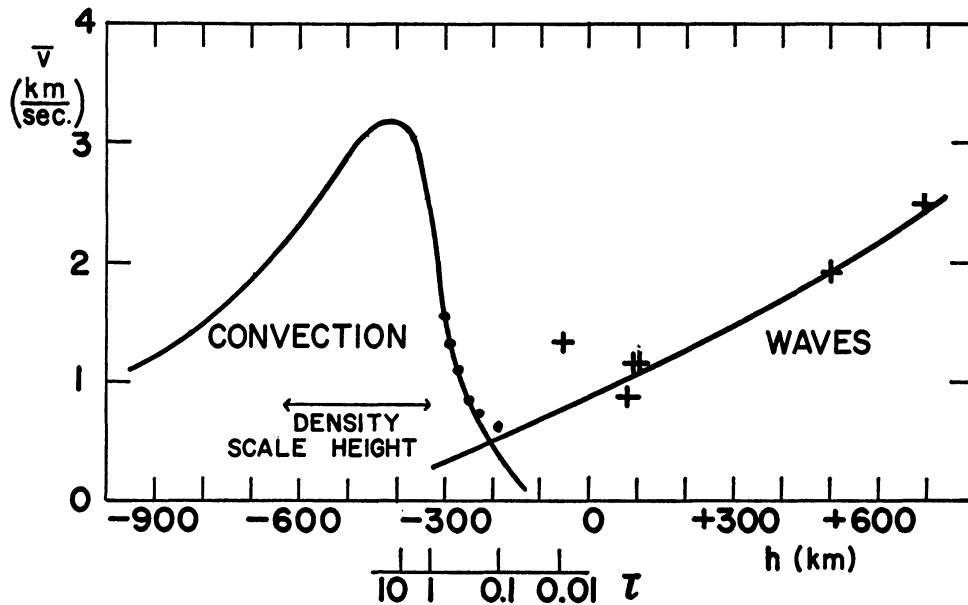


FIG. 1.—Average random velocity as a function of height in the solar atmosphere

(amounting to 1–2 km/sec) is caused by turbulent convection. But there exists also a very weak component in the photospheric velocities which, according to this picture, is caused by progressive waves and which amounts to at most 0.3 km/sec in the photosphere and still less in the subphotospheric layers. If, in fact, the turbulence here can produce waves with material velocities of this amount—small compared with the convection, but not ignorable—which then travel outward and grow in amplitude, we have a source of energy amply sufficient for the chromosphere and corona. Indeed, the energy flux carried by these upward-going waves would amount to about one-thousandth of the whole luminosity of the sun, and that is a lot more than we need for the chromosphere and corona. But it is always nice to start with something too much, so that we can be wasteful afterward.

The question then is: Can the convection in fact emit this much hydromagnetic or hydrodynamic noise? Several astronomers have applied a theory developed by Lighthill and Proudman to this astronomical problem. The theory gives an estimate of the flux of waves emitted by turbulence. If you apply this theory to the solar case, you get for the photosphere not the present 0.3 km/sec but rather less, perhaps by as much as a factor of 10. However, the theory is based on appreciable idealizations which may not

be really justified in our case. So I think that this discrepancy should not be taken too seriously at this time. It seems to me that the problem of wave emission by turbulent convection is getting ripe for a more detailed attack. When we really understand the dynamics of the solar photosphere and chromosphere by such an analysis, I think we will have made an enormous step ahead toward the understanding of the more violent phenomena in the atmospheres of red giants.

At the moment it might be particularly interesting to look into the problem of waves emitted by the solar convection in those areas in which there are large-scale magnetic fields. The problem is, I think, another example of those that can be approached by the laminar-mode method. In the hydromagnetic case of wave emission from convection, even this simple method leads to thoroughly nasty equations. However, nowadays such complications are no longer a serious stumbling block. Just as on the observational side the balloons have made possible such an encouraging step ahead, on the theoretical side large computers permit us to go into much more complicated problems than before without worrying. I feel extremely confident with regard to the development in this area.

The research we have discussed here would in large part not have been possible, if it had not been for some entirely new tools now available to astronomers, such as balloons and large computers. But balloons and computers are only samples of a fantastic array of new techniques that is just getting into our hands. The only problem I think will be whether we shall be bright enough and dedicated enough to make use of these new tools effectively and responsibly.

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