PART III.

Spherically-Symmetric Motions in Stellar Atmospheres.

B. - The Propagation of a Shock-wave in an Atmosphere of Varying Density.

Discussion.

Chairman: M. KROOK

— L. BIERMANN:

A comment on several points made by SCHATZMAN. First, as regards the energy balance of the chromosphere and corona, it is essential to take account of the energy necessary to maintain the corpuscular radiation as well as the optical radiation. The energy in this corpuscular radiation is of the order 10^5 erg cm^{-2}, which is comparable with that from the corona by optical radiation. Second is a point discussed by LÜST and myself several years ago; *viz.*, all stars having hydrogen convection zones, where occur velocities of the order a few km/s, must be expected to possess chromospheres and coronas. But for supergiants with large radii, the velocity of escape is much less than for the sun, and possibly these stars have only chromospheres, not coronas. It is known from the general properties of the mechanism of radiation loss that there is a sharp transition between the chromosphere, with T of the order 10^4, and the corona, with T of the order 10^6. Thus, a supergiant with escape velocity 20 km/s or so cannot retain a corona with thermal velocity 200 km/s or so. Third, LÜST and myself have reached the conclusion that until one reaches the chromosphere-corona interface, one cannot — except within sunspots — expect to have Alfvén waves; in the low levels of the chromosphere one has practically exclusively sound waves. Fourth, on the relative importance of ambipolar diffusion — again studied by LÜST and myself and by LEHNERT — we have both agreed that Piddington's conclusion is not really sound, that outside the spots the contribution to ambipolar diffusion is not really essential. Fifth, in discussing the evolution of the noise from granulation into shock-waves, the possible influence of a chromospheric magnetic field must be taken into account. Sixth, it is not clear to me how a shock with the small relative amplitude suggested by SCHATZMAN could produce enough dissipation to maintain the energy balance; it seems to me necessary to have material velocities near sonic.

— R. LÜST:

First, two comments on the acoustic noise generation, which in the astronomical literature — following the work by LIGHTHILL and by PROUDMAN (*Proc. Roy. Soc. Lond.*, 1952, A **211**, 564; A **214**, 119) — is usually taken proportional to M^5. KULSRUD (*Ap. J.*, 1955, **121**, 461) has investigated the effect of the presence of magnetic fields, and finds that magnetic turbulence increases considerably the generation of sound. Provided the magnetic pressure is less than the gas pressure, no Alfvén waves will be generated. On the other hand, note that the M^5 law rests on a discussion of isotropic turbulence. But in the top layers of the hydrogen convection zone, we probably do not have isotropic turbulence — the turbulence element will expand and move outward. We might expect the dependence on M to become somewhat less. This is a question for the aerodynamicists, the question of the generation of acoustic noise in the presence of a density gradient. Second, consider the question of wave propagation in a magnetic field. We have investigated — results are as yet incomplete — an atmosphere with an outward density gradient and a vertical, constant magnetic field. In the lower layers, we took gas pressure large compared to a magnetic pressure; and in the top layers, the reverse. We assumed cylindrical symmetry, and investigated how an initial pressure impulse propagates outward. We introduced an artificial viscosity into the wave so as to be able to treat shock-waves. One finds indeed that an acoustic wave is guided by a magnetic field, so that the propagation of the wave is preferentially along magnetic lines of force. The difficulties inhibiting the completion of the work are those of boundary conditions at the top of the layer, already discussed at the last session. We made several assumptions to avoid incoming waves; but always in the calculations we found instabilities occurring at the top layers, and these ultimately travel inwards. It is not yet clear to us whether these instabilities are due to assumed incorrect boundary conditions, or if there are really instabilities. So the problem has not been solved, but there appear to be good indications that a magnetic field is able to guide an acoustic wave preferentially along the magnetic line of force. This was our intent, to see if one could interpret solar spicules as arising from such an effect, noting that the spicules are very often tilted in much the same way as coronal rays and that one likes to identify the coronal ray tilt with that of the magnetic lines of force in the solar field.

— E. N. PARKER:

Let me present some ideas on the dissipation of transverse hydromagnetic waves in the solar corona. If we believe in the continual hydrodynamic expansion of the solar corona, then not only is the rate of heating larger, 10^{28} erg/s, than previously estimated considering only radiation and conduction losses,

10^{27} erg/s, but the heating must take place out to distances of several solar radii. The question is whether such extended heating is plausible.

Let us assume that by some means, such as the convection zone and/or the spicules, there are generated at low levels 10^{28} erg/s in hydromagnetic waves, in the general one gauss field of the sun. Below the corona the gas pressure p is very large compared to the magnetic pressure $B^2/8\pi$, of the general solar magnetic field. Thus, for transverse waves with an amplitude ΔB comparable to B, the motion will be essentially incompressible. The transverse incompressible wave will propagate slowly $(B/\sqrt{4\pi\varrho} \ll p/\varrho)$ out along the magnetic lines of force, which presumably extend approximately in the radial direction. Under such conditions the propagation is without dispersion. The angular frequency ω of the waves is of the order of $(10^{-2} \div 10^{-4})$ rad/s, so that the wavelengths are within a factor of ten of $(10^2 \div 10^3)$ km. Dissipation due to viscosity and resistivity is negligible.

But as the waves propagate higher in the solar atmosphere, the gas pressure — which is decreasing rapidly — becomes comparable to the magnetic pressure. The medium becomes compressible. It was shown sometime ago by perturbation methods (PARKER, 1958, *Phys. Rev.*) and more recently by reduction to a Riemann-type analysis (MONTGOMERY, 1960, *Phys. Rev. Lett.*) that the hitherto transverse wave will develop a longitudinal or compressible aspect and will rapidly steepen its front. The steepening proceeds without limit, so that eventually some sort of dissipation — resistivity, plasma instability, phase mixing, etc. — must occur, with the result that the energy originally contained in the purely transverse wave is fed into thermal motions.

Note then that if the dissipation and heating of the atmosphere should become so rapid that the gas pressure becomes much larger than $B^2/8\pi$, then the steepening of the transverse wave, and the energy dissipation, will slow down. Thus, the dissipation mechanism is self-regulating. Given a large flux of transverse hydromagnetic waves propagating up into the solar atmosphere along the radial lines of force of the general solar field, the temperature of the gas will be elevated by the dissipation of the waves to the point that p is of the same order as $B^2/8\pi$, *i.e.*, the speed of sound will become comparable to the Alfvén velocity. This relation,

$$p = o\,(B^2/8\pi)\,,$$

will be maintained for as far out in the solar atmosphere as the hydromagnetic wave flux can hold up. In terms of the temperature T and number density $N(\varrho = 2NkT)$ we have

$$T = o\left(\frac{B^2}{16Nk}\right),$$

which is in rough agreement with observations. For instance, one gauss at the photosphere extending radially outward yields 0.6 G at an altitude of $3 \cdot 10^5$ km, where $N \simeq 10^8/\text{cm}^3$. We compute, then $T = o(1.5 \cdot 10^6\,^\circ\text{K})$. We obtain slightly lower temperatures, lower down, and slightly higher temperatures (up to $3 \cdot 10^6\,^\circ$K) farther out.

We suggest that the outer corona is heated principally by the dissipation of initially transverse hydromagnetic waves, so that $p \simeq B^2/8\pi$ determines the coronal temperature of the sun. We call this regulated heating the *Mach one effect*, which we have already suggested to be operative in the interstellar generated of cosmic rays (PARKER, 1958).

— F. KAHN:

Why does the wave steepen?

— E. N. PARKER:

A quick and purely physical picture is that the velocity of propagation is most rapid where the field is strongest. Let me recall to your minds that the condition which prevails in a transverse hydromagnetic wave is that the total pressure is constant. Therefore, the gas pressure and density must be lower where the field is stronger and that part of the wave propagates faster than the part where the field is weaker and the pressure and density higher.

— A. J. DEUTSCH:

In connection with Biermann's interesting suggestion that there may be giant stars having chromospheres but not coronas, I wonder if escape velocities as low as 20 km/s are possible? For a star of one solar mass to have such, its radius must be 900 times that of the sun. Escape velocities of 100 km/s are possible, possibly 50, but lower than 50 is, I think, impossible.

Second, I would like to ask the following questions in connection with some very qualitative ideas about the nature of the flow processes in the late-type giants, details of which I will discuss in tomorrow's session. Here, one deals with stars having radii several hundred times the solar radius. One knows that matter is streaming out of these stars with velocities of the order of 10 km/s (cf. Table I of Deutsch's talk).

The gas, where we observe it, has an excitation temperature indistinguishable from zero, an ionization temperature which is very low; and a kinetic temperature which cannot be very high. We may call this a corona if we wish, it certaintly is very different from the solar corona.

We also know that these stars have emission features which suggest to us that there is a chromosphere. Now we find, by studying the dynamics, that in order for us to understand the flow at all, we must consider either very high velocities of ejection — which we do not observe — or a very high temperature region — which we also do not observe. Let us suppose, however, that there is a high temperature region near the reversing layer. The temperature need not be as high as 10^6°, but it probably will have to be over 10^5°. This region is presumably unobserved. I observe the lines produced at greater distances where the gas has cooled off and slowed down.

Is it not reasonable to suppose that the physical mechanism operating here is the following? Due ultimately to convective processes well below the photosphere, acoustic disturbances are set up which then propagate outwards through the reversing layer into the chromosphere, dissipating energy as they go and heating the gas — much the same picture we have for the solar chromosphere and corona. When we reach a sufficiently high level in the « corona », we find that the inhomogeneities due to the acoustic disturbances and shocks are pretty well evened out, and we are left with a medium which is nearly homogeneous, having a high temperature, and having some net velocity outwards. Shortly after we reach that point, we come into the regime where we can observe the gas. Is this a physically realistic picture? If so, then in order to understand it in a more quantitative way, it would be appropriate to formulate these specific gas dynamical problems. First, I would like to consider a one-dimensional problem: I will consider a gas which may have a temperature gradient, and which may have a gravitational field going through it in the « x » direction. Let us suppose that at $t = 0$, I give to a certain slab of gas a certain initial velocity which will be prescribed, and then let it go; I simply assert that at $t = 0$, I know the velocity and the mass contained in this slab. And I ask for the subsequent motion of the gas, both in front of the slab, after it has been started on its way, and behind the slab. The particular question which I would like to have answered, because it is one that's relevant to the problem that I have been discussing, is this: as a result of the impulsive motion, how many g/cm² of matter will flow through a surface which is well removed from the slab? This effectively gives me the rate of mass-loss in the plane problem. Of course, eventually I will want to consider the same problem with a spherical geometry, where I give a thin shell an impulsive disturbance and ask, as a result of that impulsive disturbance, how many grams of matter flow per second through a larger sphere? I would also like to ask what are the properties of the velocity field and of the mass transport? Will they depend upon the ratio of energy to momentum in the initial disturbance? It seems to me that they should. The characteristics of the flow and of the mass-loss might well be different, depending upon whether I start with 1 g/cm² moving at Mach 10, or 5 g/cm² moving at Mach 2. It is necessary to ask in what ways this will change the temperature, the velocity field, and the rate of mass transport. These are some of the questions which I wish to put before the hydrodynamicists as probably relevant to the kinds of flows which we have observed.

— F. N. PARKER:

I would like merely to comment that as an alternative to the ideas that Deutsch has expressed — impulsive motions deeper in the star sending ripples of material which go out into infinity — was the picture that I got from Deutsch's observational description. It is possible, using temperatures which are not in disagreement with what one sees in the atmospheres of these giants, to write down a simple spherically symmetrical steady flow out of the giant star. The hydrodynamic flow is similar to the hydrodynamic expansion of the solar corona (cf. Session III, C). The numbers would be quite different, but the solutions are the same analytical character.

— A. J. DEUTSCH:

Apparently I did not make my point clear. I have in mind a model for the flow in the region where one loses sight of the initial disturbances which are responsible for the transport of energy, momentum and mass at the base of what becomes, at some number of stellar radii, essentially a smooth spherically symmetrical flow.

— R. N. THOMAS:

Three points. First, regarding your conjecture that at a sufficiently high level in the outer atmosphere the inhomogeneities associated with the heating mechanism are essentially evened out, note that in the sun, the one star we observe in detail, we have strong inhomogeneities throughout much of the region in which we observe spectral lines. Indeed, in the region where mechanical effects enter, the lower parts appear to be homogeneous from a momentum-input standpoint; from an energetic, one doesn't yet know. But higher, the spicules appear; spike-like columns, moving outward at (10÷100) km/s, transporting enough mass to replace the corona every few hours. One might say that these spicules are the «initial state» postulated by DEUTSCH, rather than a uniform, spherically-symmetric slab. So, my second point is that several years ago I did just what Deutsch asked — only in terms of a limited (in area) block of gas rather than a spherically-symmetric flow — assuming an initial velocity for a column of gas, and asked its configuration (1950, *Ap. Journ.*, **112**, 343; earlier rough model 1948, *Ap. Journ.*, **108**, 130). I actually asked for a steady state, so that the model was essentially that of a supersonic jet in a gravitational field, with high Mach number. So the solution was Prandtl's old solution modified by a gravitational field. I did

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not push the model further, because it seemed to me it neglected the basic physical features that such a problem must include: viz., strong variation in internal energetic degrees of freedom of the gas (I used constant γ), and coupling with radiation field (which I ignored). But if one wants to ask about the flow field in a chromosphere-corona, it seems to me that a spicule field is an equally-likely starting point to a spherically-symmetric one, based on our present knowledge. However, I would like to pass no judgment on whether this rough picture of mine, or the acoustic-wave sharpened by magnetic field picture of Lüst, is preferable. Third, relative to Biermann's comment on stars without corona but with chromosphere, regions of 10^6 and 10^4 °K for the temperature, I would emphasize that the radiative stability arguments underlying this picture would lead also to additional regions having intermediate values of temperature. For the sun, we have evidence for a smooth distribution of T_e up to about $1 \cdot 10^4$; a jump to somewhere $(2 \div 5) \cdot 10^4$; evidence for values of T_e in the range $(0.7 \div 1.5) \cdot 10^5$; and it is not clear that there may not be another region between this last and a value $(0.5 \div 1) \cdot 10^6$. So one doesn't want to look at the outer atmosphere as a rigorously 2-component affair. It just so happens that most of our observations have emphasized the 10^4 and 10^6 °K regions of the outer solar atmosphere so far. I think the Wolf-Rayet stars are a good example to keep in mind, where other regions are emphasized; the solar rocket observations are doing the same for the sun.

— M. KROOK:

I must admit to some confusion as to the relative importance of sound waves and hydromagnetic waves. I hope someone will clarify the position where are the sound waves; to what extent are they inadequate for the heating, and to what extent are hydromagnetic waves needed for this?

— E. SCHATZMAN:

First, I would draw a conclusion from what has been said by BIERMANN, DEUTSCH, and PARKER relative to the giant stars. For the sun, the temperature rise in the outer atmosphere comes only in regions which are optically very thin in the visual continuum. But in the giant stars, it is likely that the dissipation behaves much differently, and we may have a rise of temperature beginning already at optical depth 0.2 in the continuum. This result has been obtained using a phenomenological theory of the velocity of propagation, dynamic pressure of the waves, etc. But I think we should add to that result the observation that if we have energy enough, part of the energy is used to push away material, as in Parker's picture of the corona. So I think that if in the region of optical depth 0.2 we would add the energy used to heat up the material and that to push it away, we could develop the theory to obtain the temperature inversion, the model of the star without a corona, and the mass-loss. Second, on the question raised by KROOK, we have some information from the sun. From Leighton's pictures, for example, we know that we may have certain regions on the solar surface where we observe an increased magnetic field, say 50 G. If we compare such pictures with those of calcium faculae, we find the faculae coinciding with the regions of enhanced field. And there is a sharp transition between these facular regions having field, and non-facular regions showing no field. So, I think we could suggest that outside the faculae, we have heating without effect of the magnetic field; and in the faculae, we have heating including the effect of magnetic field, at heights about 1 000 km above the solar photosphere. That is, we have heating by compression waves outside the faculae; while in the faculae, we have to include dissipation by transverse waves. Finally, note that it is only at very great heights, some 2 solar radii, that we might expect to have dissipation possible *only* by transverse waves, because here the mean free path is too large for dissipation by longitudinal waves.

— R. LÜST:

I only emphasize that to get this damping for the Alfvén wave, you have to be in a region where the matter is only partly ionized; for without some neutral gas, the conductivity is not sufficient to make significant damping. The only mechanism that can really contribute is the ambipolar diffusion between neutral and ionized component.

— E. N. PARKER:

I think you are overlooking a lot of possibilities, such as plasma instabilities.

— R. LÜST:

Agreed, there are certainly other possibilities, not yet completely understood, which would contribute to the damping. But I refer just to the Piddington mechanism, on which this necessity of a neutral component is a severe condition.

— C. DE JAGER:

I think it is clear that the facular regions are heated in the photosphere, above optical depth 1; but I wonder if we really can be sure they are heated in the chromosphere? They are a bit brighter in H_α and Ca II, but this does not necessarily mean they are heated more than is the surrouding region. Second, the point was made that the hottest stars should not have coronas because they do not have convection zones. This would follow if the corona in these stars arose from turbulence generated by convection. But according to Miss UNDERHILL, we observe turbulence in these stars. How it is generated, we do not know; but if it exists, I think it should produce a certain mechanical flux, thus could give a corona. Third, agreed that Alfvén waves can only be generated in regions where magnetic pressure exceeds gas pressure — thus probably only in sunspot regions — there is still a factor not yet mentioned. Such waves can be reflected, in a region of decreasing density. If we compute to see what happens to these waves generated in sunspot regions, we find the greater part reflected backwards. What happens then? The only thing I can imagine is that they are transformed into acoustic turbulence, which leads to heating of the lower parts of the spot. I would pose this problem to the aerodynamicists.

— A. A. BLANK:

There are three speeds of propagation of hydromagnetic waves. Why is the discussion confined to Alfvén waves; have you thought about the possibility that the propagation could be a more general variety? Even if one begins with a compressionless wave, a pure Alfvén wave generated in the incompressible core, as soon as the wave reaches a higher level where compression is possible, the energy will be propagated in all the available modes. This is not a mere possibility; there is a proof by Grad that, in general, the three modes cannot propagate independently through a compressible medium.

— M. KROOK:

I think one also wants to distinguish the case of a large amplitude disturbance where the resolution and propagation of individual small amplitude modes does not have any direct meaning.

— W. B. THOMPSON:

In particular, these things may happen in the corona where the hydrodynamic picture is questionable because of the long mean-free-path. There are a number of processes, not described by hydrodynamics, which might be important, especially non-collisional damping (Landau damping) which might be great enough to beat any steepening edge.

— A. UNSÖLD:

A remark from the observational viewpoint, relative to velocity fields at the chromosphere-corona boundary. We talked at various times about the spicule structure of the chromosphere; about these spikes extending some 10 000 km high and moving at some 20 km/s.

These spicules are sometimes quite similar to small prominences; and, in fact, long before one spoke about spicules, one talked about small prominences. It happens quite frequently that they assume more complicated structures and move off with higher velocities. From the spicules there is quite a continuous transition into so-called rising eruptive prominences — large masses of gas apparently of cooler temperatures up in the corona which move with very large velocities and sometimes quite suddenly speed up by hundreds of km/s. I remark that in order to remind also the aerodynamicists that there exists in the outer part of the solar atmosphere a mechanism which is able to accelerate considerable masses of gas suddenly and to very high velocities. It is generally assumed that it is connected with magnetic fields.

— M. KROOK:

Would someone sketch a quick picture of the characteristics of convection zone, photosphere, chromosphere, corona, to give a quick picture of mechanisms of heating and types of motion.

— L. BIERMANN:

Photosphere outside spot. – We have turbulence of the order 1 km/s. My own position has been that this is much nearer to turbulence in the aerodynamical sense than to some convection of the Benard type. Densities of the order 10^{17} atoms/cm^3.

Photosphere near spot. – The radiative flow is suppressed to a considerable amount — only $(20 \div 30)\%$ remains. But photometry shows that practically all the radiation that « disappears » in a spot comes up in a ring around the spot. If you take a total area of spot plus a ring, 15% or so of the radius, around it, then you get practically the same energy as in the non-spot region. So you get the picture of energy being diverted to the sides; it's just a question of how the magnetic field acts in diverting the energy flow beneath the spot. Twenty years ago, it appeared that the material in a spot was essentially quiescent; but now it appears that there are motions of several km/s there. The observational indications suggest a different type of motion inside and outside the spot, even though the velocities are the same size.

Chromosphere. – There is a transition region of roughly 300 km between photosphere and chromosphere; and the chromosphere extends up to some 10 000 km. We have velocities that are observed, statistically, to increase upwards from a few to about 20 km/s. There we have velocity fields of a certain scale, structure, and energy, which are connected with the superposition of sound waves developing into shock waves. On the other hand, THOMAS and colleagues tried many years ago to put forward a picture, starting with the observed properties of spicules at higher levels, and assuming higher velocities of 50 km/s or so at lower levels. To us there is a difficulty seeing how one gets these high velocities at lower levels. LÜST has already described our attempts to produce spicules as a consequence of the action of magnetic fields on the acoustic waves.

Corona. – The transition from the upper chromosphere to corona, from a few times 10^4 °K to a few times 10^6, is fairly steep. The corona is essentially isothermal, beginning at the level of $(10\,000 \div 20\,000)$ km. Thus, we have a transition from about $(20\,000 \div 30\,000)$ °K to $(1 \div 2) \cdot 10^6$ in less than 10 000 km. In the corona, there is one serious question: up to now, one has no real evidence of mass motions of the order of the sound velocity there, some 200 km/s. These would be difficult to observe; and observations of line-profiles are not incompatible with their presence; but the question is still open. Theoretically, we would expect them, in order to get the necessary dissipation effects. Of course, in connection with work on plasma physics, we are just beginning to learn about a variety of instabilities; and one may indeed have the result that he obtains much more dissipation than expected on the basis of pure aerodynamics.

(*Ed. note*: It should be noted that the T_e structure of the chromosphere-corona is presently violently controversial. In the regions called coronal by BIERMANN, values between $(0.5 \div 3) \cdot 10^6$ °K are variously given. Arguments for T_e in the 10^5 °K range, occurring as low as 4 000 km, between the spicules, have been given. For the coronal discussion, cf. discussion in Section IV, D (SEATON's remarks) of these proceedings and the *Proceedings of the* 1960 *Meudon Colloquium on Stellar Atmospheres* (*Ann. d'Ap.*, **23**, 807 (1960)). For the chromosphere-corona transition, cf. *Physics of the Solar Chromosphere*, THOMAS and ATHAY, 1961.)