PART III.

Spherically-Symmetric Motions in Stellar Atmospheres.

C. - Non-Catastrophic Mass-loss from Stars.

I.

Discussion.

Chairman: C. DE JAGER

(*Ed. Note*: This discussion spread over one and a half days, mainly stemming from Deutsch's emphasis on possible stellar mass-loss at a steady flow less than the escape velocity, Parker's emphasis on the « solar wind » as the very high speed mass-ejection mechanism for the sun, and the feeling of the aerodynamicists that the problems could be combined and generalized as a simulation of the diverging-converging nozzle-flow problem. So in editing, parts of the discussion have bee re-ordered, for greater continuity. Following the Chairman's original schedule, stellar problems come first, then solar. Two summarizing presentations from the nozzle-flow standpoint were presented—one by CLAUSER, one by GERMAIN. Germain's presentation is used as the initiating point of re-discussion of several aspects in this transcription; CLAUSER has expanded his remarks and published them as a contribution from the Johns Hopkins University Department of Mechanics—AFOSR TN 60-1386 Nowember 1960, so they are not included here.)

— A. UNDERHILL:

I would like to point out some inconsistencies in Deutsch's table. For the M-type giants, DEUTSCH has reported some very beautiful and detailed observations from which one can deduce that the flow at large distances is larger than the escape velocity at the same place. Very sensitive and intricate interpretations allow one to infer somewhat similar facts for the sun. In the case of the stars, rather crude and general arguments are given; and if one examines Deutsch's figures for the Wolf-Rayet and Be-type stars, one sees that the flow velocity listed is in most cases somewhat smaller than the escape velocity. We have heard that arguments based on evolutionary considerations favor values for mass-losses of the order of 10 times larger than those derived from these observations. I believe that this factor of 10 can be made up quite

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easily, simply by altering the distance from the stellar surface at which the material has motions, and the size of the motions themselves.

Take the case of the Wolf-Rayet star HD 192103, spectral class WC 7. As fas as I can tell from spectroscopic observations, this star appears to have a single spectrum, and the He II lines λ 4686, λ 3203 (from level 3), λ 5411. λ 4541 (from level 4), and λ 6527 (from level 5) all have the same total half-width, which in the case of this star is of the order of 1 000 km/s: this implies a root-mean-square velocity of roughly (500 ÷ 600) km/s. These are all emission lines. In this star one observes strongly displaced He I absorption in the lines λ 3888 and λ 3186, which originate from the metastable $2\,{}^3S$ level; these are the only strong absorption lines in the spectrum. Under conditions of high temperature and moderate geometric dilution (*i.e.* at a distance of the order of 5 stellar radii) this level becomes strongly populated. A simple uniformly expanding sphere cannot be used to explain the He II profiles, one of the difficulties with the interpretation of Wolf-Rayet spectra being that the part of a strong line projected on the disk should be self-reversed, upon this theory, thus causing absorption on the violet side of the line, but in fact no such absorption is observed. The significance of this was recognized 30 years ago. Thus, it is significant that strong absorption only occurs for spectral lines which are strengthened when the material is far from the stellar surface, and that these lines show velocities of expansion of the order of 1200 km/s. I think the intrinsically strong line λ 4686 does have some self-absorption (as it should have, since it's formed in a moderately dense region), but this self-absorption is such that it is all over the profile.

So the picture is that the Wolf-Rayet stars are composed of an inner atmosphere, which forms the emission spectrum chiefly, and an outer region at quite a distance from the star, which usually doesn't possess enough material to produce emission features, although it does produce certain absorption lines. This outer shell gives observational evidence for a definite expansion.

I repeat that most of the emission features in a Wolf-Rayet spectrum come from parts of the atmosphere where the motion is not uniformly directed. For example, the line λ 5696 of C III has the only flat-topped profile which I have observed in a Wolf-Rayet spectrum to date, and there is no doubt that in HD 192103 this profile has a width of close to 2 000 km/s, whereas the strong He I lines in this star have widths of no more than about 1 000 km/s. So I am saying that an expanding envelope exists outside the regular Wolf-Rayet atmosphere. I think that you can also infer this from some of the binaries.

— G. Elste:

May there be a strong self-absorption at the C III λ 5696 producing the flat-top?

— A. Underhill:

I do not think so. This line is intrinsically a moderately weak line. I think that if there was going to be self-absorption, you would see it first of all in C III 4650 – you do not.

— E. Spiegel:

Could you just remind us of the evidence from the Wolf-Rayet stars that are members of binaries, whether it is the case that the shell would be near the critical zero velocity surface, and can one therefore get an idea of the consistency of these ideas of the shell sizes?

— A. Underhill:

I do not remember offhand the sizes of the orbit and stars. What I remember about *V444 Cygni* is that the major part of the emission lines must be formed reasonably close to the stellar surface. You see you have a light curve and you have an eclipse; to interpret the details, you must postulate a fairly small nucleus which gives your continuous spectrum—another region that is reasonably dense—and then an extremely large region in which electron scattering dominates. Now I think this outer region probably produces these special features I have been describing.

— Mrs. Burbidge:

I would like to make a quite different argument for the existence of mass-loss in WR stars and in particular this star *V444 Cygni*. If the WR stars are massive stars, one might argue from the appearance of their spectrum that they appear to be at a late evolutionary stage. Some WR stars have strong carbon and some strong nitrogen in their spectra. Although it is difficult to say anything about real abundances in the atmospheres, because obviously departures from LTE are very important in atmospheres like this, yet the existence of carbon and nitrogen fit in well with our ideas of nucleogenesis in the interior of stars and stellar evolution. If a massive star has radiated long enough for the hydrogen in its interior to be converted to helium, and the helium core has reached quite a size, we can then get high enough temperature and density in the center for the $3a$ reaction to be triggered. $3\,{}^4\mathrm{He} \rightarrow {}^{12}\mathrm{C}$, 3 helium nuclei go to carbon 12 so one would get some carbon in the interior. Now if there has been some mixing to the surface, we have two possibilities. If the carbon came straight out to the surface, one would have evidence of excess carbon on the surface of the star. Alternatively, if the carbon went through a hydrogen-burning shell, that is, a region where hydrogen is being converted to helium by the carbon-nitrogen-oxygen cycle, then the carbon would largely be converted to nitrogen and one would have a high abundance

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of nitrogen on the surface. Now in *V444 Cygni*, the mass that was determined from the orbit was I think something like 10 solar masses for the WR component, whereas its companion, the O-type star, has a somewhat greater mass—I do not remember the figures but something on the order of 25 solar masses, I believe. Now if the WR star has reached a late evolutionary stage, while the O-type star is still on the Main sequence, how does it come about that the WR star has a lower mass than the O star? Because as DEUTSCH pointed out, the rate of consuming available fuel goes with the 2.5 power of the mass. So, I would just like to put this as a suggestion that at least one member of this binary star has lost a large amount of mass, about 15 solar masses or more in this case.

— C. DE JAGER:

There is really little *direct* evidence that WR stars are surely very old.

— J.-C. PECKER:

There is a good argument in favor of the mass loss of the WR star: the Lagrangian point falls right in the double system *V444 Cygni* at the outer limit of the shell of the WR component.

— B. E. J. PAGEL:

I do not think that it has been mentioned today that essentially there are two kinds of WR stars—at least there are two kinds of stars that show this characteristic spectrum with very broad emission lines. There are the ones that have been discussed this morning, and others which are the cores of certain planetary nebulae and which have a somewhat similar spectrum to the WR stars but probably have a considerably smaller radius, perhaps less than one solar radius. So I should like to ask whether the two groups might be considered to fit in any evolutionary scheme?

— A. UNDERHILL:

I think that the WR stars are considered to be Population I, certainly a good many of them are associated with O and B stars, which everybody considers to be Population I, that is young stars formed in spiral arms. Perhaps DEUTSCH could correct me, but are not the planetary nebulae and therefore the central stars considered to be Population II, an entirely different type of star?

— A. J. DEUTSCH:

I think the answer is yes. In putting this material on the board, I felt it was necessary to make some comments about the present ideas relating to

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stellar evolution. But I think some of this material may be a bit irrelevant to the subject matter of principal interest here, and to the extent that evolutionary questions do not bear directly upon the hydrodynamical problems perhaps you would see fit to dismiss them for the time being and concentrate on the others.

— M. J. SEATON:

I would like to raise some questions concerning where the planetary nuclei fit into this scheme. My first comment is that DEUTSCH referred to the super-novae and the planetary nebulae as both catastrophic cases, and drew a line before going on to WR stars. There I think some distinctions should be made. The super-novae are catastrophic in the most literal sense; presumably the star is completely destroyed. On the other hand, it may be that only one tenth of the mass of a star forms a planetary nebula and that is not catastrophic in the sense of complete destruction of the star. Then one might ask: Is the formation of the planetary nebulae catastrophic in the sense that it is something that takes place suddenly, in the same way that a nova ejects a shell in a more or less violent outburst? Now there are good reasons for believing that the planetary nebulae are not just remnants of novae, but it is often thought that planetaries originate in a sudden outburst. Naturally the question arises as whether such an event has ever been observed. Have we ever seen something rather like a nova that is subsequently identified as a planetary nebula? Of course, one might try to answer this just by taking statistics. We do not observe very many planetary nebulae and we could ask if it is likely that we would have observed one in the stage of sudden formation? I think on the other hand, we have to question whether in fact one must think of the planetary nebulae as being catastrophic events in this sense. It has already just been mentioned that the nuclei of some of the planetary nebulae are very similar to WR stars and these appear to have certain fairly steady rates of ejection. I must, therefore, ask whether perhaps the planetary nebulae might not result from some steady ejection process.

The second question that I would like to raise is where the planetary nuclei fit in the scheme of classification. Now, of course, planetary nebulae are by no means all the same, they cover quite a range of degrees of excitation. Central star temperatures may be determined by a variety of methods all essentially due to ZANSTRA. For low excitation nebulae one obtains $T_s \simeq 5 \cdot 10^4$ degrees and this is the sort of value that one would expect for an O star. On the other hand, if one takes really high excitation planetaries with very strong He II lines, then there is no doubt that star temperatures come at least as high as $25 \cdot 10^4$ and now, of course, we are right outside of the normal range of spectral classification. For these quite extraordinary stars, I might just say it appears that their radii are fairly small, perhaps one tenth of the solar

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radius. I would like to pose two questions. First, are there such stars that do not have planetary nebulae? One can observe them and tell something about their properties when they have a nebula associated with them, but the central stars themselves are very faint and insignificant objects. Possibly they only exist in association with nebulae. This brings me to my second question: Does a star which is as hot as $25 \cdot 10^4$ degrees necessarily eject matter and form a nebula?

— C. DE JAGER:

I agree with DEUTSCH that we should not go too far into the problem of evolution, but on the other hand the problem of mass-loss is intimately connected with the evolution, so we do have sometimes to treat the evolution problems. I think it was VORONTSHOV-VELIAMINOV who for the first time suggested that planetary nebulae would go over into a novae and then finally to a white dwarf. This suggestion was based on their place in the Hertzprung-Russell diagram and on the fact that these types of stars lose mass and so finally can arrive at a mass so low that they can go into a white dwarf. Does anybody know what the lifetime of a planetary nebula is?

— A. J. DEUTSCH:

Ten thousand years. It is my understanding that a planetary nebula is a one-shot affair. A star reaches a certain stage in its evolution when it releases one tenth of its mass, which then goes out into the interstellar medium. I don't know how long it takes the one tenth of a solar mass to flow out through a sphere drawn around the star just before this outburst takes place. It may take a year; I suspect it takes some tens of years as SEATON has suggested, maybe even quite a bit longer than this. But it happens only once; it gets rid of a tenth of a solar mass and then I believe it stops. It doesn't go on.

— R. N. THOMAS:

Would you tell us how you know? Are the ten thousand year life and the one tenth solar mass purely theoretical figures?

— A. J. DEUTSCH:

I believe that these numbers come from the following arguments. One measures the surface brightness of a planetary nebula, one knows its linear dimensions; one can, therefore, compute the total amount of mass in it. Result: One tenth of a solar mass. One also knows that rate at which the nebula is expanding, one then asks how long will it it take before this will no longer be observable. Result: 10 000 years, roughly. I would also like to re-

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mark on the point that was already made; *viz.*, there are good and sufficient astronomical reasons for distinguishing between the nuclei of the planetary nebulae and the classical Wolf-Rayet stars. The latter objects have luminosities of the order of a few thousand times that of the sun. The central stars of the planetary nebulae have luminosities which are comparable with the luminosity of the sun. In addition their kinematics and distribution in the galaxy are totally different. So, I think one cannot admit the possibility that the classical WR stars are about to become the nuclei of typical planetary nebulae. It is indeed a fact that at least two WR stars are known to have nebulae around them. The nebulae can actually be photographed against the sky. But, in neither case is the nebula at all typical planetary.

— M. J. SEATON:

I was not suggesting at all that one should bracket together the WR stars and planetary nuclei and think of them as being the same sort of object, but I do think that we should consider whether the type of steady ejection process in the WR stars is also the type of process taking place in the planetary nuclei.

I only comment on the numbers. If we take $10^{-6}M_{\odot}$/year as the ejection rate for a WR star, and a time of 10^4 years, we have $10^{-2}M_{\odot}$ ejected, which is not much less than the figure of $10^{-1}M_{\odot}$ given for planetary nebulae.

— E. N. PARKER:

To what extent is the matter in a planetary nebula composed of interstellar material swept up by the 15 km/s expansion velocity? If a large fraction is interstellar matter, then the present expansion velocity may be very much lower than the initial expansion velocity.

— M. J. SEATON:

The material could originate from interstellar matter only for nebulae which are thin shells; this could not be the case when the density is high throughout a large volume. Let me raise another point. This morning DEUTSCH presented us with the result that the electron temperature would have to be high to get ejection of matter; and if I understand correctly this is essentially a question of having enough velocity to exceed the escape velocity. On the other hand, we are very accustomed to thinking, for shells of stars and gaseous nebulae, that the kinetic temperature is about 10^4 °K. This is a problem to which UNSÖLD referred the other day in his remarks on the microscopic treatment of equilibrium phenomena. The temperature of 10^4 °K is determined by the O^{2+} ion. Also, the O^{2+} ion by its forbidden line emission provides a means of measuring the temperature; and the measured value is indeed just what one predicts from the theory of the thermal balance. But the problem may be more complicated.

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Consider a nebula containing hydrogen, helium, and oxygen. We have the three regions shown in the figure. The boundary between O^{2+} and O^{3+} coincides with the boundary between He^{+} and He^{2+} because O^{2+} and He^{+} happen to have identical ionization potentials of 54.4 eV. The usual theory —and the (O III) measurements—apply only to the region containing O^{2+} In the inner region, that containing He^{2+}, the temperature may be much higher. This is not only because there is no cooling by O^{2+}. The mechanism may be summarized as follows: In the inner region we are skimming off the really high-energy quanta of the central star, those with energies above 54.4 eV. Each of these quanta produce one quantum in the He II Lyman α line, and this in turn ionizes a hydrogen atom and gives an electron with a kinetic energy of 27.2 eV; thus, for each quantum absorbed one gains at least 27.2 eV of kinetic energy.

From this it may be shown that the kinetic temperature will be of the order 10^5 °K. The size of this inner region depends on the temperature of the star, but its high kinetic temperature is quite insensitive to the star temperature. I would like to make one further remark which I am sure is not relevant but which concerns a problem discussed at previous symposia. At the meeting in Cambridge, England, ZANSTRA suggested that condensations in nebulae might result from the rather curious equation of state which one has with the O^{2+} cooling mechanism. The idea was that dense regions would be cooler since there would be an increased amount of O^{2+} due to recombination of O^{3+}. For thick nebulae one may expect the picture drawn above to be correct. The O^{2+} and O^{3+} will then be sharply separated in space and Zanstra's condensation mechanism will not work.

	H^{+}	H^{+}	H^{0}
Star	He^{2+}	He^{+}	He^{0}
*	$O^{3+}, O^{4+}, \ldots$	O^{2+}, O^{+}	O^{0}
	$T_e \sim 10^5$	$T_e \sim 10^4$	

— C. DE JAGER:

I wonder if Mrs. BÖHM-VITENSE could come back to a point raised by her at the end of the discussion on pulsating variable stars. That is this: At a certain state during the pulsation or evolution of a star there may occur a local region where the effective gravity becomes negative. My question is whether this local region might become so extended in certain stars that it could be of importance for the mass-loss of a star.

— E. BÖHM-VITENSE:

I really do not think it could, because this unstable region will always first occur in the layer with a temperature around ten thousand degrees. Now

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if the higher layers should become unstable—that means gas could flow out of the star—one could expect at least also the deeper layers to be unstable at the same time. That means that if the gas should be able to flow out at all, this should require an extended region that would flow out, and the star certainly could not exist very long in this state. I guess it would not even be formed. The question then is whether during the evolution of the star, the star could pass through a region where this whole layer could become unstable. This could happen if the product of the absorption coefficient, $\varkappa$, times the radiation flux, F, which is proportional to the radiative acceleration, would be increased during evolution. Either $\varkappa$ or F could be raised. $\varkappa$ could be increased by increasing the pressure, but then the radiative acceleration would become important first in the deeper layers, and the only result would be that the atmosphere would be blown up slightly and the pressure would decrease again until there would be equilibrium restored. The same would happen if the flux would increase—that means if the effective temperatures would increase during evolution—then again the radiation would first make the layers around 10 000° unstable, and would blow up this part of the atmosphere, and thereby lower the pressure and the $\varkappa$ in this region until the atmosphere is stable again. The outer layers of the atmosphere would remain stable during this process.

— E. SCHATZMAN:

The question is related to the problem of generation of planetary nebulae and WR stars following the line suggested by SHKLOVSKY. When in the course of evolution a star of large mass—I do not know precisely which mass—has developed an isothermal core of largest possible value, which is about 15% of the mass of the star, then the core starts contracting. But for such stars of such mass the contraction of the core is very fast, and the radiation which is generated in that central region of the star starts pushing away the mass of the outer layers. So we have the equivalent of a piston; a shock develops and this problem can be studied from the point of view of hydrodynamics. It should be possible to find how this shock-wave develops, and how any mass outflow is initiated. In such a case we might expect that the difference in the WR stars and the planetary nebulae is a difference 1) in chemical composition and 2) in mass. Especially in the WR stars the amount of energy available suffices to push away a large mass. I compute the amount of gravitational energy available in the gravitational core as being about 10^{50} erg. Thus, this could push away, at 10^8 cm/s, about 10 solar masses.

— G. BURBIDGE:

Perhaps you can answer a question on the hypothesis of SHKLOVSKY. I have never been able to understand what physical process starts this thing

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going. One starts with the star in an equilibrium configuration and then suddenly all the radiation is absorbed and generates momentum in the outer shell. In the extreme case this would suggest that the outer shell would heat up and expand, while moving outward, but the character of the radiation that we see would change.

— E. BÖHM-VITENSE:

It seems that this hypothesis uses the same mechanism discussed before—radiation pressure. I do not think it would result in strong mass ejection, because it would just expand the star a little bit until the star would be in equilibrium again, and nothing more would happen.

— W. H. MCCREA;

Whenever you see in physics anything being thrown away from a system, it results from energy becoming concentrated into small localities, as in spray from a breaking wave. How can this apply to stars? Is there no way of concentrating energy into small bits of the atmosphere of a star, so that we get material « spraying off » rather than « flowing » off?

— A. UNDERHILL:

General considerations suggest that Be stars lose mass at a rate of 10^{-7} solar mass per year. Many people would get extremely happy if you make the Be stars live any more than 10^7 years. So 10^7 times 10^{-7} means a loss of one solar mass in the lifetime of the star. For an average Be star the mass is $5M_\odot$. Thus, a larger rate of mass-loss than estimated is required if these stars are to be able to evolve to white dwarfs.

— C. DE JAGER:

If the sun was as far from us as a star, we would not infer any mass-loss at all. Still, it looses mass—and apparently at 500 km/s. Who knows what happens in B and Be stars?

— A. UNDERHILL:

This is my point—I think all numbers have been probably greatly underestimated. One more point. In trying to understand the planetary nebulae and the shells around the WR stars and Be stars, I sometimes wonder how much magnetic fields have to do with the observed phenomena. I understand that a magnetic field can keep material trapped near a star. It can also do something to throw material out. I would like to note the perhaps significant fact that the hot stars, the O's and B's and WR stars are supposed to be formed in fairly recent astronomical times in spiral arms where there are supposed to be magnetic fields. How can you lose the magnetic field? If you bring

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gas that was in an interstellar field together to make a star, don't you have to bring a magnetic field with it? Then you will have stars with magnetic fields. Now, there is no possibility of observing these fields directly and in this way proving that there is a magnetic field. There is nothing that you can observe to show the field, but if it is there, won't it affect our ideas of mass-loss or ejection seriously?

— S. S. HUANG:

I would like to mention some work (cf. *Ann. d'Ap.*, 1959, **22**, 527) which I did recently which has some bearing on the present topic of mass-loss by a star. It is an observational result that in α *Virginis* and in other B-type spectroscopic binaries having two sets of spectral lines, the secondaries, *i.e.* the less massive and less luminous components, are always overluminous with respect to their masses. Why should they be overluminous? We can have four possible exaplanations: 1) A result of evolution. Since a more massive star evolves faster than a less massive star, one would expect that the primary component should first depart from the main sequence and become abnormally luminous. Actually it is the secondary component that is overluminous. Therefore evolution is not an explanation. 2) A result of hydrodynamic flow of matter and consequent transport of energy from the primary to the secondary component. But the two components in these binaries are on the average of 10 stellar radii apart. They are not in physical contact. Indeed both stars are much smaller than the two lobes of the critical surface which limits the sizes of both components. 3) A result of energy transfer from the primary to the secondary through electromagnetic radiation. You can rule this out immediately because the amount of added energy is simply not large enough to account for the excess energy that the secondary components of these binaries radiate. 4) A result of energy transfer by corpuscular radiation. This is my final conclusion. I assume that the excess energy radiated away is transported from the primary to the secondary component through an exchange of high-energy corpuscles. It is proposed that there exists a common envelope, which may be regarded as a common corona, around the two components of a binary system. If the density of the common corona is higher than that found in the solar corona, a plausible assumption in view of the larger masses of the component stars—the overluminous nature of the secondary component can be satisfactorily explained.

It is interesting to recall that after this work was completed, a group of physicists including KUPPERIAN and others, then in the Naval Research Laboratory and now in Goddard Space Flight Center, NASA, found in rocket flights ultraviolet radiation coming from a nebula near to α *Virginis*. The nebula is much larger than the common corona proposed in my papers. However, SHKLOVSKY has since proposed that the energy radiated in the ultra-

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violet by the nebula comes from the corpuscular radiation emitted by α *Virginis*.

I would like also to make a comment on the problem of hydrodynamic flow from stars. According to DEUTSCH, the hydrodynamic flow velocity observed in many stars is of the order of 10 km/s, which is less than one tenth of the escape velocity. How could we derive from this empirical result the conclusion that these stars are losing mass by hydrodynamic flow? Thus, it appears to me that except for novae, novalike objects, and perhaps planetary nebulae the observations are not strong enough to draw any conclusion concerning mass-loss through hydrodynamic flow.

— K. H. PRENDERGAST:

I have been asked to give an account of a theoretical investigation of gas flow in the neighborhood of a close binary system. I'll try to sketch this very quickly, and also indicate why I think it is exceedingly difficult to say anything about mass-loss from such considerations. Suppose we have two stars which move around one another in circular orbits at constant angular velocity—and also suppose that the radius of one of the stars (or possibly both) is comparable to the separation of the centers of the stars: this is what I mean by a close binary system. There exist systems in which there is gas not only in the atmospheres of the two stars, but also at a considerable distance *above* the atmospheres. Can we say anything about the motion of this gas? We have to ask two questions at the outset. First of all, how does the gas get out of the stars into the system (this I am not going to try to answer), and second what forces act on the gas once it has been removed from the atmosphere of one or the other of the stars. If we assume that the velocity field does not depend on time, the equations of motion contain the inertial term $\boldsymbol{V}\cdot\mathrm{grad}\,\boldsymbol{V}$, the Coriolis force, $2\boldsymbol{\omega}\times\boldsymbol{V}$, the centrifugal force $\boldsymbol{\omega}\times(\boldsymbol{\omega}\times\boldsymbol{r})$, the gravitational attraction of the two stars, and the pressure gradient. But what is the effective pressure? There is, of course, the gas-kinetic pressure, but there may also be important effects due to radiation pressure, or magnetic pressure, and there could very easily be significant Reynold stresses. It is impossible to consider all of these, and I have chosen to discuss the equations neglecting the pressure terms entirely. We can offer the following excuse for this procedure: The contribution of the pressure terms to the equations of motion is of the order V^2/R, where V is a small-scale velocity (whether thermal or « turbulent » does not matter), and R is the distance from an element of gas to the center of mass of the system. If this term is to be comparable to the gravitational forces, V must be of the order of a few hundred kilometer/second, and there is no observational evidence for the existence of such small-scale, high velocities.

I now construct the « gradient-wind » approximation to the solution of the equations of motion with the pressure term neglected. The velocities computed in this approximation are of the same order of magnitude as those indicated by the observations, and the flow pattern looks like the pictures that the observers have been drawing for a number of years. (Such pictures can be found in Struve's « Stellar Evolution », and a number of original papers as well.) It should be clear that these considerations have no bearing on the problem of mass-loss from close binary systems. In order to discuss mass-loss we would have to be able to follow the history of elements of gas ejected at arbitrary speeds, in arbitrary directions from various points on the surfaces of the stars. This is currently impossible for two reasons. In the first place, we do not have a physical theory which enables us to compute the effective pressure, and therefore we cannot write down the correct system of equations for the flow. Secondly, even if we knew the equations, it would be very difficult to integrate them, even on a big computer. An account of this work has appeared in the *Ap. J.*, **132**, 162 (1960).

— R. N. THOMAS:

Let me return to the H_α profiles DEUTSCH had on the board for the Be stars—the profiles consisting of a broad emission on which, not necessarily symmetrically located, is apparently a self-reversal. If I understand correctly the arguments, they are that the self-reversal represents the emission from that portion of the shell lying between the observer and the main stellar disk, the shell being large in extent compared with the radius of the main disk, and there being some kind of radial expansion of the shell. Thus, the geometrical effect gives an emission which can be regarded as either absorption in a cooler shell; or if the shell is not cooler, as coming anyway from only a relatively few atoms emitting at the maximum velocity of expansion (or even contraction), so the apparent self-reversal occurs. But consider the similar appearance of other types of lines, showing a central emission core, with a self-reversal superposed on this core. For example, Olin Wilson's observations of the later type giants, where Ca^+ H and K lines show this behavior. Or, Lyman α of H and the Mg^+ and Ca^+ lines in the sun. In these solar cases, where we know there is no question of a large shell, our suggested interpretation has been based on a simple solution of the transfer problem for a non-LTE source-function in an optically-thick, hot chromosphere. We can predict the essential observed features, with the separation of the emission peaks being a combined function of the details of the temperature gradient in the atmosphere and the Doppler width of the absorption coefficient. In the case of Wilson's observations of the H and K lines, the several suggested interpretations again do not involve an extended atmosphere. The question of the detailed profile is presently controversial; some make an interpretation only in terms of the effect of turbulence on the absorption coefficient profile; JEFFERIES and I introduce also the effect of variation of a non-LTE source-function. However, in the case

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of the Be stars, it seems to be assumed that one must have a greatly extended atmosphere, in expansion, with the details of line formation essentially irrelevant. So I want simply to raise the question—how certain is it, that one really only needs such an extended atmosphere. Are you sure that such models as used for the sun, and the Ca^+ lines observed by WILSON in the giants, need not be invoked?

— G. BURBIDGE:

I have no immediate answer, except to say try it and see if you can get the correct profile

— A. UNDERHILL:

A general remark on observational problems in astrophysics is that observations of one feature only permits several interpretations. For these stars, my preference for the present model—and the self-reversal as absorption by the narrow region in front of the disk—is based on its bringing together quite a few pieces of observation

— MRS. BURBIDGE.

We used to wonder whether the emission lines in the Be stars could be produced in some way other than in a ring around the star. But when one computes the number of emitting atoms—*e.g.* consider the great strength of the H_α emission—the level of the emission line is way above the continuum—it seems to us that it is the great number of emitting atoms that makes an extended atmosphere seem necessary.

— R. N. THOMAS:

There is no problem about the absolute emission in a line relative to the continuum—once there are enough atoms to provide an opaque chromosphere, the intensity is a function of the size and position of the temperature rise in the outer atmosphere. And, once I get an emission line in such an atmosphere, I get a self-reversed core, except for very unusual circumstance of temperature gradient.

— A. UNDERHILL:

Be stars do not have a temperature increasing outward.

— A. J. DEUTSCH:

I want to endorse the comment of A. UNDERHILL. One has to look at the spectrum of a Be star as an entity, and he then sees that, in addition to H_α, there often exist other absorption features, in many of these stars, which obviously are produced in an extended shell. There are absorption lines which arise from metastable levels only, and are therefore characteristic of a dilute radiation field; so that one knows the star in this case to be in fact surrounded by an extended envelope. Now it must be added that for some Be stars, notably among the giants, one does not have this kind of evidence, and there is reason to believe that an explanation in terms of a temperature reversal, without an extended chromosphere, may be quite possible. But for some typical Be stars, one has the additional evidence of the absorption shells; one also knows that these stars have rotational velocities which are near the stability limit, so that he might expect them to be ejecting matter; one has also evidence indicating that the matter producing the emission lines is concentrated towards the equatorial plane, etc. I think all this adds up to a pretty strong case in favor of a really extended atmosphere rather than the kind of model that you have spoken of.

Now, with regard to Olin Wilson's observations. Some of you may have noticed this morning, when I was concentrating my attention upon the profile of the circumstellar absorption lines in the M giants, that characteristically we get a profile like this at the K-line. We have the broad damping wings that are produced in the reversing layer of the star, and in addition we have a chromospheric emission line which is centrally reversed. Olin Wilson has measured the width of this emission feature, and he finds the extremely remarkable result that it is proportional to the visual luminosity of the star to the one-sixth power, independent of the spectral type, over a range of 15 magnitudes—that is, over a million-fold range in visual luminosity. Question: Does this indicate some kind of turbulent velocity fields in stellar chromospheres, fields which are correlated closely with the visual luminosity? If so, what are these velocity fields like? It is necessary to suppose that the width of the line is the same as the width of the absorption coefficient; or may it be appreciably smaller? One does not know the answers at the present time. However, the existence of emission features of this kind in all late type giants, including the M's, indicates to us the existence of chromospheres for these stars. In these stars, therefore, as in the sun, the temperature falls as we proceed outwards through the photosphere, where the continuum is produced; and then the temperature never gets up to the level of a million-degree corona; or it does get there, passes its maximum, and quickly starts down again so that in our observations we see mainly the cool outer envelope.

— M. J. Seaton:

What is the excitation temperature?

— A. J. Deutsch:

Very low; in the outer parts of the envelope, indistinguishable from zero; also the kinetic temperature must be relatively low in the envelope. I would

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not like to rule out the possibility that the temperature rises somewhere to heights comparable with what we observe in the solar corona. The parts that we observe, however, are the cooler parts. Finally, I only want to mention that in the K-line of normal giants the self-reversal normally gets shallower with advancing spectral type, until at M_0 it just about disappears. At about that point there appears a very sharp, very deep absorption feature, which represents the onset of the circumstellar absorption spectrum. As we go to still later spectral types, the violet edge of that feature appears to stay fixed, and the red edge moves longwards. In the more luminous M giants this feature becomes very strong; in the most luminous M supergiants, it may actually absorb away the whole emission line, and we see only a strong deep absorption core at the bottom of the profile of the reversing-layer K-line.

— R. B. LEIGHTON:

I would like to offer just a very brief comment about Thomas' idea of the structure of the K-line. One gathers from most of the things we have heard that the chromosphere in which the emission part of that line is formed (and the K_3 absorption) is a uniform thin shell over the outer parts of the surface of the sun and perhaps other stars. Actually we have growing evidence that the emission in the K-line on the sun comes from extremely well-defined patches distributed quite irregularly over the surface of the sun, and I for one would question very strongly the advisability of trying to interpret a line-profile, which was obtained by some kind of an averaging process over a large area of the sun, in terms of a « source-function ». I do not really see how such a « source-function » can be connected with the very spotty nature of the emission in actuality.

— R. N. THOMAS:

The « source-function » is not something you can insert or ignore as you choose—it is the ratio of emissivity to absorptivity and enters in *any* kind of discussion you make which involves transfer of radiation. Our point is simply that if you take a spherically-symmetric chromosphere—but please note the chromosphere is *not* optically thin in these lines—you predict the observed kind of self-reversed emission core. I agree completely that very probably the real chromosphere has departure from spherical-symmetry, and velocity fields, and a complete theory must include these. But note that careful observation of the variation in Ca^+ emission over the solar surface (*e.g.* E.v.p. SMITH, *Ap. J.*, **132,** 202 (1960)) shows only a change in detailed structure of the self-reversed emission core, no « absence » of these features. Second, how sure is DEUTSCH that there is really a temperature drop in the outer layers of the M giants—the self-reversed core cannot be interpreted as evidence for this, as has been sometimes done.

— A. J. DEUTSCH:

The temperature drop to which I referred is demanded by the observations of the circumstellar lines which appear in the M giants. I am unable to say whether there must be a temperature drop in chromospheres of K giants. I am perfectly prepared to admit that you can reproduce their self-reversed *K*-lines without a temperature drop. But in the M giants it is certainly there.

— S. S. HUANG:

I would like to answer Thomas' original question about whether we can use one and the same mechanism to explain the profile of hydrogen lines observed in Be stars and the similar profile of Ca II, *H* and *K* observed in the sun and in late-type stars. It appears to me that we must invoke two different mechanisms, because in the case of Be stars: 1) the separation of the two emission components is generally of the order of a few hundred km per s, which is too large to be regarded as arising from radiative transfer; 2) their spectra also show the broadened lines arising in the reversing layer of the star and characteristic of rapid rotation, indicating that the deep core of the hydrogen lines is due to a detached ring, and 3) as Miss UNDERHILL and DEUTSCH already pointed out, narrow shell lines which can only be produced in a low-pressure region appear also in their spectra, indicating again the existence of a detached ring. These observed facts led STRUVE to propose that the Be stars are rapidly rotating stars. Their rotational velocities are so large that the equatorial region becomes unstable and mass is ejected from the region. The ejected mass forms a rotating ring in the equatorial plane around the star. Such a ring suffices to produce the profile of the hydrogen lines. Thus, the profile of the hydrogen lines in Be stars is due purely to a geometric effect. This is not so in the case of Ca II, *H* and *K* lines or the Lyman α line found in the sun, because everywhere on the solar disk we find similar profiles, indicating that the profile can only be explained by radiative transfer. Thus, using the terminology which I presented a few days ago, the profiles of hydrogen lines in Be stars are a result of geometric broadening while the profile of the Lyman α line found in the sun during rocket flight and perhaps that of the *H* and *K* lines in the late-type stars are the result of physical broadening.

— C. DE JAGER:

The problem of mass-loss of stars in connection with evolution was raised for the first time some 15 years ago by FESSENKOV and Mrs. MASSEVICH in the Soviet Union. At that time they ran into a heavy discussion with the group of HOYLE, MCCREA, LYTTLETON, GOLD, on the question of whether mass-loss or accretion of mass would be the most important for the evolution, and I have a feeling that this battle ended in a draw, resulting in nobody be-

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lieving either in accretion or in mass-loss. The problem of mass-loss in connection with the evolution came up again only a few years ago, and this time it came to more general attention of astronomers. DEUTSCH was the first to give a systematical review of data on stellar mass-loss, and I think we all should be thankful to him for that. It is, of course, a first review and we hope that further investiagtions will soon increase both the accuracy and the number of the data given here.

We turn now to a discussion of the solar observations. There is various evidence for solar mass-loss. First, let me call attention to a rather indirect one, that might otherwise not be mentioned in this symposium. The occultations of the *Crab nebula* by the solar corona take place every year in the month of June. The different observations made since 1950 have shown that these can only be explained if the coronal elements, scattering the radio radiation of the *Crab nebulae*, have elongated forms. The most recent observations by HEWISH, published quite recently show that these elements have shapes as shown in the drawing (furnished by courtesy of A. HEWISH, Cambridge, England):

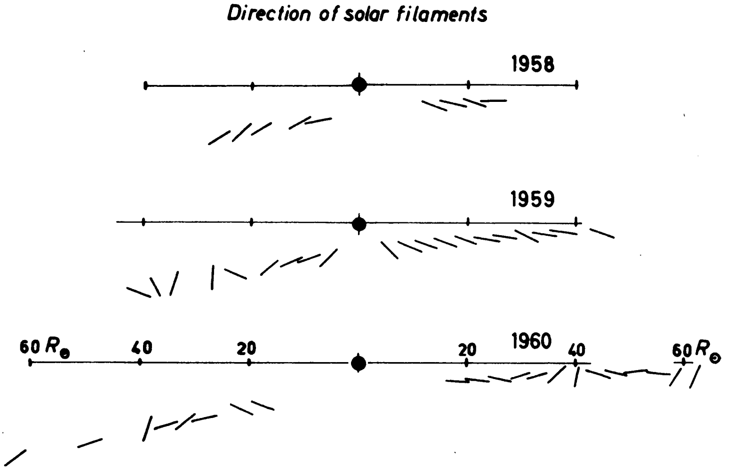

Fig. 1.

It is mostly assumed that matter flying out from the sun follows more or less the magnetic field lines. If this is true the field lines do not indicate a dipole field at great distances from the sun, although the measurements of the polar plumes, near to the sun, suggest a field close to that of a dipole. So we see here an example of matter streaming out and taking the magnetic field with it. I only wanted to mention this observation; I think nothing has been done theoretically on the interaction of outstreaming matter and the field, so it is hardly worth-while to have a discussion on this problem.

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— R. LÜST:

I will summarize the work by BIERMANN on comet tails just to give some numbers to indicate what has been found thereby on the mass-loss from the sun. Now, according to BIERMANN, the comet tails of type one, which are of gas and are ionized, can be explained by the interaction of the corpuscular stream coming out from the sun and hitting the comet. The main process which is responsible for the ionization is charge exchange between the protons in the corpuscular stream and the CO molecules in the comet tails; from the observed data on the comet tails, one can get some estimate for the necessary corpuscular flux. The most recent number for the flow is somewhat lower than the values given two years ago. This revision is mainly due to the fact that one has now better experimental observations for the charge exchange cross-section. The cross-section in this energy range is somewhat higher than expected—on the order of $3 \cdot 10^{-15}$ cm^2. Using this cross-section, the corpuscular stream at one astronomic unit is found to be of the order of 10^9 ions/cm$^2 \cdot$s at the lower level of solar activity. If one assumes an average velocity of about 500 km/s—this would lead to a density somewhat less than about 10^2 ions/cm^3. This is a rough average value, and the flux might be quite higher on days of higher activity. For instance, the value given by UNSÖLD and CHAPMAN in 1949 for the case of high activity was about 10^{13} ions/cm$^2 \cdot$s.

The above value of some 10^9 ions/cm$^2 \cdot$s gives a particle flux at the solar surface of 10^{14} ions/cm$^2 \cdot$s. Furthermore, assuming a velocity of about 10 km per second at the solar surface, this would lead to a density of 10^8/cm^3 involved in the outflow. Also note that the figure on particle flux leads to a yearly solar mass-loss of about $10^{-13} M_{\odot}$.

— A. B. SEVERNY:

Blackwell's well-known recent investigations of the corona at great distances from the sun lead to a conclusion that the density of the interplanetary plasma can hardly exceed 10^2 particles per cm^3 at distances ~ 1 a.u. If it were greater, we would be able to find appreciable widening of spectral lines in the spectrum of zodiacal light.

It is also interesting to note the recent Pariysky attempt (USSR Solar Commission session, June 1960) to evaluate the density of solar corpuscular streams by considering the counterglow as formed by these streams. He found that a particle density $\sim 10^3$ cm^{-3} is sufficient to explain the observed brightness of counterglow.

In connection with this problem of the density of interplanetary space I would like to mention the recent results of measurements of this density with the aid of three Luniks, carried out by GRINGAUS and considered by SHKLOVSKY (K. GRINGAUS, V. KURT, V. MOROS, S. SHKLOVSKY: *Astron. Journal USSR*, No. 4, 1960).

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The electrostatic capture devices permitted one to record the current produced by charged particles at energies > 200 V. Near the earth, the density of positive ions falls from 10^3 cm^{-3} at the distances 2 000 km to $\sim 10^2$ cm^3 at distances $(28 \div 30)$ thousand km and this run, probably, corresponds to the ionized component of the geo-corona. For interplanetary space between earth and moon, there are no indications of a stationary plasma with particle density exceeding $(50 \div 80)$ cm^{-3} (the usual noise level corresponds to ~ 50 cm^{-3}). Sometimes comparatively strong currents were recorded in interplanetary space corresponding to a particle flux $2 \cdot 10^8$ cm^{-2} s^{-1}.

Therefore, we think that there is at the present time strong evidence that the interplanetary particle density $n < 10^2$ cm^{-3} at distances ~ 1 a.u.

— C. DE JAGER:

Let me summarize. We distinguish between the quiet sun, the sun with an activity center, and the sun with a flare. Assume that all the particles that are found at 1 a.u. are moving out from the sun; then we see that BIERMANN, SEVERNY, and BLACKWELL find consistently values smaller than 10^2. Further UNSÖLD and CHAPMAN found that a strong flare emits some 10^5 particles$\cdot$cm^{-3} at earth's distance. Values for the sun with an activity center are more uncertain, so we put 10^3 to 10^4. From the foregoing discussion, it is already clear that flares may give rise to significant increase in the loss of matter, and this need not only apply to the sun but also to the stars.

— J.-C. PECKER:

When one looks at the table given by DEUTSCH, he can see two types of flow: catastrophic and regular. It seems to me that in the solar case, what appears as a regular flow is indeed more or less an integration over time of a lot of processes which are « locally » catastrophic (such as flares). This could be well the case for many « regular » flows. But, in some cases, one sees directly (without integration) the whole catastrophic process. Then I want to ask what happens in the case of the « flare » stars, *UV Ceti*, *T Tauri*, etc. They are similar to the sun in one way because of the surface activity not mentioned by DEUTSCH, but the amount of energy that is involved is much bigger.

— M. MINNAERT:

I should only like to add that the number of M dwarfs is so considerable that the contribution of these stars—if a great proportion of them emits flares—could be perhaps one of the most important contributions of stars to producing interstellar matter. This would mean that instead of considering only the giants there ought to be also a category of dwarfs—the more important since we have not yet found processes enough to contribute the necessary amount of matter to the interstellar gas. It might be that Mr. or Mrs. Burbidge could give an estimate about the contribution of the M dwarfs.

— G. Burbidge:

I do not think that we have the slightest idea about how much matter is ejected by these types of stars. We do know, however, that they do increase in luminosity to a considerable extent, and so we have speculated on the possibility that in the flares which presumably occur at these times, some form of nuclear activity goes on. If it does occur, we shall get large fluxes of comparatively high-energy particles—mainly protons, and these will escape. However, as far as I am aware, one has no idea about the rate at which this will occur. To say that the corpuscular radiation increases in some way proportional to luminosity would, I think, be quite false. There is no observational evidence as far as I know for outward flowing velocity—for expansion velocities in flare stars. So although, as Minnaert says, this may be an important mechanism of mass-loss from stars, I should point out that these stars are probably in a state of contraction when this process occurs. Consequently this is not quite where we would like it to occur in terms of stellar evolution.

— A. J. Deutsch:

Just to reinforce Burbidge's last point. While on the scale of the mass balance of the interstellar medium the contributions of the M dwarfs may be not unimportant, I think that they cannot resolve the difficulty to which I referred, when I pointed out that we are faced with the requirement of getting rid of large quantities of mass from the massive stars.

— N. Milford:

Is it important for this conference that we know the mass-loss from stars, since this depends on gravitational parameters which are rather independent of the velocity fields? Secondly, is it very important from the point of view of aerodynamicists whether the flow is 10^2 or 10^4 or 10^6 cm^2/s in any of theses cases?

— E. N. Parker:

Yes, it is important whether the density is 10^2 or 10^3/cm^3 at the orbit of the earth. One can obtain 10^2/cm^2 from the hydrodynamic equations of steady winds with the observed coronal temperature, but he can't by any stretch of the imagination justify 10^3/cm^3 at the orbit of the earth under steady conditions without assuming coronal densities at the sun about 10 times higher than observed.

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— N. MILFORD:

This seems to me an astrophysical answer.

— E. N. PARKER:

It seems to me an aerodynamic answer. If one solves the hydrodynamic equations, then he finds one possible range of density, but with definite limitations.

— K. H. BÖHM:

With regard to the question by MINNAERT one should add that according to the observations by HERBIG, there is an apparent outflow of gas from *T Tauri* stars in so far as emission lines in *T Tauri* stars are shifted by about 50 km/s with regard to the absorption lines. Of course, one cannot say whether this is larger than the escape velocity, because one does not know in which region and how far from the star the emission lines are formed; but I think there is some sort of observational evidence in this respect. It should perhaps be noted that HOYLE has claimed that matter is falling in, but this has always been a controversy between theoreticians and observers and so far as I know from HERBIG there has been no evidence of in-falling matter.

— A. UNSÖLD:

Just at the transition point going from observation to theory one more remark about the sun. In the solar corona matter seems to flow out chiefly in the so-called coronal rays. Now in recent times it has become more and more evident that at distances of several solar radii from the sun, matter is quite strongly concentrated in these rays. If you would draw a sphere of say 5 solar radii around the sun, only a small percent of the surface of this sphere would actually be pierced by coronal rays. That means that in considering any problems of flow of matter, or of heat conduction in the corona, we must be aware that the problem is far from spherically symmetrical; instead of the usual factor $1/R^2$ in the spherical problem we should have a factor more like $1/R$. That means to the theoretician that instead of a spherical problem we are dealing with something like the cylindrical problem and that of course may effect the solution quite considerably.

— E. N. PARKER:

You are right that when you look at the solar corona you do in fact see streamers. However, from hydrodynamics it is not obvious that a few solar radii from the sun the material actually flows in the same direction as the streamers. If you work out the equations you find that there is very little difference in the final velocity that you predict, whether you see streamers from the sun or whether you do not.

— R. Lüst:

From comets one has also an indication of some corpuscular streaming at higher latitudes. You see comets at high latitudes and one sees also some activity in these comets. If the theory of corpuscular stream and comet tails of type one is right, you would expect this indeed. Of course, the density drops somewhat if one goes away from the ecliptic, but I do not know the exact factor.

— L. Davis:

It is widely accepted that the density of the solar wind is on the order of 100 particles per cm^3, and that the velocity is of the order of 500 to 1000 km/s. Any reasonable theory of what happens when this solar wind interacts with the earth's geomagnetic field would indicate that the disturbances go down much closer to the earth that one would think, from the satellite observations made by Senett and his collaborators at Space Technology Laboratory of the geomagnetic field. There seems to be two possible ways out: One, to say that the solar wind is considerably weaker than all other data indicate—the other is to say that there is something of an aerodynamic nature going on which allows the earth's magnetic field to stick out much further than it has any business to (I do not think this last aspect is something completely understood as yet.) I do not think that you can say that the satellite observations of the magnetic field can be at fault in this point.

— A. Unsöld:

As far as the observations go, one can just say that out to 20 solar radii certainly the concentration of matter in particular places is very high. The occultations of the *Crab Nebula*, which deJager mentioned, already are most easily interpreted if one assumes that out there the local density is something like 10 times the average density for the same distance.

— A. J. Deutsch:

It is my understanding that Chamberlain has objected to the very high velocities that Davis has said now find general acceptance. Can somebody explain the situation; are there indeed observational reasons for supposing that the velocity will be of the order of 20 km/s.

— E. N. Parker:

No observational reasons.

— M. Krook:

I would point out that the radii put in the various tables may not have any direct significance for the structure or the dynamics of the stellar atmosphere. These radii are wholly based on observations and theories de-

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signed to explain observations in a very narrow spectral range—the visual range—and the stellar radius is established from observations in this visual range. It is instructive to consider what would have happened had we been able to observe only in the radio-frequency range; we would perceive only the outer parts of the corona. Would we have been able to produce a model of the sun? Where the star ends, and where the interstellar medium begins, is a vague idea; and one must be careful in discussing radii and distances.

— A. J. DEUTSCH:

I would object. Certainly the « edge » of the star is not well-defined; but it would be unfair to imply that it has no physical significance. It is the place where the mean-free-path of the average photon suddenly increases enough to permit escape into space. It is no accident that we observe stars not many octaves from the region where the continuous spectrum reaches its peak intensity.

— M. KROOK:

I think you are implying that the structure of the outer layers of the star is completely determined by what you see in the optical region. Once this is clear, everything else is. The idea is that there is no reaction back on the atmospheric structure by what lies outside its edge.

— E. N. PARKER:

I would like to pursue the proposal made two years ago that the solar corona is in a state of continual hydrodynamic expansion. Let me start by commenting on the question raised by UNSÖLD, the extent to which one can apply spherical symmetric calculations. We have investigated this problem because it is obvious, looking at eclipse photographs of the sun, that the sun is *not* spherically symmetric. We find that whether the gas moves out along a radius or along some sort of a flat fan, we do not obtain even a 15 percent difference in the velocity for a given input of coronal heating. The velocities are remarkably insensitive to the kind of geometry, since after all the stationary flow equations are nothing more than conservation of energy. So I will go ahead with the spherical case. Let me cite some of the evidence for an outflow of gas through interplanetary space—LÜST fortunately has gone over most of the observations already so that I do not need to treat them at length. I want to distinguish the quiet sun from what I call the active sun, *i.e.*, the sun immediately following a large flare. The best evidence by far, I think, is from Biermann's comet analysis, which you remember gives densities on the order of 100 particles per cm^3 at the orbit of earth and flow velocities on the order of a few hundred km/s, for the quiet sun. This is apparently a perpetual state for the sun, not only in the plane of the ecliptic, but far from the plane of the ecliptic. There is other evidence that there is continuous corpuscular radiation from the sun, and that is the quiet day aurora. Every clear night in the auroral zone one sees the aurora. If we believe that the aurora is due to corpuscles from the sun, it would imply that every day there is corpuscular radiation from the sun. In the same way one observes continual polar magnetic agitation, which presumably is also due to corpuscular radiation from the sun. Since the agitation is a continued state in the polar regions, one again comes to the conclusion that there must be continual corpuscular radiation from the sun. There are a number of other arguments that one can give here, but I think these are typical, and perhaps the best of the lot. In contrast to the quiet sun, we have the active sun, for which the estimates of particle density due to agitation at the orbit of the earth run as high as 10^5. The one or two day transit times between the observed flare and arrival of something at the earth, give velocities somewhere between one and two thousand km/s, so let me write down 1500 km/s as a typical figure. The density of $10^5/\text{cm}^3$ and the velocity 1500 km/s are entirely consistent with the low latitude aurora and with the magnetic storms which one sees to follow the flare.

Now the question is, what is the origin of this solar corpuscular radiation. I want to pursue the suggestion made two years ago that the « solar corpuscular radiation » is nothing more than hydrodynamic expansion of the solar corona. The solar corona is very hot, and it is simply a matter of solving the hydrodynamic equations to see if the corona is hot enough to expand with the velocities and densities just given. We ask under what circumstances the corona of a star such as the sun will expand, and under what other circumstances might it be static. Suppose that I can observe the temperature of the corona of a star out to some distance r, and beyond that I cannot observe them. I must therefore speculate as to what happens beyond r. Let me take the best case that I can for a static corona. Suppose that the temperature out to r is $T(r) = 2\cdot 10^6$ °K. How might I best maintain this corona in static equilibrium? You begin exactly at the limit of observation and put an adiabatic atmosphere on top. You cannot have the temperature drop more rapidly that the adiabatic gradient beyond r, because you would then get convective overturning —but you can postulate that it drops adiabatically. A static atmosphere requires that $\lambda = GM_s M/rkT(r) \geqslant \frac{5}{2}$, where M_s is the mass of the star, M the mean mass of an atom in the corona, and G the gravitational constant, *i.e.*, the gravitational energy must be larger than some fraction of the thermal energy, or it cannot hold an adiabatic atmosphere. Unless the inequality is satisfied, the pressure does not fall to zero at infinity; you would have to enclose the star in a box to maintain its atmosphere static. So look at the sun and ask what the criterion tells us there. Pottasch and Chapman have recently independently investigated the temperature of the corona at sunspot

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minimum using Blackwell's density observations out to about 22 solar radii. From the density observations, one can compute the gradient of the density. The density gradient and temperature can be related if we are talking about a static corona, so we assume that the corona is static, as did POTTASCH and CHAPMAN. This will give us a lower limit on the temperature. One finds that the number λ in close to the sun is very large. The gravitational forces are large enough to satisfy the inequality. But λ decreases outward from the sun. We find that λ reaches $\frac{5}{2}$ at the 22 $R_{\odot}$ limit of observations. It is hard to make a definite statement of accuracy here; for BLACKWELL gives no definite statement on the accuracy of his observations in this region. POTTASCH and CHAPMAN differ by about 7 percent, so I use that as some kind of estimate of error, which makes the value of λ very close to $\frac{5}{2}$. So either the corona must become exactly adiabatic, or I cannot have a static corona.

— S. POTTASCH:

Beginning at about 10 solar radii, Blackwell's values of density depend somewhat on unavailable knowledge of the brightness much further out. So the densities are uncertain by about a factor 2.

— E. N. PARKER:

Since the density is changing by factors of ten, you do not need much accuracy to tell the difference between, *e.g.* the isothermal and adiabatic cases. So it seems to me necessary to abandon a static corona. Then, we turn to investigate the possibility of expansion.

In the same notation, the hydrodynamic equations of motion are, for spherical symmetry:

(1) dynamic: $$NM\,\mathrm{d}v/\mathrm{d}r = -\,\mathrm{d}(2NkT)/\mathrm{d}r - GM_{\odot}NM/r^2$$

(2) continuity: $$Nvr^2 = N_0 v_0 r_0^2$$

we assume that T and N are related by the polytropic law:

(3) $$T = T_0(N/N_0)^{\alpha-1}\,.$$

We can integrate the equations completely for a general value of α. Let ψ be the kinetic energy of the average ion in units of the initial thermal energy at the base of the corona, which I take to be $r = a$. Let λ be the gravitational parameter as defined above. This is essentially the gravitational potential energy per atom at the base of the corona in units of the thermal energy at that same point. Let $\xi \equiv r/a$. The solutions can then be written in this

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form:

$$\psi = \frac{Mv^2}{2kT_0}, \tag{4}$$

$$\psi = \psi_0 - \lambda\left(1 - \frac{1}{\xi}\right) + \frac{2\alpha}{\alpha - 1}\left\{1 - \left(\frac{\psi_0}{\psi\xi^4}\right)^{(\alpha-1)/2}\right\}, \tag{5}$$

$$\psi - \ln\psi = \psi_0 - \ln\psi_0 + 4\ln\xi - \lambda(1 - 1/\xi)\,. \tag{6}$$

We denote by ψ_0 the value of ψ at $r = a$. We arbitrarily choose $a = 16$ km to be the base of the corona at which we specify the density N_0, temperature T_0, and velocity v_0. We ask what solutions are appropriate to the sun. It is not interesting to start above the speed of sound at $r = a$ because that begs the question. Let me start with some exceedingly low velocity, 1 km/s. I find that my solution has the property: it rises and then soon falls again. I find that in fact it falls so rapidly that the density and pressure do not go to zero at infinity, where we have only the very small interstellar gas pressure of perhaps 10^{-14} dyne/cm². In fact the pressure at infinity for this solution is a few percent of that at the base of the corona —enormous!

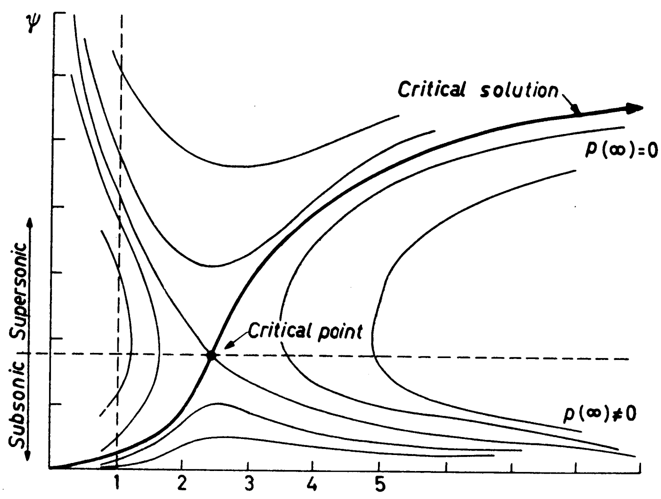

Fig. 2.

Consider such a solution for which $\psi_0 \ll 1$, so that the pressure does not go to zero at infinity. Obviously I need a box, enclosing the sun at infinity, to maintain the pressure and the stationary character of the flow. Suppose that I slowly dissolve the box. With the decreasing back pressure at infinity, the flow will accelerate. ψ_0 will slowly increase. This does not cause the pressure at infinity to go to zero until finally I come to the critical solution, shown in the figure, which starts at relatively low velocity on the sun, with ψ_0 corresponding to 50 km/s at $r = a$. At this point the solution changes its asymptotic form at large ξ. The velocity ceases to go to zero, and the density and pressure suddenly do go to zero. There are no physically meaningful solutions for ψ_0 greater than the critical value. Thus, we have the critical so-

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lution, which you see breaks away from the solutions on both sides of it and goes up to a finite velocity at infinity. The only exception to this arises when you insist upon an isothermal corona all the way to infinity, in which case it goes up logarithmically. For all other cases it levels off, giving you constant velocity, and therefore a density which goes to zero like $1/r^2$, with zero pressure at infinity. This is, in fact, the required boundary condition. Such are the characteristics of these general solutions. We have plotted them for $1 \leqslant \alpha \leqslant \frac{5}{3}$ and for many values of λ. One can compute the many asymptotic relations, but that is not terribly interesting for a qualitative discussion such as this. The point is that if we believe the pressure to vanish at infinity, *i.e.* no box enclosing the sun, then the corona must follow this critical solution. The velocity at the base of the corona is of the order of tens of km/s.

(Note that « base of the corona » refers to a radial distance $3 \cdot 10^5$ km above the photosphere—10^6 km from the center of the sun—where the particle density is about $3 \cdot 10^7/\text{cm}^3$, probably a bit lower at sunspot minimum, and a bit higher at maximum.)

— P. LEDOUX:

What is the total mass of the expanding atmosphere?

— E. N. PARKER:

The same as the existing corona, because the mass of the corona is essentially contained in the first scale-height.

— P. LEDOUX:

But you have a density decreasing as R^{-2} which, integrated to infinity, gives an infinite mass, If you assume that you consider a transient stage, you will have some kind of front at the most external point reached, and boundary conditions there should fix the motion—you will not need to go to infinity. But the solution you have discussed is selected on the basis of a boundary condition at infinity—not to have a finite pressure there.

— E. N. PARKER:

The point on the integration would be true if you really took the solution extending to infinity, but sooner or later the interstellar medium must stop the flow. And, on specifying the pressure, all I need to do is replace zero by 10^{-14} dyne·cm^{-2}, the pressure of the interstellar medium, and I will reach the same conclusion on the results. The critical aspect of the solution is to end up with a very low pressure, rather than with a factor only 10 or 10^2 less than that at the base of the corona, which is some 10^{-2} dyne·cm^{-2} corresponding to the values assumed.

— F. KAHN:

Am I right in thinking that these equations are the same as Bondi's equations for the case of spherically symmetrical accretion?

— E. N. PARKER:

I am sure that they must be. Only the sign of the velocity would be changed.

— F. KAHN:

In that case, would the stable solution be the one which is subsonic at ∞ and supersonic near the star?

— E. N. PARKER:

He would probably have used an effective $\alpha \leqslant \frac{5}{3}$ with small velocity and pressure at infinity. It is hard to compare two such unlike situations.

— F. KAHN:

There must be some reason why one flow is stable for outward motions and unstable for inward motion.

— A. J. DEUTSCH:

I believe that this solution is probably the one appropriate to describe the solar corona, but I cannot agree with the remark that one recovers the same result if he requires either the solution which goes to 10^{-14} dyne/cm^2 at infinity, or that which goes to zero. One can obtain adiabatic solutions which go exactly to 10^{-14} dyne/cm^2, or to any other prescribed value, and they get there with zero velocity. Parker's solutions do not have that character. If this is of any physical relevance (and I am not persuaded that it is or is not), it may well be quite an important distinction. The next point, which is related to this: it is not immediately clear to me why, if one is uncomfortable with a solution which yields a pressure many orders of magnitude too high at infinity to be balanced by the interstellar medium—and incidentally a non-zero velocity—he is nevertheless prepared to accept a solution which yields a pressure many orders of magnitude too low, and a finite velocity.

— F. KAHN:

I wonder whether there is any use in having a solution which is subsonic at infinity. You have an interstellar medium moving relatively to the stars at a speed that is much higher than the sonic velocity at infinity. The usual speeds are to 10 to 20 km/s, sonic speeds are of the order of 1 km/s. So you could not possibily fit a subsonic solution at all.

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— W. M. McCrea:

These are indeed exactly the same questions as for symmetrical inflow which were solved first by Bondi, and about the same time by Ebert, and they obtained the complete set of solutions as Parker has drawn it. Now for inflow —as I showed afterwards, the likely thing is for the motion to follow one solution curve a certain distance and then to get a shock and then to follow a different solution curve. I do not know whether the aerodynamicists agree but I think that this is what happens. Now I spent a lot of time, myself and a pupil, trying to get outflow, and I could not get anything plausible. I got this solution that Parker has talked about. But you have got to give the material the velocity at some level. And that is the whole problem—you can always get the solution if you get the exact initial condition, but we could not see how this could come about in reality. The only hope that I could see is if you have a solution that is essentially non-steady, and carry the solution right down to the center of the star, and have a star which instead of being in static equilibrium is somehow an expanding system, where the velocity of expansion deep down is small but not zero and is appreciable in the region of interest. But that was too difficult a problem for me. I think that aerodynamicists ought to say what they think.

— A. J. Deutsch:

I would keep open the possibility that the conditions of some appropriate fit onto the interstellar medium may indeed produce a disturbance that will propagate into a distance of not many stellar radii. I would agree that the fitting requirements on the interstellar medium may make no difference at the surface of the star, but I am not prepared to admit that the corona even as close as 2-3 radii is equally insensitive. My principal reason for this insistence is that if we admit the relevance of these boundary conditions, we can reproduce within an order of magnitude *both* the temperature and density of the solar corona. This impresses me as being too much for chance coincidence.

— E. N. Parker:

If you feel that there is something to this point, it should be calculated. I think you are right that the temperature and density are due to the interstellar medium rather than to the convective zone of the sun.

— L. Davis:

If one is concerned about boundary conditions between expanding medium and interstellar space, he should include the pressure of the interstellar magnetic fields, which are probably 100 times the gas pressure and are anisotropic. So long as you are not worried about these boundary conditions, you do not have to worry about the interstellar magnetic field.

— L. BIERMANN:

I would suggest this magnetic pressure might be as large as 10^{-11} dyne/cm^2.

— E. N. PARKER:

What would happen is that the solar wind would rush outward until its momentum density equals whatever pressures there are, and at that point you get some sort of disordered interface.

— C. DE JAGER:

In the case of the sun, we are dealing with rather special boundary conditions because the sun is surrounded by the planetary system, and I have the feeling that in the region of the planetoids there is an additional source of turbulent magnetic fields. The system of the planetoids surrounding the sun is known to consist of a number of large planetoids, a greater number of smaller particles, an enormous number of little stones and dust and so on, and the many collisions between these particles may produce, by evaporation, turbulent gas, that will be ionized by the sun. The turbulent masses of gas could produce turbulent magnetic fields. Evidence for these turbulent magnetic fields is found by cosmic ray observations. As SIMPSON showed, these observations of scattering of solar cosmic rays may be explained by assuming that the interplanetary magnetic fields are greatly turbulent, say, at distances beyond the distance of the earth. So, I have the feeling that the magnetic pressure at this distance may be considerably higher than the magnetic pressure in the interstellar medium. So, if we want to discuss the problem of the outflow of matter from the sun, we must take into account that already at distances of about one or two a.u. it collides against this turbulent field, which will act as a filter to the outgoing matter, which still will go out finally, but much more slowly.

— A. B. SEVERNY:

I would appreciate a comment on the applicability of a hydrodynamical treatment of the problem, because the mean-free-path may be something like the sun-earth distance.

— E. N. PARKER:

The mean-free-path is comparatively short—about 1/10-th the characteristic dynamical length.

— E. SCHATZMAN:

You may have at least two kinds of discontinuity: You may have an ordinary shock and you also can have a change from an H II to an H I region

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and then in the ξ, ψ plane I think that you shift from one point on one of your curves to some other point. I think that at least in some cases that is part of the difficulty.

— E. N. PARKER:

Let me put some dimensions on here. The critical point occurs within 2 or 3 solar radii of the sun, and once you are well past the critical point you can completely neglect the internal state of gas—it is on its way at 500 km/s. It is true that if I were trying to compute the temperature of the gas at the orbit of the earth I would have to pay very close attention to these things. But as a matter of fact the velocity and density of the mass flow depend upon them hardly at all. The expanding gas has climbed out of a 600 km/s potential well, which is equivalent to about one kilovolt per hydrogen atom.

Let me make one comment about McCrea's question as to what I do about the initial velocity of the order of 20 or 30 km/s in the corona. The initial velocity at the arbitrary radius $r = a$ goes to zero if I chose to decrease a. If I arbitrarily start at one million km from the center of the sun—of course the velocity is not zero.

— W. M. MCCREA:

That is the point I was drawing attention to on the other board—it is not a steady problem essentially. You can't go right to the center. You would have point sources.

— E. N. PARKER:

I would like now to comment upon the extension of the solar magnetic field into interplanetary space. If there is, in fact, hydrodynamic radial expansion of the solar corona, then it is obvious that the expansion must pull out the general solar field into a radial configuration. I don't know whether the general field is a dipole—but whatever it is, any lines leaving the surface of the sun which are not stronger than about one gauss are pulled out into some more or less radial configuration; I am sure that the fields play a role in the formation of the coronal streamers, as UNSÖLD has mentioned, but none the less they are stretched out in what I would call a roughly radial configuration. Because the sun rotates, they spiral slightly. To give an estimate of the spiraling, a 400 km/s wind at the orbit of the earth results in a spiral which just reaches 45°. Hence inside the orbit of the earth the field is principally radial, and outside the orbit of the earth it is principally azimuthal. I make this point because I shall need it when speaking briefly on the active sun.

Some times on the sun in an active region there is an enormous flare, and one observes that the solar corona over a large region above the flare rises very quickly to sometimes 4 million degrees. As soon as the flare is over you see that the temperature is roughly doubled, and it remains high for a day or two or three—sometimes even a week—thereafter. The point I would like to make is that this suggests that a hydrodynamic explosion must take place. So I have investigated the hydrodynamics of blast waves from the sun. Again I assume spherical symmetry, and this time I agree that it is not a very good approximation. But let me make this one point—if I assume spherical symmetry about the center of the sun, I will get a lower limit on the velocity, for the same temperature profile. So I am not overestimating the velocities with the spherical symmetry approximation. One uses the standard techniques $\eta = \tau/r^{\chi}$—the similarity variables for progressive waves—it is all in COURANT and FRIEDRICHS, and I will not bother you with it.

The density ahead of these waves falls off like $1/r^2$, from the quiet day solar wind model where the velocity is roughly constant at large radial distance. One assumes that the thermal velocities are small compared to the shock velocities, so the Mach number is large—so you get a factor of 4 increase in the density across the shock at the head of the blast wave. Now, if a flare, and the corona over the flare, were merely a single explosion, so that the energy were all added in an hour and no energy added thereafter, then you have a true blast wave—the density would rise by a factor of 4 and fall again behind the shock, with the parameter $\chi = \frac{3}{2}$. ROGERS worked out this solution several years ago. Now it is observed that the solar corona remains at its elevated temperature of four million degrees for a day or so thereafter. So there is the possibility that it will expand and continue to push on the back of the blast wave. That is what the cases represent, $1 \leqslant \chi \leqslant \frac{3}{2}$. If I take an extreme case where I assume that the corona pushes on the blast wave so hard that the kinetic energy of the blast wave increases linearly with time, then I get the step wave for $\chi = 1$, which is quite thin. On the sunward side of the rear of the blast wave there is nothing but hot coronal gas pushing outwards. I think that $\chi = 1$ is an extreme case, of course. Now if the corona pushes so that the energy goes up like $t^{\frac{1}{2}}$, one obtains another curve, etc. So I offer you a sequence of blast wave profiles—one will have to decide from the observations which is appropriate.

The question is, do these blast waves have the right velocities and densities to agree with what rough observations we have? At $4 \cdot 10^6$ °K we go to our stationary solutions for the hot coronal gas driving the blast wave, and ask are there any solutions flowing outward with a thousand or 1500 km/s velocities? The answer is yes, 1200 km per s is a rough estimate of the rate at which the rear of the blast wave might be driven outward by a $4 \cdot 10^6$ °K corona. The front of the blast wave automatically goes faster, at 1500 km per s or higher. The most extreme figure quoted for the density is $10^5/\text{cm}^3$ at earth, and all I can say is that the blast wave densities can match that.

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If the corona is several million degrees all the way to the back of the blast wave, then the pressures are so high in back of the blast wave that the density can in fact be that high. I do not wish to argue the point one way or another. The blast wave from a $4 \cdot 10^6$ °K corona can duplicate the 1500 km/s and $10^5/cm^3$ suggested from observation. Our hydrodynamic model of both the quiet and the active sun accounts for the observed solar corpuscular radiation.

Now consider the cosmic ray intensity in interplanetary space, which is how I got started on this whole calculation. I am immediately concerned with the magnetic field configuration in interplanetary space. In the quiet-day solar wind, the lines of force of the general one gauss solar field are drawn out in Archimedes spirals, reaching 45° from the radial direction at earth, as I have already mentioned. The blast wave energy, increasing like t^ν, distorts the quiet day field. And now what effect does this have on the cosmic ray intensity? It is very easy to compute the extent to which cosmic rays are swept out of the inner solar system by outward sweeping magnetic fields. You find that you get up to 40 percent cosmic ray decreases with an energy dependence which is something like reciprocal magnetic rigidity, which is in rough agreement with the crude observation that currently exists. Such a decrease is immediately recognizable as the Forbush type cosmic ray decrease, observed following a given flare on the sun, simultaneously with a magnetic storm on earth.

— W. H. McCrea:

Is this the dipole field that you are distorting?

— E. N. Parker:

I do not wish to commit myself as to whether the general solar field is a dipole. Any lines of force ($B \leqslant$ one gauss) will be drawn out as I have shown them, regardless of how the field density varies over the surface of the sun.

— L. Davis:

We have seen some figures showing magnetic fields in the solar system and heard some discussion of this. In the last few months Pioneer V has reported back observations on one component of the magnetic field in the solar system over a period of some 50 days, and following around the orbit of the earth, but with the satellite going somewhat towards the orbit of Venus during this time. The magnetic field during this time as reported by Senett and his collaborators at Space Technology Laboratories is very hard to fit to any ideas that one has. It is just possible that the apparatus is inhabited by a gremlin that is trying to upset it—but there is nothing in the experiment that

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would indicate this except that the results are so surprising. Something like a sixth to a quarter of the time the field is surprisingly uniform and steady at about 2.5 times 10^{-5} gauss. More than half of the time the fields show quite a bit of irregularity. It may run up to $4 \cdot 10^{-4}$ gauss on some occasions. These disturbances seem to be correlated with geomagnetic disturbances which would indicate that it really was not due to gremlins. In any case they seem to indicate that more than half the time there is a much more irregular magnetic field than Parker's figures would seem to indicate, and the direction seems completely wrong.

— A. UNSÖLD:

A word on the problem of the temperature and density gradients in the corona. The coronal density gradient leads to temperatures of about $1.6 \cdot 10^6$ while the line-profiles consistently give $2.5 \cdot 10^6$. I have wondered at the reason for this discrepancy, and I think it is the ray-structure of the corona. The corona essentially follows systems of lines of force, and VAN DE HULST showed long ago that a tube of magnetic lines of force is filled up from below like an isothermal atmosphere without regard to sideward limitation. If one wants to bring into agreement the stratification measured by BLACKWELL for the average corona, on the one hand, and the higher temperature of $2.5 \cdot 10^6$ on the other hand, one must make suitable assumptions as to how the coronal rays thin away further away from the sun. One finds *e.g.* that at 5 solar radii, roughly $\frac{1}{5}$ of the sphere is crossed by such rays. Such a degree of inhomogeneity agrees well with what ALLEN deduced several years ago from the waviness of the observed isophotes. This picture seems to have various implications. For instance, the more or less explosive way described by PARKER of producing transient phenomena is certainly not the only possibility. Radio observers know since a long time that the so-called slowly variable radio frequency radiation in the decimeter range is due to « coronal condensations » which often pass into coronal rays. These are often produced by the following mechanism. The amount of matter in each tube of force is simply proportional to the density at which the coronal region begins, and that critical density is higher if the mechanical flux is higher. So one would explain these rather quiet columns of gas essentially by just assuming that they have the same density gradient as their less dense surroundings, but that the density is everywhere multiplied by a certain factor which observations show to be about 5 or 10. It may of course be that there are other possibilities of getting matter into higher layers.

— E. N. PARKER:

This is an interesting idea. Do you have in mind the corpuscular radiation following a flare, when you discuss this more or less quiet streaming?

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— A. UNSÖLD:

No. My opinion is that we have two different possibilities which apply to two different phenomena. Also, our opinions seem to differ somewhat as to the coronal temperature—I put the average value a bit higher. Then, I would emphasize that the curves fitted to Blackwell's data largely reflect how the magnetic tubes of force thin out.

— A. UNDERHILL:

The flow velocities from stars do not seem to exceed a value of 10^3 km/s by more than a factor of 2 except in the supernovae. Is there any hydrodynamical reason why we do not observe velocities as high as 10^4 km/s? It does not matter what kind of star you take—the outflow velocities apparently lie between 10 and about 1000 km/s.

— E. N. PARKER:

I will attempt an answer. Simply remember that the velocity goes up only as the square-root of the energy; and even if you take 10 or 20 million degrees, it is hard to beat 1000 km/s.

— W. H. MCCREA:

I make one simple observation, which is not meant to be cynical, although it sounds like it. Fifteen years ago, HOYLE, BONDI, and LYTTLETON explained the solar corona by infalling material and got a suitable density gradient. Here, you reverse all the velocities; naturally you get the same density gradient.