i) This arc (ζ_∞) lies below the arc $(\zeta_*)_s$ which goes through A. The corresponding (ζ_∞) curve goes through the origin and checks the assumption b). Thus, it is a solution. No other solution is possible because according to (15), such an arc (ζ_∞) would be the image of a flow lying downwards of a shock, if the image of the upward flow were a (ζ) curve lying above the two branches of the critical curve (ζ_*) and thus condition b) would not be satisfied. (ii) Or this arc (ζ_∞) lies above $(\zeta_*)_s$. A shock must be present. For the reason given in i), the only possibility to check assumption b) is to have as image of the upward part of the flow an arc of curve $(\zeta_*)_s$ which corresponds to a flow starting as a subsonic flow for $x=0$ and which is accelerated smoothly through the trans-sonic regime (saddle point A).

The position of the shock is defined as the intersection of the (ζ_∞) curve with curve (ζ), which represents the locus of flows lying just behind a shock when the above state lies on the $(\zeta_*)_s$ branch. It may be checked that these two curves have one and only one point of intersection.

The special case $\gamma=\frac{5}{3}$ is a limiting case whose discussion is left to the reader. We want to emphasize that assumption b) is necessary to guarantee the uniqueness property.

6. – Signification of the results.

The values of τ_∞, T_∞, B are given by the data of the interstellar medium. But the value of m depends on data on the state observed on the surface of the star. Let us note with a subscript the corresponding values of various quantities $(r_0, \tau_0, T_0 \dots)$

$$m = u_0 r_0^2 \tau_0^{-1} .$$

In order to compute m, u_0 and τ_0 (or T_0 and τ_0 according to the Bernoulli equation) must be known. To compare the theory with experimental data, one must check that the flow on the star is subsonic and that the flow at r_0 may be compatible with the data at infinity thanks to the uniquely defined flow in **5**.

Another way to test the validity of such a theory is to notice that data for various stars must be fitted as explained above with the same interstellar medium.

Discussion:

— A. J. DEUTSCH:

I should like to clarify several points. First, I think it is not quite correct to say that there is a controversy between PARKER and myself. As I understand the situation, there is not necessarily any controversy. We depart from different sets of observations, with different physical problems in view, and attempt, each of us, to find the solution appropriate to his particular problem. It would be nice, of course, if we could relate these two. As I understand it, Parker's problem is this; he believes there is good observational evidence for a high temperature solar wind of the order of 200 particles per cubic centimeter, at one astronomical unit from the sun, moving with speeds of the order of 500 km/s. My observational evidence, on the spectra of the M-giants, relates to a situation in which I observe gas flowing outwards with speeds of the order of 10 km/s; I do not know what the density is; I do have some indication of what the temperature is, and it appears to be very low. So, there are major differences in the point of departure.

Consider again Fig. 8 from my paper, which summarizes the regimes that are possible so long as I restrict myself to the case of continuous adiabatic flows. The abscissa is the value at the base of the flow of the ratio of the escape velocity to the thermal velocity, and the ordinate is the initial Mach number. From this Figure I see that I have the option of going to flows which at the base are characterized either by very large flow velocities—that means large β_0, or by very high temperatures—that means large $(V_{th})_0$; or both. If, for example, I go in the direction of low initial thermal velocities, so that the gas is cool near the surface of the star, then I must move to the right in this diagram. But I find that I cannot go very far in that direction in the subsonic regime before I run into the region where no continuous flow is possible, and I am therefore forced up into the supersonic regime, where the initial flow velocities are at least comparable with the escape velocity; and I say that the observations of the M-giants do not admit this possibility. For if I have a cool gas, which is expanding from the star with flow velocities comparable with the escape velocities, I must expect to observe this, and I do not observe it. And therefore I take the only option which is left to me within this framework, which is to say; there may be a high temperature region around the star, sufficiently high in temperature that I cannot observe it. Let's see how well I can get along, by moving off more to the left in this diagram, and simultaneously attempting to keep the flow velocities as low as I possibly can.

Return to Fig. 10, (page 254), which illustrates the characteristics of a typical supersonic adiabatic flow. Notice here that the initial flow velocity is almost equal to the escape velocity; but the initial temperature is rather high, so that I need not to be too disturbed that I fail to observe flow velocities that are comparable with the escape velocity. However, I find that by the time I get out to 10 stellar radii, the flow velocity has risen to about 150 km/s. The temperature is till too high here for me to observe the gas. But the flow velocity stays high; it has already essentially reached its asymptotic limit, and beyond $x = 10$, I must expect the velocities to remain high, right into the observable cool region. This clearly is inadmissible from the point of view of the observations. To the best of my knowledge, all of the supersonic adia-

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batic flows will have this characteristic, and are therefore inadmissible in attempting to explain the observations of the M-giants. γ is $\frac{5}{3}$ here.

Now, in endeavouring to discuss the subsonic adiabatic flows, I asked myself whether one can limit the possibilities to flows which will merge smoothly with the interstellar medium—that is to say, in which the velocity goes to zero, as indeed it does in all of these subsonic adiabatic flows, and where simultaneously the temperature and density go to the values appropriate for the interstellar medium. And I find that I can do this, if I limit the possible range of values of the initial Mach number and the ratio $(V_{es}/V_{th})_0$. Fig. 11 (page 255), again reminds us of what those limitations are. I find that, corresponding to a given escape velocity, the initial flow velocity may have any value which lies below the line labeled V_0. The initial temperature, however, is very closely prescribed; it must lie on the dashed line if the initial Mach number is zero, and it must lie on the full line if the initial Mach number is 1. Moreover, it must lie between these two lines regardless of the value which is assigned to the temperature of the interstellar medium, whether this temperature be 100° or somewhat more than 10 000°. Similarly the initial density is closely prescribed by the outer boundary condition. At this point, for the first time, I noticed that at the abscissa 2.5, which corresponds to the escape velocity at four solar radii from the center of the sun, the diagram predicts a temperature of 3 million degrees, and a density of $3 \cdot 10^5$ protons/cm^3. Since these numbers correctly represent the solar corona within a factor of 2 or 3, I naturally wondered whether the same notions, which I was developing in the context of the M-giants, also have an application to the case of the solar corona. I throw this out as a question. Certainly, if we go to this kind of an interpretation, then we cannot reproduce the high velocities for which PARKER believes there is good observational evidence in the neighborhood of the earth.

Now, I have considered in most detail a case where the initial velocity is 44 km/s. However, since the initial temperature is 270 000°, I probably will not be able to observe the gas at the point where it moves with this velocity. I find that in its subsequent motion the gas quickly decelerates, until by the time I've reached 100 stellar radii, the velocity is down to 2 or 3 km/s. I notice that the gas always moves with a velocity less than the local escape velocity, V_{es}, which is given by the straight line. To find whether a flow of this kind is consistent with the observations, I compute the projected density of Ca II in the line of sight, taking rough account of the ionization gradient in the flow; and I also compute the mean expansion velocity along the line of sight. The computed surface density turns out to be less than 1 percent of the value observed in the red giants; and the computed expansion velocity is also small, by a factor of nearly 10. It must be noted here, however, that there is a grave question as to whether one can give any physical justification

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for assuming that the flow approximates adiabasy. The one investigation of this question that has been made, by R. WEYMANN in his thesis at Princeton indicates that radiation causes gross departures from adiabasy, at least for the case of flows which are somewhat more dense than the one which I considered here. In any case, it looks as though most of the M-giant stars are losing mass at a rate which violates the limit set by the nozzle that we were talking about earlier. It looks very much as though we have rates of mass-loss which exceed by several thousand times what this kind of flow can give.

— E. SCHATZMAN:

I think that the adiabatic condition is too correct to obtain the right dependence of temperature, density, and pressure in these layers, and that the difficulties you have with the late giants can come from that.

— A. J. DEUTSCH:

I have the impression from Weynmann's work that the difficulties become more severe as the density goes up. The difficulties are apt to be much less in the case of the early M-giants. I'm not sure that the adiabatic approximation is too bad there, but when the density goes up and the radiative processes take over, then they remove the basis for this whole picture.

— R. LÜST:

I want to put a question. Would it not be possible that these kinds of stars would be surrounded by a hot corona? In this case there might be no such difficulties with the mass-loss; and the same picture which PARKER has applied to the sun, would then be applicable for these kind of stars.

— A. J. DEUTSCH:

Yes, I think it is. I think that at the base of flow which I just described there is something that approximates a corona. The temperature may be something like 300 000°; one does not need a temperature as high as 1 million degrees. I should like to know whether, starting with a very much higher temperature—say a temperature ten times higher—one can contrive to keep the velocities in the observable region down in the range which I observe. I'm unable myself to see how to do this, particularly if I have to go to supersonic flows at large distances.

— P. GERMAIN:

May I ask DEUTSCH if he can show us which part of the integral curve he has considered, especially where he locates the initial value of R in the diagram. It is the subsonic part which is after all the critical value of R.

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— A. J. DEUTSCH:

The difficulty is that Germain's discussion breaks down for the particular case I have discussed, where γ equals $\frac{5}{3}$. I said that the critical point moves off to infinity. It does not, it moves off to zero. I think the curves can always be divided into two classes, can they not? In one class, the velocity always decreases monotonically and goes to zero; in the other class, the velocity increases monotonically. I'm not sure of the latter. I think it goes logarithmically to infinity.

— W. V. R. MALKUS:

The process of accretion has been touched on occasionally in the symposium. Could DEUTSCH comment on any evidence that stars in the denser regions of the interstellar gas have spectra which may indicate inflow? Possibly CLAUSER or PARKER could comment on the inflow case?

— A. J. DEUTSCH:

I think a fair statement of the case would be that we are pretty much guided in the choice of the theoretical problems we investigate by the observational problems with which we are confronted. There are sound theoretical reasons for believing that stars must indeed be condensed out of the interstellar medium; and we know some places where we think we can see this happening. The details of this process, however, are extremely small. But in recent years we have been confronted with a lot of evidence that indicates that we can see before our eyes a wide variety of stars spraying matter out into the interstellar medium. Therefore the emphasis has been laid, I think reasonably, upon these problems. I think the statement made earlier, about our having little or no evidence for seeing matter fall into stars, is correct. This must happen; but it's awfully hard to observe it.

— N. MILFORD:

I would like to address a simple question to the aerodynamicists. Would it make any significant difference if the boundary conditions at infinity change, as they do, because of the variations in density and velocity of the interstellar matter? If these changes occur in a time of order 1000 years, would there be any significant feedback into the inner regions?

— H. LIEPMANN:

I think that we should actually ask the question a little broader. We have discussed so far only stationary solutions; and the problem cannot be stationary, I think, since probably you have explosive formations on the surface of the stars, and you have changes at infinity. So, if you like, we should maybe

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take a few minutes and discuss the possibilities of non-stationary outflow, or influence of non-stationary conditions at infinity on the outflow.

— R. B. LEIGHTON:

Also, will it really be true that the flow can ever be isotropic, because the star is moving through the interstellar medium?

— A. J. DEUTSCH:

Certainly these complications require consideration. However, the observations suggest that they probably represent second order effects. That is, there is observational evidence, supported by theoretical arguments, that the processes we are considering here are quasi-stationary. I think we should expect to be able to give a fairly good account of the observations in terms of a stationary theory. But there may be some very interesting spectroscopic problems relating to non-stationary problems.

— H. PETSCHEK:

Could one give a criterion for when he can treat the flow as quasi-stationary? I think this would be when the time it takes the particles to go through the flow field is shorter than the time in which the boundary conditions change.

— A. J. DEUTSCH:

That condition is satisfied. You see, at 10 km/s, matter moves 10 parsec in one million years. The average distance between the stars is of the order of 1 parsec.

— N. MILFORD:

I don't think that there is general agreement that the fluctuations in density of the interstellar medium are necessarily of the same order as the distances between the stars; we don't actually know what the scale of the density fluctuations is. In previous meetings of this series we have had several different scales given for these fluctuations.

— F. KAHN:

I would have thought that the scale of fluctuations was very much larger than the distance between the stars, with clouds possibly 5 parsec across and maybe 100 parsec apart, so, in fact, if you make the scale of fluctuations about 1 parsec you are making a gross underestimate.

— F. H. CLAUSER:

I think that I can give a statement as to what to look for in this non-stationary case. You can see this in your own washtub, if you allow the water

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from the faucet to strike a flat plate there. You will find it goes down a column, and spreads out, and reaches a supersonic water velocity. You find that the flow spreads into a very thin, high speed layer; and then out at a certain distance, it goes through a shock-wave, in which the height of the layer increases manyfold, and the velocities become very low. This is my picture of what happens in the stars, that you get this supersonic outflow, and that out at a great distance there is a shock-wave, that converts the flow back to a higher density, higher pressure, lower velocity flow. Now you ask what happens if I begin to disturb something in the interstellar medium; and in particular, what would happen, if one gave a flow to the interstellar medium. All that happens in the bath tub analogy is that the circular ring that forms will be distorted; if you bring water in from this side, the ring will move over. The supersonic portion will be absolutely unaffected, and have no knowledge that any of this has happened; so that the entire set of boundary conditions given by movement of the shock-waves and all of the interstellar discontinuities, non-stationary effects, etc., will be reflected by a movement of the shock-wave. So anything that you do out in the interstellar medium will have no influence on the supersonic flow—it is effectively isolated by the supersonic flow, and there is no way that things can move upstream. The entire change, due to the presence of the non-stationary effects and fiddling in the interstellar medium, will appear to be a movement in and out of the shock-wave boundary.

— A. J. DEUTSCH:

Let me ask two questions: First, whether in the astronomical context it's possible to give now some estimate of the order of magnitude of the radius of the standing shock-wave; and second, would you expect that this shock would lead to any observational consequences?

— E. N. PARKER:

You can estimate the shock position by the following argument. Coming out from the sun is a flow that has essentially constant velocity after about $(20 \div 30)$ solar radii. Thus, density falls off as $1/r^2$ and can be computed from its estimated value at the earth. The condition giving the shock mentioned by CLAUSER is simply that the pressure of the solar wind after passing through the shock must balance the interstellar pressure. The pressure across the shock is essentially ϱv^2. Take a velocity of a few hundred km/s and a density of some 10^2 particles/cm² at the earth. If the interstellar pressure is 10^{-14} dyne/cm², the radius of the shock is 5000 a.u. If we introduce a magnetic pressure —which BIERMANN suggested might be a factor of 10^3 higher than the gas pressure—the radius is reduced roughly by 10^3, or to about 160 a.u.

The observable consequences of such a shock are probably not visual, be-

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cause the temperature is very high beyond the shock. I think that the most important consequence of the shock is its cosmic ray effects. The cosmic ray intensity in the inner solar system, during the years of solar activity at least, is rather low compared to the intensity in interstellar space. And this is apparently due to outward convection of cosmic rays in disordered magnetic fields, which occurs probably on the near side of the shock boundary as well as beyond. Thus, the 11-year cosmic ray intensity variation observed at earth probably originates in part at the shock transition and beyond.

— A. J. DEUTSCH:

In the case of standing shocks around stars, is there any hope that one might attempt to relate these to the generation of non-thermal radio noise?

— E. N. PARKER:

I am sure one would find a relation, because you would generate a lot of high-energy particles as a result of such a shock. The point is, that at sunspot minima, one observes a cosmic ray intensity which is high relative to when the sun is active. Cosmic rays come from outside the solar system except for brief intervals when the sun generates a few. Moreover, whatever is depressing the cosmic ray intensity must lie well beyond the orbit of the earth because there is no gradient at the earth. Finally, the only way one can exclude cosmic radiation is with a magnetic field. So, we conclude that the sun does something to depress cosmic rays, and we note that the shock just discussed occurs well beyond the earth orbit and will be the first thing encountered by the incoming cosmic radiation, by way of disordered magnetic fields. Any magnetic field will have some discontinuous configuration across the shock. To settle the cosmic ray problem completely, one must relate everything that goes on in the way of disordered magnetic fields from outside the shock clear through into the inner solar system.

— L. DAVIS:

There are actually two things that I want to say. First, of course, this question of what happens in the region around the sun as the gas flows out towards the interstellar magnetic field is a complicated one which has been discussed by cosmic ray physicists for about five years. There are a variety of models—PARKER says probably none of them is right—but some of them are more right than others and one can combine features from them. There is one thing in the model that he mentioned that I think I wanted modified. As the activity of the sun changes, the position of that shock front is going to creep in and out, just as Clauser's ring moves in and out when you turn the tap on and off in your bathtub. This changing volume of the region in which the cosmic rays have difficulty in penetrating will also produce effects. Another reason that the cosmic rays have difficulty in getting into the region near the sun, in addition to the difficulty of diffusion through disordered fields, is just the fact that the cosmic rays in the galaxy are going along the galactic lines of force which don't come into the region inside the shock-wave at the interface between the solar wind and the galactic field. They can easily get into this region only at the ends where the galactic field splits or along irregularities. Well, I say that this indicates that the model is more complicated—which of course PARKER knew from the beginning as he told us.

The other point concerns what one might think would happen when this solar wind comes out and strikes a magnetic field. And here I come back to the point of view of which I seem to be the sole representative—of looking at the information that one gets from satellite observations. Let us consider not a galactic magnetic field against which a solar wind blows, but rather the earth with its dipole field, and let us look to see what happens in the region where we think the solar wind is blowing on the earth's dipole field. We find that in 2 satellites—one which went out within 10° of the earth's sun line and one which went out within about 45° of the sun line—there is evidence of the same thing happening. Unfortunately the evidence is not clear enough so you say precisely what happened but you can give some idea of what it is. First, the solar wind did not push the geomagnetic field in as far as one would have expected from the simple momentum balance that PARKER gave. Correspondingly, one would think that it might not push the boundary

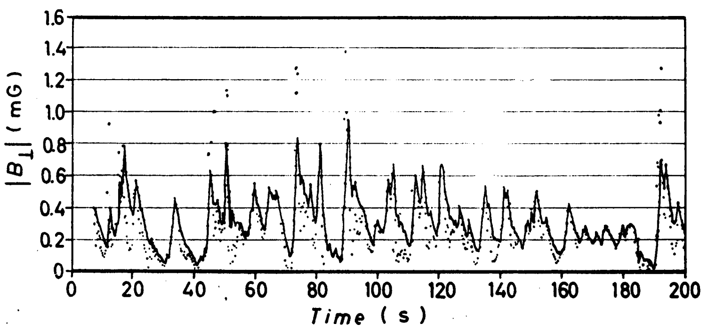

Fig. 5. – The component of the geomagnetic field normal to the spin axis of Pioneer I in the region of 80 000 km from the center of the earth.

between the solar wind and the galactic magnetic field out as far as the momentum balance would lead one to expect. The second thing is found in the rather thick boundary region between 80 000 km from the earth's center and 120 000 km, where the last trace of the dipole field is found. In this region there were very irregular magnetic fields as shown in Fig. 5, which is taken

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from the work of Dr. C. P. SONETT and his collaborators at Space Technology Laboratory. It shows the magnetic field plotted against time, as observed by the satellite. The expected dipole field that one would get is about 30 gamma, that is about $30 \cdot 10^{-5}$ gauss. That is about the average field, I think. What one sees is a field that has wriggles in it. The time scale is something like 10, 20, 30 s between peaks. It falls down to very much less than 30 gammas in some places. This could be a geometric effect since the satellite measures only the component of the field normal to its spin axis and, if the field varies in direction, you could observe only a small component of a large field at some points. These peaks though will go up—some of them to 100, many of them to 60 or 80 gamma. Many of them are quite symmetrical, many others start out to be fairly symmetrical but show a filling in on the back side. These peaks do not seem to be stationary structures, existing in the outer atmosphere of the earth as a constant high magnetic field in a little region. This just seems impossible. They look more like waves, probably propagating inward—if they are propagating inward they have slightly steeper fronts than backs. They have more resemblance to a symmetrical solitary wave than to the classical shock, which I suppose one would think of as a steep front and fairly flat back. The times of sharp rise and fall are a few seconds. It would appear then that these waves transfer the momentum of the solar wind across weak fields rather than blowing the field away; they probably also carry substantial amounts of energy. Perhaps a little later on it may be possible to say more about what the nature of these waves is. But I think one can regard this as a kind of a laboratory scale observation of some phenomena which probably have importance in many of these astrophysical situations—if one only sees how to transfer it.

— H. LIEPMANN:

It looks to me as if this is one case where you may infer that the non-stationary problems are important in discussing the solar wind.

— F. KAHN:

I think that the question has been raised whether one is justified in talking about a hydrodynamical approach to the question of the outflow of material from the sun—whether an adiabatic approximation is good in that case or not. And I think one can find some numbers to show that there is nothing much to worry about. Let us first consider whether the mean-free-path is small compared with the scale of the motion. If we start with a completely ionized gas, we get a collision cross-section of this form

$$\sigma_{\text{collis}} \text{ is of the order } \frac{\pi e^4}{(kT)^2} \log \left\{ \frac{(kT)^3}{4\pi N e^6} \right\},$$

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N being the particle density. Now in the case of the sun, starting from a place where particle density is 10^7, temperature = one million degrees—we find a collision cross-section which is about $\sigma = 3 \cdot 10^{-16}\,\text{cm}^2$, and again putting at $N = 10^7$ we find that the mean free path is by two orders of magnitude smaller than the radius of the sun. Since we probably start considerably further out than at the surface of the sun, this seems to be entirely satisfactory.

Now the next point is, how does this vary in any reasonable flow? And we find that the further away we go from the sun the more satisfactory our approximation gets, because we have that, apart from a log factor which does not change too much, the length of the mean-free-path varies as $(Nk^2T^2)^{-1}$, which is proportional to $1/Na^4$. In the adiabatic flow of a monotonic gas, Na^3 is constant. Thus, the mean-free-path varies as a, and its ratio to the typical length scale of the flow, R, varies as a/R. k = Boltzmann's constant, a=sound speed, R = distance from sun. Now you see, as we go away from the star, that we enter the region of supersonic flow ahead of the shock, and here the sonic speed drops all the time; R increases all the time. In fact, the approximation gets better and better, provided we are considering motion inside the stream. Of course, if the stream runs into another mass of gas, the mean-free-path for the collision will be determined by the relative velocity of the encounter and our mean-free-path formula would be wrong.

The next point is, can the gas stay ionized? There is a very rough formula for the rate of recombination of electrons with hydrogen ions which is valid —more or less—in a reasonable temperature range. The rate of recombination is $5 \cdot 10^{-11}\, n/\sqrt{T}$ recombinations/s per ion, where n is the electron density, T the temperature in °K. At the surface of the sun, or at least in the region where the stream sets out, the recombination rate works out to be $5 \cdot 10^{-7}\,\text{s}^{-1}$. You would therefore have to wait about $2 \cdot 10^6$ s before a given particle recombines if it stays in the corona. This is about a month, and, of course, much longer than the time it takes for a particle to get away from the region. As time goes on the rate of recombination goes as $n/T^{\frac{1}{2}}$. Once again n varies as a^3, $T^{\frac{1}{2}}$ varies as a, and $n/\sqrt{T}$ varies as a^2. The speed of sound keeps decreasing and therefore the recombination rate goes down and down. We are interested, in fact, in the recombination rate compared with the time a particle spends in a given region. This time is given by the distance from the sun divided by the speed of the stream. The ratio that we are after is thus a^2R/u; finally we have from the condition of continuity, or flux condition that NuR^2 is a constant of the motion. N again is proportional to a a^3, so that a^3uR^2 = constant, and we find that a^2R/u varies as $a^{\frac{1}{2}}/u^{\frac{3}{2}}$. Now u increases to a constant value; and since a again keeps on decreasing, the recombination rate multiplied by the time scale typical of the motion also keeps on going down. Thus, if the gas doesn't recombine while it is near the solar corona, it is never going to recombine at all. This also excludes the possibility that the sun raises

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the temperature of the gas by photoelectric heating, the particles just don't recombine—so you can't heat them up by ionizing them again. The only thing that one might have to consider is whether waves from the sun, such as are supposed to heat the corona, can travel out further into the gas when it is moving away from the sun. But I don't want to comment on that.

— H. LIEPMANN:

A comment on one point which SEVERNY brought up; namely, the question whether radial outflow of this type can be considered hydrodynamically stable, *i.e.* whether you expect radial velocities only, or whether you expect in a problem like this to get velocity components in a non-radial direction. Has anybody a strong opinion on this? My own opinion would be that it is stable—I think that radial outflow is stable and radial velocities dominate except of course during explosive processes like that on the sun.

— S. GOLDSTEIN:

Not only is a spherically symmetrical flow with a shock stable, but a flow which is non-symmetrical to begin with will, in a short time, approach symmetry.

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