PART IV.

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

D. - Collision-Free Shock-Waves.

Discussion.

Chairman: L. DAVIS

— R. LÜST:

Let me comment on this one-dimensional treatment and its connection with shock-waves. The first question is why we can treat the plasma as a fluid if we assume no collisions. Normally we apply the fluid picture if you are sure some set of particles move in some way together. This is possible if *e.g.*, the collisions serve as a mechanism to keep the particles together. If there is a magnetic field, and the particles spiral around it, then this provides a mechanism to keep the particles together even in the extreme case of no collisions. But in such a situation, the pressures parallel to, and perpendicular to, the magnetic field differ; so in your equations this difference in pressures must be included. Second, if you want to describe the plasma in this way, you have to be a bit more careful in the terms you retain. Normally, you apply the one-fluid hydromagnetic description, which means the normal hydrodynamic equations plus the $V \times B$ magnetic term. Also, you apply infinite conductivity, which means zero electric field in a moving system. Especially this last assumption is no more valid in the extreme case we consider. That is, in the conductivity—or more generally call it the diffusion—equation you have

$$(\ldots)\, \mathrm{d}j/\mathrm{d}t = E + (V \times B)/c + \ldots ,$$

where you normally set the sum of the first two terms in the right side equal to zero and neglect the other terms. But in the case considered here, when frequencies become comparable to the ion Larmor frequencies, these neglected terms become important. This is the analysis we have applied, including these terms. The 2-fluid description of the NYU group also includes these terms.

So then if you look for solutions which should describe shock-waves, you first find solitary waves. Their wave-lengths are of the order of the geometric mean of the Larmor radii of electron and ion. These are not really shock-waves at all, since all quantities return to their original value after passage of the wave. To describe a shock, we require some element of irreversibility; and the question is whether we can get it under this picture just described. This question has not been answered yet as far as our treatment goes; I think the NYU group makes the same statement.

It should now be noted that you can get shocks if you are not completely in the collision-free case. In this situation, these solitary waves may be of great interest in creating shocks, because with even a fex collisions, these solitary waves get damped and you finally end up with a wave-train where you have the right increase of entropy.

The other possibilities would be to find other ways of increasing the entropy, PETSCHEK has rejected the solitary-wave kind of solution in favor of a set of essentially unstable waves. There are other possibilities of instability; *e.g.*, involving the two streams of electrons and ions. But as a summary-conclusion, it can only be said that we have not yet really obtained a shock-wave in the absence of collisions.

— F. KAHN:

In this problem you are looking for a mean of dissipating energy in a plasma-magnetic wave, and electrostatic instabilities may do that for you. Suppose we have a plasma-magnetic wave propagating perpendicularly to a magnetic field.

The question is: Can the energy of this wave be dissipated without invoking collisions of individual particles. In the regions where the magnetic field is changing rapidly—the regions of increase toward, and decrease from, the wave-crest—the electron and the ion velocities will be considerably different. The ions will not be affected too strongly by the changing magnetic field; and thus you might have a situation analogous to what you get in the ordinary two-stream instability for plasmas, with an electron stream trying to get through an ion plasma. Now, it wouldn't, of course, be right to isolate one region of high current density, and just consider the two-stream plasma instability which might arise there, without information from all the other regions of high density. But I suggest that it would be a relatively easy problem actually to see whether the whole field of such a wave suffers from electrostatic plasma instabilities, because the problem is after all linear. What is more you can readily set up the undisturbed state following the motion of the electrons and of the protons, and then try to disturb it to see if there are any complex frequencies thrown up. That's my first point.

The second point concerns the solitary waves which are known to exist in the presence of a magnetic field. And now I will take an entirely different point of view; supposing in the end it proved impossible for us to construct a collision-free shock, would this be a tremendous disaster? If you consider the problem as it was first put by PETSCHEK, you are essentially asking what

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happens when a piston moves into a plasma. Somehow the plasma further ahead has to find out what's goind on behind, otherwise you get confusion. Now, if we were doing gas dynamics, we would say that no wave can be propagated fast enough, and therefore we must put some discontinuity into the fluid, which we shall call a shock; in the region of the discontinuity, the equations have to be a little bit modified because we are going to take viscosity into account. But if I'm right in interpreting some of the results on solitary waves in plasma-magnetics, one can get solitary waves of all speeds propagated into a plasma, provided only that one leaves oneself free to consider cases where electrons follow looped trajectories. This case proves rather difficult to consider and I believe in the paper by DAVIS, LÜST, and SCHLÜTER it was not treated. But if such a case is taken into account, I think there is no reason to believe a solitary wave cannot be found which will propagate at any speed you like into the medium ahead. Then, would it not be possible for the piston to find its right place, at any time, in such a solitary wave and to propagate the disturbance ahead in that way?

— H. PETSCHEK:

A similar problem to what you describe has been considered by ROSENBLUTH and LONGMEYER: taking a piston, which is impulsively put into motion with a constant velocity, and then calculating the time-dependent equations of motion in a way that is analogous to what LÜST described. I believe that the result that they got was that the density pattern had some wiggles in it, and the thing remained time-dependent. The density did drop off in a distance which was comparable to the thickness of this pulse, which incidentally is about 1/40 of the characteristic length that I used, so that if one looks at this grossly it would look like a shock-wave with a dimension of the order of $\sqrt{m_e c^2/4\pi N e^2}$.

— R. LÜST:

For some astrophysical situations, it is not too essential to get collision-free shocks; but there are situations where it is important. The first time the question has been raised was in connection with accelerating cosmic rays. There, the situation is that the gyro-radius of the cosmic rays is larger than that of the particles having thermal speed in the interstellar medium, but it is smaller than the free path of these thermal particles. If you want to deflect the cosmic rays by shocks in the interstellar medium, you need a shock thickness smaller than the gyro-radius of the cosmic ray particle. So if you have a shock structure which is of the order of the geometric mean of cosmic ray and thermal particle gyro-radii, you have problems; if it were of the order of the mean-free-path of thermal particles, this would be fine.

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Then I would only comment that I just did not want to bring in this question of loop trajectories. Neither we, nor the NYU group—with one important exception—have treated them.

— L. Davis:

I have a feeling that most of the people who have worked on non-loop trajectories have thought about the loop trajectory problem. You find the equations are much more complicated, but it looks as though the gross character of things would not be very much different if you could manage mathematically to handle the loop trajectories. So, this may be an over optimistic statement, but you start with the simple cases and you hope the others will not be too different.

— A. A. Blank:

I confine myself to the one question: Is there really any hope for a one-dimensional collision-free shock of the kind indicated by conventional one-fluid analysis? Apparently there is. The work is due essentially to Morawetz at NYU and I shall describe it in an intuitive way without writing equations or deriving numbers.

We are seeking irreversible transitions or shocks in a two-particle model. Imagine that we are observing a steady shock accompanied by a magnetic disturbance and that the fluid is passing through the shock from right to left parallel to the x-axis and perpendicular to the magnetic field.

In general we consider a two-dimensional distribution of particle velocities perpendicular to the magnetic field. The mean velocity is parallel to the x-axis and we denote its value before the shock by u_0 and after u_1. At present it is impossible to consider a complete Maxwellian distribution of velocities. Instead Prof. Morawetz took a cut-off distribution in the form of a circle centered at $(u_0, 0)$ in velocity space. Certain particles of sufficiently low velocity can loop, those corresponding to a low velocity sector of the circle in velocity space; the others cannot.

Without going into details of the analysis, let's see how the velocity distribution is altered upon passing through the shock. What we are looking for is some evidence of the Gibbs mechanism for irreversibility, namely phase mixing. After the shock the particles that loop change the shape of the circle grossly. We obtain a picture like Fig. 1*b* in our paper. There is a long ear which follows along the circle closely. You might anticipate that repeated looping will produce further ears, and ears upon ears. This begins to look like the phase mixing necessary to produce a macroscopic entropy jump.

Morawetz's computations yield a transition which shows a rise to a certain height, followed by a periodic wave train at a definitely elevated height. The computations are significant only to a certain distance and the ultimate behavior is unknown.

One feels happy because this result looks like the beginning of the irreversible shock transition we are seeking. One feels unhappy because we can draw no conclusion of what happens far behind the shock. After a certain distance we may return to the initial state, and the irreversible transition be lost. It should be added that in Morawetz's case this seems most unlikely. Once an ear is formed it is hard to see how it could be retracted. Even in this eventuality there is a last hope. Let me remind you of the old story of the Poincaré recurrent time: if we have a box containing a gas and initially 10% of the gas is in one half of the box and 90% in the other, the time will arrive when the gas returns to its initial distribution, but we are not going to wait for it. It is a gross extrapolation in our case, but it is perfectly possible, even if there is a return to the initial state, that the time elapsed may be so large as to be beyond all physical significance. We see, then, that even if it should prove that the transition is reversible in the strict technical sense, there remains the strong possibility that it is for all practical purposes, irreversible.

— L. Davis:

While we are still on the subject of solitary waves, let me point out that there is a little difficulty in the terminology here. The words « solitary wave » are now reasonably well recognized as describing a type of electromagnetic wave in a plasma in which the inertial properties of the current-carrying electrons are important, thus giving the wave a remarkably short scale—a remarkably rapid frequency. This is, however, only a more or less singular solution out of all the solutions here. One has a great many kinds of running waves which have the same character of wavelength and frequency and there it not any well-recognized name for these, so they usually get left out in the discussion. But if someone is talking about the motion of an astrophysical medium it may be a very convenient thing to know that one solution of the equations of motion is that of running waves. Now they may be unstable and they may break down after a while—but they do form a useful component in terms of which to describe the whole motions—at least there is some hope they would.

— A. A. Blank:

We call them periodic wave trains. I have an additional point. There is a result here that does not come out of any other theory. We get differential heating of the electrons and the ions. I do not think there is anything else which gives you this kind of thing.

— F. H. Clauser:

Blank made the statement that Mrs. Morawetz worked through the calculations that resulted in the periodic wave train; but did this in fact have the possibility of this phase mixing that you had guessed.

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— A. A. BLANK:

Yes, the « ear » did appear.

— F. H. CLAUSER:

Would her calculations have given the additional ear?

— A. A. BLANK:

In a higher order theory, one would expect the next ear to come out one Larmor period later.

— V. D. SHAFRANOV:

I would like to summarize some recent work by R. SAGDEYEV, on some aspects of collisionless shocks in a cold plasma ($knT \ll H^2/8\pi$). He has observed a certain analogy between gravitational surface waves in water of finite depth (following the classical analogy: « shallow water » and plane motion in conventional gas-dynamics). In both cases we have non-linear steady waves of similar structure, a solitary wave, for example. In a magnetic plasma solitary waves exist for $M < 2$, (M = Mach number); for water waves, the critical Mach number is equal to ~ 1.7. In this region the shock front has a regular oscillatory structure, provided the non-linear wave is stable. Calculation predicts for weak waves ($M - 1 \leqslant 8\pi nTk/H^2$) stability with respect to some special disturbances; for example, plasma oscillations. Thus, the small amplitude shock thickness appears to be

$$\Lambda \approx l \frac{m_e}{m_i} \cdot \sqrt{\frac{H^2}{8\pi nTk}} \ln (M - 1)^{-1},$$

where l is the free path of the electron.

In the opposite limiting case of high M, an « overturning » of the front appears. The region of multistreaming motion, generated by overturning, does not increase indefinitely. In the case of water waves the force of gravity acts as a turning force. For ions in a plasma the magnetic field plays the same role which turns back the ions in a distance of the order of the Larmor radii.

$$\frac{C_H}{\omega_{H_i}} = \frac{C}{\Omega_0}, \qquad \left(C_H = \frac{H}{\sqrt{4\pi n m_i k}}, \qquad \omega_{H_i} = \frac{eH}{m_i C}, \qquad \Omega_0^2 = \frac{4\pi e^2 nk}{m_i} \right).$$

In both cases the multistreaming motion is unstable. In the hydrodynamical « bore » the cause is instability of the tangential discontinuity. In the plasma this is instability of the interpenetrating ionic streams, moving across the magnetic field. Such instability develops at a distance of the order

$$\Lambda \sim \frac{C}{\omega_0}, \qquad \left(\omega_0^2 = \frac{4\pi n e^2 k}{m_e}\right).$$

This leads to an irregular « turbulent » structure of the shock front.

In an intermediate region $(1 + (8\pi nTk/H^2) \leqslant M < 2)$ instabilities of other types may exist, which arise if the regular electronic velocity (in the transverse direction) exceeds the thermal velocity. This corresponds to a characteristic length of order

$$\Lambda \sim \frac{C}{\omega_0}\sqrt{\frac{H^2}{8\pi nTk}} \cdot \frac{1}{M-1}.$$

— R. Lüst:

Let me raise some questions on the one experiment that has been done, by Patrick. I raise these, not to challenge the experiment, but for better understanding. First, a question on Fig. 1 showing the shock thickness results. If you had just looked at the experimental points, and been unaware of the theoretical curve, would you not possibly have simply drawn a horizontal line, coinciding with the channel width? Second, for the calculation of mean-free-paths, one needs to know the temperature, thus already applies some kind of theory; *e.g.*, the Rankine-Hugoniot equations across the shock. So, how good are the temperature estimates you use? Third, I saw one experimental point, dealing with shock width, lying above the collision shock thickness; what does this mean? Fourth, what is the dependence upon magnetic field direction? This last is important conceptually, since one would expect that for propagation along the field, no collision-free shock should be possible, since only the ordinary free path enters. On the other hand, if I understood Petschek correctly, under his theory one should expect a similar thing to occur for propagation along the field.

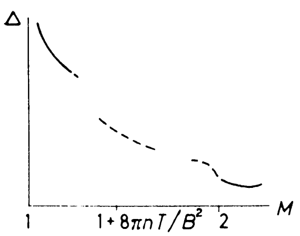

Fig. 1. The dependence of shock thickness on Mach number.

— H. Petschek:

On question 1, whether the experimental points distinguish between the line drawn and the channel width, I think it obvious they do not. There are two possible effects of the channel width. One is that we are dealing with a cylindrical geometry, and the magnetic field which is driving the shock is stronger on the inside of the annulus than it is on the outside because it drops off as $1/R$. This could produce a tilt of the shock which would look like a width

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on such oscillograms. We have tried to look for a possible tilt in the shock by looking at different angles. This is rather crude—but seems to indicate that the shock is fairly plane—we are doing further investigations by making a larger radius and same size annulus, to check this point further. The other point is the one raised by LIEPMANN. I think it was that the friction on the walls could reduce the shock thickness. There is not a very clear cut answer. In some cases, we have gotten shocks less than a quarter of the annulus thickness, which makes that somewhat doubtful but it is certainly not beyond being questioned. Now the question about calculating the mean free path. It was pointed out by LÜST that one needs something, possibly the Rankine-Hugoniot equations to calculate the mean-free-path. Now the experimental check of this that we have is that the light intensity measurements tell us the density behind the shock front. Now this slide shows the intensity as a function of initial density; this should go as the square of the initial density, and have a value depending on the density ratio across the shock-wave. Now the upper line corresponds to some rather lower temperature experiments which were done for a shock-wave going along the magnetic field. These were not in the collision-free region—in that case the density ratio should be four and this agrees quite well. In the case of the magnetic field in the plane of the shock-wave, the density ratio across the shock is reduced to 2.2, and we get agreement as far as the density.

This is calculated for $\gamma = \frac{5}{3}$; the density ratio here is rather independent of the γ. You see the main compression across the shock is that of the magnetic field. This is for a shock-wave of about Mach number 2. We tried to see, for example, whether one could tell the difference between $\gamma = \frac{5}{3}$ for particles and a gamma of $\frac{4}{3}$ which one would expect if one takes the wave picture seriously, and the density difference was less than 10%. So it is not observable. Now once one has the density, he knows the difference in velocity between the streams ahead and behind the shock. And one can calculate the mean-free-path from this. The next question: there are a set of points where according to the picture, the collision shocks should limit the thickness and the thickness is apparently about a factor of 2 greater than the curve. This is not clearly outside the accuracy of this curve. But it is somewhat of a discrepancy. The last question: PATRICK tells me that one cannot run the device as a whole without some axial-magnetic field. That is, you do not get anything that looks like a shock-wave—it's not clear whether this has something to do with a shock-wave itself; or more probably that it's associated with the pre-ionization mechanisms and uniformity around the ring—somewhat extraneous experimental results—so that the minimum angle of the magnetic field to the plane of the shock that has been used is about 15°. He has used stronger axial components of the magnetic field to vary the angle from 15° to 35°. I have not been able to see any dependence on angle; ac-

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cording to our theory—one would expect about a factor of 2 between zero and 30 degrees—and this you see is still within the scatter of the experiment so that there is no disagreement.

— R. Lüst:

What about 90°?

— H. Petschek:

You mean experimentally? One can take the theory that I presented, which has in it the parameters of the mean wave number and the β that one gets, and these change as the angle changes—the β changes because the Rankine-Hugoniot conditions are different when you compress or do not compress the magnetic field. And the wave number changes because waves travel faster along field lines than perpendicular to them. This gives the result that the shock thickness, as the angle increases—goes up by the factor of 2 which was shown and then decreases, and would wind up about a factor of 2 below the zero degree case. The experimental situation on this was that initially the experiments were done with only an axial field as the 90° case; and when one got the condition where the mean-free-path became larger than the annulus, one got no reproducible results. Which is suspicious of the fact that for the 90° case the shock did not exist. However, I think that these experiments should be repeated. So that situation is completely up in the air.

— H. Liepmann:

I agree with these points but I have two more. One is that the shock has to do the ionizing — in front you have no conductivity — so you have to come up through the shock to the conductivity you need, and this looks to me somewhat difficult to intepret theoretically. I realize very well, of course, how difficult the experiments are. The apperance of a shock with the axial-magnetic field could be interpreted differently. It looks to me that with an ordinary shock-wave, with pressures and diameters that you have you don't get a shock, not so much because of friction at the wall but because of heat conduction to the wall. Because nearly the whole mass of the gas is boundary layer; the wall is cool, the density high, and consequently the whole mass of the gas is there and what happens is that you have a negative displacement thickness. So you don't form a shock. So it is possible that the axial-magnetic field mainly produces the heat transfer to the wall. If this is the case you will find no shock for zero axial-magnetic fields. This is, I think, a very strong effect on the possibility of even forming a shock, collision-free or otherwise.

— H. Petschek:

Yes, the argument for always having some axial field is that this tends to keep the gas off the walls. Now the type of thing you suggest — of the gas

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cooling the walls. — would lead to a measured density which was off from the one you would expect. So the agreement of the density curves to some extent supports the idea that the walls are not particularly important. As far as the question of the ionization of the gas — that is a shock-wave going into hydrogen at room temperature conditions — the temperature that you would get behind the shock at these shock velocities corresponds to a million degrees. Now at something like 100 thousand degrees, the gas should already be well-ionized. If one calculates the rate of ionization, this depends on electron collisions.

— H. LIEPMANN:

Not initially?

— H. PETSCHEK:

Initially it is not clear — if one has some electrons, the time in which the electrons would double thenselves is still an order of magnitude smaller than the shock thickness we are discussing. So, presumably, what is happening is that in the first part of the shock, until one gets to say 100 000 degrees — there are still collisional effects important and ionization is going on, but this is only the first 10 percent of the shock-wave.

— L. BIERMANN:

Do I understand correctly that one should expect some non thermal electromagnetic radiation from the fluctuations of these wave packets which move about in your picture. If so, did you develop the theory of the emission in the frequency range of some multiple of the gyration frequency of the ions or neighboring frequency ranges? And did you make any attempt to discover experimentally whether excess radiation exists — that might be a good means, if the theoretical expression can be derived. There might be a possibility to discover experimentally whether you actually get the fluctuations as a sort of turbulence or not.

— H. PETSCHEK:

The large amplitude waves which are present in the plasma around the ion cyclotron frequency should give rise to some radiation which could be observed outside. It is somewhat difficult to estimate how much, because as these waves hit the boundary from a very sharp boundary they would be reflected. It the boundary is more gradual, it is not clear how much is reflected and how much is trasmitted. We are in the process of trying to measure field fluctuations just outside the plasma with a pickup coil. These experiments have not produced anything yet. Another experiment which is in progress is to try and find whether the waves do exist by shooting an electron beam which goes through a curved path and comes back. The position where it comes back tells you the strength of the magnetic field — the average magnetic field which is an interesting quantity. The de-focusing of the beam should tell you the fluctuations that are present. This experiment has just barely gotten started; there are no data yet that I know of.

— M. Krook:

We have been confronted with two kinds of treatment of the collisionless shock this morning, and told there were allegedly only two schools of thought. There are at least three: Liepmann remarked in an aside earlier that he did not believe any of it. So if these are three views, there is a fourth. My own theory is that the way to discuss the collisionless shock is not by invoking the Boltzmann-Vlasov equation but actually to reexamine the problem anew and to find new equations of motion. Once we abandon the collisions in the Boltzmann equation, then we have to find a dissipative mechanism to take its place. Let me sketch an approach.

In the ordinary treatments one has a set of distribution functions for each kind of particles, one-particle distribution functions. We have an electric field E and other fields as well. We write down equations of motion for the distribution function. This is the Boltzmann-Vlasov equation in which the E is determined through an equation involving the charge density. Now then we throw away the Boltzmann collision term, which is itself an idealized representation of molecular interactions, — that is that particles interact only when they get close together. Once we have thrown that away we have lost the major dissipative mechanism, and have to re-examine the equations of motion, to actually remove another idealization — which is inherent in those equations of motion. Now one way of doing this is to replace this E by an $E_0 + E_1$ and to say that E_0 is determined by the Poisson equation, and the E_1 can only be specified stochastically, because there are microscopic fluctuations which are averaged when we write down the Boltzmann equation. When you do this, this fluctuating field E_1 now gives you a dissipative mechanism. Actually Thompson has discussed conductivity from this point of view by putting in this field, and Florence at Harvard has considered the scattering of plasma oscillations by these fluctuations E_1.

— H. Petschek:

You say one should emphasize the long range forces due to charge accumulation. Now this type of thing leads to the wave motions, and this is precisely the term that we have emphasized.

— M. KROOK:

What I mean here is that the E which you put into the Boltzmann equation is a macroscopic E. It is a macroscopic field — a self-field — due to charge accumulation and so on. The E_1 is a fluctuation in the two-particle correlation function. It is not enough to just write down the two-particle correlation function itself, but you have in fact to work out what the fluctuations are in the field that a particle sees, due to screeming and so on. The characteristic length for this E_1 is of the order of a Debye's length — where there is no magnetic field, and the characteristic time is something of the order of the plasma frequency. For example, if you work out the scattering of plasma oscillations, with E_1 absent — in other words just with the E_0 — and then put E_1 in as a perturbation — then the quantities that enter are components of E_1 at x and t, and $E_1(x)$ at x' and t'. The average values of the correlation function are involved. These are microscopic fluctuations as opposed to the character of E_0, which is a macroscopic field. The plasma oscillation is an organized collective motion of the medium as a whole, E_1 is not a collective motion — it is collective only at most whithin the Debye's screening radius — or rather a radius of that order.

— R. LÜST:

I completely agree that one has really to investigate what are the proper equations for the plasma if the collisions are not there, therefore one has to investigate fluctuations and their influence.

From the other side I think the situation might be somewhat different if you have a strong magnetic field. And this is our attitude, for instance, when we say that we still have a fluid description. Then if we take the Vlasov equation, we have to add the magnetic field terms, and our approach is that these are the largest terms and may replace the collision terms.

— M. KROOK:

I think my criticism is based on a much more fundamental starting point than the one mentioned here, if I may say something about that. This is actually involved in the definition of the distribution function. There are two possible ways in which one can define such a distribution function. There is the classical way, where one tries to define this as a function of v, x, and t. To define such a distribution function, you take a small volume, and if you read CHAPMANN-COWLING, you are told that this volume must be sufficiently large to contain a large enough number of particles so as to smooth out fluctuations, and at the same time so small that you can effectively regard it as infinitesimal. Now, of course, mathematically this is nonsense; and what one would have to do is to write down say difference equations, and after a time you would not know from which particular cell a particular particle came. All you can say about it in this treatment is that it was in that particular cell, and at a later time it could be in one of a number of different cells. One way out of this apparently is to go to an ensemble, and to say that in fact we consider a large number of identical systems — prepared in exactly the same way as the system of interest, and then we can define this by taking an average over the ensemble. But when we write down the equations of motion for these distribution functions, they are not then the equations of motion of any real system, because the forces to which particles are subjected are forces averaged over the ensemble. In other words fluctuations have been smoothed out, and your equations of motion are not the equations of motion of a real system at all. If you want to take account of fluctuations, you have to put in some new physical assumption. In the Boltzmann case it is the molecular chaos assumption, where you in effect say that once a particle has been in collision you do not follow it out, but next time it collides you specify only that the impact parameter has a certain probability distribution. Once you throw away collisions you must take out some other idealization, which is inherent in this kind of a definition of a distribution function, and the writing down of an equation of motion for it. One way of doing it is to allow for the fluctuations not only of the electric field but of all physical quantities involved. And I think this would also operate in the case of the presence of a strong magnetic field. It may not be important in those cases—I would not go so far as to say that it is always the dominant term. You may be right, that if there is a strong magnetic field—then this fluctuating term has a minor influence as compared to others.

— H. Liepmann:

I am wondering whether your ideas are related to some recent work of Green (H. S. Green: *Phys. of Fluids*, **2**, 341 (1959)), who derives macroscopic equations for a conducting gas. Green rejects the usual phenomenological introduction of Ohm's law and instead discusses statistically the collective influence of all other particles upon the forces exerted on a particular one. In this fashion the effective dielectric parameter and the conductivity can be expressed in terms of correlation functions.

— M. Krook:

What is the dielectric constant?

— H. Liepmann:

That is the question which Green discussed, isn't that right?

— M. Krook:

Yes, I think so, but not quite in this context.

— W. V. R. MALKUS:

In most of these studies, particularly the laboratory studies, one is talking about situations with magnetic fields initially imposed upon the system. Then one explores plasma instabilities, and the more microscopic instabilities which result from the existence of the field. Certainly one good reason this is done is that these instability problems are then linear. Now there also exists the possibility, particularly in this shock column we have been talking about, that we have a finite amplitude instability which involves the production not only of fluctuating velocity fields, but fluctuating magnetic fields. This is a generation problem in which both fields arise simultaneously from the available potential energy, or in this case the available organized kinetic energy, which is then turned into disordered kinetic and magnetic energy. Now the imposition of a magnetic field to rationalize the degeneration or the production of the shock leaves one of course the problem of where the magnetic field came from. Many of the fields that are being discussed astrophysically are of such scale that they cannot have existed primordially, and therefore they must be produced by some local kinetic process. I suggest that the two are the same; that the instabilities that are being explored, and the magnetic fields that are in this argument used to generate them, are both produced by the same instability. Now one can have some faint test of this. We have been listening to detailed mechanistic inquiries into possibilities—perhaps a less mechanistic approach would be to inquire into extremes that the instability could produce in terms of absorbing energy from the available organized flow and putting it into a disordered flow. In some work in the *Astrophysical Journal* last summer, I explored another explicit mechanism for the production of—let's call them extreme magnetic fields—and in that case and perhaps in this case too—if one wants to absorb as much energy as possible from the streaming organized flow, put it into disordered flow, one strikes a balance between the advection of momentum $V\cdot\nabla V$ and the advection of momentum by the magnetic flow $(\mu/4\pi\varrho)(H\cdot\nabla H)$. This balance permits the greatest release of the available organized energy into the disorganized form which arises from the instability. Now an estimate of whether this is indeed the case, can perhaps be made by comparing the magnitude of these quantities in this shock—or the shock one might expect if indeed such a quasi-mechanistic equipartition occurred. In interstellar space I recall we are discussing a region where densities of 10^{-24} and velocities of $3\cdot10^{7}$ cm/s prevail. By comparing the magnitude of the fluctuations that must exist across the shock—one can get some estimate of whether the magnetic fields that result are in keeping with those one anticipates there. Such a balance is also an equipartition of magnetic and kinetic energy. We have then

$$\frac{1}{2}\varrho V^2 = \frac{\mu}{8\pi}H^2\,,$$

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where V, the fluctuating velocity, is $\frac{1}{2}$ times the organized flow velocity, the temperature behind the high speed streaming being very small. This suggests that H would be of the order of 10^{-4} gauss in the vicinity of the shock. I do not know if that is an unreasonable astrophysical number—the mean numbers in the interstellar region are smaller than this from the other observational data; but perhaps just in the region where the field is being produced, one might anticipate it being larger, diffusion mechanisms perhaps would make it smaller over a broader scale. Now, I might note that if the mechanisms proposed—of Alfvén waves being the principle motions that exist as the macroscopic, randomizing process in the shockfront—are justified, they also have the property of an equipartition between their velocity and their magnetic field. Therefore this argument is not incompatible with the thought that the principal physical quantities involved in this a-mechanistic equipartition momentum balance are Alfvén waves,

$$V_A \simeq \left(\frac{\mu}{4\pi\varrho}\right)^{\frac{1}{2}} H_A .$$

Is that right; is, in an Alfvén wave, energy equipartitioned between the fluctuating velocity and fluctuating magnetic field?

— H. Petscheck:

For an Alfvén wave at frequencies below the ion cyclotron frequencies—that is true. However, the wave would be important at frequencies somewhat above the cyclotron frequency, and in that case the magnetic energy in the wave is larger by a factor ω divided by the ion cyclotron frequency.

— W. V. R. Malkus:

That being so, this would be the minimum value for the magnetic field: so that in fact you expect larger magnetic fields associated with the fluctuations. One last point I might make is that I do not think the kinetic theory viewpoint is incompatible at all with a continuous mechanism. We cannot make any plausible arguments that can be tested about the independence of one unstable wave in its initial period of growth from all others, but the idea that there is available energy for the growth of waves and they will grow from the white background noise in disordered fashion and lead to a disordering of the available kinetic or potential energy is far from implausible. I wanted to note that it is the basis of the theory of turbulence that I presented to you earlier in this conference.

— L. Davis:

This was an interesting discussion of essentially the dynamo theory of the generation of magnetic fields, by motions of matter. This is an important thing because clearly the origin of all these magnetic fields that we talk about

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is highly important. However, it is somewhat outside the field of our present discussion. It has been discussed and a good deal has been published on it in earlier times, so I suggest that we discuss the other things that seem more directly concerned with stellar atmospheres. I would only remark that for the interstellar space, which we are now discussing, the velocity of $5 \cdot 10^7$ cm/s is at least one and probably two orders of magnitude higher than most people would allow, except in exceedingly trivial size regions. This velocity is nearer the values discussed for the solar wind.

— N. MILFORD:

I wish to comment a little on the picture of the shock region between the solar wind and the interstellar gas. We have the usual picture of the solar wind, with a density of 100 particles cm^{-3} and velocity of 500 km/s at one a.u. It was suggested that somewhere in the region of 100 to 1000 a.u. we had the beginning of the shock front. The density there is down to 10^{-3} and the velocity is supposed to be effectively unchanged. Then in the shock region beyond this distance, presumably, the density is of about the same order and the temperature is very high; and then out a bit further we have the interstellar gas with densities of the order of 0.1, velocities of the order of 10 km/s, low temperatures, and mostly neutral.

Now if this shock thickness is to be anything like one collision-free path— let's discuss the situation without magnetic fields and no other stars present— then with these densities and temperatures you get a distance of the order of 10^5 or 10^6 a.u. If we have a relative speed of 10 km/s between the interstellar gas and the sun, then it seems to me that the interstellar gas will tend to penetrate much further into the system than is indicated by the appearance of this shock, and that it will come into a distance from the sun of the order of some few a.u. with the sort of figures we listed before. The actual result depends, of course, upon the collision cross-section for charge transfer and can be put into the approximate form, $R_{\text{a.u.}} \sim 16[(1+10^{-16}/\sigma)^2-1]^{-1}$, where σ is the cross-section for charge transfer; if you put $\sigma \sim 10^{-16}$ cm^2 this gives us about 5 a.u. So at this distance the interstellar gas becomes substantially ionized, then it is stopped by the solar wind in a short distance, and finally swept out with the solar wind. The picture that we have then for the density in the interplanetary medium near the solar system is something as follows: the inner part is the same as previously postulated, but a little further out, say at about 5 a.u., you have the solar wind interacting with the interstellar gas. The interstellar gas probably tends to pile up to some extent in this region and may have a density ten times larger than its original value. Further out we have a region in which the solar wind density appears to be less than the interstellar gas density, before we come to this shock front which was mentioned last week.

Now this is without magnetic fields. If we put in magnetic fields presumably the region is compressed somewhat towards the sun; but unless there is a tremendous increase in density somewhere, the interstellar gas will still penetrate a considerable distance into the solar system with this value of 10^{-16} cm^2 for the charge transfer cross-section. So the questions that I would like to ask the aerodynamicists are as follows: 1) What would they call such a region of interaction if they believe it exists? 2) Under the conditions we have talked about, in the presence of magnetic fields, how far do they think the shock front would be liable to extend, particularly in view of the variable nature of the interstellar gas further out (variable density and probably velocity fluctuations also)? Finally, if there were large fluctuations in the solar wind, for example during times of solar disturbance, and there were a shock front as suggested, what would happen if a very large flux of particles came along? Presumably it would punch some type of hole in the shock front and region out to some distance, but what would happen after that—would there be then some sort of oscillation of this protruding front, or would it just die away gradually?

— E. N. Parker:

The galactic magnetic field will in fact stop the solar wind, at say 500 or 1000 a.u. if the present figures are correct. I do not believe the 10^5 a.u. model.

— M. J. Seaton:

I would like to raise the question of the temperature of the solar corona, particularly from the standpoint of the energy distribution that one would expect in a medium heated by shock-waves. I think it is worth summarizing the position and suggesting what the possible explanations might be.

It has been mentioned once or twice before that one can get the temperatures of the corona from the ionization equilibrium; the temperature usually quoted is $1 \cdot 10^6$. One can also get a temperature from line widths; this is usually given as $2 \cdot 10^6$. I think there is a general impression that this is a discrepancy which will be cleared up, but in fact it seems now fairly certain that this discrepancy is real. Let me give a little more detail. The ionization that is of interest is for ions Fe X to Fe XIV. These ionization equilibria depend only on the energy of the electrons and one could say this gives a temperature of the electrons. It has been thought that the most uncertain quantity entering is the collision cross-section. Recent work by Burgess in London has been concerned with a systematic study of cross-sections for highly ionized atoms. This work indicates that one cannot in fact change the adopted cross-sections very much. Similar work has recently been done in Munich by Miss Trefftz. With these latest results one has a figure rather less than $1 \cdot 10^6$, although not very much less. We than have $T_e \leqslant 1 \cdot 10^6$ for the electrons. In order to get up to $2 \cdot 10^6$, the cross-sections would have to be in error by a factor of 30 and this seems extremely improbable. On the other hand, consider the line-width measurements. This is a very direct measurement, and the latest results give a value rather bigger than $2 \cdot 10^6$, more like $2.5 \cdot 10^6$. It would seem that the measurement is sufficiently definite there that one could say that $T_A \geqslant 2 \cdot 10^6$, where T_A refers to the temperature of the emitting atoms.

If this is a real discrepancy, consider what might be the possible explanations. One would be that the electrons do not have the same temperature as the atoms. This depends on relative magnitudes of various relaxation times, and the general opinion seems to be that one would have the correct numbers only in the outer corona—although I have not checked the numbers myself. A second possibility might be that there are non-thermal velocities; in that case this would be important for the atom, but the corresponding velocities for the electrons would not be important. If one took these as turbulent velocities, he would have the turbulent velocity of the same order of magnitude as the thermal velocity of the heavy particles. The third possibility might be that there is no temperature at all—that is to say that we do not have Maxwellian distributions. And in this case one notes that, while the ionization does not depend on the extreme tail of the distribution, it does depend on the distribution of rather high-energy particles—rather higher than the average. The line width will, of course, depend on something like the mean velocity. I would like to have comments about the sort of velocity distribution people might expect for the electrons and atoms for a medium heated in the way that the corona might be heated.

— E. N. PARKER:

The only comment I can make on the possibility of a non-Maxwellian velocity distribution is that once a long time ago we applied the order-of-magnitude numbers appropriate to the corona to some general models for Fermi acceleration of particles, and concluded that while we could rather easily make the ion velocity non-Maxwellian, we saw no possibility of making the electron velocity distribution very non-Maxwellian. I am not saying that the possibility does not exist but it is not obvious how electrons could be accelerated on the basis of the kinds of things considered so far for particle acceleration.

— C. DE JAGER:

Could not the non-Maxwellian shape of the velocity distribution function arise from the fact that the high-energy tail of the distribution is continuously lost to space?

— E. N. PARKER (and others):

Discussion on direction of the effect. Agreed this is a possibility—but must be computed.

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— N. PETSCHEK:

If one assumes that all the heating goes initially into the ions, did you say that one *cannot* explain this difference?

— M. J. SEATON:

This is the sort of possibility that has recently been considered by ALLEN. Certainly most of the radiative cooling might be by the electrons, and one must consider the possibility of the heating going in the first place to the atoms. But this is a matter of just computing relaxation times for these processes; such calculations made some years ago suggest the in-balance only occurs at very low densities, thus in the outer corona.

— N. MILFORD:

Perhaps in Kahn's absence I can quote his dinner table estimates of relaxation times in the lower part of the corona. I believe that he estimated relaxation times of the order of some few minutes.

— R. N. THOMAS:

KROOK should correct me if I make a misstatement here. But we calculated this difference between electron temperature and atom kinetic temperature some time ago. We did it for somewhat higher densities, and neglected the effect of impurity cooling; that is, we considered only hydrogen, not things like the metals, or carbon, oxygen, and nitrogen. We got essentially no difference in temperature—something like $\Delta T = 1\,000$ degrees at 10^7 °K. But if one puts in some impurities, then our results become completely uncertain.

I think this last is certainly something that can be done once we know what the energy is that can be put out by these impurities. This last is the unknown part—term diagrams, f-values, collision cross-sections; not the methodology of making the calculations. So, our investigations should be re-done for coronal conditions.