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DISCUSSION FOLLOWING THE INTRODUCTORY TALK BY DELACHE

Souffrin — I would like to ask where are the large and the small amplitude velocities that you talk about?

Delache — You may have "large" values for the boundary condition on the velocity, which means really a "large" value of the mass flux, if it would cover the whole Sun, while the numerical value for the actual velocity remains small. This is what happens in the lower part of the transition region.

Thomas — In other words, you end up with a small mass flux down below and a large mass flux up above.

Delache — The important condition is that the mass flux over the whole Sun remains constant. Also, I don't want to go into detail about the field structure. This whole picture I've given is very macroscopic.

Thomas — **I** haven't pushed you to open or closed fields. I've just pushed you to large or small mass fluxes, that's all.

Cayrel — Is it not true that if you multiply the mass of the spicules by the appropriate velocity you get the same order of magnitude as the mass flux of the solar wind?

Delache — Yes, I think Beckers has the answer, which I believe is yes.

Beckers — The upward transport in the spicules is two orders of magnitude greater than is required to balance the solar wind; but the energy available from the spicules is two orders of magnitude less than that required to balance the losses of the corona due to conduction down and other losses. So the spicules can easily provide the coronal mass losses, but not the coronal energy losses.

Underhill — I'm wondering if this has any relevance to a fairly commonly observed phenomenon in stars. In certain late type stars with extended atmospheres, you see what are called clouds. These clouds refer to the fact that one day you see two or three displaced calcium absorption lines and the next day you don't. This common type of observation can be explained qualitatively by irregularities in a more or less steady flow. I wonder if this solar-type flow you're describing here might be what is taking place? Could this kind of thing develop irregularities?

Delache — Yes, we know that this kind of thing can develop irregularities because the magnetic field structure is changing with time, often very rapidly. In the solar case, you must go to the filamentary structure. In observations of coronal streamers made from balloons, you see the structure changing in two or three hours.

Pecker — I'm a little troubled by the temperature picture that comes out of your model. The temperature within the spicules and the temperature outside the spicules seem to vary at such rates that it implies little connection between the thermal structure and the magnetic structure.

Delache — This is not really a complete model. For consistency, you have to demand something like pressure equilibrium between the two columns. That would require a further step than I have taken.

Skumanich — I think what all of you are saying is that some systematic flow is needed. On the other hand, this avoids the question that Thomas

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raised long ago of the possible role of spicules in heating the corona. We tend to view the spicules now as though they arose from energy deposited in the corona and conducted back down into the chromosphere; but what about the possibility that they arise as a result of some hydromagnetic effect. We don't know what this effect may be, of course. We wave our hands and say Petcheck mechanism or induction mechanism, but the point is, couldn't some magnetic field effect be responsible for the spicules and might they not play some role in coronal heating? Do we have to go all the way down to the convection zone for the source of heat for the corona? Can we deduce anything from the ending of chromospheres along the main sequence? Can we say anything about how this convection decays as we go off the main sequence? Does the type of self excited instability that Ulrich has studied prevail along the main sequence? How does this scale?

Athay — One thing that excites me in your work is that you've taken data which have no spacial resolution and inferred an inhomogeneous structure for the Sun. This has important implications for stellar work. It's interesting that in the case of the Sun people working from a different direction arrive at the same results you discuss.

Pecker — I was intrigued by Anne Underline's earlier comment. I wish she would make more clear to us exactly what stellar observations are relevant to these ideas of Delache.

Skumanich — I would like to know more about structure in extended atmospheres.

Underhill — The most pertinent observations are those given by Petore McKellar, and Wright on the 31 Cygni type stars. Regarding irregularities in the flow, you have variations in the tops of emission lines in the Wolf-Rayet type stars. Also in Be stars and B supergiants, when you can scan the profiles rapidly. You find they're changing in a matter of minutes, or at least a half hour. You just have to conclude from looking at the data that inhomogeneities exist.

Wright — The figure I discussed yesterday (Figure H-43) represents probably the best example we have of the satellite .lines in the K line of 31 Cygni. This is a series taken during the eclipse of 1961, and I hope to observe a similar effect before May of this year. Here we have -the normal K type spectrum with the emission produced by the K_t and K_2, and superimposed on that are the chromospheric lines as you come into totality. This series started in July of 1961, and by the time we got into August we saw evidence for these clouds or whatever you want to call them. This particular one lasted for three full days, August 6 to August 8. Then it disappeared and there was only a single component. Then in

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September the additional chromospheric lines appeared again. Not in the same position, but since these are just velocity effects, this is probably due to the fact that different portions of the atmosphere are moving with different velocities at different times. Hence, the interpretation as prominences or clouds, whatever you want to call them. Sometimes you see several components. It doesn't show up too well here, but in 32 Cygni, particularly in 1965 and later on, I have suggested that they may be as many as four or five components at a single time. I'm not too positive about some of the multiple-component lines, but they do seem to be present. When you get deeper into the atmosphere, the lines tend to broaden out, and I am interpreting this broadening as the sum of several components. Finally, as you come out of totality, you begin to see the damping wings and get the true K line of calcium. But these must be velocity effects, I think. You have broad lines getting narrow and then broad again, all of which is evidence for the type of clouds that Anne Underhill was talking about.

Skumanich — Couldn't this be a binary effect, i.e., a gravitational perturbation, rather than a structural difference in the atmosphere?

Wright — **I** don't think so with these stars, you have the atmosphere extending out three stellar diameters, with the B type star just a little thing. There's no evidence I can find for mass exchange in 31 Cygni or Zeta Allrigal, for which we have this kind of data. There does seem to be mass exchange in UV Cephei, however, and it therefore qualifies as a close binary.

Pecker — I think those observations are exceedingly interesting, and I would be tempted to react in the same way Anne did. But this kind of thing could not be observed on the Sun at a distance, because the spicules are too small and numerous. So if you are to see the kind of thing discussed here, the elements must be of a sufficiently large size. So I ask the question to Philippe Delache of how one can apply the equations and conditions of energy, momentum, and mass conservation to these objects, keeping in mind the fact that much of the flow may occur out the side of the spicule-like inhomogenities.

Delache — I don't think you can do it, because the magnetic field is needed to confine the flow, and we don't know anything about the magnetic field structure of these stars.

Pecker — But the magnetic field *only confines* the flow. It does not alter the general picture regarding conservation of mass, momentum, and energy in the flow.

Thomas - The magnetic field only establishes the boundary physics — not the internal or overall physics.

Ulrich - I have a somewhat different point of view on this. I feel that the granule size or scale is governed by the pressure scale height somewhat below the surface of the Sun. Now the pressure scale height in these late type stars is a much larger fraction of the total radius of the star than in the case of the Sun. If you scale the granulation up proportional to the pressure scale height, you conclude that a granule in these stars is something like 8% of the radius of the star. Therefore the spicules are going to be very large objects. This does assume that the spicules are rather directly associated with the granules. Consequently, you don't necessarily have to have something like prominences to explain the observations; it could be, something associated with the granulation. I would say that the case of 32 Cygni, the grazing eclipse, is an example of this.

Skumanich — A caution about scaling. As Durney has discovered in his work with Leibacher, one cannot always scale solar to stellar results, at least with the solar wind. They found that, in trying to scale the solar wind to late type supergiants, the sonic point was reached inside the radius of the star. So the wind there is more than the corresponding solar wind would be. It's a very dominant feature of the atmosphere. Although it's very useful to use the Sun as a standard, we should be particularly careful when we go off the main sequence. There are many changes to be taken into account.

Cayrel — In looking at the components of these K lines, I would like to ask what part comes from the main component of the atmosphere and what part comes from interspicular material?

Peterson - Isn't that really what Pasachoff has been observing? When we observe the K line with high resolution, aren't the changes due to the different chromospheric components we are observing?

Skumanich — I would be very cautious about that. I've looked at Pasachoff s results, and if we take, as as measure of the region we are looking at, the energy in the line over a one Angstrom band, we find that he was looking at only one network region. This problem of statistics does plague us, and we must be careful that we are looking at a representative solar region. Most of Pasachoff's data are from the cells. They are not from the network boundary. On the other hand, everyone has seen pictures of the Sun in Ha, .6 Angstroms from line center. Here we see little fingers which, if we identify them with spicules, show that they tend to cluster *around* the network boundary, presumably where the magnetic field is strong. Thus, you can see the K line from above without

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having superimposed on it the time dependent spicule contribution. If not, we have a harder problem to solve. If we cannot assume a steady state, then we have to solve a dynamical transfer problem, and that is difficult.

Underhil! — That's saying that you have radiative irregularities as well as spatial irregularities.

Skumanich - More than that. It's saying that while we've let the dynamicists worry about the time variable, we've ignored it in the transfer problem. I think that, in the network, that's all right. But in the spicules, that may not be all right. The spicules are a dynamic phenomenon, with time scales comparable to the reaction rates of interest.

Underhill — I can imagine a situation where you see the spicule for a while, and then you don't; so you think it has gone away. But maybe if hasn't, really. Rather, the spectral feature you were observing to detect the spicule has faded.

Skumanich - May *I* now call for more detailed questions on the two introductory papers.

Defouw — I'd like to make a comment on the first paper first. Jordan noted that shock waves may begin at an altitude of 1000 km. This is the altitude at which there is an abrupt temperature jump, and he implied that this temperature jump may be caused by this shock formation and the subsequent dissipation. I'd like to point out that the amount of dissipation that is required is not determined by the temperature but by the radiation rate. That is, an abrupt increase in temperature does not imply an abrupt increase in the heatirtg rate. In fact, you can have an abrupt jump in temperature even if the heating rate is uniform throughout the whole atmosphere. To show this I will make an elementary calculation using the net heat-loss function, L, which is the cooling rate (in ergs cm"^{3} sec"^{1} or ergs gm"^{1} sec"^{1}) minus the heating rate. The energy equation in which I am interested is L = 0. I would like to consider the simplest case where L depends only on the local values of the electron temperature, T, and the gas pressure, P, If we differentiate the heat equation L(TJ?) = 0 with respect to height, h,

Skumanich — Excuse me. Just for clarification, what is in your heating function L?

Defouw — I'm going to consider the heating rate in L to be a constant. The funtion L includes the mechanical heating but is otherwise unspecified.

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Skumanich - Do we know how to differentiate it?

Defouw — I'll differentiate it as follows:

$$\frac{9L}{3T}\frac{dT}{dh} + \frac{9L}{3P}\frac{dP}{dh} = 0$$

Now I'm going to ignore momentum transfer by waves, which Delache likes to include. If we just consider ordinary hydrostatic equilibrium (dP/dh =-pg), we find that the temperature gradient is

$$\frac{dT}{dh} = \rho g \frac{3L/3P}{3L/3T}$$

By the assumption L = L(T,P), I've assumed an optically thin atmosphere. I'll draw on the board the radiation rate for an optically thin gas as a function of the electron temperature for a fixed value of density This curve was first calculated in essence by Pottasch and most recently by Cox and Tucker. You have a maximum around 20,000 K due to hydrogen emission and a maximum around 100,000 K due to emission from ions of carbon and oxygen.

Skumanich — At what density?

Defouw — At any fixed density. If we consider fixed pressure, which is what we want for the derivative 9L/9T, the cooling curve is similar but the maxima are shifted to slightly lower temperature.

Skumanich — Is there any particular density you would use?

Defouw — No, as long as the gas is optically thin.

Skumanich — I come back to my comment during the first day That is not sufficient, there is a length scale that has to come into the problem.

Defouw — In this case, I'm just assuming an optically thin atmosphere. I don't believe the chromosphere is really optically thin. This is just an illustrative calculation. Now, the numerator (3L/9P) of the above expression for dT/dh is always positive because it is essentially a density derivative of the radiation rate. The sign of the denominator (3L/3T) is determined by which side of a maximum in the cooling curve you are on. If you are on the low-temperature side of one of the maxima, the denominator is positive, and the temperature must increase with height in order to keep the radiation rate equal to the heating rate. As you get closer to the maximum, the radiation rate becomes less sensitive to the temperature, and therefore the temperature has to increase more rapidly with height. Finally, at the maximum, 3L/3T vanishes and the temperature gradient becomes infinite. By this time conduction has become important.

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If you look at models such as the one Vernazza presented the other day, and early models of Thomas and Athay, you see two temperature jumps which I think you can associate with the two maxima in the cooling curve. I admit that optical thinness is not a valid assumption near $T = 10^4$°K, but I think it is reasonable to assume that the temperature dependence of radiation will show a maximum due to hydrogen emission. The first temperature jump near $T = 10^4$°K, should be attributed to this hydrogen maximum while the chromosphere-corona transition is due to the carbon-oxygen maximum in the cooling curve. The largest temperature gradient occurs where the maximum in the radiation rate is found. It follows that we are not jumping to coronal temperatures because we need more efficient radiation—we are already at the maximum of radiation efficiency. Because we are at the maximum, the denominator in the above expression for dT/dh vanishes, and we have an infinite temperature gradient. That is my first point.

Now I'd like to comment on the model of Delache. This comment may be wrong because I'm not sure I understand the model. If so, please correct me. We have the large conductive flux from the corona. How do you dispose of this flux? Radiation cannot dispose of it because the temperature gradient near $T = 10^5$ K is so large that the large conductive flux from the corona is deposited in a shell only a few kilometers thick. This problem was first pointed out by Giovanelli in 1949 Now, what Delache proposes to do is to balance this conductive flux with an enthalpy flux associated with some fluid flow. He finds first, doing a one-dimensional calculation with no horizontal structure, that, if the enthalpy flux is to be large enough, you need a fluid velocity of 50 km/sec, or several tens of km/sec. The mass flux you get for these velocities is much larger than the mass flux in the solar wind. To get around this, he says that the velocities are occurring just over a fraction of the disc, to reduce the mass flux. So he still has velocities of 50 km/sec.

Skumanich — I think 10 km/sec was the value.

Defouw — OK. 10 km/sec. As I understand it, he has not done the energy calculation for this new configuration. One thing he is obviously going to have to include is what Kopp and Kuperus pointed out. The conductive flux is also going to be channeled by the magnetic field and it's going to be magnified by the same factor that the mass flux is reduced. So it is not at all clear to me that the enthalpy flux associated with the mass flow will still balance the conductive flux.

Delache — I think that the answer is yes, that the kinetic flux is channeled by the magnetic field, but you must realize that you do not necessarily have conservation of the whole conductive flux, as is the case for the mass flux.

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Defouw — Then is it true that you are no longer balancing the enthalpy flux with the conductive flux?

Delache — If we include radiative losses, that is true. As I said, you are increasing the gas conductivity and lowering the temperature gradient, which increases the omission measure and the amount of material which can emit radiation.

Defouw — Now do you think the radiation can take over?

Delache — No, it can only partially take over.

Defouw — Have you done the complete calculation for the configuration?

Delache — As I have said, this model is very naive, with one thing on top of another. We have to go through this region with a variable cross section, which I have not done yet. But in this discontinuous model, the basic quantities are conserved: mass flux, and energy (flow, conduction, radiation).

Defouw — My last comment is that I no longer believe in my theory of spicules. The reason I don't is that the temperature of spicules seems to be going down. The most recent estimates are about 8000°K. For thermal-convective instability you need at least 12,000°K.

Skumanich — They are 8000°K if steady state is assumed. So we are hiding a sinner in the basket, for, if steady state does not hold in spicules, the estimate of the temperature may be in error. I don't know by how much, but I don't feel that your suggestion is necessarily thrown out by current low temperature values based on a steady state assumption.

Defouw — I believe that my explanation of the temperature jumps is essentially correct, although some details like opacity effects and the height dependence of the true heating rate will require some modifications.

Ulmschneider — It seems to me that observations show radiation losses in Lymana, and so on, that are much greater than the C, N, and 0 radiation loss.

Defouw — But the observed line intensities depend on the temperature gradients in the respective regions of line formation.

Ulmschneider — This curve that you plot here should be such that the H peak should be very large and the C, N, and the 0 peaks quite small, on the tail of the H. (Editor's note: This curve does not appear in these Proceedings.)

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Defouw — You can't proceed from observations on this matter, because the observed intensity of a line depends on the thickness of the region of line formation.

Skumanich — Well, that's not quite correct, because I think we do have to accept the spectroscopic model of Mr. Vernazza.

Defouw — I think that in Vernazza's model, Lyman a is produced in a region 100 - 200 km thick, whereas the important carbon and oxygen lines are formed around T= 10^s °K, where the length scale is only about 5 km.

Schwartz — Let me make a comment on what Defouw just described in a qualitative way for a constant heating rate, and say that it probably occurs even in a more realistic situation, Figure III-7 shows the results of a calculation showing, in a quasi-realistic way, the heating and cooling of this region. It is a numerical experiment, where you take the atmosphere

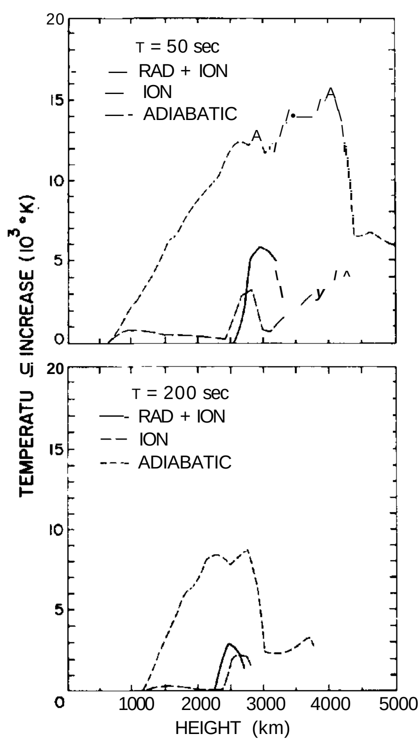

Figure III-7

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and tickle it from below, and then watch and see what happens. The upper curve is for a half-sine pulse with a width of 50 seconds (for a full sine wave it corresponds to a 100 sec period). The first pass at the problem didn't include any radiation at all to cool the atmosphere. We just set the radiative cooling equal to zero, and the temperature just went shooting up as soon as the shock was formed. When we put in the sort of cooling that Defouw talked about, the temperature rise was rather modest until you got to something like the transition region; then the temperature shot up again. However, the net dissipation of mechanical energy as a function of height was nearly the same for these two calculations. You see that the inclusion of radiation causes a rather radical change in the temperature distribution.

Skumanich — It sounds as though the mechanical dissipation is temperature insensitive.

Schwartz — The dissipation was fairly temperature insensitive, but the temperature rise produced by that dissipation is affected very much by the radiation.

Stein — I would like to make three comments about shocks, before Bob Schwartz continues with the results of our computer experiments. First, an isolated pulse and a train of waves behave very differently For an isolated pulse the shock strength increases indefinitely as the wave propagates outward. In an isothermal atmosphere,

$$(M-1) = (M-1)_0 \frac{e^{z/2H}}{\left[\frac{4-5.}{\lambda_0}(M-1)_0\left(e^{z/2H}-1\right)+1\right]^{1/2}},$$

where M is the Mach number and the subscript 0 indicates initial value. When the atmosphere has a more complicated structure, corresponding formulas can be obtained, but I just want to show the simplest case. For a train of waves, however, the shock strength, instead of increasing with height, approaches a constant asymptotic value",

$$M-1 = (M-1)_0 \frac{e^{z/2H}}{\frac{4-iL}{0}(M-1)_0\left(e^{z/2H}-1\right)+1}.$$

So the first thing you should decide when making a model is which is the realistic situation for the Sun.

Second, weak shock theory is an infinite frequency theory. It includes stratification, but neglects the dynamic effects of gravity This effect is dramatically illustrated by some of our results which Bob will present.

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Finally, even for high frequency waves, weak shock theory gives incorrect dissipation. A wave must propagate some distance before it forms a shock and begins to dissipate, so weak shock theory which assumes that a shock already exists cannot be applied starting at the place where the wave is produced. On the other hand, after a shock has travelled a distance, non-linear effects increase the dissipation above the weak shock value.

Skumanich — Does what you say depend on the temperature structure of the atmosphere?

Stein — Not really. Weak shock theory with these simple formulas is for an isothermal atmosphere. The same thing happens if the atmosphere has some nonisothermal temperature structure, but then the formulas come out in terms of an integral over height. Qualitatively, the behavior is the same.

Ulmschneider - I would like to make one comment. Until now, the weak shock theory was only applied a considerable distance above temperature minimum to insure that the shock would be fully developed. Therefore the dissipation is naturally too large at low heights, if weak shock theory is erroneously used there.

Skumanich — So what you are saying is that you can "fudge" where you put the energy by where you introduce the shock. Is that correct?

Ulmschneider — Yes, to date, when we used a fully developed weak shock theory, we didn't start from the temperature minimum, but started from an observational point further up.

Skumanich - How can you justify that? What is the reason for putting the boundary where it is?

Ulmschneider — Because I know that the shock is not developed lower down and that you cannot expect the result of a fully developed weak shock to be correct there.

Jordan — I agree with Ulmschneider that one can invoke fully developed, weak shock theory in the manner he indicated and still obtain reasonable dissipation estimates above the temperature minimum. This is because, from independent studies, it is just above the minimum that we expect significant departures from radiative equilibrium, largely due to H~, and also because the shock strength settles down to a value of about 1/3 over most of the low chromosphere, rather independent of the initial value $r}_0$ for the strength chosen (in a reasonable range of $0.1 < rj_0 < 1/3$). Thus, any overestimate would be confined to a narrow region just above the minimum. Furthermore, one can follow the development of an initially sinusoidal sound wave from the low photosphere, where it is generated, to

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the chromosphere. Many independent studies suggest that these waves will become fully developed shocks (for short period waves with period $T < 100$ sec) by the time they have reached the low chromosphere above the minimum.

Skumanich — It seems to me that the correct solution is to get Vernazza's model. He gives you the mechanical flux gradient without the conduction term taken into consideration; but he presumably can put that in. The question is, are you getting that kind of dissipation of energy with height, or with density, as Vernazza suggests, or not? It is not clear that you are. You seem to put the lower boundary wherever it suits your purpose.

Jordan -- But if the computed net radiative losses as a function of height correspond to the computed mechanical dissipation as a function of height for the same model, as the calculations we have done with the HSRA so far indicate, then this is reason to believe that the initial formulation chosen for the problem is not too unreasonable. This is the criterion you have just stated.

Schwartz — Let me tell you where to put the boundary. If you start at height zero with a wave of initial velocity v_0 and period equal to r, then the wave goes a distance $AZ = 2H\ln(l + 7/2(7+1) \bullet gr/v_0)$ before the crest of the wave has caught up with the trough and it has formed (in some sense) a fully developed shock. If you start off the wave with a given velocity amplitude, then this formula tells you at what height you can begin to apply these weak shock formulas.

Souffrin — Is that the distance where you start pushing the gas?

Schwartz — That is where the shock is formed.

Souffrin — You put in v_0 and you get the shock at some distance?

Schwartz — That's correct.

Souffrin — In the distance travelled in a period or two, you reach the place where the shock forms?

Schwartz — That's correct. This says if you take longer periods the wave gets higher up before it shocks. But remember this is an oversimplification which neglects gravity.

Skumanich - What are the free parameters in this? It looks like they are v_0 and the height at which you hit the atmosphere with v_0 with an infinite plane wave.

Schwartz — This is for an isothermal atmosphere which is the only condition in which you can work it out analytically.

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Skumanich — But what are the parameters? v_0, the height at which you start the pulse, and what else?

Stein — In the Sun, essentially nothing. Even the height at which you start is rather unimportant because, once you get into the convection zone, the density scale height becomes large and, therefore, starting the wave deeper in the convection zone will not change very much the height at which the shock forms.

Schwartz — In other words, this formula fails, because the atmosphere is not isothermal below the top of the convection zone.

A. Wilson — If I give you some numbers, I wonder if you can tell us what that Az would be under those circumstances. Try a period of 10-20 sec, and an amplitude of 1.2 km/sec. What is Az?

Stein — We didn't actually do it for a 20 sec, but for a 50 sec, pulse in our paper and it came out to be $3^l A$ scale heights. This is the point at which you start getting dissipation.

Skumanich — Where does that put you relative to h = 0 on the limb?

Stein — A few hundred kilometers higher.

Skumanich — But isn't that too low?

Stein — No, not for a high frequency, like the 50 sec pulse represents. Longer period waves, on the other hand, don't form shocks until they reach greater heights.

Ulrich — **I** want to make a general comment about all of this sound wave heating. I think unless you put some treatment of the radiative interaction in the dynamics of the sound wave you are not likely to get the correct answer. This is a very dominant effect. It makes the calculations messy. I don't know how the energetics work out. I would be surprised if you got the same results.

Stein — We also did the calculation with radiation. We included H~ and hydrogen recombination to excited states in an optically thin approximation. Direct radiative damping of the waves occurs in the photosphere and low chromosphere, and can remove up to 2/3 of the wave's energy. The rate of shock dissipation is insensitive to radiation, but instead of the temperature increasing this energy goes into ionization and radiation. The temperature rise is small until hydrogen is ionized.

Souffrin — Regarding what you just said, and what Ulrich just said before, about pulses and wave trains. It is somewhat like the difference between an initial value analysis and a boundary value analysis. Consider the question of dissipation. It turns out that it is just impossible to guess

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ahead what effect radiation will have on either analysis. If you look at the initial value problem, you have some motion given at an initial time. Then suppose you have some radiative damping. It turns out that radiation smooths out the motion in a given time. If you look at the boundary value analysis, you just can't guess, due to stratification, anything about the spatial damping in the wave train problem just by looking at the time damping in the initial value problem. It is very important in all these questions concerning heating to have a good idea about the physics of the excitation of the observed motion.

Schwartz - For the weak shock theory, which Jordan just talked about, we **did** another numerical experiment. This time we excited the atmosphere with a wave at the bottom with a period of 100 sec. and let it run for about 40 periods to let the transients die down and enable the atmosphere to achieve something like a steady state. The velocity profile is shown on Figure III-8. This resembles a classic N wave, as everybody has assumed in treating this problem. However, you will notice that the

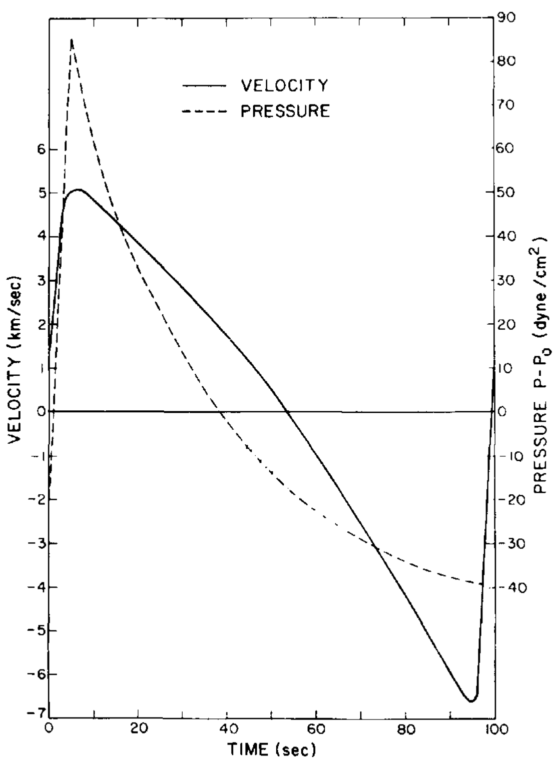

Figure III-8

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pressure variation is much more sharply peaked than the velocity variation. That might be of interest for people who look for oscillations in intensity. Figure III-9 shows the same calculation for a wave with a period of 400 sec, about half the acoustic cutoff frequency, twice the critical period. This is in the non-propagating region in the low chromosphere. Here, at 1000 km above the photosphere, it still looks very sinusoidal, and the pressure variation is very smooth. You will notice that, in this wave, the pressure is out of phase with the velocity. Look at the relationship Jordan wrote down this morning for the energy propagation: energy flux =

$$F_m = \frac{1}{Vl} \int_0^T (? \quad P_n) V^{dt} \cdot$$

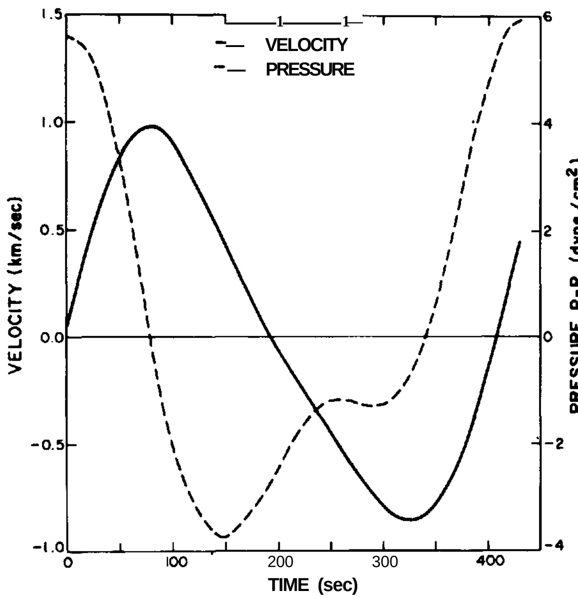

Figure III-9

Since the pressure and velocity are 90° out of phase, then the wave is not propagating energy; it is like a standing wave.

Figure 111-10 shows the dissipation caused by the first wave (100 sec period).. The dotted line is the fully developed weak shock theory and the solid curve is the result of the actual non-linear numerical calculation. Although it has the same asymktotic form, it still disagrees by an order of magnitude in the asymptotic regime, even though this is the regime where you might expect the weak shock theory to^s hold. Of course, as

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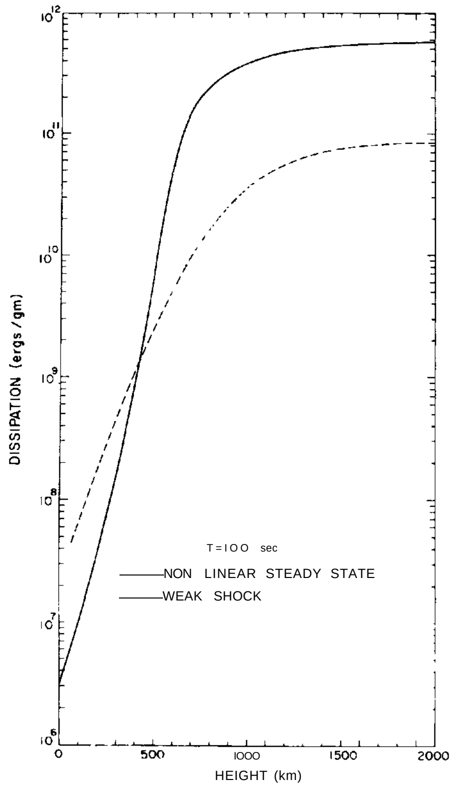

Figure 111-10

Ulmschneider just remarked, the weak shock theory gives excess dissipation in the lower atmosphere, because it assumes a fully developed shock at all heights.

Ulmschneider — Nobody has done a calculation down there using the weak shock theory.

Schwartz — That's correct, for the reason you pointed out. You know the weak shock theory will give the wrong result down there.

Skumanich — What, exactly, are you comparing to what?

Schwartz — I am comparing the weak shock theory to the exact integration of the equations of motion, for this particular model, the isothermal atmosphere. Whether or not it has anything to do with the Sun or not is another question. However, in this example, the heating which is given by the weak shock theory above 1200 km. is an order of magnitude less than the exact solution which the weak shock theory

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purports to approximate. If you think this is bad, look at Figure III-ll. That gives results for a long period wave, the 400 sec one. We see the

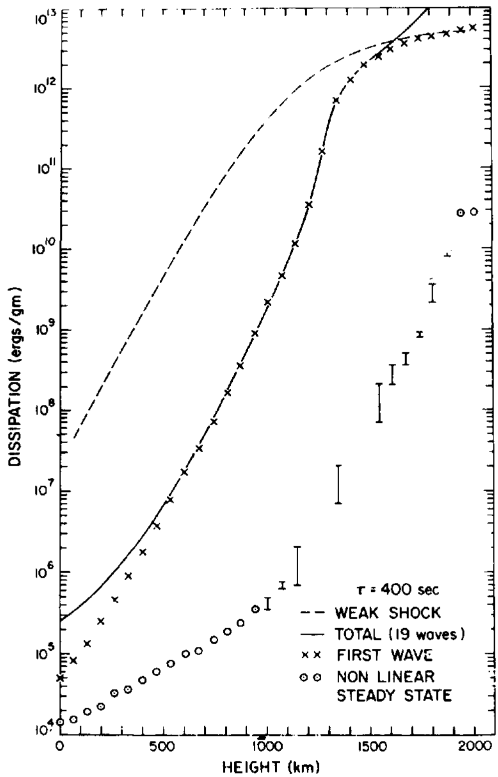

Figure III-ll

weak shock result for the dissipation here, and the steady state result for the non-linear calculation down below by 5-6 powers of ten. So the application of the weak shock theory to waves with periods longer than the acoustic cutoff is nonsense, even qualitatively.

Ulrich — May I please ask whether your exact equations of motion included the radiation curve?

Stein — In this particular case, to make comparisons easier, the comparisons were between the weak shock and an exact integration of the equations of motion for an isothermal wave in an isothermal atmosphere.

Schwartz — We have obtained results which include radiation, in the optically thin approximation.

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Souffrin — For an isothermal shock in an isothermal atmosphere there is an isothermal deformation, and the dissipation goes to zero.

Schwartz — If you have a shock you have dissipation.

Souffrin — For an isothermal wave, its dissipation goes to zero. Gamma is one.

Schwartz — No, it is /Tds and the entropy changes across the shock. But you assume, by saying it is isothermal, that this is a very unphysical radiation shock. As soon as you raise the temperature slightly above the ambient temperature, the system gets rid of all the energy by radiation as fast as you dump it in. That is physically what this means. But it's not a physical result.

Skumanich — What you are saying is that you are throwing away the entropy generated by the shocks, so it doesn't go into internal energy.

Schwartz — That's right, but you are keeping track of the total amount you have thrown away; that is what this dissipation is. This is admittedly an unrealistic case, but it was the only case for which we could write down an analytic formula for the solution of the weak shock equations to compare it to the numerical results.

Skumanich — **I** infer that you are then saying, "Don't trust weak shock theory." Now, do the weak shock theorists want to stand up and say something in rebuttal.

Ulmschneider — This work was done with waves of higher period. The weak shock theory is mostly done with waves of about 25 sec period. This is a factor of four below the waves discussed here. So I would suggest that by extrapolating in Schwartz's graphs from the 400 sec to the 100 sec and from there to the 25 sec waves that the weak shock result would be much better for the higher frequency waves.

Jordan — To that, I would like to add something that has already been pointed out; namely, in the application of this theory, we do not have to assume a fully developed shock at the zero point on those graphs. So I would completely agree with everything on Schwartz's graphs, and, yet, I think that the weak shock theory does have a useful range of validity for high frequency waves in the low chromosphere.

Skumanich — Then the question is; are they high frequency or low frequency waves?

Stein — In the discussion this morning, Beckers talked about the observations and said that they had looked at phenomena with good enough time resolution so that if there had been shock waves with periods of about 50 sec they should have seen them, and they didn't.

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Thomas — Wait a minute. You have given a shock wave and you should predict what you should expect to see. Then you ask whether Beckers has seen it or not.

Stein — Wolfgang, Kalkofen and I are in the process of doing that.

Schwartz — It should be noted that it is a bit misleading to talk about high and low frequencies in this context. Although a wave may be thought of as starting out with a certain frequency or superposition of frequencies, the situation is altered once the shock forms. Since the shock travels faster than the sound speed, it catches up whatever waves may be present ahead of it and converts some of their energy into shock energy. Thus, although you may have, for instance, some transient low-frequency waves present in the atmosphere which would never form shocks by themselves, that does not preclude their contributing to the strength of a shock by this nonlinear interaction.

Ulmschneider — **By** considering what kind of mechanical flux you have, you find that the amplitude of the shock wave is not very large. You don't expect a very high velocity of the shock. You expect a shock velocity that is almost equal to the sound velocity. In that way the shock doesn't eat up the other distrubances. It appears that the high frequency waves which have periods of around 10-20 sec develop into shocks first at low heights. You can get an idea of how such a shock develops out of a sound wave by considering the simple wave theory. This shows that if you assume the same initial flux, then a wave of high frequency will develop into a shock earlier. This was the basis for my work, in which I assume that at low heights, 200 km above the temperature minimum, high frequency shock waves are formed. In the case of short period waves, as we just saw, I suppose it isn't too bad to assume weak shock theory I think everything is self consistent and consistent with the computation of acoustic flux dons by Stein. The graphs shown by Jordan show that a large part of the acoustic flux comes in short period waves. Now. why don't you see them? I think, because of the contribution function, the height interval in which you contribute to the line emission is about 300 km. But if you have a 10 sec period wave and a 7 km/sec sound speed, then you have a wavelength of 70 km. This fits several times into 300 km, so you shouldn't see it. You shouldn't see effects of high frequency sound waves in spectral lines. It will add to the microturbulence of the medium, so you see a broadened profile. But will not see a periodic shift of any kind. So I don't think this is an argument against high frequency waves.

Thomas — But how much can it add to the microturbulence? You had a very high frequency weak shock, so how much does the material velocity

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change across that shock? If it's not much, then it doesn't do anything to the microturbulence.

Ulrich — There must be a sound speed difference across the front or it's not a shock.

Thomas — An infinitesimally weak shock has no material velocity amplitude, and that's what I measure. The propagation velocity is the sound velocity, but not the material velocity change.

Souffrin — There is a difference between turbulent excitation and pulse excitation. If you think of just one shock traveling, then, after some time, the initial situation is restored. Now from a stationary excitation there must be a stationary structure, something that is not a traveling shock.

Skumanich — One comment. We have been talking about two modes of excitation. One is from the turbulent convective region, and it propagates and then undergoes non-linear interaction. The second one is just like a piston hitting bottom of the atmosphere with some characteristic time. Which one is involved in producing these high frequency waves? Also, will the mechanical energy theorists please give the observationalists some guide as to what they should be observing. The theorists have to constrain the domain of applicability so that some observational parameters can be found.

Ulrich - There are four main points I wish to make. I will start off by some discussion of shock waves, because I think the evidence in favor of them is weak from the observational standpoint. At least for the longer period oscillations, the observed line core intensity fluctuations are too small to be compatible with shocks. If there are shocks, they must occur at higher frequency. I think this particular effect was demonstrated by the observations of Simon and Shimabakuro (Ap. J., **168,** 525), who looked at the electron temperature of the gas somewhere about 2000 km above the temperature minimum. They found that the brightness of this gas showed only a slight correlation with five minutes. Their time resolution probably was not able to provide useful results for periods shorter than 100 sec.

Concerning the power spectra calculated by Stein, I would point out that, in the region of the frequency diagram between 100 and 300 sec, it gives exactly the wrong slope compared to the observed power. Anyone using this theory should find a reason for this error and come up with another power spectrum which is more in agreement with the observations. Until this is done, I, for one, will remain a little skeptical of this peak at 50 sec. That is something that the high frequency shock people must do in order to make their theory more believable to me.

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In talking about heating today, we have had a good deal of discussion about the derivative of the mechanical flux. Figure 111-12 illustrates some of the relevant expressions. Most people have used the first expression for

CHROMOSPHERE HEATING WITHOUT SHOCK WAVES

F = FLUX OF ENERGY ASSOCIATED WITH OSCILLATORY MOTIONS

$$16\,\sigma\,T^3\,\rho\kappa\left(T - T_{eq}\right) = -\frac{dF}{dZ}$$: ACOUSTIC ENERGY BECOMES RADIATIVE ENERGY

WHAT IS F? THE CIRCLED QUANTITIES ARE CLAIMED TO BE F

ECKART : $\rho\frac{D\mathcal{E}}{Dt} + \nabla\cdot(P\underline{u})\ P\underline{g}\ \underline{u} + \rho q$

LANDAU & LIFSHITZ : $\frac{\partial}{\partial t}(\rho\mathcal{E}) + \nabla\cdot(\rho H u) = \rho q$

WHERE $\mathcal{E} = \frac{1}{2}\,|u|^2 + E + gz$

$$q = -16 a T^3 T?(T - T_{eq})$$

Figure 111-12

the mechanical flux. I have tried to find the source of this expression, and the references lead to the book by Eckart on "Hydrodynamics of Oceans and Atmospheres" in which this equation is indicated. This is the total derivative of the energy per gram of the fluid following the motion. This equation seems perfectly valid. However, I think it's hardly clear that the circled quantity is a proper flux since the divergence of this must follow the motion of the fluid. Additionally, there are two extra terms. Landau and Lifshitz, on the other hand, derive this equation, and point to this term where the script H is the enthalpy and claim that this is the mechanical flux. On the left hand side is a time derivative fixed in space so that Landau and Lifshitz's flux looks like a more legitimate flux since the divergence of it gives a time derivative of the energy density. I have adopted this as my definition of the mechanical flux. In the case of non-adiabatic oscillations this expression gives and additional entropy derivative which must be included in the flux. Another thing to notice in this equation is that I have written the emissivky schematically in a crude form. It is precisely this same quantity divided by the density which appears in the equations of motion of the sound wave.

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I have studied the propagation of acoustic waves in the presence of a temperature gradient and radiative interactions. Figure III-13 shows the assumptions that I have used to get a tractable dispersion relationship. The critical assumption here is that the opacity is given by LTE.

ASSUMPTIONS

1 SMALL AMPLITUDE OSCILLATIONS ABOUT A PLANE PARALLEL MODEL IN HYDROSTATIC EQUILIBRIUM.

2. PERFECT GAS EQUATION OF STATE.

3. OPACITY GIVEN BY INSTANTANEOUS LTE.

4. NO VISCOSITY

5. T?T^3 = CONSTANT

Figure 111-13

Relaxation of that assumption would give a different radiative cooling rate and possibly a phase shift. This assumption of LTE says that the radiative cooling goes towards wiping out a temperature difference between the average medium and the displaced parcel.

Now for the second point. I want to demonstrate that overstability is possible whenever the temperature gradient exceeds a critical value which is less than the adiabatic gradient. As a way_m of convincing you that this is at least possible, I would like to present the following rough argument. Consider an atmosphere initially in hydrostatic equilibrium. Label mass shells in this atmosphere by their undisplaced altitudes z and consider plane parallel displacements £(z) about these altitudes. The continuity equation then gives

$$\frac{Ap}{'\,p} = \frac{3\S}{3z} \quad '$$

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and the momentum equation gives

$$\frac{3^2 g}{912} = \frac{3AP}{8z} \; '$$

The quantities AP and Ap are the changes in pressure and density *following* the motion. In the case of an isothermal atmosphere and adiabatic displacement, the solution to these equations is well known and is (see Lamb, 1940, "Hydrodynamics," § 309)

$$\frac{\partial(\rho^{1/2}\xi)}{3z} = \left(\frac{\omega^2 - \omega_o^2}{c^2}\right)^{1/2} \rho^{1/2}\xi$$

where co is the frequency of the wave, co_0 is the acoustic cutoff frequency and c is the adiabatic sound velocity At co = co_0 we see that $3(p^{1/2}£)/9z = 0$. Therefore d£/Pz = £/(2H), where H is the pressure scale height. Using the continuity equation we conclude that

$$\frac{A£}{p} = \frac{L}{2H}$$

This implies an adiabatic temperature change of

$$\frac{A\,l}{T} = -\left(\frac{T}{VT} \cdot i\right) \frac{AP}{p} = -\left(\frac{7}{K1} - I\right) \frac{J}{2H}$$

The condition for overstability in the case of slow radiative heat exchange requires that the rate of change of the temperature in the blob | YT/£ I exceeds the rate of change of temperature in the surroundings | dT/dz |. In terms of the logarithmic temperature gradient this condition is

$$v > \frac{\gamma \quad 1}{2} \,,$$

which is always weaker than

$$\frac{\gamma \quad 1}{\gamma}$$

This condition differs from the usual condition for the onset of convection because the pressure in the displaced blob of matter is not equal to the average pressure. I find this same condition from the correct

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solution to the equations of motion with a temperature gradient in the case of slow radiative heat exchange. In the case of a very rapid radiative heat exchange I find the condition is

$$\nabla = \nabla_{ad}/2$$

but at present I do not have a short derivation of this condition.

The third point I want to make concerns the temperature rise. After computing the divergence of the flux associated with an acoustic wave, you can determine the temperature rise required to dispose of this energy. The equation I find is

$$T - T_{(\mathrm{rad.\ equi.})} = 350^{\circ}\mathrm{K}\left\{\left(V_{\mathrm{rms}}^{2}\right) \bullet K\left(\frac{\omega}{\omega_o}, \frac{\beta}{\omega_o}\right) \cdot \frac{2.5\mathcal{R}}{C_\rho\ \mu}\right\},$$

where v_{rms} is the material velocity in km sec^{-1} , K is a function which varys in value according to the graphs of Figures 111-14 and 111-15 as a function of radiative damping parameter |3 and the ratio co/co_o, Cp is the specific heat at constant pressure, <R is the gas constant, and ju is the mean molecular weight. The largest values of K occur for the lowest frequency and co and the smallest values of |3. At small j3 and low frequency you get a fairly large factor. If you put in an rms velocity like 4 km/sec, and if this value occurs at low frequency, then you get a very large temperature rise from this formula.

Another thing to note is that, for small |3, T T($_{rad.\ equi}$) is independent of j3. At small j3, if the medium can dispose of the energy quickly, then it gets a large share of the acoustic flux which comes by. On the other hand, a section of matter which cannot radiate easily does not get a very large share of the passing acoustic flux. This type of heating seems to be a rather democratic process where those who pay (radiate) receive a large share of the money (energy) and vice versa.

A final point which concerns the five minute oscillations is something of a puzzle. If you believe that the 5 min. oscillations are heating the chromosphere, then you have the disconcerting observation that the amplitude of the five min. oscillations is less under plage regions than under quiet regions. This means that you are generating less energy, since the energy generation in overstable acoustic waves is proportional to the square of the amplitude. Yet, there seems to be more emission in the higher layers. This is a puzzle. I think one possible explanation is that in a magnetic region, the required emission is redistributed and it is easier for an upper layer to radiate the energy which is being generated. So you need a smaller amplitude to drive the whole thing. This explanation does

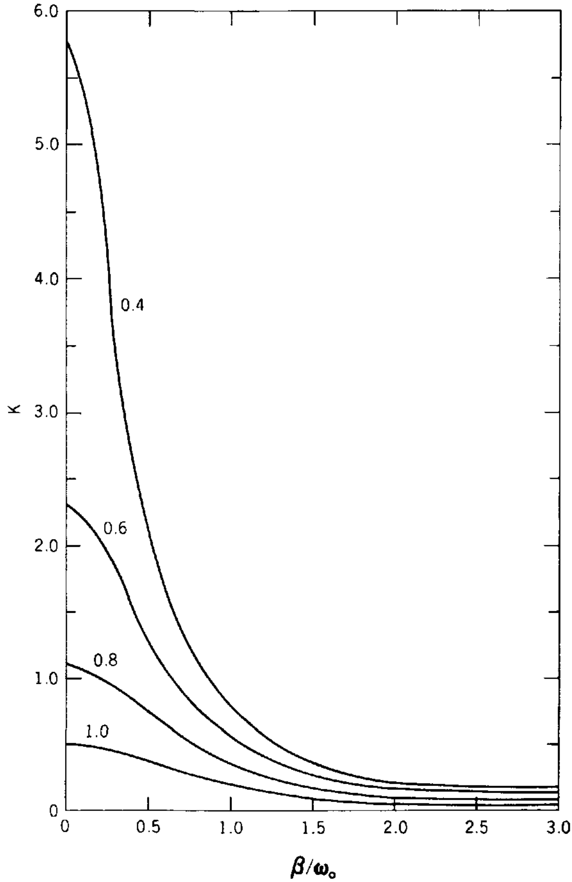

Figure 111-14

not seem very satisfactory, however, and, yet, I don't have a better one. Possibly there is another energy source over plage regions which is dominant.

Skumanich - What is the reservoir from which the work comes, is it the radiation field?

Ulrich — It's the oscillations in the convection zone. Ultimately, that is the source. The energy emitted locally in the low chromosphere and observed as a temperature excess has as its immediate source the pressure variations of the underlying layers. These in turn are generated by the interaction of the radiation exchange and the temperature gradient. The temperature gradient permits a displaced parcel of fluid to be cooler than average when it is in the compressed portion of the nscillation cycle. As

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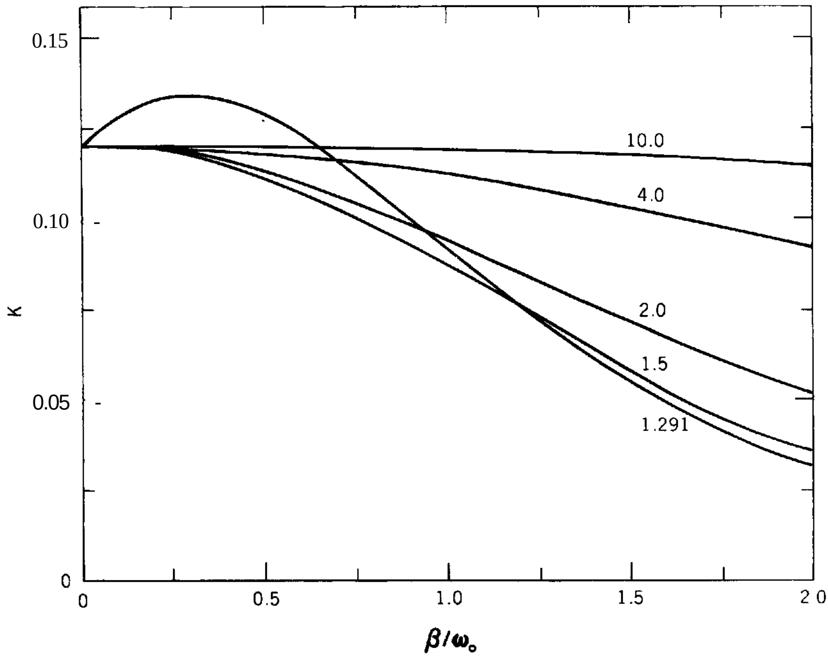

Figure 111-15

long as the matter gains thermal energy at this phase, then the oscillations will be driven as in a classical heat engine. As far as the immediate source of energy for the excess radiation is concerned, it doesn't matter what drives the acoustic waves. Also I should say that, at this point, I haven't said anything about the overall energy balance of the oscillations. This must be considered to determine the amplitude.

Thomas - I don't understand that remark.

Ulrich — In terms of the 5 min. oscillations, I think the analysis must be essentially non-linear, such as that done by Leibacher.

Thomas — What is your basic coupling mechanism between the aerodynamics of the motion and the radiative energy balance in the electron continuum?

Ulrich — This is just the work which is done over a cycle by the compression.

Thomas — So if **I** have a big radiative energy loss, I can hold this amplitude down.

Ulrich — If you have a big radiative energy loss, then, for the same amplitude of oscillation, you get a larger portion of the work out of it.

Thomas — I am talking about the temperature amplitude now. If I have a big radiative energy loss, I can hold this amplitude way down. I can't see from your equations where you have these things put in. There must be a coupling term somehow.

Ulrich — Perhaps I can clarify a crucial aspect of the temperature rise calculation. I write that

$$T(z,t) = T_{R.E.}(z) + [T_o(z) - T_{R.E.}(z)] + [T(z,t) - T_o(z)]$$

The radiative heat loss by the matter is then proportional to

$$\beta\,[T(z,t) - T_{R.E.}(z)] = \beta\,[T_o(z) - T_{R.E.}(z)] + \beta\,[T(z,t) - T_o(z)]$$

The time-independent portion of this expression is cancelled by the divergence of the acoustic flux which is a second-order average of the first-order solution to the equations of motion. The radiative heat exchange term which enters the equations of motion and effects phase relations is then the second term on the right hand side which is a first-order. As you say, a large value of β *will* hold down $T(z,t)$—$T_0(z)$. However, for $\beta \sim \infty$, the divergence of the acoustic flux is proportional to β, so that $T_o(z)$—$T_{R.E.}(z)$ is independent of β. Finally, an important point I haven't included in all this is that the coupling constant could be complex. In this case, you get phase lags between compression and cooling. I am almost sure that you will get this in the non-LTE regime.

Thomas — Your work just seems to lead to an awfully big temperature amplitude on the right hand side of the T—$T_{(rad.\ equi.)}$ equation.

Souffrin — To go back to the problem of wave generation, I would like to make a statement concerning the physical picture for the excitation of the 300 second waves. Where does the instability come from? I understand it as a mechanism which can be traced back to Chandrasekhar and Cowling as a general possible cause of pulsational instability. Its relationship with acoustic modes was clarified by Spiegel and later by Spiegel and Moore. It works the following way. To give rise to that instability, a system needs three kinds of things. It needs a superadiabatic temperature gradient, a mechanism providing a restoring force, and a dissipative process such as heat conduction. A system with these three properties can exhibit pulsational instability. The convective zone has the right temperature gradient. Radiation gives the necessary smoothing of temperature

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differences. The extra restoring force can be due to, say, a gradient of molecular weight which enhances the density stratification, or to a magnetic field, or to anything else you want. In the case considered here it is provided by compressibility, i.e., by the acoustic or pressure modes. That acoustic modes provide a restoring force inside a convective zone amounts to the fact that high frequency acoustic modes exist and are stable in such a zone. That is to say, for instance, that one can make noise inside a convective zone.

Let me sketch now how the mechanism works. Suppose an element of material is pushed out of its equilibrium position. Let the restoring force due to pressure prevail over the buoyancy force so that the system is dynamically stable, although the Schworzschild criterion indicates instability The parcel is then decelerated and ultimately turns back towards its initial position. Due to, say, radiative transfer, the temperature of the parcel tends to reach the temperature of the surrounding material, so that the parcel experiences a buoyancy force (upwards) at any level, which is smaller when it comes back towards its equilibrium position than when it first went up. Since the restoring pressure forces are not much altered by the heat exchanges, it is immediately seen that, along half a cycle, the balance between the pressure and the buoyancy forces is modified to produce a situation which is clearly pulsationally unstable. My belief is that the one very clear mechanism for producing the 300 sec oscillations is the one considered by Ulrich in his numerical calculations, applied to the convection zone.

Ulrich — The only point I want to make is that resonant acoustic waves are basically pressure modes, where the pressure variations are large compared to the average pressure. In the more familiar gravity modes you have described, the pressure variations may always be neglected.

Kippenhahn — **If I** understand your mechanism correctly then it is the same which produces overstability when V = d8nT/d8nP fulfills the condition $V_{ad} < V < V_{ad}$ + dfin ju/dCP *QJL* molecular weight). But then V_ad is the gradient critical for the problem while you have this puzzling factor 2.

Linsky — I'd like to make a comment to Ulrich about interpreting data, namely spectroheliograms taken in the cores of strong lines. There seems to be a strong tendency for the various elements of the solar chromosphere to segregate themselves into two camps—the brights and the darks. There is no true gray gradation between light and dark regions. I suspect that bright regions, in general, have higher densities and temperatures, and that an instability is indicated by the spectroheliograph data. Namely, a region that is slightly overdense absorbs more acoustic energy and becomes overheated relative to the rest of the chromosphere.

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Skumanich — We want an interaction with the radiation transfer people. We want to find what are the key observations which fix the free parameters in the dynamical theory

Souffrin — I ask for the following observations. The observations are to discriminate between the theories by locating the energy at any level in the atmosphere in terms of frequency and horizontal wave number in the (k,co) plane. This is not bound directly from observations. But the analysis of the observations in k and co is the only one useful for the theory.

Skumanich — So you suggest that a variety of lines at different heights should be observed.

Souffrin — Any line at any one height is good, if you can tell us how much energy of oscillation you find, not only at a given frequency, but also at that frequency *and* horizontal wave number. Space-time observations, two dimensional observations, at any altitude are what we need. It would be even better if you could give the density in the diagnostic (k,co) plane, with amplitude. This would make it possible for us to say if the unstable oscillation of Ulrich is real. If it turns out to be real, it could give us a lot of information about the stratification of the adiabatic gradient, as Ulrich mentions in his paper.

Skumanich — You said that we need simultaneous space, time observations, i.e. in the (k, eo) plane.

Sheeley — He is saying that the meeting place between the observationalists and the theorists is on the (k, to) plane.

Skumanich — But you can't see this, whether you like it or not you are born in the (x,t) plane.

A, **Wilson** — Nobody seems to point out that Frazier has already done this. This data already exists.

Souffrin — But we need it even better.

Skumanich — It is unfair to say as good as you can get it, because there are compromises that the observationalists have to make. So we really need to know where one should struggle very hard, and where it's not so essential.

Ulrich — Regarding the (k,co) plane, I would like to point out that the long horizontal wavelengths are the most important, because these are the ones which penetrate the deepest in the convective zone. I would caution the observers who are looking for evidence for long horizontal wavelengths that they must be sure that they don't have some power at short

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wavelengths, where the amplitude seems to be greater, mixed up with their observations. This could be most confusing.

Underhill — Talking about observations to prove a theory, theory is supposed to represent a fundamental behaviour of material. I would like to point out that the non-solar stars are useful. It is not necessary, as far as I have understood the suggested mechanism, that it occur only in a few thousand km long lengths near the surface of the Sun. If the mechanism is universal, it seems plausible that, under conditions on a star with different gravities and radiation fields, the scale may be larger. If so, then you could look at the stars and find brightness variations, in selected wavelength regions, of these short periods. Rather rapid pulsations of certain stars are known. Whether they are relevant to this mechanism or not I don't know. I haven't quite got the physical picture. But I think it is worth exploring. The mechanism might operate under different scales, and then occur on the appropriate stars.

Skumanich — You do give up space resolution when you do stellar observations. If the concern is, in fact, to use the space scales to pin down which of the mechanisms is operating, this could hurt you.

Underhill — I have not understood from the discussion that they have said they need a tiny space scale.

Stein — Two types of observations are possible: a statistical approach which looks for the location of power in the (K_{Horiz} *co)* plane, and the analysis of individual wave packets. Studies of individual wave packets could determine the polarization relationships between Au, AB, AP, k, B_o, as well as the vertical and hoizontal propagation velocities, and the shapes and sizes of the packets. The directions of Au, AB, B_o and k can't be easily determined, but their relative amplitudes, and the variation of the relative amplitudes with height and from center to limb can be observed. Because the magnetic field, B_o, is more or less vertical in the network in the chromosphere, and because the ratio of Alfren to sound speed changes with height, such studies will give information on the type of wave.

Skumanich — Why do you people give temperature increases, and not the rate of energy dissipation?

Stein — We have that. You would begin to see a temperature rise where substantial dissipation begins, if there were no radiation. Radiation has little effect on the dissipation. The wave is still dissipating the same energy, but that energy is now going into ionization and radiation, not heating.

A. Wilson - The point I got out of today's discussions was that there are two schools of thought about the heating problem. These are either: (a) We need high frequency, short wavelength acoustic waves with very large amplitudes (1-2 km/sec). No mechanism for generating such motions has been suggested and they are not detectable observationally (b) The main cause of the heating above the minimum is the energy dissipation of the running wave component of the 300 sec oscillation. A small amount of energy in the form of high frequency waves may be needed lower down with small amplitude (.25-.5 km/sec).

These two alternatives bring us face to face in the Sun with a most important contemporary problem in the study of stellar atmospheres. What is microturbulence? As you know every line in the solar spectrum shows an increasing width towards the limb. No theory of line formation predicts this effect. It is always ascribed to microturbulence. The microturbulence has an amplitude of 1-2 km/sec and is on a small enough spatial scale to evade detection by wiggling line bisectors, etc.

Apart from the intrinsic interest of the heating problem, it throws a great deal of light on the subject of microturbulence. If the velocity field postulated in suggestion (a) can be shown to be really necessary to heat the atmosphere we must accept that microturbulence exists. We then have a rather stiff hydrodynamic problem; that of working out how it is generated and propagated. If suggestion (b) wins the day, as I think it will, we have a rather interesting situation. Firstly, the 300 sec oscillation will not give rise to the anisotropic microturbulence required in the photosphere because: its amplitude is too small, its z dependence is exponential, not sinusoidal, and it is a primarily vertical oscillation. Secondly, any small wave motions required to start the heating at low heights will have far too small an amplitude to act as microturbulence.

As you know, the history of microturbulence is very unhappy. It was operationally defined in the days when our understanding of line formation wasn't even roughly correct. Microturbulence is simply a discrepancy factor. Its importance lies in the fact that it plays a central role in methods of determining element abundances.

Now suppose that we can heat the solar atmosphere adequately without using a microturbulent velocity field. What then causes the increase in width of the lines towards the limb? We can look for breakdowns in our descriptive scheme at two points:

- In the theory of line formation: Here we can ask if the assumptions of a frequency independent and isotropic line source function are adequate. Any discussion of these questions must rest on our ability to obtain the radiation field bathing the atom and

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the redistribution function for scattering in the atom's rest frame. The problem of obtaining the redistribution function has now been solved. We have discussed that of obtaining the radiation field bathing the atom below: Now let us consider the second independent source of error in our theory.

- Error in the description of the solar atmosphere: Here we can ask the following question: Does the present assumption of a homogeneous atmosphere with anisotropic microturbulent motions provide an adequate description of the inhomogeneous state of the actual atmosphere?

Clearly any attempt to form the radiation field bathing the atom by directly analyzing observational data must solve the inhomogeneity problem first. Recent work of mine has shown that:

- No self consistent explanation of the core profiles of the D lines is possible if the solar atmosphere is homogeneous and does not contain microturbulance.

- No consistent explanation of the center limb variation of the 4571 102 A of Mgl is possible in a homogeneous atmosphere. One is forced to the conclusion that the inhomogeneity of the atmosphere is not well approximated, using the microturbulence model. Therefore the development of the subject should be as follows: (a) Observationally we must obtain sufficient spatial resolution to obtain limb darkening curves at each point in the structure pattern of the inhomogeneity. (b) These limb darkening curves must then be inverted to yield a first order structure. The inversion will assume the simplest line formation physics.

But we have now returned again to the problem of self consistency of the source function (which of course now depends upon position in the atmospheric structure). Only when our data set closes along all resolution axes have we any right to expect adequate agreement between our theory and the observations. Until this time we shall be plagued by non uniqueness arising from insufficient resolution in wavelength, space or time.

Finally I should like to emphasize again the importance of microturbulence in the solar atmosphere. If it is present, it is by far the dominant part of the velocity field. It does absolutely everything. It looks as if solar hydrodynamicists have already tacitly assumed it does *not* exist, as they have made no attempt to explain its generation on propagation. The majority of the lines used in abundance analysis fall on the flat portion of the curve of growth, and are very sensitive to the value of microturbulent velocity adopted. Until the present confusion about the nature of

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microturbulence is cleared up, we can have little idea of the accuracy of the abundances estimated from such lines on the accuracy of our line formation theory.

Skumanich — I would like to suggest a possible experimental, observational test in stars, as Anne Underhill would like us to do, for whether you have a convection driven heating, or a self excited heating, that presumably exists along the main sequence and is not due to the presence of a convection zone as I am guessing. As Wilson suggested, lets look at the diagram of the b-y index versus the absolute power emitted by the Call chromosphere. This is the actual power output; it is not normalized to the luminosity of the star. You have a curve, for example for the Hyades; that is still rising near where the observations become difficult and disappear. Does this continue to rise? Do we find, very close to here, the rapid turnaround, because the convection zone is disappearing? I don't think the evidence is yet in.

The spectral types here are F6, F7 etc. There is a difficulty in obtaining measurements as we go to earlier stars, because the continuum is rising rapidly. The line itself is being affected by the higher effective temperature, and the ionization changes the line opacity, but we should see the turnaround if it is there.

O. Wilson I started the Hyades at just an arbitrary point. Perhaps I didn't go far enough towards early type stars.

Skumanich — From looking at your data, I couldn't find evidence of even saturation.

0. Wilson — I think it is because we didn't look there, we didn't go there.

Skumanich — I am then repeating your suggestion that we should look at this end of the main sequence, and see if there is a turnaround where we believe convection is dying out.

Wilson — I think it dies out very rapidly, if you find where rotation ceases.

Underhill — But that doesn't mean that you don't have a chromosphere, because you could have these mechanical pulsations excited in another way. You pould have shearing on rotation. You just need some little disturbance in density to have it grow.

Skumanich — The only two mechanisms I have heard about have been the overstability, and the convection zone driving an oscillation field. I don't know much about rotationally driven overstabilities. You may possibly be right. This may not be a test of these ideas. It would certainly be interesting to know whether there is a turnaround or not.

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Wilson — You know about this point on the main sequence, which is a (b-y) of 0.28. You no longer see strong chromospheric activity. But Procyon does have weak chromospheric acitivity. It lies above the cutoff of rapid rotation. If that power point marks the onset, or the end of deep convection, then you still have some chromospheric activity above that, but it is very weak. But we are looking here at a rather narrow range of spectral types. Procyon is F3, and the cutoff point is F4 or F5. As it refers to (b-y), it's a little early.

Skumanich — It's also a subgiant.

O. Wilson — It lies in the main sequence band according to Strömgren.

Skumanich — That's true, but is there evidence that it is going horizontally across?

O. **Wilson** — This I don't know.

Underhill — This comes back to the problem of defining chromospheres. We've got to stick to the definition of a temperature increasing outwards.

Skumanich — My definition would include my guess that whatever produces calcium emission on the Sun produces it in the main sequence stars of earlier type. I am using an homologous shift of the Sun up and down the main sequence.

Cayrel — Is the Lighthill theory able to predict the magnitude of the mechanical flux of energy coming from the noise in the convection zone?

Skumanich — I have looked at the work in the field, so I will try to answer the question. The Lighthill theory was first done by Proudman and he got a factor in the coefficient on the order of 50. Stein did it again and found that the power put into the tail of the turbulence spectrum governs the coefficient very sensitively. You are going from 50 to 1000 depending upon how you decay the energy in the high k, high co, part of this diagnostic diagram we have heard so much about. This makes me afraid. When you have an answer that is so sensitive to what you do with the tail of the spectrum, how can we trust the energy estimates? How can we possibly understand the tail of the spectrum, if we don't know the physics of turbulence?

Souffrin — You are quite correct. That theory is a dimensional analysis. It just tells us how much we will modify the output, if we modify the source in some way. That is not very useful for observations.

Cayrel — At least has the flux been computed with exactly the same assumption for a dwarf and a giant, for example?

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Skumanich — The problem with the dwarf and giant stars is that in the giants the flow is essentially supersonic. You get into the difficulty that the theory breaks down for Mach numbers close to 1.

Stein — About 4 years ago Strom and I computed the flux the Lighthill theory would predict for a series of main sequence stars and giants. We wanted to see the results of applying the same wave generation assumptions to all the stars. I don't know how you measure the extent of a chromosphere, but we found exactly the opposite from what some of you seem to think is the case. Namely, the ratio of the mechanical energy input to the luminosity of the star goes down as you go down the main sequence to cooler stars, rather than going up. This is why we never published the paper. However, it does go up for the giants, but that is much more uncertain, because you get into much higher flow velocity

Skumanich — There is no theory for sonic turbulence.

Jordan — I'd like to present the results of some calculations by de Loore. He used the Lighthill theory for generation of sound waves by turbulence in the convection zone of stars. To calculate convection zone models along the main sequence, from A5 to KO, he used the Bb'hm-Vitense mixing length theory. He found that the hottest, densest coronas could be expected for the late A and early F type stars, with coronal temperatures as high as four million degrees, and electron densities up around 10^{10} Figure **III-**16 shows some of his results. The numbers in the left hand graph are effective temperatures; in the right hand graph, they are relative magnitudes for the mechanical energy flux. He normalized things with respect to the Sun, and got for the solar corona a 1.1×10^6 °K corona and $N_e = 10^9$ In order to get that, he had to assume that the flux value was generated only over 10% of the solar surface. He did not include this normalization in his other calculations. His calculation in the convection zone for small r is inferior to a technique employed by Kyoji Noriai, and, therefore, de Loore tends to overestimate the convective flux, particularly for the earlier type stars where the convection occurs more in the surface regions. I mention this work without any comment, because, in view of all the assumptions and uncertainies in it, it is impossible to evaluate how relevant the calculation is.

Skumanich - What are the observational implications?

Jordan — One of the implications is that one should look at strong ultraviolet lines in coronas of late A and early F type stars. If they do have such hot, dense coronas then you should see these lines. These atmospheres may even be optically thick in some of these lines, due to higher predicted coronal densities, if de Loore is right.

Ulrich - Convection seems to exist in rather early stars, according to de Loore.

Jordan - It is true that, even for stars earlier than A5 and for effective temperature up to 41,000° K, de Loore always found some convective instability. However, if you notice the vertical dashed line in Figure 111-16 for stars earlier than A5, the region that carried the most convection had a ratio of convective to total flux of less than 20%, and this dropped off

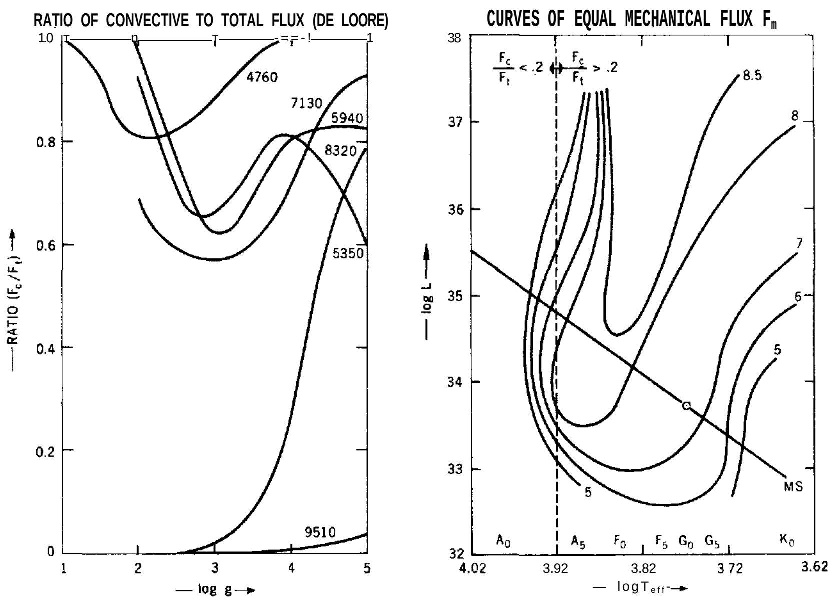

Figure 111-16 Ratio of convective to total flux(de Loore) (left) curves of equal mechanical flux F_m (right).

so sharply that he did not predict strong chromospheric activity for stars earlier than type A5.

Mullan — The results of de Loore, and also the results of Castellani *et al* (Astrophys. & Space Sci. 10, 136, 1971), were computed using the formula F $\sim M^5 v^3$ for the mechanical energy flux. Here, v is the convective velocity and M is the Mach number associated with this velocity. These authors have applied this formula even in cases where M is as large as unity However this formula was derived theoretically in the limit of small M, say M < 0.1, and the accuracy of the formula is expected to become very low as M approached unity. And even if the formula turns out to be accurate, the uncertainties in v due to uncertainties in convection theory are enormously amplified in F Further

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uncertainties arise if magnetic fields are present, so theoretical estimates in mechanical energy fluxes computed in these papers can hardly be considered accurate, even to within an order of magnitude.

Jordan — I agree.

Skumanich - We have to go backwards from the observations to inferences about what is the mechanical flux, and further yet to inferences about what is the convective source. We need more from the theorists in terms of a simple physical picture.

Leibacher — First, I have two comments on my own work. The heating calculation is being done for the Speigel mechanism right now. It may take a long time. Second, concerning the cause of the heating, Figure 111-17 shows how we discriminate between various theories. This is a picture of velocity versus time at a number of different heights where the zero height is the $T_{5000A} = 1$ point. There has been some argument

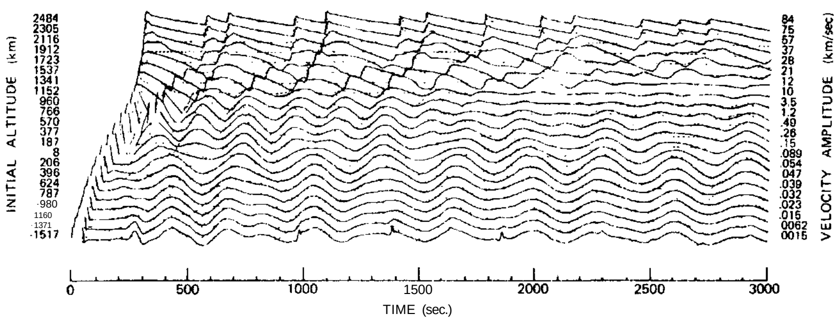

Figure III-17

about there being observational evidence for shock waves in the solar chromosphere. If you look at these profiles you will see that they are very symmetric up through 1000 km, up to the height where Ca K is formed. These are the highest lines we can see from the surface of the earth. Right now it is very difficult for us, with the observed amplitudes here at the earth's surface, to expect to see shock waves in the chromosphere. In Figure 111-17 we are looking at the velocity profile from a computer experiment. In some way we create an oscillation which has the correct amplitude here at the surface. This is a 0.2 km/sec oscillation. Now. the question is, as a result of this correspondence with observations, what would we expect to see in the chromosphere? Would we expect to see shock waves higher up? Can we decide on one of the various heating mechanisms? The answer is no; we would not from the surface of the earth. You have to go to the higher lines formed above 1500 km, where

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you see the velocity profiles become asymmetric, and the pressure profiles become very narrow. The dotted lines are pressures and you can see here, nearer the base of the temperature rise to the corona, the pressure is very constant. It has a very narrow, in time, over-pressure. Again, those cannot be seen from the surface; we will have to wait for OSO I observations. Now I would like to report on a number of contributions from the informal meeting yesterday afternoon.

Underhill — The temperatures, densities and pressures in the solar chromosphere vary somewhat like those in the atmospheres of early type stars. Only early type stars are much larger than the Sun, so we have a lot more material. It's very well known that you get sporadic emissions in some short period pulsating variables. You also get, as Fischel found, sporadic disappearance of the C IV resonance line. Pulsation like you show may occur in early type stars, and you might not need very much at all to trigger them off. You might not even need a convection zone to start them. But the result of those shocks is superheating. I don't like the idea of saying chromospheres exist only for stars with convection zones.

Leibacher — I would first like to consider two sets of observations by Musman and Beckers on the presence of exploding granules in the surface layers of the Sun. For a long time there has been a series of observations by Rösch of the appearance of very bright spots on the solar surface, which then expand into a ring and disappear. These are continuum observations. With the new Tower Telescope at Sacramento Peak, Musman has made movies of these appearances, and has made some hydrodynamic models of them which are very similar to cumulus clouds models. Beckers has been doing similar observations with his velocity filtergram system. It has velocity pictures and short velocity movies, and hopefully in the near future longer velocity movies of these exploding granules. To the extent to which oscillation and heating theories depend upon excitation by granulation, I think all of a sudden we are moving ahead very rapidly.

However, it should be noted that recent work of Sheeley and Bhatnagai indicates that the granulation and oscillation horizontal scales differ by a factor of three.

A second area of discussion was the observations of the 5 min. oscillations on the solar surface, and the reliability of these observations. Figure 111-18 is for those who have been talking about the horizontal scales that are involved here. This is the famous diagnostic diagram. The isophotes here are iso-power lines and are the results of some observational work by Frazier. A great deal of effort has been placed on trying to understand the double peak nature of the oscillatory motion. There are two distinct peaks. If you look at a power spectra, Fourier analyzing the

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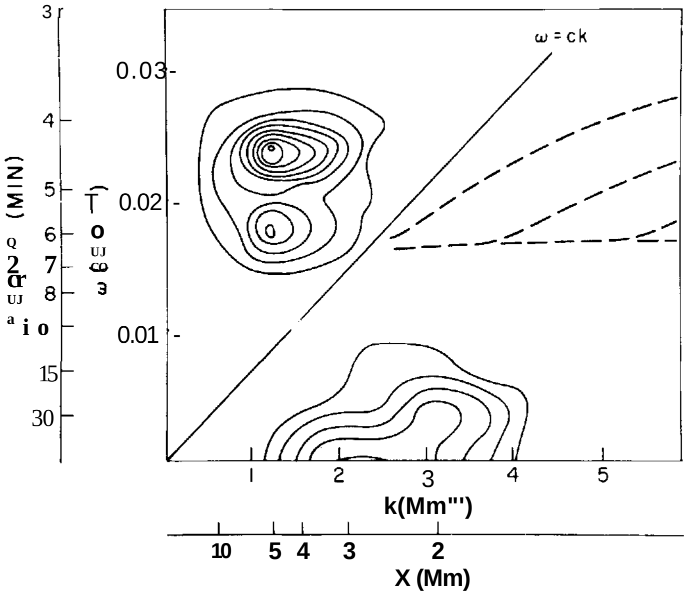

Figure 111-18

velocities, compressing everything onto zero horizontal wave number, power as a function of frequency shows a number of very narrow peaks which correspond to very long coherence for oscillations. Figure III-19 is a very long record obtained at Mt. Wilson by Howard Which has been analyzed by Cha and White at HAO. You see here, for instance, what

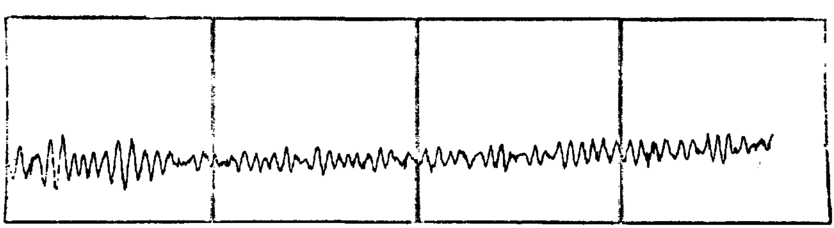

Figure 111-19

appears to be an extremely long, in phase, series of oscillations. It is the length of that packet, then, that gives rise to the very narrow peaks in the power spectra. A lot of effort has gone into the interpretation of the multiple peaks and their positions. The result is now emerging from White and Cha, and separately from Deiibner, that the very peaked nature of the power spectrum is a result of statistical uncertainty in the records.

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There aren't enough independent data points. More satisfying to White and Delibner are single peaked envelopes, which are stable in time. There is bound to be some reluctance in accepting a change as drastic as this. One of the most convincing arguments in favor of it is White's ability to reproduce the observations in the statisitical sense, from introduction into a power spectrum such as this of purely random noise. In other words, he can take a filter and filter noise and get the observations back out. I have some of White's pictures. His work will be published shortly. In summary of their findings: the best representation they have for the observations of the 5 min. oscillations are that it is a narrow band random process with the emphasis placed upon the randomness.

As a last point, there has always been some debate between the granulation excitation, as I have mentioned, and over-stability arguments such as Ulrich, and Stein and I have been proposing. If you look at the structure of the individual packets, you find you can get essentially whatever you want. If you look at frequency vs. time in a packet, some packets have their frequency increasing with time, until the packet disappears; others have the frequency decreasing with time. If you look at the amplitude vs. time in a packet, in other words, and ask if the packet starts off very large and then dies out, you find just that. You also find the same result with time running in the opposite direction. So any theory you want to justify, you can find a section of the record that will reproduce it. If you take enough of the record a statistically significant sample of the oscillation, all the determinism drops out.

For the theory of the 5 min oscillation, there exist two primary schools of thought. One is currently represented by the people at the University of Rochester, Al Clark and John Thomas, who have been proposing that the oscillation consists of trapped internal gravity waves. Internal gravity waves are essentially bouyancy waves, to some extent similar to the waves one sees on the surface of the ocean. They are trapped by the temperature structure of the solar temperature minimum. The other school, represented by Ulrich, Stein and myself, sees the energy concentration of the 5 min oscillation as being sub-photospheric, and the model is more represented by an organ pipe, the upper surface of which is the top of the hydrogen convection zone. The lower surface is several mega-meters beneath the surface. The observations in fact, relate to evanescent waves, non-propagating waves that are tunneling through the temperature minimum.

Skumanich — **In** view of its importance, I would like to reopen a discussion of the convection zone.

E. Böhm-Vitense — Unreasonable results are obtained by people who apply the mixing-length theory in cases where the convection zone is

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thinner than the mixing length. It doesn't make sense to take a mixing length equal to the scale height, and obtain a convection zone which is only one half a scale height thick. Second, the mixing-length theory, as it stands, is certainly not the ideal theory. I think we can estimate the velocities without relying on the theory. If at some point, essentially the whole energy is transported by convection then by putting the convective energy transport equal to the total energy transport you can derive the average velocity at this point without too much uncertainty. There is less than a factor of two uncertainty in the velocity which you get this way, as long as you are sure that at that point the total energy is transported by convection.

Skumanich — Does this mean that the velocities become large.

E. Böhm-Vitense — They do increase with lower densities, which means they increase with increasing temperature or increasing luminosity until the convection becomes ineffective. On the main sequence, that happens at about 8000 °K.

Peytrenann — I have a question about the graph of de Loore which shows the ratio of convective flux to total flux: to what depth do they refer?

Jordan — It varied. Certainly for the late A and early F stars, it was above mean optical depth unity.

Stein — What we calculated, when Strom and I did it, was not the ratio of convective but of mechanical energy flux to luminosity deduced using the Lighthill theory.

Peytremann — The mixing length theory you all use can be criticized, but whether it is wrong or not, it should lead to the same results when used by various people. The small ratio of convective to total flux in de Loore's graph (Figure 1 (Jordan) left hand graph) may result from the fact that this theory is certainly not valid when applied to layers thinner than the mixing length itself, as. may have been the case for the hotter stars de Loore treated.

Skumanich — I think that the idea of using a scale length the order of a scale height is not crazy at all. In fact, my own work in 1955 shows, this was for convection in a poly tropic atmosphere, and that as you change the horizontal length scale, the flow packs itself into that scale height which is like the horizontal size. You fix this size, as Böhm has found out, by damping effects. We are still investigating whether or not there is convection in the early type stars.

Peytremann — In all the models I have calculated, I never had any convection in atmospheres earlier than spectral type A.

Kandel — The question is what are you using mixing-length theory for. If you are talking about energy transport, which is internal energy flux, then the mixing-length approach may be satisfactory; it may give reasonable results; and you get velocities out of that which are certain average velocities which work very well. For the purpose of computing a mechanical energy flux which will perhaps heat a chromosphere and corona, you have a very different type of average over the velocity. You are working with the tail of the distribution. I don't think the mixing-length people would say that they could tell you what the tail of the distribution will be, when it is involved with some average over velocity to the eighth power (as Mullan said). You have this enormous uncertainty which makes it very very hard to believe any of these predictions.

Skumanich — I agree.

Mullan — Just how accurate are these models? The fact is that we do not know the run of Mach number with depth in any star. We do know that certain M dwarfs (the flare stars) have coronas, for they have been observed to have radio bursts somewhat similar to Type III solar bursts. Kahn and Gershberg have found that gas densities in the coronas and chromospheres of the flare stars are up to 100 times greater than in the Sun. However, the maximum convective velocity in an M dwarf is expected to be smaller than in the Sun according to current convection theories. If this is so, then a star with small convective velocity is somehow able to generate sufficient mechanical energy to support a corona 100 times denser than that in a star with a higher convective velocity.

Skumanich — That's a good point. We certainly have avoided the variation of model and dynamical properties along the main sequence. I think part of that is that we don't have a full understanding of the observations. The observations exist, thanks to Wilson.

Stein — It may be that turbulent noise generation by the Lighthill mechanism, while present and important for heating the chromosphere, is not the primary source of mechanical energy for heating the corona. The calculations of Leibacher and myself suggest that the 5 minute oscillations are heating the corona. Such long period waves will get their energy up to the corona more easily. We are in the process of calculating the generation of the 5 minute oscillations by thermal instability in the superadiabatic convection zone. This process will presumably have a different dependence on stellar properties along the main sequence than the Lighthill mechanism.

Underhill — There is a theory that suggests that stars with magnetic fields are rotating underneath. These magnetic fields will become wound up and

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they can become very strong. This theory explained how to get a magnetic field in a white dwarf. At the same time, the star blew off its atmosphere, so you had the white dwarf left over. If the white dwarf has a fair amount of cool expanded atmosphere around it, it will give you a nebular spectrum. If those magnetic fields somehow accelerate the material, you will get x-rays. Perhaps some of the highly excited atmospheres are not heated by mechanical energy, but may be heated by soft x-rays which we cannot observe because of their attenuation between us and the object. It's not impossible.

Skumanich — I think, by arguments of homology, that along the main sequence the fields can be ignored. The observations of the solar wind, which is driven by the energies deposited in the corona, seem to be independent of the magnetic cycle of the Sun. I am not sure that this argues that they are secularly independent. All we know is that they don't follow the actual oscillation. But they may follow the mean amplitude of the magnetic field. Whether the field can act as the energizer of the gas, as you suggest, I leave to the white dwarf men. However, whether flares represent, in some generalized sense, some heating mechanism for the corona, I think that that would also be cycle dependent, which we don't observe.

Mullan — Observers cannot depend on theoreticians for guidelines as to what should be observed, simply because uncertainties in the theory of mechanical energy generation are so great. In fact the problem must be inverted; and I would like to ask the observers to present theoreticians with a value for the solar mechanical energy flux deduced from observations. Theoreticians might then profitably use this as a constraint on the various free parameters at their disposal.

Skumanich — One problem is that some of the theoreticians give us a model for the dissipation as a function of height in the atmosphere, while others give us temperature and density models, but the two don't spend enough time checking each other. One might say that the atmosphere is a filter and what we really want is the pass band of the filter.

Souffrin - **I** would like to suggest that people not look too closely at the observations. Many excellent theories in science would not have been developed had people had very detailed observations. A number of large scale effects on the Sun which have been discovered would not have come to light if people had been concerned only with more detailed observations. This is not to say that I advocate no observations, but only that I think the theory should be better developed so that we at least understand the large scale phenomena.

Skumanich — So long as we always keep in mind that, in the absence of laboratory experiments, theory and observations must bootstrap each other in astronomy if we're to understand anything at all.

Schwartz — Concerning observations, I would just like to emphasize that observations of velocity fields are not observations of the power which is propagating through the atmosphere. The only way to learn if there is energy propagating from velocity field measurements is to measure the phase relations between pressure and velocity. If these two quantities are in phase, then you know energy is propagating. If the pressure and velocity are 90 degrees out of phase, then it doesn't matter how large the velocity you have is, you aren't propagating any energy. So what we fluid dynamics people need is for the radiation transfer people to solve the transfer problem to give us information on the pressure from the intensity variations. I know this is a tall order, as it means doing the transfer problem many times (10 or 12) during the 300 sec period, rather than once; but it is what we need.

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