MECHANICAL HEATING IN STELLAR CHROMOSPHERES USING THE SUN AS A TEST CASE

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INTRODUCTION

The remarks in this talk will apply only to chromospheres of comparatively late type stars which have significant convective envelopes. This is not to imply that mechanical heating does not occur in other stars, but only that, to the best of my knowledge, little or no satisfactory progress in applying mechanical heating theories to the outer atmospheres of non-solar type stars (without convective envelopes) has been made. Indeed, practically all of the progress that has occurred has been in solar work, so most of my remarks will pertain to the Sun.

The serious work on solar atmospheric heating began in the late 1940's and, since then, has included treatments of wave modes which might be involved and the development of observational techniques to detect them. Definite results up to the mid-1960's included strong theoretical support for some kind of gravity-modified sound wave as the source of at least some heating via shock dissipation, and the earliest observations of the now well known (but still not well understood) 300 sec periodic variations in the line central brightness and position of many upper photospheric and low chromospheric lines.

Comparatively recent efforts in the past six years have emphasized more detailed numerical calculations, including some non-linear effects, to determine the generation, propagation, and dissipation of various wave modes for more realistic solar atmospheric models. In addition, the corresponding observational work has been directed toward studying phase relations among oscillations at different heights (using lines of different strengths) and toward getting both high spacial (1 arc sec) and time (5-10 sec) resolution spectra, in the hope of inferring directly from the observations information on the heating and the associated velocity fields.

With that background, I'd like to offer a brief review of the principal wave modes proposed and studied for the heating, along with where they are generated and how they propagate. Then I'll review the solar heating picture as it stands today.

WAVE GENERATION AND PROPAGATION

The general problem of wave propagation in a compressional atmosphere with gravity and a magnetic field is treated by Ferraro and Plumpton (1958) and many others. Since it is difficult to solve the propagation equation with all the terms in it, the usual procedure has been to obtain solutions for simpler cases where one or more of the three basic parameters (medium compressibility, magnetic field, and gravity) are left out. For the moment, I'll ignore the magnetic field parameter.

Extensive studies of the gravity-modified sound wave have resulted from the original suggestions of Biermann (1946) and Schwayschild (1948) that these waves heat the outer atmosphere by shock dissipation. In particular, numerous applications of the Lighthill (1952) theory for generation of sound waves by isotropic turbulence have followed his pioneering work. One comparatively recent and important contribution by Stein (1968) included several calculations of both the total acoustic power generated and the frequency distribution of the acoustic emission. To do this calculation, it is necessary to know the turbulent velocity amplitudes and also the turbulence spectrum (spacial and frequency dependence) in the generating region. Since these parameters are currently difficult to infer from observations, reliance on a convection zone model and theoretical turbulence spectra is necessary. Stein, like many others before him, had to use an admittedly rough model for the convection zone, based on the earlier Böhn-Vitense (1953) mixing length theory. He then did the calculation for several different turbulence spectra. His results demonstrated that the total acoustic power output is highly sensitive to the high frequency tails of these spectra. This situation, added to the already well known sensitivity of the result to the turbulent velocity amplitudes (the acoustic emission varies as the fifth power of the turbulent Mach number), introduces considerable uncertainty into the computed acoustic flux. Stein's computations yielded an uncertainty of about an order of magnitude in the acoustic flux, but the further uncertainties in the convection zone model and in the method used for the calculation, which ignored the interaction between sound and turbulence, suggests an even greater final uncertainty in the results.

In spite of all these difficulties in this extremely elaborate treatment, Stein's results are important for two reasons. First, even if his *lower* limit for the upward flux of sound waves is an *overestimate* by an order of magnitude, this flux would still be of the order of 10^6 ergs cm^{-2} sec^{-1}, which now seems adequate to balance the net radiative losses in the lower chromospheric region by dissipation of weak shocks. Since the empirical evidence of the solar granulation, as well as simple theoretical arguments based on Rayleigh and Reynolds numbers, lends continuing support to this general picture of sound wave generation at the top of the convection zone, Stein's results are encouraging. Second, the calculated frequency dependence of his acoustic emission exhibits a peak far above the critical angular frequency $\omega_s = \gamma g/2c_s$ (γ = specific heats ratio, g = gravity acceleration, c_s = sound speed) below which all sound waves are reflected. If this were not true, vertical transport of the sound waves through the temperature minimum could not occur. This important result was true for all turbulence spectra used. Figure III-1 is a graphic demonstration of this second conclusion, where acoustic flux spectra are graphed for the three turbulence spectra used by Stein. An immediate consequence of this

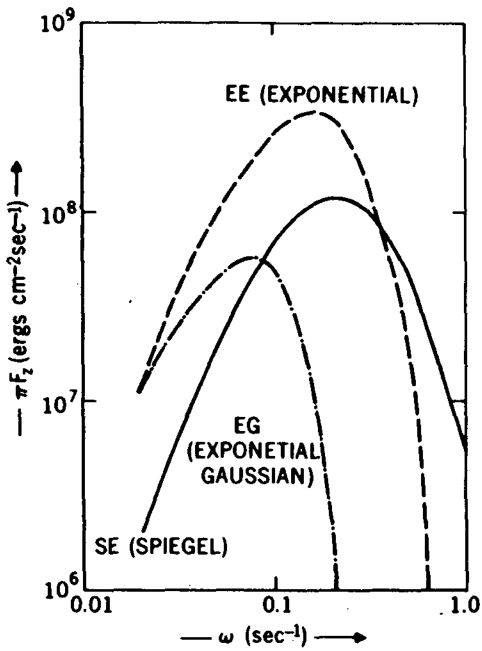

Figure III-1 Steins solar acoustic flux spectrum.

result was that people working on the chromospheric dissipation of waves generated by turbulence in the low photosphere returned to their work with renewed confidence that they were doing something relevant to the Sun. The general picture of chromospheric heating now seems still more involved than when Stein's results appeared, as we shall see presently, but the two main conclusions mentioned still stand, to the best of my knowledge.

So far, I have deliberately avoided mentioning magnetic fields. We know they must play some role in the heating problem. One has only to note the strikingly different behavior in the temperature sensitive H and K lines over plages and the so called normal chromosphere. What role do the magnetic fields play?

This is a difficult question to answer, because the introduction of the magnetic field complicates the mathematical problem considerably, particularly by introducing significant non-linear terms into the propagation equation (Pikel'ner and Livshitz, 1965). Understandably, less progress has been made here than in treating the simpler case of zero magnetic field. Fortunately, there is one rather strong statement that can be made. It *may* be possible to ignore the magnetic field and still obtain a relevant model for the solar chromosphere. By relevant, I mean an approximate, one-dimensional, theoretical model, based on a mechanical heating theory which ignores magnetic fields, and yet, which is in substantial agreement with one-dimensional models derived from observational data. If this proves true, it would have direct bearing on the theoretical treatment of chromospheres of non-solar, main sequence stars with convective envelopes. Difficult as it may be to devise ways of computing non-radiative equilibrium models for these stars with a relatively simple heating theory it would be extremely difficult to do it with the non-linear (and, possibly, multi-dimensional) aspects the problem would assume with strong magnetic fields.

To demonstrate this simplifying possibility, consider the dimensionless parameter

$$\frac{c_A}{c_s} = K \frac{B}{\sqrt{\rho T}}$$

where c_A and c_s are the Alfven and sound speeds, respectively, and B, ρ, and T are the magnetic field strength, mass density, and kinetic temperature. The quantity K is an almost constant function of the mean molecular weight and the specific heats ratio. From the wave equation for propagation in a medium with magnetic field, we can readily see that, when $c_A/c_s < 1$, the wave propagates more like an ordinary sound wave as the ratio becomes progressively smaller. In the language of Osterbrock's (1961) well known study, the fast hydromagnetic mode becomes the sound mode. But it is easy to substitute the appropriate numbers to see that this is exactly what happens in the solar low chromosphere and photosphere outside of plage and spicule regions, which comprise a small fraction of the total gas mass at these heights in the atmosphere. So, barring the possibility that the magnetic structure of the bulk of the gas is a small scale, unobservable, high-fields-of-opposing-polarity situation, it follows that, below and possibly within much of the transition region, the heating occurs mainly in regions of negligible magnetic field.

These remarks are meant only to show one way in which the magenetic field might be negligible in treating one part of the heating problem. As chromospheric densities drop rapidly with height, we soon enter a situation, somewhere in the transition region, where $c_A/c_S \geqslant 1$, even for a field of one gauss. Also, any treatment of heating in plages and spicules requires inclusion of magnetic field effects. Finally, the magnetic field will play some role, perhaps a vital one, in wave generation (cf. Kulsrud, 1955), again, where $c_A/c_S \geqslant 1$. So the current research on how to treat various hydromagnetic modes and their interactions with each other and the non-uniform propagation medium is very important and should certainly be pursued vigorously. On the other hand, the comparative insensitivity of the solar wind to the solar cycle (Hundhausen, 1968) suggests, though it does not prove, that at least the total amount of steady state mass and mechanical energy flux from the subphotospheric regions is constant and, thus, not strongly dependent on magnetic activity. Perhaps many (important) details of the steady state heating will prove to be strongly dependent on the magnetic field, while the total magnitude of the heating will not. These are major questions for which we currently lack answers.

Another wave mode that has been treated extensively as a possible heating mode is the gravity wave, the relatively low frequency, long wavelength, two dimensional wave characterized by elliptical (rather than longitudinal, as in the case of sound waves) particle motion in the vertical plane passing through the wave propagation vector. This mode represents one possible solution of the wave equation, leaving out the magnetic field, but including medium compressibility and gravity. Given a suitable perturbation, this mode is certainly present in the solar atmosphere wherever the radiative relaxation time is not too fast to suppress it. Whitaker (1963) injected the gravity wave into the solar heating problem because sound waves with (relatively low) frequencies characteristic of photospheric granules (Bahng and Schwarzschild, 1961) could not propagate through the temperature minimum region. This was before Stein showed that the frequencies for sound waves generated by the Lighthill mechanism lay much higher than the critical cut-off frequency $\gamma g/2c_S$. Thus, Whitaker's original motivation for proposing the gravity wave no longer exists.

This situation can be illustrated by the diagnostic diagram in Figure III-2. This diagnostic diagram is simply a plot of the dispersion relation $F(\omega, k_x) = 0$ for different vertical wave numbers k_z and a set of physical parameters characterizing the solar temperature minimum region. (Mean

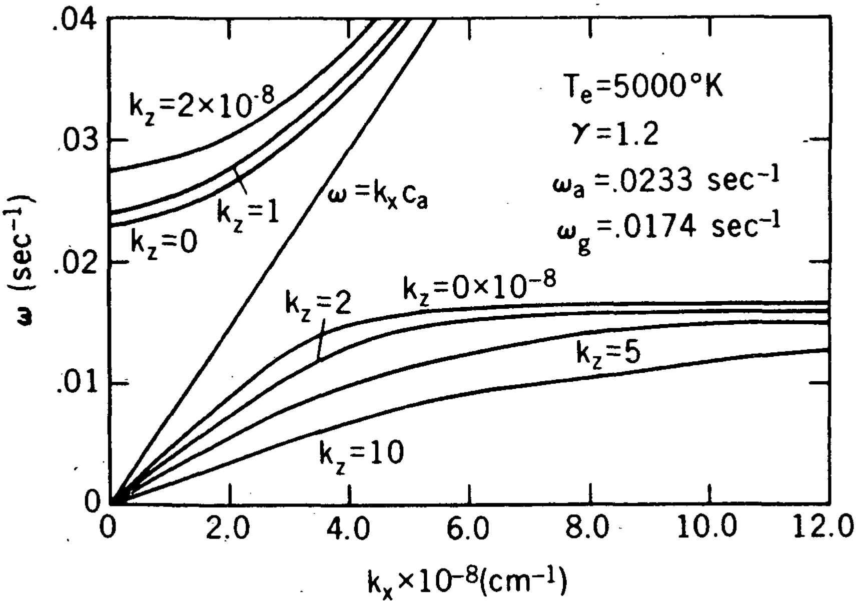

Figure III-2 Diagnostic Diagram for T_e (min) region.

molecular weight unity is also used.) The values given are those chosen by Whitaker, but, although T_e should be lower, it doesn't change the general picture. ω_g is the Väisälä-Brunt frequency above which vertical propagation of gravity waves cannot occur. It is given by $\omega_g = g(\gamma - 1)^{1/2}/c_s$. The straight line solution $\omega = k_x c_s$ is a pure sound wave in a zero gravity medium, that is, a horizontal sound wave in the Sun. The solutions in the upper left-hand corner represent the gravity modified sound waves which, as we see, cannot propagate vertically for $\omega < \omega_s = .0233$ sec^{-1}. Thus, for example, a 300 sec sound wave could not propagate up through this region. Of course, now we believe that 30 sec is a more representative period for the high frequency sound wave, and this latter period lies well below the limiting value for vertical propagation. The gravity waves, on the other hand, have dispersion relations more like those of the photospheric granulation with which Whitaker seems to have identified them. Hence, we see his preference for gravity waves. In addition to the fact that the gravity waves no longer seem necessary in the low photosphere, there is a more serious objection to associating them with this region. That is, as Souffrin (1966) pointed out, the rapid radiative relaxation time, of the order of one second, would quickly eliminate these oscillations in this region.

It would seem that gravity waves play no role in solar atmospheric heating and that the preceding discussion is somewhat irrelevant, but this is not necessarily the case. There is now convincing observational (Frazier, 1968) and theoretical (Moore, 1966) evidence that a significant convective flux penetrates above the rather artificial boundary separating the convection zone from the radiative equilibrium photosphere to heights where the radiative relaxation time has increased enough for the atmosphere to support gravity waves. Given a reasonably high efficiency for gravity wave generation (and this is predicted), it is still quite possible that the gravity wave flux might be as high as 10^6 ergs cm^{-2} sec^{-1}. Although no known dissipation mechanism makes these slow, low frequency waves a candidate for chromospheric heating, they must still be considered for coronal heating, where various 'frictional' and conductive processes may liberate the energy over a long path length, or where conversion to a different, hydromagnetic mode may occur. In addition, the possibility exists that the penetrative convection, in the presence of magnetic fields of 10 gauss or more in the low chromosphere, might give rise to torsional oscillations which propagate upward along magnetic lines of force, dissipating their energy by Joule heating of the atmosphere. Howe (1969) performed a linearized calculation and concluded that such a mechanism could account for spicules, although the conclusion is highly tentative and illustrates the difficulty of treating problems where medium compressibility, gravity, and magnetic field may all play a role.

It is safe to say that, while Whitaker's original ideas on gravity waves in the Sun have not stood up, the gravity mode and other modes generated by penetrative convection in the upper photosphere and low chromosphere are probably present, and that they may play an important role in heating both the corona and chromospheric, particularly in regions of magnetic field strength exceeding 10 gauss.

A discussion of waves in the chromosphere would be utterly incomplete without a consideration of the 300 sec velocity field oscillations which have actually been directly observed, in contrast to the high frequency sound waves, hydromagnetic modes, and gravity waves for which the evidence is, at best, more indirect. Ever since their chief characteristic features were first described by Leighton, Noyes, and Simon (1962), the question has been raised as to what role these oscillations might play in heating the outer atmosphere. Frazier (1968) obtained power spectra for both velocity and intensity fluctuations in three lines spanning the photosphere from the top of the convection zone to the temperature minimum, with sufficient resolution and observing time to break up the 300 sec oscillation into two, long duration, constant period velocity fluctuations of 265 sec and 345 sec. Furthermore, the amplitude ratio of the short to the long period oscillation was found to grow with height. In addition, a strong, low frequency, convective component of the velocity field was found to persist right up to the temperature minimum. Finally, the duration of the velocity fluctuations suggested little or no correlation with the granulation. The implications of these and other observations analyzed during the past few years have stimulated a new round of theoretical activity which we are still experiencing right now.

It was immediately recognized that the granulation, which is our observational evidence for the turbulence which we believe generates the relatively high frequency acoustic spectrum studied by Stein, is in no direct way connected with the 300 sec oscillation, in contrast to the earlier notion that granule "pistons" might be driving them. Also, the observational evidence for penetrative convection at the temperature minimum kept alive the possibility that gravity waves might play a role in atmospheric heating, as already mentioned.

The most significant development to follow Frazier's work, however, in my opinion, is the two studies by Ulrich (1970) and Leibacher (1971), in which what seems to be a plausible mechanism for the 300 sec oscillations is discussed, and where the resulting eigenmodes are followed through much of the photosphere and chromosphere, where they begin to lose their energy rapidly through non-linear (shock) dissipation.

Ulrich's work concentrates on the generation of the oscillations; Leibacher's, on the propagation and dissipation. Both agree that the observed oscillations in the photosphere cannot be standing waves in the sense of running waves constructively interfering as they move back and forth between reflecting boundaries. The critical frequency for sound wave propagation is too high in this region, as we have already noted. In the absence of a forced, but decaying, oscillation pumped by the granulation, what are we really observing in the photosphere? Ulrich may have supplied the answer by recalling that small pertubations can lead to overstable oscillations in the presence of a superadiabatic temperature gradient in the presence of radiative cooling, a condition which is described by Moore and Spiegel (1966) and applies to the top of the solar hydrogen convection zone. Given this situation, Ulrich noted that the upper convection zone could trap standing acoustic waves, which would then drive the photosphere at the appropriate eigenfrequencies determined by the boundaries of the resonant cavity below. Although the waves could not propagate as running waves into the "forbidden" region around the temperature minimum, it is easy to show that the decay distance for the energy density $1/2\ \rho v^2$ (v = material velocity) is quite long there. (The notion of reflection at the boundary follows from ray acoustics and is highly approximate here, as the ratio of the very long, > 1000 km, wave length to scale height is quite large.) Detailed calculations show that attenuation is not too rapid. Indeed, the velocity amplitude actually increases with height in the atmosphere, so small is the density scale height.

The reason for the trapping follows readily from a cursory examination of the dispersion relationship for waves in a compressional atmosphere with gravity (again zero magnetic field for simplicity). It is necessary to apply this relationship, which follows, to a non-isothermal atmosphere such as the top part of the convection zone. The dispersion relationship is

$$k_z^2 = \frac{\omega^2 - \omega_s^2}{c_s^2} - k_x^2\left(1 - \frac{\omega_g^2}{\omega^2}\right) \tag{1}$$

where all the quantities were defined in discussing Whitaker's work, except here,

$$\omega_g^2 = g\left(\frac{\gamma - 1}{c_s^2}\, g + \frac{1}{T}\frac{dT}{dz}\right)$$

should be used for the Vaisala-Brunt frequency in this non-thermal situation (cf. Kuperus, 1965). The lower boundary occurs where the inwardly increasing temperature decreases the first term on the right hand side of equation (1) so that, for a given finite (non-zero) value for the horizontal wave number k_x, it becomes equal to the second term, which will be of opposite sign for $\omega_g < \omega < \omega_s$, the frequency range in which the observed oscillations lie. Thus, $k_z = 0$ results, defining a lower reflecting boundary. The upper boundary occurs where the two terms again cancel, this time because, for a given ω, the outwardly decreasing temperature causes a correspondingly increasing ω_s to approach ω in value. The result is a resonant cavity for eigenmode (ω, k_x), given a model for the upper convection zone and photosphere.

To actually obtain eigensolutions, one must, of course, solve the appropriate wave equation with boundary conditions which depend on the eigensolutions (ω, k_x). Ulrich obtains a simple, workable, lower boundary condition from equation (1), by noting that $\omega_g \to 0$ as one goes into the convection zone. Then he determines the upper boundary by finding the mode which has the smallest velocity amplitude above the temperature minimum, on the grounds that this mode should be distorted least by shock formation in the upper atmosphere and, thus, provide the most reliable boundary matching. His eigensolutions include a fundamental mode and first-overtone mode which pass through the peaks of Frazier's published power spectra. To establish that these oscillations are overstable, Ulrich is forced, by his method of handling the outer boundary condition, to consider the energy balance. When he does this, he finds that the fundamental mode and first two or three overtone modes are overstable. In addition, he estimates the outward energy flux in these oscillations is greater than 10^6 ergs cm^{-2} sec^{-1}, or roughly in agreement with estimated net radiative losses from the outer atmosphere reported by Athay (1966). Although I would take issue with his speculations as to what happens to the waves as they heat the outer atmosphere (conversion to heat through some hydromagnetic interaction), it seems to me that Ulrich has come closer than anyone, to date, to providing insight into the *origin* of the 300 sec oscillations. In addition, he concludes his article by outlining the kind of observations necessary to further check some of these ideas.

Leibacher, on the other hand, while concluding independently that the mechanism of subphotospheric standing waves is responsible for the observed photospheric oscillations, concentrates on the properties of the observed "evanescent" oscillations themselves. He shows how the evanescent waves become propagating waves once more, due to the chromospheric temperature rise, and calculates the atmospheric heating through non-linear dissipation. Further results which I'll mention in a more detailed treatment of the heating make this seem very plausible. That is, there is good reason to believe that 300 sec progressive waves will develop very quickly into strong shocks, so that complicated hydromagnetic interactions are unnecessary. Therefore, these interactions, mentioned by Ulrich would seem less likely to be important in heating the upper chromosphere or transition region, at least, outside of plages and spicules. The position of the evanescent waves in an isothermal temperature trough is shown on the diagnostic diagram of Figure III-3, which appears in Leibacher's thesis. We see immediately that their range of (ω, k_x), which corresponds to observed values, is quite incompatible with propagating acoustic or gravity waves. They are on the other hand, completely compatible with the picture provided by the more recent work.

This concludes what I want to say about the 300 sec oscillations. There isn't time to review past theoretical efforts to understand them. Most of these efforts have run into serious objections, often as refined observations clarify what the Sun is doing. An earlier effort by Moore and

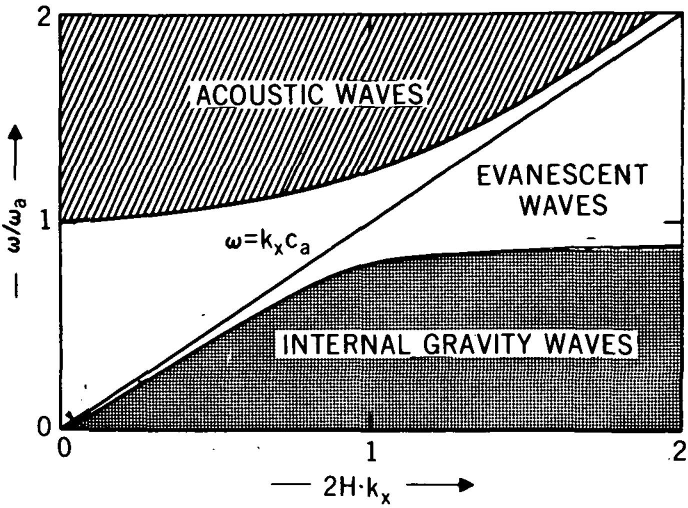

Figure III-3 Evanescent waves on solar diagnostic diagram.

Spiegle (1964) suggested the evanescent wave interpretation, which now seems promising, without offering the explanation of underlying standing waves. Time and better observations, particularly of phase relations in two dimensions, will permit us to check the more recent work of Ulrich and Leibacher.

SOLAR ATMOSPHERIC HEATING

Keeping all these remarks on wave modes in mind, I'd like to turn to the heating question. Since most of the quantitative work on this question has been restricted to the chromosphere, it is useful to start there and work up.

The earliest idea, already discussed, was that sound waves generated by turbulence at the top of the convection zone would build up into shock waves, as they propagate out into the sharp negative density gradient, and rapidly give up their energy, thus producing the abrupt transition to coronal temperatures and heating the corona itself. Recent detailed work (cf. Ulmschneider, 1970, 1971 a,b), using the theoretical acoustic spectra of Stein – Figure III-1 again – has modified the original picture in several ways.

By following the growth of the sound waves from their point of generation up through the photosphere and low chromosphere of a typical solar atmospheric model, Ulmschneider has shown that a fully developed shock wave (crest of an initially sinusoidal wave has caught up with the trough) develops after the wave has traversed a few scale heights, i.e., several hundred kilometers. This particular conclusion is in substantial agreement with several earlier studies. The result is important in insuring that significant shock heating will occur around or slightly above the temperature minimum, where, as we shall see, some mechanical heating appears to be necessary. A departure from the original picture occurs, however, when Ulmschneider solves the weak shock propagation equation for these waves. He shows that, for the relatively high frequencies of the Stein acoustic spectra (typically 30 sec period), the shock Mach number remains small enough in the low chromosphere to preserve the validity of the theory; and this permits estimates of the local mechanical heating to be made by using it. He then calculates the heating in this way, and finds good agreement between the heating and the local net radiative losses due to H^-, which are computed using the same model. This is illustrated in Figure III-4. Earlier studies either ignored the situation in the low

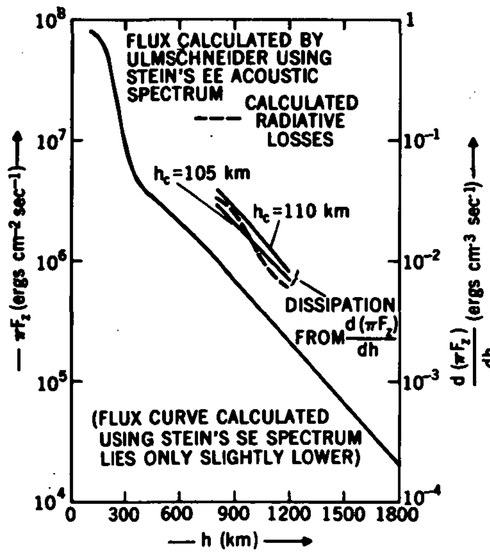

Figure III-4 Mechanical flux and dissipation in chromosphere

chromosphere or treated it very approximately. Furthermore, the earlier notion that the waves generated by the turbulent convection are responsible for the chromosphere-corona transition and the high coronal temperature now seems wrong. It is the low chromosphere, alone, below the sharp upward temperature transition, where these waves seem to be effective. Higher up, we appear to need the 300 sec progressive waves and, possibly, other modes.

The importance of Ulmschneider's results can best be seen, I feel, if we keep two things in mind. First, it is useful to recall that, if the low solar chromosphere does require mechanical heating, as now seems well established (Athay 1970), the net radiative losses from this region of almost negligible extent (compared to, say, the corona) are probably equal to the sum of all the other net radiative losses from all other sources in the entire outer atmosphere beyond the temperature minimum. This is due, of course, to the relatively high densities in the chromosphere compared to the corona, notwithstanding the much higher coronal temperature. This observation, though reported often, does not seem to have made much impression on some astronomers who talk about the heating problem as if coronal heating were the sum of it. Obviously, a region, however small, is fundamentally important if (1) much of the heating must, ultimately, occur there, and if (2) the waves responsible for heating all the higher regions must pass through it. Incidentally, this problem of energy balance in the chromosphere is a principle reason for energetic efforts to determine, from observations, the optical depth, breadth, and value of the minimum temperature. These efforts, which sometimes involve considerable expense–for high altitude infrared observations, for example–are certainly worthwhile.

Consequently, Ulmschneider's rather satisfactory treatment of the low chromosphere has importance in its own right. Looking ahead, it keeps alive the hope, already mentioned, that a relatively simple heating theory may be applicable to building one-dimensional non-radiative equilibrium atmospheric models for a large class of late type stars with convective envelopes.

This brings us to the upper chromosphere and/or the transition region.* What causes it? This is certainly still an unanswered question, but recent work on shock theory offers one interesting possibility in the magnetic field free regions. Several recent calculations show that the relatively low frequency waves associated with 300 sec oscillations will develop into strong shocks in the upper chromosphere, and the sudden release of a large burst of energy in this way could cause the transition to coronal temperatures, if the atmosphere cannot lose the energy over a shocking cycle under chromospheric conditions (Jordan, 1970). This mechanism raises as many questions as it attempts to answer and says nothing about the complex spicule phenomenon, but it has the merit of simplicity and, recently, some additional support, both from the theoretical picture of the 300 sec oscillations and how they develop when they become progressive waves, as well as from some recent observations from the

*I'll use these two terms interchangably. Usage varies.

OSO-7 satellite (Chapman et al., 1972). These satellite data give evidence for periodic changes in upper transition region conditions, as inferred from approximately 300 sec periodic changes in intensities of lines from He II, Mg VIII, and Mg IX. These changes could be caused by periodic temperature fluctuations due to strong shock waves passing through this region, consistent with Leibacher's theoretical calculations.

One of the serious problems that the strong shock hypothesis runs into is refraction and, to a somewhat lesser extent, reflection from the sharp temperature rise. These effects could reduce the outward flux in these waves below the value required to balance energy losses in the corona. Even more to the point here, the sharp temperature rise implies a strong conductive flux from the corona back down into the chromosphere. All of these processes will be further complicated where there are magnetic fields.

These complications do not preclude shock heating in the transition region, but they do show that the total heating picture is probably much more involved. In particular, until we have a reliable observationally determined temperature model of the transition region, it will be difficult to determine the conductive flux at various points and, hence, the conductive heating. One real hope for progress soon is that planned high resolution satellite spectra in transition region lines will provide us a sufficiently good model to permit the shock heating and conductive heating calculations to be made there. Then we can not only discriminate better among various possible transition region heating modes, but also determine better what waves can continue on into the corona.

One summary picture of solar chromospheric heating, consistent with the work reported and restricted to that great bulk of gas for which the magnetic field is negligible ($\lesssim$ 10 gauss), might appear as follows: Sound waves are generated by turbulent convection in the low photosphere and, thanks to their comparatively high frequencies, they pass through the temperature trough and develop quickly into weak shock waves. As such, they deliver their energy to the low chromosphere, balancing the net radiative losses in H^- and a number of medium to strong spectral lines, and then pass into the transition region where their behavior is less well known, but their residual energy flux, and hence their effect, is small, perhaps negligible. On the other hand, the 300 sec periodic oscillations in the temperature trough have been transformed, by the outward rise in low chromospheric temperature, from non-propagating, evanescent waves into progressive sound waves and develop quickly into strong shocks, capable of producing a rapid temperature rise by heating the gas (ionizing hydrogen) beyond its capacity to remain thermally stable at low chromospheric temperatures. A significant conductive flux back-down will result from this rapid temperature rise, and the heating associated with this flux will, along with the strong shock heating and the radiative cooling, determine the final temperature structure and energy balance of the transition region.

Since this summary picture is necessarily tentative, it might be useful to mention several critiques of the above ideas. I'll then indicate why the above picture still seems the most compelling to me.

First, we cannot discount completely the possibility that the temperature rise in the low chromosphere is produced in radiative equilibrium, eliminating the need for mechanical heating there (Cayrel, 1963). Some of us, including myself, felt that this idea was fundamentally incompatible with the non-LTE situation in the H^- continuum, but this proved to be wrong, due to the non-coherence of the continuum scattering (Skumanich, 1970). Thus, it was evident that only detailed calculations could settle this issue. In particular, given a reasonable density distribution for the chromosphere, and the effects of line blanketing on the temperature there, *the question becomes: will a radiative equilibrium, blanketed model* exhibit temperatures as high as those obtained from current observationally determined models. Athay, (1970) did this calculation and concluded that, although no mechanical heating would be needed to produce a temperature minimum of 4400° K at τ_5 (normal optical depth at 5000 Å) = 10^{-4}, mechanical energy would be required above this point. This agrees with a calculation I have done, using Athay's blanketing functions and a formulation of the problem similar to Gebbie and Thomas (1970). At this stage, it appears that the cooling due to line blanketing above the temperature minimum more than offsets the tendency of the non-LTE Cayrel mechanism to increase the temperature. Consequently, mechanical heating will be necessary to produce a temperature rise in the low solar chromosphere.

I might mention here a subject I am not competent to evaluate, but one which is very important. This is the possibility of radiative equilibrium temperature rises in early type stars, discussed briefly in Mihalas (1970) and, *in greater detail, in a series of papers by* Mihalas and Auer which appeared in the Astrophysical Journal over the late 1960's. If this rise occurs in radiative equilibrium, up to the color temperature of the background continuum (otherwise, the second law of thermodynamics is violated), this could reduce the requirements for mechanical heating significantly. Finding a source of mechanical energy is a serious problem for these hot, early type stars, as they have radiative, not convective, subphotospheric envelopes.

Another possibility for the solar chromosphere, advanced by one of the participants, is the suggestion by Ulrich (1972) that radiative dissipation of sound waves might produce the temperature rise. Ulrich questions the shock hypothesis on the grounds that evidence for the waves is lacking, but it is not obvious that we have taken the observations or properly analyzed the data to confirm or rule out the shocks. Quite to the contrary, this is the object of several current research programs. It is probably premature to judge the radiative damping mechanism, which depends strongly on such parameters as wave frequency, radiative relaxation time (hence, non-LTE effects), and material velocity in the chromosphere. Nevertheless, given the sharp negative density gradients in the low chromosphere, and considering the granulation evidence for a turbulent region in which the necessary high frequency sound waves can be generated, not to mention the results of weak shock calculations, it would seem that the shock heating mechanism still offers the most natural way to heat the low chromosphere.

Subject to these alternate possibilities, the shock heating picture looks very promising. In view of this, it might be worthwhile pointing out what form of the weak shock theory is valid for chromospheric calculations, where the Mach number does not greatly exceed unity. Some conflicting results have appeared in the literature, and it is now clear how this conflict arose.

Osterbrock (1961) is the first person to publish an application of what we call weak shock theory to the chromospheric heating problem, to estimate mechanical heating as a function of height for a given temperature-density model. As we have seen, his conclusion that weak shocks probably heat the low chromosphere seems as likely today as it did then. On the other hand, much else has changed, and it is somewhat ironical that this original conclusion still stands. First, current chromospheric models have a much smaller density scale height than the van de Hulst (1953) model used by Osterbrock. Second, we now believe that wave periods around 30 sec are more apt to characterize the turbulence generated sound that the 100-300 sec range used prior to Stein's (1968) work. Third, it is easy to show that, for these short period waves in the chromosphere, the approximation used by Osterbrock to evaluate the mechanical flux integral leads to serious over-estimates in computing the growth of the shock strength and the dissipation.

Ulmschneider, in the studies referenced earlier, has performed the evaluation correctly, provided the shock is truly weak. Such a weak shock is represented by a P(t) curve calculated by Schwartz and Stein (1972) for an initially sinusoidal disturbance of period 100 sec under low chromosphere conditions. The P(t) relation behind the shock front is almost linear. This linear relation is equivalent to assuming that the relaxation phase of the wave's passage can be represented by a simple wave in a perfect gas (cf. Landau and Litshitz, 1959, p. 367). This is not unreasonable if the entropy change during the relaxation phase is not too abrupt (in marked contrast to the initial "shocking" phase). So by assuming a linear P(t) relation over a shocking cycle, one can evaluate analytically the mechanical flux integral

$$\pi F_+ \text{ (mech)} = \frac{1}{T} \int_0^T (P(t) - P_o)\, u(t)\, dt, \tag{2}$$

where P_0 is P(t=0) and T (here) is the period, for a given, simple rest frame velocity u(t), usually chosen to be a sawtooth N-wave. In fact, it can be shown that the result of integration is almost independent of the ratio of the velocity relaxation time to the period, as long as this ratio does not become much smaller than 1/3. Using the resulting expression for πF_+ (mech) in the shock propagation equation, it can be solved for a given atmospheric model. This is what Ulmschneider did. His results confirm Osterbrock's original conjecture, but only because the tendency of new, small scale height models to cause explosive growth of the shock is offset by the shorter period and less approximate method for evaluating the mechanical flux integral. We have come full circle in a decade.

The work of Schwartz and Stein, just mentioned, and its antecedent (Stein and Schwartz, 1972) bear directly on this question of ranges of validity for the weak shock theory. They show that, as expected, for a relatively short period wave (100 sec vs. 400 sec), where weak shock theory begins to become applicable, a careful treatment of the growth of the initially sinusoidal disturbance is necessary to prevent an overestimate of the heating low in the atmosphere, and the weak shock theory will seriously underestimate the heating as the Mach number approaches 2. Fortunately, Ulmschneider's calculations exhibit a lower Mach number throughout the low chromosphere.

It seems that periods of around 100 sec (corresponding to roughly twice the acoustic cutoff frequency ω_a) represent the upper limit for a weak shock treatment of chromospheric waves. Figure III-5 shows the results of a calculation I did, using the Harvard Smithsonian Reference Atmosphere (Gingerich, Noyes, and Kalkofen, 1971) and solving the shock propagation equation exactly as Ulmschneider did. We see that, for a 30 sec shock, the shock strength parameter η remains almost constant with height as Ulmschneider concluded. For a 95 sec shock (the velocity relaxation time τ_0 differs by a negligible amount here – it was varied during the calculation), η grows rapidly with height and eventually exceeds the range for validity of the weak shock theory, thus yielding spurious values for the

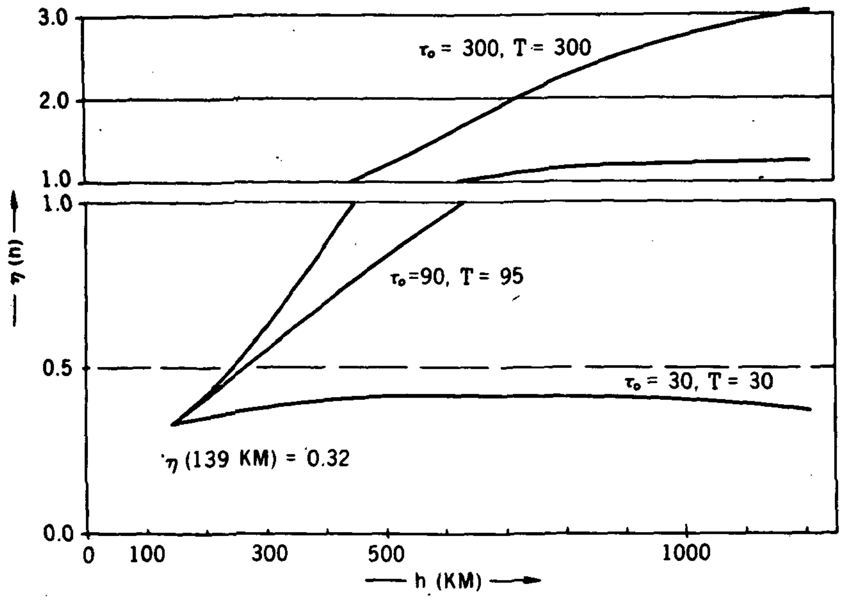

Figure III-5 Shock strength parameter η(h) vs. H for HSRA model and different periods τ (sec).

dissipation, as noted by Schwartz and Stein. Finally, for a 300 sec shock, the wave, once assumed to be fully developed, grows explosively, and cannot be treated by the weak shock method, consistent with the previous work of all of us.

This concludes a survey of the situation in the chromosphere, including the transition region, and brings us into the solar corona. What heats the corona? We don't know. It's even hard to make an educated guess, because there are problems with all the wave modes proposed.

The Alfven mode is the favorite candidate of a number of authors, for several reasons. First, one important effect of a magnetic field will be to couple the different wave modes in the chromosphere, leading to a transfer of energy from the fast mode (which, you will recall, is just a sound wave in a zero magnetic field) into the Alfven mode, in regions where the Alfven speed exceeds the sound speed. Since the Alfven speed is given by $c_A = B/\sqrt{4\pi\rho}$, and since density drops off faster than temperature increases up to the transition region (or, more to the point, $c_A \uparrow$ faster than $c_s \uparrow$ as h $\uparrow$), we see that this situation will exist everywhere in the chromosphere where $B \gtrsim 10$ gauss. The Alfven mode has the right propagation properties for coronal heating too; namely, it can penetrate to the corona without appreciable dissipation. This is largely due to the non-compressible feature of the Alfven wave, which will follow magnetic field lines up into the corona. The problem is that no one, to my knowledge, has offered a satisfactory dissipation mechanism for these waves in coronal gas, whose low densities appear to make the various collisional mechanisms inefficient.

A similar problem exists for the gravity mode, which Frazier's (1968) observations suggest should be present due to the presence of penetrative convection near the temperature minimum. Again, how is the energy dissipated in the low density corona? The long wavelength and low frequency gravity wave does not lend itself to shock dissipation there, and and linear dissipation processes appear too inefficient.

One useful bit of information bearing on this problem would be to determine, once and for all, if the quiet solar corona, observed at sunspot minimum, is a phenomenon of *only* regions of significant magnetic field strength, with material at essentially interplanetary densities between the magnetic regions (cf. Billings, 1966, Chapter 3.). If this proves true, it would restrict our search to waves and heating mechanisms effective in these regions. In particular, it would favor the Alfven wave hypothesis, or, perhaps, the one proposed by Howe (1969) and mentioned earlier over heating by ordinary gravity waves.

A popular hypothesis over the years has been that the progressive waves generated near the top of the convection zone heat the corona by shock dissipation. This raises just the opposite problem from the Alfven and gravity modes. Dissipation by shocking could heat the gas, but getting these progressive waves into the corona with an adequate energy flux looks difficult. The high frequency sound waves which are likely to heat the low chromosphere dissipate practically all of their energy there, according to all our recent calculations, which are of course, model dependent. The 300 sec waves may carry sufficient energy to the base of the transition region, but refraction and reflection off the sharp temperature rise probably reduce this flux several orders of magnitude, so, while these waves can easily heat the transition region right up to the 10^6 °K corona, they may not have sufficient vertical flux to balance the various coronal losses. Again, this conclusion is model dependent, and could change as we get better models for the transition region.

CONCLUSION

It should be evident from these remarks that one of the crucial theoretical problems is the behavior of a system of waves under chromospheric conditions in the presence of a magnetic field. How do they interact with the medium and with each other? What new modes appear as a result of this interaction? Frisch (1964) has addressed himself to this problem, which involves some unpleasant non-linearities, and finds that with a WKB approximation the rotation of the magnetic field couples the modes. Stein and Uchida, among others, are working on the problem now, and many of us await their results eagerly.

I'll close this survey of solar atmospheric heating on the optimistic note that, thanks to the high spacial resolution possible on currently flying and planned future solar satellites, coupled with good time and spectral resolution, we can confidently expect to learn much more about oscillatory velocity fields and general chromospheric and coronal structure in the 1970's. The two pointed experiments on OSO-I, scheduled for an early 1974 launch, will obtain simultaneous spectra in a large number of uv lines, with spacial resolution approaching 1 arc sec, time resolution of 10 sec, and spectral resolution of .05 Å or better. This will permit us to do many things, like testing the chromosphere for the presence of high frequency waves in the region where the core of the strong MgII resonance doublet is formed. This is the very region where we expect strong dissipation from these waves.

For those of you interested mainly in non-solar stars, I hope this review has demonstrated two things: (1) The shock dissipation hypothesis still seems the most attractive for the Sun, outside of, possibly, the corona. (2) Nevertheless, there are still other candidates for the heating, so great caution must be exercised in treating chromospheric/coronal heating of non-solar stars with strong convective envelopes by some shock dissipation theory.

Several efforts have been made to treat late-type stellar atmospheres in this spirit over the past decade. In this afternoon's discussion, I'll attempt a critique of one of the latest and most comprehensive of these studies.

REFERENCES

Athay, R.G. 1966, *Ap. J.*, **146**, 223.
Athay, R.G. 1970, *Ap. J.*, **161**, 713.
Bahng, J., and Schwarzschild, M. 1961, *Ap.J.*, **134** 312.
Biermann, L. 1946, *Naturwiss.*, **33**, 118.
Billings, D.E. 1966, "A Guide to the Solar Corona," New York: Academic Press.
Bohm-Vitense, E. 1958, *Z. Astrophys.*, **46**, 108.
Cayrel, R. 1963, *Comp. Rend. Acad. Sc. Paris*, **257**, 3309.
Chapman, R.D., Jordan, S.J., Neupert, W.M., and Thomas, R.J. 1972, *Ap. J.*, **174**, L97.
Ferraro, V.C.A., and Plumpton, C. 1958, *Ap. J.*, **127**, 459.
Frazier, E.N. 1968, *Ap. J.*, **152**, 557.
Frisch, U. 1964, *Ann.d' Ap.*, **27**, 224.

Gebbie, K.B., and Thomas, R.N. 1970, **161**, 229.
Gingerich, O., Noyes, R.W., and Kalkofen, W. 1971, *Solar Physics,* **18**, 347.
Howe, M.S. 1969, *Ap. J.,* **156**, 27.
Hulst, H.C. van de, 1953, chapt. 5 in "The Sun," ed. G.P. Kuiper, Chicago: Univ. of Chicago Press.
Hundhausen, A.J. 1968, *Space Science Rev.,* **8**, 690.
Jordan, S.D. 1970, *Ap. J.,* **161**, 1189.
Kulsrud, R.M. 1955, *Ap. J.,* **121**, 461.
Kuperus, M. 1965, "The Transfer of Mechanical Energy in the Sun and the Heating of the Corona," Dordrecht, Holland: Reidel.
Landau, L.D., and Lifshitz, E.M. 1959, "Fluid Mechanics," London: Pergamon Press.
Leibacher, J. 1971, Thesis, Harvard University
Leighton, R.B., Noyes, R.W., and Simon, G.W. 1962, *Ap. J.,* **135**, 474.
Lighthill, M.J. 1952, *Proc. Roy. Soc. London,* **A211**, 564.
Mihalas, D. 1970, "Stellar Atmospheres," San Francisco: Freeman.
Moore, D.E., and Spiegel, E.A. 1964, *Ap. J.,* **139**, 48.
. 1966, *ibid,* **143**, 871.
Moore, D.E. 1967, part II C in "Fifth Symposium on Cosmical Gas Dynamics" ed. R.N. Thomas, London: Academic Press.
Osterbrock, D.E. 1961, *Ap. J.,* **134**, 347.
Pikel'ner, S.B., and Livshitz, M.A. 1965, *Soviet Astronomy,* 8, 808.
Schwartz, R.A. and Stein, R.F. 1972, to be published.
Schwarzschild, M. 1948, *Ap. J.,* **107**, 1.
Skumanich, A. 1970, *Ap. J.,* **159**, 1077.
Souffrin, P. 1966, *Ann. d'Ap.,* **29**, 55.
Stein, R.F. 1968, *Ap. J.,* **154**, 297.
Stein, R.F., and Schwartz, R.A. 1972, to be published.
Ulmschneider, P. 1970, *Solar Physics,* **12**, 403.
. 1971a, *Astron. & Ap.,* 12, 297.
. 1971b, *ibid,* **14**, 275.
Ulrich, R. 1970, *Ap. J.,* **162**, 993.
. 1972, to be published.
Whitaker, W.A. 1963, *Ap. J.,* **137**, 914.

DISCUSSION FOLLOWING THE INTRODUCTORY TALK BY JORDAN

Skumanich – I would like to ask a question about the zeroth order atmosphere for which you are doing the calculation of this heating. Do you start with the models that we radiative transfer types give you?

Jordan – Yes. The calculations in my talk were done for a number of models including a current version of the Harvard-Smithsonian Reference