THEORETICAL UNDERSTANDING OF CHROMOSPHERIC INHOMOGENEITIES

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"By assuming that the atmosphere is homogeneous at each depth, we are immeasurably adding to the numerical tractability of the problem at the expense of ignoring 80 years' worth of data on chromospheric inhomogeneities"

LINSKY and AVRETT (1970)

INTRODUCTION

To the spatial inhomogeneity, Linsky and Avrett could have added the variations with time which are also well known, well observed characteristics of the solar chromosphere. Let me quote also Praderie (1969): large asymmetries are observed in stellar K_2 components which vary with time, "so that it seems difficult to think of any interpretation of the K line profile that would ignore motions and inhomogeneities in the atmosphere of those stars". And let me borrow a conclusion from Thomas (1969): "So what we need are ingenious ideas for empirical inference; or theoretical generalization from experience with the solar case". I wonder if the solar experience is sufficient at the present time to permit any theoretical generalization, as has been the case for the solar wind. In order to simplify, I shall restrict the scope of this contribution to the quiet solar chromosphere, and focus only on spicules. It is quite possible that, in ignoring plages and active phenomena, we miss an important clue to the understanding of inhomogeneous structures. But we also have to "add to the tractability of the problem".

Now, one *basic observed property* of the solar chromosphere is undoubtedly its inhomogeneous structure; at the present time, the *basic physical property* seems to be the mechanical energy deposited. So a first question could be: how fundamental is the relation between mechanical energy deposition and inhomogeneities? The answer is not clear, since the way is very long which has to go from the origin of mechanical energy, it's transport (or propagation), it's deposition, it's effect on the state parameters, on the macroscopic structures, and then the prediction of escaping radiation, which is what we observe. We must not forget that we have at our disposal numerous studies where the inhomogeneous structure is an essential starting point (or conclusion) together with completely homogeneous theories, some of which are successful. Our question could then be replaced by the following: in neglecting the temporal and spatial factors, do we loose a significant amount of the physics? and how complicated would it be to include the $(t, \vec{r})$ parameters in the existing theories?

The chromosphere-corona transition region should obviously be included in our study, since its structure is continuously connected to the chromosphere. This continuity, essentially with respect to mass flow, has been stressed by Zirker (1971).

In the following, we shall start from the observations. As we shall see, it has been possible to infer from them some empirical models, in which, very often, a great many theoretical considerations are embedded; generally, the transfer problems are partially solved, whereas the dynamical equations are not considered. I shall call this type of approach "descriptive theories".

Then we shall consider the mechanisms of some dynamical models that have been proposed to explain the machinery which is responsible for inhomogeneities.

After having stressed that, with little effort, we have at our disposal some simple tools for studying inhomogeneities, I shall give a brief account of a recent work in which the inhomogeneous structure of the chromosphere-corona transition region shows up very simply, from dynamical considerations applied to observations averaged over the whole disk of the Sun.

OBSERVATIONS – DESCRIPTIVE THEORIES

Spicules can be seen on the limb, and also on the disk, even if there still exists some disagreement on the correct detailed identification. They form families (brushes, coarse mottles) lying at the boundary of the supergranulation cells, where the magnetic field is known to be relatively strong. Most of the available information on spicules can be found in the very extensive survey made by Beckers (1968). More recent observations, essentially pertaining to the H and K problem, have been made with high resolution (spatial, temporal, spectral); for example by Bappu and Sivaraman (1971) who propose that the boundary of the supergranulation should obey the Wilson-Bappu relationship. It is possible to construct simple models for individual spicules and for the chromospheric background (sometimes called "interspicular" matter). As Zirin and Dietz (1963) mentioned, this kind of descriptive model may account for the observations, but generally it does not answer the fundamental questions: what is the heating mechanism, and what makes spicules? Recently Krat and Krat (1971) deduced that the classical model of a rotating spicule made of a Ca II core with a Helium envelope is still adequate for the interpretation of their high spatial resolution observations in Hα, Hβ, D_3, H and K. The question of the dynamical state of such a structure is avoided in saying simply that it is compatible with the model of Kuperus and Athay (1967). Going to the chromosphere-corona transition region, Withbroe (1971) also gave a crude description of a spicular structure that is needed to explain center-to-limb XUV observations. Beckers (1968) also gave a descriptive model of a two component chromosphere, and very carefully made warnings on the validity of such an approach. First, he obtains a pressure inversion in the interstellar region. (Note that Delache (1969) has given a possible interpretation in terms of momentum transported by the heating waves.) Second, he questions the validity of a statistically steady state; as an example, the recombination time for a proton and an electron (T_e = 15 000°, n_e = 10^{11} cm^{-3}) to the first and second level is 1.0 or 2.5 min respectively. Similarly the quasi-static behaviour of the radiation field could also be questioned. The random walk of a photon in an optically thick spicule can take a long time! Preliminary work shows that the process can be described in a diffusion approximation (Delache, Froeschle, 1972; Le Guet, 1972).

Since, clearly, one cannot avoid going to the dynamical models, let me list some observational requirements, as given by Beckers (1968):

- A spicule moves up (≅ 25 km s^{-1}), slows down, and approaches a standstill; "it is likely that it returns to the photosphere after it becomes invisible".
- At two different heights, the accelerations are practically simultaneous: the accelerating force propagates with velocity $v > 500$ km sec^{-1}.
- Spicules appear in the magnetic regions which outline the solar supergranulation (B≈ 25-50 gauss).
- Spicule diameters, birth rates, and lifetimes are similar to that of the granulation.
- Temperature T_e is nearly constant above 2000 km.
- Before the death of a spicule, its diameter increases.
- Left and right hand sides of a spicule are different, possibly indicating a rotation.

SOME DYNAMICAL MODELS

The first step in trying to put another kind of physics, besides just radiative transfer, into what I have called descriptive theories is, of course, to look at the energy problem. Thus, the various dynamical theories differ essentially in the heat supply. If mechanical energy is deposited in an inhomogeneous, time dependent pattern, this can be due to either (or both) of two reasons:

- The amount of energy available for absorption depends on $\underset{\sim}{r}$ and t.
- The process by which the energy is absorbed depends on $\underset{\sim}{r}$, t.

In both cases, the currently accepted heating mechanisms can be responsible for the spicular structure; some of them have been studied in the homogeneous case, like shock wave dissipation, or heat conduction, together with the departure from radiative equilibrium. A recent review by Frisch (1972) describes the results obtained in coupling the heating mechanism with the radiation field in a stratified atmosphere. This kind of mechanical energy may, or may not, be available in an inhomogeneous pattern. For example, Kuperus and Athay (1967) propose that spicules be driven by the conductive heat flux. The latter is inhomogeneous from the very beginning due to the magnetic structure of the transition regions. On the contrary, Defouw (1970) describes a local instability sensitive to the magnetic field, which borrows the energy from a constant homogeneous source.

Other types of energy sources have been described which are basically inhomogeneous, as the kinetic energy of horizontal motion in the supergranulation, or the Petschek mechanism of magnetic line reconnection, as proposed in a qualitative manner by Pikel'ner (1971). As there is no reason why the starting inhomogeneities would be similar to one another, it is hard to see why the resulting spicules are so alike. However, the role of local parameters in fixing the $\underset{\sim}{r}$, t properties of the dissipation are not excluded, and again, it seems worthwhile to study in some detail the "local machinery" that may lead to a relaxation, or unstable situation.

For Kuperus and Athay (1967), as we have said, the heat conducted backward from the corona in the steep temperature gradient of the transition region is responsible for the onset of a Rayleigh-Taylor instability. The authors describe the instability as caused by the upward pressure force in the dense layer, replacing the downward gravitational field of the classical instability. The quantitative analysis is missing; in fact Defouw (1970a) concluded their picture would lead to a stable situation.

In his paper, Defouw describes "thermal instability" but does not deal with the real heating mechanism. He assumes simply that there exists a heat loss function $\mathcal{L}$ (energy loss minus energy gain per unit mass per unit time). The rate of energy input is assumed to be constant. Then, the instability is described. The initial idea goes back to Thomas and Athay (1961): if the hydrogen plasma is heated, it may become less and less able to get rid of its internal energy by radiation. Defouw finds that, depending on the temperature range, the temperature gradient, and the value of the density, one can have unstable situations. The presence of magnetic fields reinforces the instability. Growth rates, temperatures, and electron densities are in satisfactory agreement with the spicule observation. However, the radiation field is treated in the quasi-static, effectively thin approximation, and the energy supply is left unspecified.

At this point, I would like to make a general comment on "descriptive" and "dynamical" models, which comes from the coronal experience.

If one takes into account the energy equation, and the hydrostatic equilibrium for a fully ionized plasma, one can predict a static spherically symmetric solar corona (Chapman, 1959). One needs only to specify T_e, n_e at a boundary point, e.g., at the base of the corona. But this corona has a finite pressure far away from the Sun; one needs an artificial wall to sustain it. Once the wall is removed, the static corona is no longer stable. Is it going to show relaxation into inhomogeneous structures? This seems to be a very complicated idea. One has only to allow for a spherically symmetrical expansion; we add the mass conservation equation and wait for the steady state to establish itself. We do not have to impose any further physical boundary condition. In particular, the velocity v at our boundary point is fixed. The solution (Parker, 1965) is thus viewed as the asymptotic behaviour of a time dependent problem.

Thus, precisely because we think that the chromosphere can be locally unstable, the mass motion should be taken into account from the very beginning. In a following paragraph we shall see how this simple principle can yield to some interesting ideas in the chromosphere-corona transition region, possible connected with spicular structure.

SOME TOOLS FOR THEORETICAL STUDIES IN CHROMOSPHERIC INHOMOGENEITIES

In this section I would like to show, with three examples, that the tools that we need to begin are available or can be found with little effort in the existing literature.

FIRST EXAMPLE:

Local description of the instability condition by Defouw: after some calculations, one finds that a necessary criterion for instability is:

$$\Delta = \left(\mathcal{L}_x - \frac{\rho}{1+x}\mathcal{L}_\rho\right)\left(\mathcal{J}_T - \frac{\rho}{T}\mathcal{J}_\rho\right) - \left(\mathcal{L}_T - \frac{\rho}{T}\mathcal{L}_\rho\right)\left(\mathcal{J}_x - \frac{\rho}{1+x}\mathcal{J}_\rho\right) < 0$$

($\mathcal{L}$ is the heat loss function, $\mathcal{J}$ is the number of ionizations per unit mass per unit time, ρ, T, x are the density, temperature, ionization degree, and $\mathcal{L}_x$ stands for $\frac{\partial \mathcal{L}}{\partial x}$, etc.)

This result has a simple local physical interpretation. In the equilibrium state, a given mass element has well defined energy E, number of particles $\mathcal{N}$, and volume V. This reads:

$$\begin{aligned} E &= \text{cst} && \rightarrow && \mathcal{L}(x, \rho, T) = 0 \\ \mathcal{N} &= \text{cst} && \rightarrow && \mathcal{J}(x, \rho, T) = 0 \\ V &= \text{cst} && \rightarrow && P(x, \rho, T) = P_{ext} \end{aligned}$$

(P is the pressure of the mass element, P_{ext} is the "external" pressure.)

What is the condition for the existence of an equilibrium (neutrally stable) x, ρ, T? (Which is the starting point for a discussion of thermal instability, as in Souffrin, 1971.)

The answer is straightforward: $\delta\mathcal{L} = \delta\mathcal{J} = \delta P = 0$, i.e.

$$\mathcal{L}_x \delta x + \mathcal{L}_\rho \delta\rho + \mathcal{L}_T \delta T = 0$$

$$\mathcal{J}_x \delta x + \mathcal{J}_\rho \delta\rho + \mathcal{J}_T \delta T = 0$$

$$P_{ext}\left[\frac{\delta x}{1+x} + \frac{\delta\rho}{\rho} + \frac{\delta T}{T}\right] = 0$$

(since $P \propto (1+x)\,\rho T$).

A solution for δx, $\delta\rho$ δT different from zero can be found only if $\Delta = 0$. Thus $\Delta = 0$ is the condition for marginal stability. A closer examination will show which side has the instability.(*)

This does not mean that the complete calculation made by Defouw is useless. On the contrary, it is really necessary for a detailed description. This was intended simply to show that it is often possible to extract simple descriptions imbedded in stratified geometries or abstract calculations. These simple descriptions can be more than qualitative and can give valuable support to the intuition.

SECOND EXAMPLE:

This example is non-local, and mixes the heating process together with radiative transfer. Frisch (1970, 1971) has solved numerically the problem of radiative and conductive coupled transports in a stratified atmosphere. In her results, there seem to appear two regions; as a matter of fact it has been shown by Cess (1972) that an approximate solution can be found analytically within the framework of singular perturbations; the boundary layer can be treated separately from the interior. Again, from detailed results, it has been possible to infer an approximate, but much simpler,

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(*) Note added in the final manuscript after a remark by R. J. Defouw.

The question is not really very simple: for example Defouw (1970b) interprets the procedure in the following way: Suppose that $\delta T = \delta P = 0$ and we calculate $\delta\mathcal{L}$ as a function of δT.

$$\delta\mathcal{L} = \frac{-\Delta}{\mathcal{J}_x - \frac{\rho}{1+x}\mathcal{J}_\rho}\,\delta T,$$

as $\mathcal{J}_x < 0$, $\mathcal{J}_\rho = 0$, the thermal instability criterion $\frac{\delta\mathcal{L}}{\delta T} < 0$ is equivalent to $\Delta < 0$.

One can object that it also seems legitimate to calculate $\delta\mathcal{J}$ as a function of δx if $\delta\mathcal{L} = \delta\rho = 0$.

Then

$$\delta\mathcal{J} = \frac{-\Delta}{\mathcal{L}_T - \frac{\rho}{T}\mathcal{L}_\rho}\,\delta x,$$

as $\mathcal{L}_q < 0$, $\mathcal{L}_\rho > 0$, one finds that if $\Delta > 0$, $\frac{\delta\mathcal{J}}{\delta x} > 0$ which seems to also yield an unstable situation.

Obviously in both cases we are not dealing with the correct proper perturbations corresponding to eigenvalues of the damping constant (or growth rate).

description of the physical process. Obviously, the stratified medium assumption is no longer fundamental in Cess's treatment.

THIRD EXAMPLE:

This last example is well known. It is simply the non-LTE radiative transfer problem, and the concept of thermalization length Λ, first introduced by Jefferies (1960).

In Mihalas's recent book (1970) the rather simple result obtained by Avrett and Hummer (1965), namely

$$\Lambda \approx \frac{1}{\epsilon} \; ; \; \frac{8}{9\epsilon} \; ; \; \frac{8a}{9\epsilon} \quad \text{(Doppler, Lorentz, Voigt)},$$

results from long calculations whose physical meaning is not obvious.

While the physical usefulness of Λ was demonstrated, for example by Rybicki (1971), for rapid calculations of non-LTE multilevel transfer problems, Athay and Skumanich (1971) succeeded in calculating orders of magnitude for Λ from very simple physical considerations. Notice again that the validity of this kind of procedure is demonstrated only because the "exact" solution in known! A series of papers by Finn and Jefferies (1968) and Finn (1971, 1972) also has to be mentioned; it deals with the probabilistic interpretation of radiative transfer. It is interesting to see the amount of formalism decrease while the physical insight given to the reader increases. The present tendency seems thus to eliminate most of the algebra, especially that connected with plane parallel geometry, and concentrates on the physical meaning of the local parameters. For example Athay (1972) proposes that the optical depth τ has to be replaced by the "mean number N of scatterings that a photon has to suffer before it escapes". Obviously there is a one to one correspondence between N and τ, but N is not related to a particular geometry.

In conclusion, I think that one can be optimistic about the possibilities that we now have to attack the problem of understanding the *local* machinery which makes the spicules, if we are careful to consider the right local parameters, and if we first try to get good local descriptions of physical processes.

INHOMOGENEITIES IN THE CHROMOSPHERE – CORONA TRANSITION REGION: MASS FLOW?

This paragraph is a brief account of a recent work (Delache, 1972) based on the two principles that have been stressed in the previous paragraphs:

- Try to define the local quantities which stand at the midpoint between observations and theoretical predictions. The proposal is to take the temperature as the independent variable (instead of altitude h, or optical depth), and to study the "thermal differential emission measure" f(T) defined by

$$f(T)\ dT = n_e^2\ dh.$$

- Relax the condition of a static atmosphere.

The equations are very similar to that of solar wind theory, except for radiative losses which are taken into account. In a first step, they are treated in a one dimensional analysis. The value of the velocity v, or mass flow $n_e v$, at a boundary will be physically fixed by the steady state, as usual, and will depend on the amount of energy deposited in the corona (I assume no energy deposition in the transition region). As this is outside the domain of the study, one will need the observations to infer v, either "local" observations (XUV spectrum or radiospectrum) or extrapolations of the solar wind flow.

First, one finds that f(T) is, in fact, simply related to observations, either XUV or radio. If the pressure is assumed to be nearly constant in the transition region, then $f^{-1}(T) \propto T^2 \frac{dT}{dh}$; this last quantity is not very different from $T^{5/2} \frac{dT}{dh}$, which is the expression of the conductive flux. Thus, it is not surprising that simple reductions of observations lead so often to simple predictions of this flux. For example, Chiuderi et al. (1971) proposed a simple parametric representation of the radio observation. One can show that this particular form necessarily implies a constant conductive flux!

But the main result is the following: f(T) can have two very different kinds of behaviour, depending on the value of the mass flow:

- If the mass flow is in the "low regime" (which would correspond to the solar wind flow, or less) then $f(T) \propto \sqrt{T}$, thus leading to a constant conductive flux and agreement with XUV observations for lines emitted at $T > 2{:}10^5$ °K. This confirms Athay's previous result (1966), and is represented by the straight lines on Figure III-6 which is taken from Pottasch's classical work (1964). However, as can be seen on the figure, this behaviour does not match the observation for low values of T, nor does it match the radio observations (Lantos, 1971).
- If the mass flow is in the "high regime" (say 50 times higher than the prediction of a spherically symmetric extrapolation of the solar

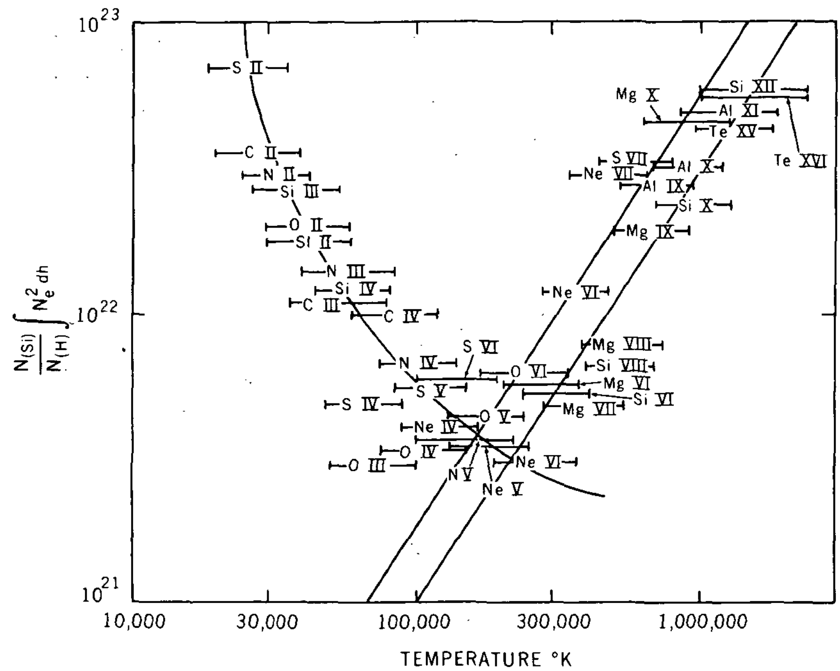

Figure III-6

wind), then $f(T) \propto T^{3/2}\ (T\text{-}T_0)^{-2}$ which agrees with radio observations and XUV observations for $T < 2{\cdot}20^5$ °K as shown on the left part of Figure III-6.

The two regimes can be reconciled in a single model in which the vertical coordinate is guided by the magnetic field. The cross section of the magnetic tubes of force open to the solar wind flow is increasing from the bottom (chromosphere) to the top by a factor of 50. Thus the mass flow $n_e v$ can be large locally, while it remains constant when integrated over the whole solar surface. This sort of morphology for the magnetic structures is known from observations of course, but it is striking that it can be deduced from observations which integrate the complete disk. The picture can be qualitatively completed: in regions of closed magnetic lines (i.e. the two ends are connected to the solar surface) the conduction perpendicular to the field is lowered; the outflow is prevented; the transition region should be very low in the atmosphere and very thin; it does not contribute to the emission measure for $T < 2\ 10^5$ °K.

In this model, the transition region structure is dominated by the conductive flux for $T < 2\ 10^5$ °K above spicules (open field regions) and for all T in the closed field regions. Below $T = 2\ 10^5$ °K, in the open field regions, the enthalpy flux plays a major role. The motion of matter is important. (It has already been noticed by Kuperus and Athay (1967) that the energy flow due to motions in spicules was important.) The temperature gradient is not so steep. The amount of material in a given temperature range is increased.

CONCLUSION

It seems that we are now in a position of starting detailed physical studies of inhomogeneities. Local theories are being developed in dynamics as well as in radiative transfer. The mass flow has to be taken into account, as it is almost certainly a consequence of energy deposition. The momentum equation should also be looked at in detail, as the energy flow and deposition lead nearly always to momentum flow and deposition. (Pressure is exerted by the heating waves, especially in inhomogeneous structures, where they can be refracted.). The stability problem has to be solved after the non-static steady state is fully described. In the previous paragraph we have seen a crude theory starting on those basic principles, applied to a region where dynamics and radiative transfer are disentangled; one is really tempted to connect what is described there with spicular structure.

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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.