# PART III

# THE UPPER CHROMOSPHERE

# FINE STRUCTURE OF THE UPPER CHROMOSPHERE

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**Abstract.** The paper contains a review of attempts to understand observational data on the inhomogeneous structure of the upper solar chromosphere, particularly the spicules. Observations over the electromagnetic spectrum from millimetric to the ultraviolet give equivocal results; and in spite of recent progress, models for these inhomogeneous structures still face difficulties in accounting for the observed radiation. New observations in the visible are needed in order to answer even the simplest questions of kinematics, while the interpretation of the line spectra still poses many difficulties. The acquisition of EUV data at high spatial resolution should lead to major progress in the field, just as it already has done in showing the EUV emission to be concentrated over the network boundaries.

## 1. Introduction

That the upper chromosphere is inhomogeneous is plainly evident from direct filter photographs taken at the limb or in the strong chromospheric lines on the disk. Spectra of the disk and chromosphere taken either during or outside eclipse show a corresponding structure – cf. Suemoto and Hiei (1962), Pierce (1965). The principal inhomogeneities are seen to lie on the network boundaries and to resolve into structures of the order of one arc second in size – as is most clearly shown in Dunn's Hα photographs taken with the Sacramento Peak tower telescope and illustrated, for example, in Figure 3 of Michard's review. These inhomogeneities are undoubtedly spicules. Other inhomogeneities, lying interior to the network, have been described by Liu (1972) and Sawyer (1972), but very little is known of these evanescent features, and they are generally agreed to form a less important aspect of the characterization of the chromospheric structure. This judgment may, however, reflect our ignorance rather more than a balanced assessment. While recognizing that the picture is certainly only a first approximation at best, we shall here follow what seems the normal practice and represent the upper chromosphere in terms of one region, lying along the boundary of the supergranular network, and another, more or less homogeneous in nature, lying interior to this. In these terms, we shall discuss recent efforts to elucidate the physical characteristics of such a model, and shall try to indicate some of the difficulties facing the various pictures which have been advanced.

Quantitative information on the inhomogeneous structure of the chromosphere has been derived from the disk observations in the EUV, and in the millimetric and centimetric radio, as well as from disk and limb observations in the visible. The latter data have the immense advantage of giving high spatial resolution, which has so far not been possible in either the radio or the EUV; nevertheless we shall see that some information on inhomogeneous structure can be obtained from analyses of spectral data in those wavelengths.

In Section 2 we discuss the inferences which have been drawn mainly from optical data, while in Sections 3 and 4, we concentrate on analyses of radio and EUV ob-

*R. Grant Athay (ed.), Chromospheric Fine Structure,* 71–88. 

servations respectively. This format has obvious limitations, but is at least partly justified in convenience as well as in reflecting the historical development of the subject. In Section 5, we shall endeavor to bring together the results obtained by the different observing techniques to show points of agreement as well as those of dispute.

## 2. Studies at Visible Wavelengths

### 2.1. Observations at the Limb

#### 2.1.1. *Line Intensities and the Spicule Model*

Direct, narrow-band photographs, such as those obtained by R. B. Dunn at Sacramento Peak, have provided the basic data for understanding such questions as the lifetime, size, and spatial distribution of the chromospheric inhomogeneities. However, spectroscopic information in lines of H, He, and $Ca^+$ has been the source data for most of our knowledge of the physical conditions – particularly the temperature, density, and state of motion. The spatial resolution needed for such studies is obtainable only under the best of conditions, while the faintness of the emission makes their observation above the sky background difficult in any but the strongest lines except for the rare occasions provided by total eclipses. Until observations from space vehicles allow us to acquire data with high angular resolution over a wider wavelength range, any direct inferences on the structure of spicules will necessarily be limited to regions where the temperature is less than 10000 to 20000 K; reflecting the conditions under which the strong lines of H, He, and $Ca^+$ are most emissive in the chromosphere. This is unfortunate, since the nature of the transition region around spicules is of profound importance to understanding the origin of the inhomogeneous structure of the chromosphere. A first indication of this transition region structure has recently been obtained by Brueckner and Nicolas (1973) on the basis of ultraviolet eclipse data; further observations in the ultraviolet will be eagerly awaited.

Giovanelli (1967) and Beckers (1968, 1972) have derived spicule models using intensity data gathered by different investigators (generally outside of eclipse) for different heights above the limb. Their approach has been essentially to compute the emitted intensity of a model spicule of given thickness as a function of electron density ($n$) and temperature ($T$), and then to determine the values of these parameters which are consistent with the observed intensity of the various spectral lines for which data are available. An example of Beckers' results is shown in Figure 1 for the height of 4000 km above the limb. The various curves should intersect at one point, which would determine a unique ($n$, $T$) if the spicule were representable in those terms. As can be seen from Figure 1, the data do determine $n$ and $T$ quite well and, bearing in mind the limited accuracy, all these intensity data can indeed be matched with a single temperature and density. This is consistent with the observations that the emission in these lines arises from the same feature in the solar atmosphere; see e.g., Suemoto and Hiei (1962), and Pasachoff *et al.* (1968).

Beckers' spicule model covers a height range of about 3000 to 11000 km, over which the density varies from $2 \times 10^{11}$ to $3 \times 10^{10}$ $cm^{-3}$, while the temperature increases from 9000 to 16000 K with the largest gradient occuring in the lowest 2000 km. The conditions at low heights depend on a few difficult measurements of the H and He lines at 3889 Å, and cannot be regarded as very reliable – there is, therefore, no compulsive case in this analysis that a spicule's temperature does, in fact, vary with height.

An analysis of the observed K-line emission by Avery and House (1969) led to a

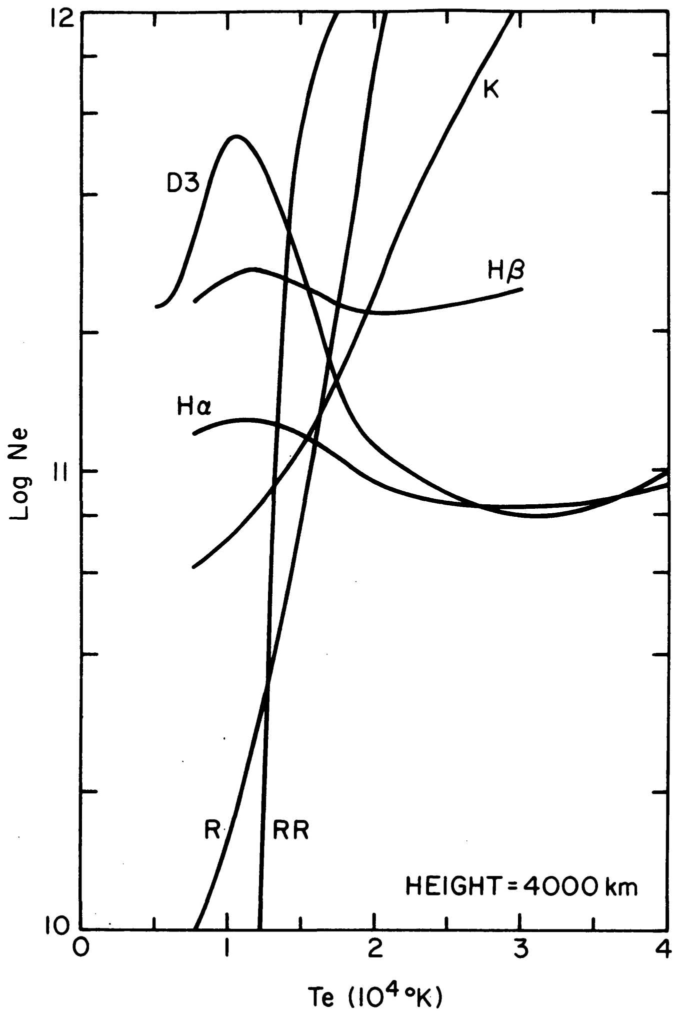


Fig. 1. The curves show the simultaneous values of electron density and temperature which are consistent with the observed intensity, in the indicated lines of the spicular emission at 4000 km above the solar limb. The curve labelled *R* refers to the ratio of the intensities of the helium and hydrogen line at 3889 Å, while *RR* refers to the ratio of the $Ca^{+}$ 8542 and the $D_3$ line – curves *R* and *RR* refer to eclipse data. After Beckers (1972).

rather different model with a generally lower temperature and higher density at a given height. The difference is probably due mainly to the different atomic data used for transition rates and to the fact that the analysis used information on only one spectral line, although the cylindrical geometry adopted would lead to systematic differences with Beckers' conclusions.

### 2.1.2. *Line Profiles*

In principle, one would also expect to obtain useful data from the shapes of the strong spicule emission lines at different heights. Although future work in the visible and EUV may achieve this hope, studies so far have been disappointing – little that is definitive to the spicule model has yet been obtained in this way, although several very puzzling questions have been posed.

The limb profiles in H, K, and H$\alpha$ have central reversals at low heights and, according to Pasachoff *et al.* (1968), are frequently asymmetric. These authors maintain that the wavelength of the minimum is unshifted whether the profile is asymmetric or not. Since the central reversal would then not appear to share in the Doppler shift of the rest of the line, they conclude that the reversal has a non-spicular origin, arising in foreground attenuation – as Michard (1959) suggested – rather than in the spicule itself, which seems at first sight more natural. The case is far from clear; however this question is important since if the origin of the self-reversal were intrinsic to the spicule this would establish that its optical depth in the K and H$\alpha$ lines were significant and so set some important constraints on the model. If it arises in foreground absorption on the other hand, then it places restraints on the characteristics of the (interspicular) matter.

The H and K lines in spicules are usually anomalously wide; cf. Zirker (1962), Pasachoff *et al.* (1968) and Athay and Bessy (1964). The origin of this broadening is as much of a mystery today as when it was first discovered over a decade ago; indeed, it is more so since it now seems clear that the width does not arise in overlapping Doppler-shifted features, as was first proposed, since it persists to great heights where the number of spicules is so small as to make overlap very unlikely. Attempts to explain the widths in terms of a non-thermal random velocity ('microturbulence') have not been convincing; we simply do not know what is doing the broadening, but it certainly does not seem that it arises in a Gaussian distribution of non-thermal motions. This question, also, needs added study.

### 2.1.3. *Line Tilts – Spicule Rotation*

A second strange feature is the 'tilt' of the line; i.e., the inclination of the line profile of the spicule to the direction of dispersion. These are invariably, and clearly, seen in H, K, and H$\alpha$ spectra of spicules and again cannot be explained in terms of an 'overlap' of different features, since even under the finest seeing and the greatest heights they are present. The natural explanation of the tilt is that the spicule is rotating, and this is now widely accepted. The angular velocities are substantial, corresponding typically to a centripetal acceleration some 10 times that of solar gravity. This rotational motion must have some fundamental bearing on the origin of spicules though we have little idea just what. The existence of the rotation may, however, help to explain the anomalous line widths, since the rotational velocity of some 20 km $s^{-1}$ is about the same as the K-line width. Again, further systematic study of this phenomenon is merited to ascertain whether the rotational velocity is the same in different spectral lines, to confirm whether it changes in sense, as Pasachoff *et al.* (1968) suggest, and to find how the rotation characteristics change with height.

### 2.2. Observations on the Disk

Disk observations allow us to see the spatial distribution of the inhomogeneities on the solar surface in a way precluded by limb observations. Such data suggest very naturally the characterization – cf., e.g., Grossman-Doerth and von Uexküll (1973) – of the chromospheric model as a quasi-spherically symmetric background layer lying interior to the network (and for which spatial averages suffice to characterize the gas), together with a circumscribing boundary region representable only in terms of independent components – the bright and dark mottles.

For many years a debate has centered on just what disk features correspond to the limb spicules; there is still no clearcut resolution. In terms of their geometrical distribution, lifetimes, and such spectroscopic properties as Doppler width, source function, and optical depth, a rather clear case seems to exist for identification of the network 'mottles' with spicules. The line-of-sight velocity found for these disk features, however, is not as large as the limb observations would require, and this remains a matter of some concern. Perhaps Grossman-Doerth and von Uexküll (1971, 1973) are correct in believing this to be a consequence of our inability to resolve structures down to a few tenths of an arc second. Perhaps it is due to a substantial vertical velocity gradient being present in the spicule, which then places the fast moving material at small optical depths when viewed vertically downwards. Observations in the higher lying transition region lines might aid in resolving this question. Certainly, there is abundant evidence for vertical velocity gradients in the asymmetric profiles of the Ca II K and H lines. Until the limb and disk velocities are put into correspondence, however, this will remain a problem.

Disk spectra and spectroheliograms in the H and K line of singly ionized calcium have provided a rich field for discussion and dispute. The characteristic double-peaked profiles are widely held to originate in the chromospheric temperature rise, following the mechanism suggested by Jefferies and Thomas (1959). On this basis, we would interpret the observed K-line profile in terms of a basic background chromospheric layer, within which (at network boundaries) are located regions where the density and/or the temperature increases. A qualitative explanation is readily and naturally obtained with this theory for such characteristics of the K-line profile as the increase, towards the limb, of the $K_2$ separation, the variability of the $K_2$ emission from point to point, across the network and in the quiet regions, as well as the observed asymmetry.

Pasachoff (1970) has advanced an alternative suggestion for the K-line reversals under which the line shapes are due to the superposition, on a basic underlying profile, of the emission from independent elements – some of which are moving up, some moving down. The velocities must necessarily be within narrowly defined limits in order to place the $K_2$ emission at so consistently the same position in the line as it is observed to be. Pasachoff's picture was motivated by the observation that, when viewed under high enough spatial resolution, the K-line profile often shows single peaks either to the red, or, more frequently, to the violet. There is no doubt that single-peaked profiles do occur, but we believe that these are simply the more extreme cases of asymmetry. There seems to be little doubt that double-peaked profiles do exist. Indeed, according to Liu (1972), single-peaked profiles arise only in the inhomogeneities interior to the network (though double-peaked profiles occur there also) while the network boundary elements (spicules) give double profiles in which the $K_2$ violet peak is commonly the stronger. The quiet interior region has, at best, a weak double reversal. Pasachoff's explanation faces several other difficulties (some indeed anticipated by its author). The asymmetries which originally stimulated the idea can be easily explained in terms of vertical velocity gradients – cf., Kulander (1968), Athay (1970), Cram (1972). We believe that the chromospheric mechanism for the formation of H and K is firmly enough established that it can be reliably used as a basis for analysis both of the line intensity as well as its asymmetry. Even so, it is not easy to visualize the geometrical structure of the chromospheric temperature rise associated with the network elements. Is it, in fact, localized at the base of these 'spicules'; does the temperature transition occur along the whole vertical height of the spicule; does it occur in a sheathing region? Neither the visible observations nor indeed those in the XUV and EUV have yet yielded much information on the three-dimensional temperature structure in the network element.

## 3. Inferences from Radio Observations

Several attempts have been made to obtain information on the structure of the solar atmosphere using center-to-limb intensity variations (usually obtained at eclipse) in the millimetric radio region. The interpretation of the results is not straightforward, however, since invariably the limb brightening observed, if present at all, is smaller than would be produced by a homogeneous model. Thus, as Coates *et al.* (1958) first discovered, the millimetric radio data forces an inhomogeneous model for the upper chromosphere.

As Simon and Zirin (1969) point out, this is, in fact, a problem common to most observed wavelengths from the X-ray to the decametric radio. For the upper chromosphere, the conflict is particularly clearly shown from the simultaneous appearance of a brightness temperature increasing with wavelength in the millimetric to centimetric range (i.e., temperature increasing with height where this radiation is formed) without any significant limb brightening.

Millimetric observations obtained at the 1970 eclipse have been discussed by Hagen *et al.* (1971) and by Simon (1971) with somewhat different conclusions. Hagen and his collaborators obtained data at 3.2 and 8.3 mm, from a site in the band of totality, using an antenna with a beamwidth across the line of contacts of about 6′ and some 2° along the contact line. To determine the specific intensity near the limb, they first averaged over the scans across the two limbs and then performed a deconvolution of the complex two-dimensional average over the uncovered solar surface and the antenna beam pattern, which yield the measured flux at each instant. The deconvolution along the line of contacts amounts to a differentiation of the flux-versus-time curve, however this yields only an average over a crescent shaped area contiguous to the Moon's limb and a second deconvolution is needed to obtain the surface brightness, or specific intensity. Hagen *et al.* completed this second step assuming an azimuthally symmetric intensity distribution of the Sun. In this way, they finally obtained a radial brightness distribution, having a double peak near the limb. They suggest this may arise from a peculiar temperature structure – perhaps an inversion – in the upper chromosphere.

Simon (1971) questions the reality of this double peak, particularly since the innermost maximum at 1 min from the limb does not appear on disk radioheliograms taken with the NRAO millimeter telescope at Kitt Peak where the eclipse was only partial. His data, supplemented by measures at longer wavelength, indicate simply a 'rough' chromosphere without limb brightening and with a scale size for inhomogeneities smaller than 20000 km, and with a small number of such inhomogeneities in an area of 20000 km$^2$. His data are in fact "...consistent with a multicomponent model of this region in which spicules protrude from the low chromosphere with the inbetween volume filled in by coronal material."

Whether or not a double-peaked radial intensity distribution is real (and we are bound to say that this seems unlikely) can only be found from observations with substantially higher spatial resolution.

However, a significant problem arises in attempting to account quantitatively for the center-to-limb intensity variation, as Vernazza and Noyes (1972) point out. The radiation temperature in the millimetric range is of order 6000 K and increases with wavelength (i.e., with height of origin). Thus, we would predict that the sun would be limb brightened at these wavelengths. The fact that the observations are not consistent with this expectation – cf. Kundu (1971), Lantos and Kundu (1972), Beckman and Clark (1973) – has been attributed to absorption near the limb by cold spicules which, of course, become more and more significant the closer one gets to the limb. A difficulty with this picture is simply that the spicule model derived by Beckers has nowhere a temperature as low as 6000 K and were that model correct we would actually expect to find an excess (rather than the observed diminution) of limb brightening.

Lantos and Kundu (1972) have shown, however, that it is possible to account simultaneously for the brightness temperature and limb darkening at three wavelengths (1.2, 3.5, and 9 mm) by accepting the lower temperature model of Avery and House (1969) and by modifying (below 3000 km) Beckers' estimate of the fraction of the solar surface at a given height which is covered by spicules. Since this fractional coverage can only be well estimated above about 5000 km, where individual spicules become visible, and since the temperature density model is, at best, only poorly known below 5000 km, Lantos and Kundu's explanation of the observed limb darkening seems very reasonable, but still requires independent confirmation, especially in regard to the physical structure of spicules at low heights.

## 4. Ultraviolet Studies

Direct information on the structure of the upper chromosphere is given by EUV spectroheliograms such as those obtained by Tousey (1971) and his collaborators. Such data are most important in showing that the network structure, seen so clearly in the visible, is also prominent in the EUV, extending in fact into the transition region – at least up to temperatures of 250000 K, where O V is formed. Whether or not a fine network can be discerned in the coronal line spectroheliograms is still under discussion. Most contemporary opinion holds that the coronal and chromospheric emission maps the magnetic field distribution. It is, therefore, very important to our understanding of the near and of the far corona to determine the emission pattern in a range of coronal lines, and so to follow, in particular, the change in the field configuration from the tight patterns seen in the chromosphere and the transition region, into the broad, diffuse patterns which seem characteristic of the corona.

Just as for radio observations, center-to-limb intensity measurements from rockets and satellites in the EUV have shown the need for an inhomogeneous model in the upper chromosphere. Indeed, the reasoning leading to this conclusion is quite similar. Furthermore, the lack of spatial resolution, again limits the degree to which we can determine the precise details of the physical structure of the inhomogeneous layers.

As for the radio data, a purely homogeneous model does not allow us to account simultaneously for the observed intensity distribution among different spectral lines and for their center-to-limb variation. Thus as shown by Athay (1966), and Dupree and Goldberg (1967), the relative intensities of different lines lead in a straightforward way, and with few assumptions, to a specific temperature-versus-height distribution which a homogeneous atmosphere must have to account for the line intensities. From such a model it is a simple matter to compute the limb darkening or brightening in the various (optically thin) lines and to the extent that this disagrees with observations, we must examine our assumptions. That most frequently questioned is, of course, that of homogeneity.

Examples of such studies are found in the work of Withbroe (1970a). In the first of these, he studied spectra of Li-like ions from N V to Si XII (lines probably all formed in the upper part of the transition region or the corona), and found no conflict between the measured center-to-limb variation and that computed using the homogeneous Dupree-Goldberg model. Presumably, therefore, inhomogeneities play only a slight role in the lower corona – at least insofar as the spectroscopic properties are concerned.

Systematic differences between prediction and observation do occur, however, for

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some lines formed wholly in the transition region as Withbroe (1970b) showed in a later study in which he essentially repeated the above analysis and found that all of the lines with wavelengths shortward of the Lyman limit (911 Å) had a predicted limb brightening greater than that observed. The natural explanation is that the radiation in these lines is absorbed by the neutral hydrogen in the inhomogeneities (spicules) which lie between the more or less homogeneous basic background layer. This argument is acceptable in terms of the known geometrical height of spicules (> 5000 km), in terms of the inferred height of the transition region – about 2000 km according to Athay (1966) – and also from the fact that spicules cannot significantly affect the intensity near the disk center, since they cover a very small area whereas, near the limb, projection effects can make them all-important.

Withbroe also used the difference between the degree of absorption found for the transition region lines and that for a coronal line (like Ne VIII), together with the known geometrical properties of spicules, to confirm the height of the transition region of 2000 km (with a substantial uncertainty) above the visible solar surface. Were the transition region to lie lower than this, the coronal lines with $\lambda < 911$ Å would show spicular absorption, contrary to observation; were it substantially higher, the degree of absorption in the transition region lines would be less than observed.

Since spicules extend characteristically to heights of 6000–10000 km, their surroundings over most of their height would be distinctly coronal in character on this model. Correspondingly, they should themselves be sheathed in a transition region between the more or less chromospheric spicular gas and the more or less coronal surroundings. However such a transition region will emit radiation – an effect neglected in Withbroe's work. If his picture is to be consistent, therefore, the transition zone must emit far less radiation per unit area than the interspicular transition region – cf. Noyes (1971). This, in turn, might arise from the zone being thin, or of low density.

Brueckner and Nicolas (1973), however, reach an opposite conclusion from their rocket spectra obtained at the 1970 eclipse. Indeed, they find that certain of the 'transition-region lines' extend to heights of at least 11000 km above the white-light limb and that the data can be well accounted for on the basis of a spicular model in which each spicule is sheathed in a transition region having essentially the same physical characteristics as that determined from EUV data with a homogeneous model.

The inhomogeneous structure of the upper chromosphere has also been studied in the Ly-c itself by Vernazza and Noyes (1972). They have adopted the same model of a basic homogeneous layer in which are embedded a random distribution of absorbers which are optically thick at the Ly-c head and are identified with spicules. Proceeding from the observed center-to-limb variation at various wavelengths within the Ly-c, they correct first for the shielding by spicules (assuming them again to be 'cold' absorbers) to obtain a center-to-limb variation representative of the homogeneous background. An empirical analysis of these data at different frequencies then allows them to determine the characteristics of the homogeneous background model. Since this agrees with the homogeneous model derived on theoretical grounds by

Noyes and Kalkofen (1970) using the central disk intensity distribution in the Ly-c, Vernazza and Noyes believe themselves justified in adopting a model of a homogeneous layer through which spicules protrude.

It turns out, however, not to be easy to reconcile this picture with the spicule models derived by Beckers (1968). Thus, both Giovanelli's (1967) calculations and those of Beckers show that the computed Ly-c source function in the spicules is very much larger than that in the quiet homogeneous chromosphere, which would imply that optically thick spicules would increase, rather than diminish, the Ly-c intensity towards the limb. For consistency, the inhomogeneous absorbing features introduced above must have temperatures well below those implied by the visible (H$\alpha$, H, K) emission. Vernazza and Noyes suggest that this clear conflict may be resolved if the spicule has a steep temperature gradient increasing from 5000 K near its base ($\sim$1500 km), which cannot be seen in the visible, to 15000 K at the heights ($\gtrsim$6000 km), where the visible radiation has its origin.

One might hope to be able to test the credibility of such a model through a close comparative study of the relative variation of intensity toward the limb of a set of lines, or continua, whose heights (or temperatures) of formation varied in some known fashion. Indeed, if there is merit to this picture at all, one can readily envisage a scheme which would use the relative limb darkening of EUV lines with $\lambda < 911$ Å to diagnose not only the structure of spicules, but also the temperature/height relation in the interspicular region. This idea was, in fact, applied by Withbroe (1970b), but much more precision is needed to define the intensity variation near the limb before this can be applied with confidence.

## 5. Models

The model adopted to explain the EUV spectra consists simply of a homogeneous layer through which cold spicules protrude. The layer has the familiar photosphere-chromosphere structure and at about 2000 km a thin ($\sim$200 km) transition region occurs with an essentially coronal gas lying above. This model has the difficulties outlined above in reconciling the required properties of the spicules with those found from the limb spectra. Insofar as the background interspicular model goes, some observations favor it, some are in direct contrast.

That the corona extends down to a few thousand kilometers from the limb can be inferred from the variation of intensity with height on slitless spectra taken at eclipses. Differentiation of such data yields a surface brightness, which in turn gives some indication of the variation of the emissivity with height. In this way, Athay and Roberts (1955), analyzing the observations of $\lambda$7892 of Fe XI obtained at the 1952 eclipse, were able to show that the coronal temperature extended far below 10000 km. This conclusion has been strengthened by Weart (1968) and by Kanno *et al.* (1971) – from coronal red- and green-line data in both cases – who agree in placing the 'base of the corona' much below 10000 km. In fact, Kanno *et al.* conclude that the maximum surface brightness in these coronal lines occurs near the base of the interspicular region ($\sim$2000 km); they also find the red-line surface brightness to increase downward faster than that of the green line, indicating an outward temperature increase in the interspicular region.

In direct conflict with this however is the model derived by Beckers (1968) of the homogeneous interspicular region. He obtained a height scale from the electron density distribution, $n(h)$, derived from K-corona eclipse measures, and adopting a temperature profile $T(h)$, computed the radio brightness temperature $T(\nu)$ for millimetric and centimetric wavelengths. By adjusting $T(h)$ for best agreement with the observed $T(\nu)$ Beckers obtained a temperature distribution having a far smaller gradient than that derived from the EUV analyses. Thus, in Beckers' model we find the temperature to be sub-coronal (250000 K) at 16000 km. The observations and their analyses are sufficiently straightforward that we must regard the conflict as real – the most immediate way out of it is to assume that the radio emission has its origin in a part of the gas which is entirely different from that where the EUV or forbidden coronal lines originate.

In fact, we know that the EUV lines are concentrated over the network boundaries so that the EUV analyses must presumably be referring to conditions in the general area where the spicules are formed. In that case, the applicability of the model underlying the analyses by Withbroe and Vernazza and Noyes must be questioned. A more natural model indeed would be one in which the radio radiation arises from the regions interior to the network while the coronal-line radiation arises from a hotter, denser gas lying above a transition region which is part of the network boundary. The spicules lie within this region.

Filter photographs show that spicules move up (and sometimes down) in the sky plane at an apparent velocity of about 25 km $s^{-1}$ – an individual feature rising typically to 10000 km before fading or occasionally returning to the chromosphere. Spectrograms taken above the limb show these same features to have a Doppler motion of about 10 km $s^{-1}$. These two velocities are consistent with what we know about the inclinations of spicules to the radial direction, and so suggest that the movement in the sky plane is a real material motion along the axis of the spicule and not a passage of an excitation front. While Nikolskii and Platova (1971) maintain that a large velocity transverse to the spicule axis is typical, most opinion is agreed that it is uncommon to find transverse velocities greater than a few kilometers per second. The question is fundamental to spicule origin and evolution, however, and needs to be clarified.

More significant perhaps than the simple state of motion, is the fact that spicules carry a large mass into the corona, if they do in fact move radially with high velocity. Thus, with a typical velocity of 25 km $s^{-1}$, and a proton density of $10^{11}$ $cm^{-3}$, spicules carry $2.5 \times 10^{17}$ protons per $cm^2$ of their area each second into the coronal levels and if, following Beckers (1972), we suppose that they occupy $6 \times 10^{-3}$ of the surface of the Sun, then $10^{38}$ protons per second find their way into the corona via spicules. Since the total coronal particle content is of order $10^{42}$, the spicules could replenish the corona in only a few hours. This material does not escape however; its flux is two orders of magnitude more than that contained in the solar wind. It must therefore return to the chromosphere. However, the concensus seems to be that far less cold material is seen returning (in Hα for example) than is seen going up. If this is really so, then we must ask more closely the fate of the material ejected into the corona. The dynamic aspects of the circulatory pattern which must be set up to satisfy the mass balance could well be of fundamental importance in the overall energy balance of the corona-chromosphere. It is idle to speculate, however, before we have more detailed data on the state of the matter returning to the chromosphere after ejection as spicules into the corona.

Piddington (1972) has introduced a dynamic model including an upward hot-gas flux, which to some extent counteracts the downward conductive flux thus leading to a transition region which is thicker above the network boundary tending to concentrate the EUV emission there. Piddington's picture seems to require that the net movement of spicules, as observed in Hα, be downward in order to compensate for the upward mass flux which carries the thermal energy. This does not appear in keeping with observations.

## 6. Summary and Conclusions

Neither in terms of the observations nor of their analyses are we able to discern much agreement between the different workers. Even after many years of observations of chromospheric inhomogeneities we lack definitive data on the kinematics, an understanding of the line profiles, or even of the line intensities. In spite of recent progress, models still face difficulties in accounting simultaneously for the observed radiation in the EUV, radio, and visible, and about all that does seem accepted is that a homogeneous model is not adequate to account for the data in any one of these spectral ranges. Given the appearance of spectroheliograms in chromospheric lines this is scarcely an unexpected result.

The acquisition of EUV data at high spatial resolution will surely lead to major progress in this field. Such data have already allowed us to eliminate a class of models which places the EUV emission over the interior network regions; comparison between lines reflecting different heights of formation will be most instructive in clarifying the interaction between inhomogeneities and corona. Data in the EUV at high spectral and spatial resolution will help us to understand the evolution of the spicule gas after it is projected up toward the corona.

In the visible many (indeed, most) critical observations have been disputed. As a simple example, does cold spicular material return to the chromosphere, indeed, does the matter in a spicule really move upward? This, as indeed most aspects of spicule dynamics, is still an unsolved question.

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## DISCUSSION

(*Ed. Note*) The discussion of Dr Jefferies' review paper was essentially interwoven with his speech. The presentation of the discussion centers around the various parts of the review paper in which discussion arose and has been summarized by J.-C. Pecker.

1. The first point which led to some discussion was the *diagnostic of the observations in the visible* (Section 2.1.1.) and can be abstracted as follows:

(a) The first question concerned the accuracy of the $n_e T_e$ determinations:

*Pecker:* This analysis (Figure 1) relies on two types of data: (1) the measurements (i.e., $D_3$, H$\alpha$, K line, etc. as measured), (2) the assumptions used to compute those curves. The fact that they intersect in a rather vague area but not in a point means either that there is some dispersion in the measurements, or that there is some inaccuracy in the computation. In other terms, when you describe the 'spicules' (let us say 'features') you may have the tendency to say that there is some average model of the features, $T_e n_e$ being physically well-defined; or on the contrary, there is possibly no such thing as an 'average feature' (a feature being like a star in a cluster; no two stars in a cluster are identical; hence different features might be the same object, at different stages of its evolution). How do you see this dilemma?

*Jefferies:* The dispersion in the figure is certainly both a dispersion of the measurements (sometimes a considerable dispersion) as well as an uncertainty in the calculation of the emergent intensity.

*Beckers:* These curves are based on the average intensities of very diverse spicules. The H$\alpha$ observation refers to a different spicule than the H$\beta$ observations but this was all that was available at the time when I made the calculations shown in the slide. There is a study now on the way by Allissandrakis and myself to look at the spectrum of individual spicules and determine the intensity of a number of individual spicules and the changes with time and height.

*Pecker:* I am glad to learn that! From yesterday's discussion I got the impression that we are seeing trees of various species – some pine trees, some bushes, a lot of things – and if we are to see what the forest is we should not take an average tree!

*Beckers:* We carefully calibrate those spectra! Some of these curves cross close to one point, but some of the others do not; this is not only because of measurement inaccuracies but also, of course, because of theoretical inaccuracies.

(b) But can we determine *directly* $n_e$?

*Athay:* In order to predict the $D_3$ intensity, you have to know what the local ultraviolet radiation field is, which you don't know except in an average; so that's certainly one source of error! There is a way to get the electron density more directly – this is the way used by M. Makita (1972, *Solar Phys.* **24**, 59) where you deduce $n_e$ from white-light images of the spicules. The result Makita obtained was $n_e \sim 1.8 \times 10^{11}$. We failed to improve on this data at the last eclipse (bad seeing) but the method is worth repeating.

(c) The second way of asking the same question is: "Is the same volume responsible for all emission lines?" This question, asked by *Brueckner*, was replied to as follows:

*Jefferies:* Yes, it is assumed in these computations. But that of course is one of the questions that one wishes to answer.

*Beckers:* I have assumed in my calculations *homogeneous* spicule temperatures and densities. If the different ($N_e$, $T_e$) curves do not agree with one another, the spread being more than could be tolerated in view of the observational and the theoretical inaccuracies, one has a basis for inferring an inhomogeneous spicule model. But if they do cross as in the figure (i.e. within the uncertainty of the observations and theory) one has no basis for an inhomogeneous spicule model. I do not claim that the spicule is homogeneous, but that we have insufficient data to derive an inhomogeneous spicule model.

*Zirin:* This method seems a very clever way of defining the physical regions where these lines are coming from; looking at the curves it is clear that one must expect a very high change in intensity, for some of these lines as a function of temperature and density; yet the spectra of these phenomena look monotonously the same. The changes with height do not seem to be well marked either. This seems to me to be pointing at the existence of 'plateaus' and stable physical conditions – the sort of stuff that Athay and Thomas talked about, years ago.

*Jefferies:* I don't think we should get carried away by this figure. I think that, as Beckers says, the way to do it is to look at individual spicules and see how a diagram of this kind works for those individual spicules. But Figure 1 is representing a broad range of different observations – averages, with a large variation from one observer to another; it is the best that could be done at that particular time.

(d) The numerical accuracy of the determination on $n_e$, $T_e$ is also obviously a function of the choice of the parameters. This point was then discussed, as follows:

*Jefferies:* I think indeed that a sensible way of approaching the problem is to get diagnostic indicators that are more or less at right angles to one another (the old problem of surveying with á long baseline!). In our case, a line like H$\alpha$ measures the electron density but doesn't give any information on the temperature. Some other lines, or line ratios, give, on the contrary, good information on temperatures but nothing on electron density.

*Pecker:* It would be very useful to assign an 'error-width' to each of those lines, derived both from experiment and from theory, but I gather it is pretty uncertain!

*Jefferies:* I think that's really pressing this curve beyond the point where it was intended – the method is the important thing.

(e) Whatever may be the basic correctness of the methodology, it relies upon theoretical computations. Are they foolproof? The question is raised by *Cannon*:

*Cannon:* The limitations to a model like this is that you are neglecting lateral transfer of radiation. I have very little faith or trust in what can be derived from such a diagram in those conditions!

*Jefferies:* I think you have the wrong idea of the model. At least above 5000 km, the spicule model is a simple slab. One could take a cylinder, but I doubt it would make any significant difference at the level of accuracy of interest here.

*Cannon:* I just don't think you can take a slab and put it in an atmosphere and say: "OK, let us bombard that slab with radiation, and compute its equilibrium." You have got to take into account how the slab interacts with the medium!

2. The second important point to be discussed was *the interpretation of self-reversal* (Section II, A, 2) in spectral lines. Are they the effect of interspicular medium, or are they basically part of the spicular spectrum, and reflect the trend of source functions within the spicule itself? Are there other interpretations, as suggested by *Wilson* – other spicules –? The discussion went essentially as follows:

*Athay:* It seems to me that it is incumbent upon those who propose the 'cool absorbing mechanism' to suggest some way that it can be done. I personally don't know how you can reduce the source function in Hα down to that point because it is determined essentially by the radiation coming from below, and I just don't see any mechanism for reducing either the radiation intensity or the source function.

*Wilson:* Isn't there a third possibility for the self-reversal? If you have a spicule forest and if you are observing them at the limb, you are also looking through some absorbing material of the same nature as the spicule on the way through to the particular one that you are seeing. This indeed seemed to come out in Michard's spectra of Hα that were shown yesterday by Rösch: above the limb, at heights of about 6000 km, there was no central reversal, but at 2000 km there was a clear central reversal and it appeared also that the width of the whole emission was increased*. That would indicate that, near the line center, you are not just looking through a whole forest of spicular material; this is forcing you to look further out into the wings of the line in order to see the intensities in that particular spicule.

*Giovanelli:* Another type of observation can help in the interpretation (though not make it unique): the central reversal does not show the structure that is present in the wings. In Michard's spicule spectra, the individual features can be traced right across the center of Hα at the greater heights, whereas low down the spicules are seen in the wings but not in the line center; the central absorption appears to be rather amorphous, any structures present being of much lower contrast. It appears from this particular type of observation that the absorption occurs outside the spicule – but there is no proof either that the absorption is distributed uniformly – or that it occurs in the tops of optically thin spicules (such as Peter Wilson has suggested).

*Newkirk:* Is this effect seen on eclipse spectra or exclusively on out-of-eclipse spectra? If this is not seen on eclipse spectra one would be inclined to believe that it is due to scattered light in the terrestrial atmosphere rather than the low layers of the solar atmosphere.

*Giovanelli:* I don't know the reply.

*Athay:* I don't see why an explanation based upon overlapping spicules in the line of sight doesn't hold. One could say that in the center of Hα the spicules have enough opacity so that you see a combination of spicules in the line of sight and you simply lose the structure. You pick it up again in the wings of the line where they become transparent. Then you don't need the interspicular material!

*Zirin:* It's clear experimentally in the pictures of Dick Dunn (scannings across the line). They show very clearly that you cannot see spicule structure in the limb band inside about 7/8 Å from the line center in the region between 0 and 3000 km, and therefore the spectrum that we are looking at, between + and – 7/8 Å is the 'general chromosphere' and not the spicular structure. We are seeing, when we go far off the center of the line, most of the spicules, and that is confirmed by looking higher up.

The discussion makes clear that the 'general chromosphere' is not a clear concept for everyone: as *Zirin* comments, if things are clear at about 4000 km, where we see a few spicules, what do we see at lower altitude: a 'general chromosphere', more or less homogeneous, or a 'forest of spicules'?

*Jordan:* If you take the available spicule statistics and calculate the areas that they add up to at the limb,

* *Rösch* made the following comment to the editor: "one slide showed Hα wider at 4000 km than at 6000 km, and without reversal; another one showed Hα and Hγ obtained simultaneously at about 2000 km: a strong reversal is observed in Hα not in Hγ, which demonstrates, as pointed out by Michard, that it cannot be due to scattered light. The reversal is fairly symmetrical and uniform along the slit, whereas the wings are not."

you find that they would merge completely at a height of about 3000 km. On the other hand, if you take EUV data and look at the height of the transition region, you come out to about 1700 km, so I think that what you are seeing from 1500 say to 3000 km is simply merged spicules because the EUV data show that there is no 'general chromosphere' there.

*Giovanelli:* I was just going to report that the central absorption in the strong lines of the Hα and H and K lines does not appear above 3000 km; these lines have flat tops at about 3000 km. It is only below this height that central absorption occurs, although it is strong at about 2000 km, and below.

*Pierce:* We are certainly seeing an interspicular homogeneous chromosphere by the observations of the lines of the rare earths: they have absolutely no spicular structure. Therefore unless you postulate separation of elements, the interspicular chromosphere must exist.

*Thomas:* I think you have to think very carefully about this geometry. I made a lot of calculations in about 1960 on this 'transition region' that you are talking about now. Only there we were getting it from the eclipse data. One of the things that was most important to the analysis was the optical thickness of Hα as a function of height. I tend to agree with Carole Jordan that in that region between 1000—2000 km and 1700–1800 km you have an enormous change in the opacity of any interspicular region in the Balmer lines, which is what you are looking at. So somehow it is needed to consider an effective bunch of spicules emerging. Now if that happens, then I think you ought to think very carefully about the diagrams that Cram put on the board yesterday; because if I interpret them in terms of velocity fields I have a choice – either we need rotation within a single spicule or we just need a radial velocity coming from the spicule inclination. And if I am looking at a bunch of spicules there is no reason that they are all inclined in the same direction. So along a tangential line-of-sight what I am looking at is several columns, whose line-of-sight component is sometimes up and sometimes down. This is not a single spicule, this is a superposition of many things. If I could persuade Cram to show some calculations that he made, I think you will see how difficult it is (even when you have these two emission peaks) to look at the velocities which you observe from these two peaks as compared to any 'central velocity' observed from the displacement of the central self-reversal – you get all kinds of things!

3. The third point which gave rise to a lively discussion was that of the *interpretation of the single-peaked features observed in the K line* (Section 2.1.).

This discussion being essentially already summarized in Jefferies' paper, we shall omit it here.

Another exchange followed later (Sivaraman, Jefferies, Athay) about the relation between double-peaked lines and supergranular cells: single peaks are generally found inside of a cell, not at the boundary.

4. The velocity fields in spicules as deduced from both disk and limb observations (2.2.) – can they be reconciled? *Grössman-Doerth* asks the following question:

*Grössman-Doerth:* Jefferies suggested that the fact that one does not see high velocities on the disk spectra may be attributed to a high gradient in the velocity. Maybe if you look at the disk you see further down, so you have perhaps lower velocities; if you observe at the limb, you see individual features and therefore higher velocities. Now, considering the fact that the spicules seen at the limb have an optical thickness of the order of unity across their diameter, should you not expect to see this gradient if you look along the axis from above, i.e. on the disk?

*Jefferies:* I don't really know that the optical thickness measured through the spicule at 5000–6000 km in the K line is in fact $\sim 1$. I don't think we know enough about the physical conditions in these objects to have much idea what the optical thickness is either vertically (as on the disk) or horizontally (as on the limb).

*Beckers:* I would like to warn against assuming that the velocity of spicules outside the limb reaches 25 km s$^{-1}$. Those are the apparent motions, they are to some extent substantiated by Doppler shift measurements, but those Doppler shift measurements are generally made from strong lines. With weak line observations it seems that Doppler velocities could be much less, but I don't think that these have been measured yet.

An exchange between *Wilson*, *Jefferies* (essentially included in the paper, hence excluded here) points to the fact that when one observes a red peak, the absorbing region has not been necessarily shifted to the red, as 'conventional wisdom' would lead to (see *Cram*'s intervention, in Section II of the symposium).

5. The UV studies (Section 4 of the review) gave place to a discussion about the influence of optical depth effects upon the analysis of the UV lines, between *C. Jordan* and *Jefferies*. The meaning of the word

'spicule' in Vernazza-Noyes' analysis is also discussed: Jefferies' reply to this question essentially included an additional doubt put on the sentence "are identified with spicules" in Section 4 of this paper, in the paragraph starting by "the inhomogeneous structure..." etc. *Brueckner* added the following comments to the discussion:

*Brueckner:* I would like to make a remark here about this problem. In one of the recent issues of *Solar Physics* we published a paper about the 1970 eclipse. We can resolve the problem by assuming that the cool spicule center is surrounded by a transition zone similar to the one which all researchers have assumed since the transition zone became fashionable. And then when you integrate along the line-of-sight, using the optically thin lines like Çɪv, Si ɪɪɪ, N v, and plot their intensities as the Moon covers the limb, you get a good agreement between the observed change and the prediction – if you assume Jacques Becker's spicule count as a function of height. The eclipse data can never be explained by a homogeneous transition shell around the Sun. I want however to warn about these observations: they are not very accurate.

6. Then some discussion about the final models took place (Section 5 of the review paper). *Athay* and *Beckers* do not seem to think that the discrepancies noted by *J. T. Jefferies* between two types of models (such as Kanno's and Beckers') are so difficult to remove. The Kopp-Kuperus model is criticized by *Gabriel*, as follows:

*Gabriel:* In the Kopp and Kuperus model, the thick transition region they obtain in the network center is the result of an error in their treatment. They choose as a 'typical' path a singular field line through a neutral point, which is not at all typical of most of the network center region. In addition to this comment, I have two criticisms to make of the model you put forward:

(1) The suggestion that the network center transition region is very thick and rarefied is not acceptable on the grounds of pressure balance. I would prefer to assume that it is normal density, but much thinner than in the network regions. This is possible since it is closely parallel to the magnetic field, which inhibits thermal conduction across it.

(2) If the upward flow in spicules of 20 km s$^{-1}$ is balanced by a downward flow in the same region of coronal temperature gas, then the downward velocity has to be 100 × higher or 2000 km s$^{-1}$. This is clearly not correct. Even if the area occupied by the downward flow is allowed to increase, this still leads to very high and measurable velocities.

Some attention was paid (*Giovanelli, Thomas, Jefferies*) to the exact location of the 'base of the transiton regions'. It is around 12–1500 km, where $n_e$ is equal to about $10^{11}$ and above; $T_e$ rises quickly from about 9000° to about 20000°, remains near 20000° for a few hundred kilometers then rises quickly to coronal values.

During the general discussion which followed Jefferies' presentation *Giovanelli* introduced a question as to the homogeneity of the region below 1500 km, as such:

*Giovanelli:* I would like to show a slide dealing with the homogeneous region – I think the top of the homogeneous region – we'll just see how homogeneous a homogeneous region is. This slide shows two photographs, one obtained at H$\alpha+\frac{1}{4}$Å, the other at H$\alpha-\frac{1}{4}$Å. There is a little spot group (just a day or two old) and shortly outside, the region is rather undisturbed. Over the center of supergranules, one can see a *granular* region. The grains also appear between the fibrils that stretch out either from the active region or from the supergranule boundary. These grains are clearly at a lower level than the overlying fibrils. They are not photospheric grains, but definitely chromospheric! An exchange (*Gabriel, Thomas*) deleted here because of duplication with what has already been expressed shows that the important problem of mass balance is not simple!

The problem of the 'inhomogeneities' of the 'homogeneous' region is the object of some interventions (*Pasachoff, Pecker, Athay, Schmidt, Kundu*) from which the following can be extracted:

*Pasachoff:* A very quick calculation I made is the following: Taking a single spicule as a cylinder 500 km across and 5000 km high, multiplying that by some mean number of spicules on the Sun (approximately half a million) we come out approximately the same surface area on the side of spicules as there is in between.

*Kundu* to *Jefferies:* Shouldn't your model show radio waves – if it is millimeter waves that you are referring to – as coming from below the transition zone?

*Athay:* I want to make a comment about the homogeneous model below 1500 km. Nearly everybody agrees that the structure is inhomogeneous – you can see all kinds of structures in all kinds of lines. Somehow we have to reduce the problem to some kind of quantitative estimate of the importance of those inhomogeneities. One way of doing this is to look at different models of the chromosphere constructed from different kinds of data. We are in the fortunate position now of being able to construct chromospheric models from optically thick data, seen at the center of the disk, from optically thin data, and spectral features observed at the limb during eclipse; we have radio data, continuum data, line data – a great variety of data. I would argue that if the inhomogeneous structure is really an essential feature of the model we ought to see different models in different spectral features. We ought to see a different model in the optically thick features than we see from the thin features, and we ought to see a different model on the limb than we see in the center of the disk. The point of this to those of us engaged in model-building is that, as time goes on, the different models are all converging into a more-or-less single type of model or very close to it.

The new models, based primarily on optically thick data observed at the center of the disk, are in very close agreement with models based on optically thin data (such as the one Dick Thomas and I derived fifteen years ago, from limb data). Until we find some clear discrepancy between the models, from the center of the disk out to the limb, we have no basis for saying that the inhomogeneous structure is affecting them in a pronounced way.