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A. OBSERVATIONAL STUDIES OF VELOCITY FIELDS IN THE
SOLAR PHOTOSPHERE AND CHROMOSPHERE

CHAIRMAN: R. Michard, Observatoire de Meudon

SUMMARY-INTRODUCTION

ROBERT W. NOYES

(Smithsonian Astrophysical Observatory and Harvard College Observatory,
Cambridge, Massachusetts)

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I. INTRODUCTORY REMARKS

During the IAU Symposium no. 12 (Varenna, 1960) observations were presented (Leighton, 1960) on two new types of velocity fields in the solar atmosphere: (a) Vertical oscillatory motions on a small scale, with a period of about 5 minutes, and (b) large-scale (30,000 km) horizontal flows in a more or less cellular pattern (the 'supergranulation').

In the five years between that meeting and this, our empirical knowledge of these velocity fields has grown enormously. Studies of the small-scale oscillation have filled in a wealth of details in what five years ago was only the sketchiest of pictures. Much more is also known about the nature of the supergranulation; in particular, it is now becoming clear that many other chromospheric phenomena are intimately related to this fundamental flow pattern.

It is the purpose of this review to discuss critically the observational evidence obtained in the last few years on the nature of velocity fields in the solar photosphere and chromosphere. We shall suggest that the properties of virtually all these velocity fields stem more or less directly from the properties of the granulation and the supergranulation, with magnetic fields playing an important role. In other words, given the motions of the granulation and the supergranulation, plus the strength of the general magnetic field, we should in principle be able to predict the nature of all chromospheric velocity fields.

Unfortunately, the observations do not yet permit much insight into the basic question of the physical relationship, if any, between the granulation and the supergranulation. This problem is a major challenge to observers and theoreticians alike.

In the remainder of this introduction we shall discuss briefly the observational techniques and the assumptions underlying the interpretation of the observations. In part 2, we shall discuss the small-scale oscillatory motions, and in part 3 the large-scale supergranulation and related phenomena. In part 4 we shall summarize these observations and examine the possible interrelation between the small- and large-scale velocity fields. We shall conclude with a discussion of future observational and theoretical studies that might clarify the present observations.

(a) Techniques for Observing Velocity Fields

(i) Spectrographic techniques

High-resolution solar spectrographs can combine extremely fine spectral resolution with a spatial resolution sometimes as good as 1 arc sec (700 km on the Sun). In addition, spectrograms may be obtained in rapid sequence, permitting high-time resolution.
Finally, the spectrum may be recorded simultaneously in many lines. Thus, the spectrograph provides detailed information on the physical state and time-development of a long narrow strip of the solar atmosphere. For example, one can deduce the line-of-sight velocity field at different levels of the atmosphere by comparing Doppler shifts as measured in different lines. Similarly, from measures of residual intensity in the line one can determine the source function for different lines at different heights. One must pay the price of obtaining information only along a line on the Sun, rather than over an area; however, for many applications this is by no means a serious limitation.

(ii) Spectroheliographic techniques

The spectroheliograph was first directly applied to the measurement of solar velocity fields by Leighton, Noyes, and Simon (1962). The method is to obtain simultaneous spectroheliograms in the two wings of a spectral line and then subtract them photographically. If certain rather reasonable assumptions are made about the shape of the line profile and the linearity of the photographic film, then the resulting image is a two-dimensional picture of the velocity field. In other words, the density fluctuations at any point are proportional to the line-of-sight velocity at that point.*

For the privilege of obtaining a two-dimensional map of the velocity field at one wavelength in one line, we pay a high price: Most of the information about the line profile is lost, as well as all information about other lines. It is also difficult to get good time resolution over a large area, due to the slow scanning speed of a spectroheliograph.

We also reap huge gains. The importance of a two-dimensional picture for gaining qualitative insight into the nature of the velocity field is hard to overrate. In addition, for certain quantitative statistical analyses, such as the determination of spatial autocorrelation functions, the amount of useful information available on a two-dimensional area enormously surpasses that available from a one-dimensional spectrum.

(b) Assumptions Underlying the Analysis

(i) Data concerning Doppler shifts

From either spectrograms or spectroheliograms, we may eventually derive a number \( \Delta \lambda \) that refers to a particular point on the Sun at a particular time. This number is then equated to \( v \lambda / c \), where \( v \) is the line-of-sight velocity. However, \( \Delta \lambda \) is inferred from a measured brightness change at two points equidistant from the center of the line. In order that the velocity be meaningful, it must be assumed that the unshifted and shifted lines both have a symmetric profile, or if there is a symmetry, it is the same for both. Otherwise, it is impossible to distinguish between nonsymmetric brightness fluctuations and actual Doppler shifts.

Further, it is usually more or less tacitly assumed that \( v \) refers to some velocity characteristic of a single point, or at least a homogeneous region, in the solar atmosphere. Actually, however, \( v \) represents a weighted average of the velocities within a rather sizable volume of the solar atmosphere. This volume may be approximated by a cylinder whose diameter is determined by the resolution of the optical system and by the image quality (seeing), and whose vertical extent is determined by the extremes in depth of the contribution function for the light passing through the spectrograph slit. Under conditions

* For a complete discussion of the method and the errors inherent in it, see Leighton, Noyes, and Simon (1962) or Noyes (1963).
of excellent seeing, the diameter of the cylinder may be as small as 1 second of arc (700 km), but diameters of 1000 to 2000 km are more usual. Typical depth variations of the contribution functions are about 100 km for weak lines (see, e.g., Elste, 1955). For strong lines, the depth variation is much more serious, especially near the line core. Athay (1963) calculates, for instance, that in the core of the strong Mg i 5172 line the depth variation is 900 km. If the spectral resolution is not high, the smearing may be even worse.

At this point two remarks are in order: (a) Because of the inadequate horizontal spatial resolution, measurements of r.m.s. fluctuations in velocity or intensity in a given line may be considerably too low. Evans (1965) has found, for instance, that there is a high correlation between measured r.m.s. velocity and the image quality, or seeing, on individual spectrograms of a time sequence. (b) The amplitudes of the velocity fluctuations, especially the oscillatory motions, appear to increase rapidly with height. As we shall see, observations in photospheric lines suggest an increase of about 50% in r.m.s. velocity over a height range of a few hundred km. Therefore, over a minimum resolvable height range of as much as 900 km, the velocity may change significantly.

Thus we conclude that the interpretation of measured velocities or intensities as characteristic of homogeneous regions of the atmosphere must be viewed with caution.

(ii) Data concerning line intensities

When we turn to the problem of interpreting observed intensity fluctuations, we must consider not only the effects of limited spatial and depth resolution, which we have just discussed, but also an additional, even more formidable problem. While the interpretation of $\Delta \lambda$ in terms of line-of-sight velocity is rather straightforward, the interpretation of $\Delta I$ in terms of any meaningful physical quantity at the point of observation is not always obvious.

Let us assume that we have translated the observed $\Delta I_\nu$ into an inferred $\Delta S_\nu$ at some level in the atmosphere, say by the approximation $\Delta I_\nu (\alpha, \mu) = \Delta S_\nu (\tau_\nu = \mu)$. Then what can $S_\nu (\tau_\nu)$ tell us about the physical conditions at $\tau_\nu$?

If we make the assumption of LTE, then $\Delta S_\nu = \Delta B_\nu$, and we immediately obtain the temperature fluctuations. This is certainly not a good approximation for most of the lines studied by the spectroheliographic technique, which demands reasonably strong lines for its use; it may not be too bad for some of the weakest (e.g., C i 5052) lines studied with the spectrograph.

If we confine our study to lines which are collisionally controlled (this would appear to include virtually all lines studied, except for the Balmer lines and the helium lines), we may relax the LTE assumption and still extract meaningful physical data about the local properties at the effective level of formation of the line. In this case $\Delta S$ reflects fluctuations in the collisional source term $\epsilon B$ of the integral equation of transfer (see, e.g., Thomas, 1965). An increase in line intensity then means an increase in either $T_\nu$, $n_e$, or both at $\tau_\nu \sim \mu$.

For a photoionization-dominated line like Hα, the situation is much more difficult. Here there seems to be no substitute for detailed calculation of a model and comparison of the predicted radiation field with observations. While this approach has been used with some success for spicule spectra, it has received little application to the studies of inhomogeneities on the disk.
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(iii) The determination of heights of formation of absorption lines

The observations we shall discuss are made at various positions in the profiles of lines of various strengths. It is clearly crucial for an adequate interpretation of the observations to assign relative heights of formation of the various lines; more desirable are absolute values, for instance above the height of the solar limb.

The most straightforward way of determining heights of formation is by direct integration of the line-plus-continuum opacity, using a solar model, to determine that physical height where \( \tau_L = \tau_c = 1 \). (Here, \( \tau_L \) is the optical depth due to atomic transitions in the line; \( \tau_c \) is the continuous opacity at the wavelength of the line.) In many cases this calculation is rather difficult, due to the necessity of computing departure coefficients from the LTE atomic-level populations.

Observations of the top height of the chromosphere seen on the limb in spectroheliograms made in chromospheric lines can be converted into an estimate of the height at which \( \tau_L + \tau_c = 1 \) by simple geometry (de Jager, 1959). In an analogous fashion, if an emission line observed at a certain height off the limb is found to be free of self-absorption, an upper limit to the radial optical depth to that level may be found; from this an upper bound to the height at which \( \tau_L + \tau_c = 1 \) may be obtained (Athey 1963).

To date, application of these methods to the observations is somewhat lacking. Reference is often made to a qualitative tabulation of heights of formation of chromospheric lines by de Jager (1959). More precise calculations are clearly necessary. Athay (1963) has carried out such calculations for a number of Mg I lines, and obtained results somewhat different from de Jager's.

In the present paper, due to the absence of detailed calculations, we shall often resort to a simple ordering of the lines according to their strength.

2. THE OSCILLATORY MOTIONS

(a) Velocity Oscillations

(i) General characteristics

The small-scale (size less than 5000 km) velocity field in the solar photosphere and low chromosphere was found from observations at Mount Wilson in 1960 and 1961 to have the following characteristics (Leighton, Noyes, and Simon 1962):

(a) The motions are primarily vertical and oscillatory, with a predominant period of about 300 seconds. The period decreases slowly with height, varying by about 15 seconds through the upper photosphere and low chromosphere.

(b) The amplitude of the oscillatory motions increases from about 0.4 km s\(^{-1}\) r.m.s. in the medium-strength Fe 6102 line to about 0.6 km s\(^{-1}\) r.m.s. in the strong Na 5896 line.

(c) The lifetime of the oscillation, as measured by the decay of the velocity time-autocorrelation function, is only about 500 seconds. It was not clear at the time whether this is a true lifetime for individual oscillating elements or whether it represents the characteristic time for phase changes in the oscillatory field.

(d) The size of the elements undergoing the oscillation increases rapidly with height, from about 1600 km in the Fe 6102 line to about 3500 km in the Na 5896 line.

(ii) The power spectrum of the velocity field

In the past few years, a much more detailed study of the oscillatory motions has been carried out, largely by the Sacramento Peak Observatory workers and their colleagues.
This discussion will lean heavily on their work (Evans and Michard 1962; Jensen and Orrall 1963; Evans, Michard, and Servajean 1963; O"rrall 1965, 1966; Edmonds, Michard, and Servajean 1965).

The Sacramento Peak data consist of sets of carefully guided spectrograms separated in time by 20 or 30 seconds and lasting up to nearly 30 minutes. From these have been obtained values of \( v(x_i, t_j) \), which express the velocity at position \( x_i \) along the spectrograph slit and at time \( t_j \). The velocity is defined as \( c(\Delta \lambda/\lambda) \), where \( \Delta \lambda \) is the average shift of the spectral line, measured at two points equidistant from the line core and in opposite wings on the line profile.

From the \( v(x_i, t_j) \) data a time-autocorrelation curve \( C(\tau) \) may be calculated in the usual fashion, and its Fourier transform taken to determine the power spectrum \( G(\nu) \). In order to compare the behavior in two different lines a and b, the cross-correlation function \( C_{ab}(\tau) \) is obtained and Fourier-transformed to yield the cross-power spectrum \( G_{ab}(\nu) \). In general, the cross-correlation function may be divided into an even and an odd part and therefore the cross-power spectrum contains a real and an imaginary part. The phase \( Z(\nu) \) is defined as \( \tan^{-1}[\text{Im}(G_{ab}(\nu))/\text{Re}(G_{ab}(\nu))] \), and the coherence \( P_{ab}(\nu) = G_{ab}(\nu)/[G_a(\nu)G_b(\nu)]^{1/2} \) (see Evans, Michard, and Servajean 1963).

Figure 1 illustrates some power spectra that have been obtained from several different observational sequences.* The curves are arranged in height in the figure according to line strength, with the strongest (Ca+K) line at the top and the weakest (continuum) at the bottom. Thus, they approximate the ordering in height in the solar atmosphere. The total height range spanned by the lines is probably about 3500 km (de Jager 1959).

The curves for Fe 5050 and Fe 8514 are typical of what is found in medium strong Fraunhofer lines, such as those in which the oscillation was initially observed at Mount Wilson. A rather sharp peak in the power is seen at \( \nu \sim 3 \times 10^{-5} \) Hz, or \( T \sim 300 \) s.

In addition to this ‘resonance’ peak in the power, the diagram shows several striking changes with altitude that were totally unsuspected from the Mount Wilson observations: (1) As we progress upward from the region where the power spectrum is sharply peaked, we find in the Mg I 5172 line the first appearance of a ‘high-frequency tail’ to the power spectrum, located at \( \nu \sim 6.6 \times 10^{-5} \) Hz, or \( T \sim 150 \) s. This becomes stronger in the Ca II 8498 line, becomes equal in power to the ‘resonance’ peak in the Ca II 8542 line, and finally dominates the power spectrum of the Ca+K line. (2) As we progress downward from the region where the spectrum is sharply peaked, we encounter a ‘low-frequency tail’ to the power spectrum, which increases in importance until it dominates the spectrum for the weak Ca 5052 line. Finally, the continuum intensity fluctuations show very little extra power near the ‘resonance’ frequency. Table 1, obtained from planimetry of Figure 1, illustrates how the percentage power in the low-frequency tail, the ‘resonance’ region, and the high-frequency tail depends on line strength. The figures in parentheses pertaining to the low-frequency tail probably reflect the slowly evolving chromospheric network associated with the supergranulation (for the Ca II 3933 line, this steady evolution was artificially removed from the data).

(iii) Phase relations in the velocity field

The first indication of an upward propagation of the oscillatory motion came with Evans and Michard’s (1962) observation that the appearance of a brightening in the continuum is followed about 40 s later by the onset of oscillations in the Ti 5174 and

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*See footnote to Table 1.
Fig. 1. Power spectra of the velocity field for lines of varying strength, and for the intensity fluctuations in the continuum. The strength of the line increases from bottom to top of the figure. All spectra are normalized to maximum power of unity.
Mg 5172 lines. Further, there was a lag of, on the average, about 15 seconds between the onset of the oscillation in the weaker Ti 5174 line and that in the stronger Mg 5172 line. However, the phase lag between events in these two lines appeared to decrease toward zero with increasing time after the onset of the oscillation. In one or two periods the high-level motions were able to overcome their initial delay relative to the low-level motions, after which the oscillation remained in phase at the two levels (Figure 2).

Evans and Michard suggested that the time lags observed between brightening in the continuum and the onset of oscillations in the two lines did not differ seriously from those expected for upward propagation of an impulse at sonic velocity (about 7 km s\(^{-1}\)). Athay (1963) concludes, on the other hand, from calculations of the height of formation of the two lines that the propagation velocity between the continuum and Ti 5174 must be less than about 1 km s\(^{-1}\), while between Ti 5174 and Mg 5172 it lies between 2 and 10 km s\(^{-1}\).

Recent analyses by Edmonds, Michard, and Servajean (1965) have shown that the actual situation is far more complicated than the simple picture in Figure 2 would suggest. They have found that the phase lag between continuum fluctuations and velocity fluctuations varies greatly between the 'resonance' region and 'convective' region of the power spectrum. In the resonance range, velocity lags the continuum intensity by some 25-50 seconds, in agreement with earlier observations. But in the region of the 'convective tail', the velocity leads the continuum brightness by some 100 seconds. Edmonds, Michard, and Servajean (1965) suggest that rising hot convective cells within the convective zone perhaps are able to induce motions in the visible region sometime before the excess heat penetrates to the same region.

The phase lag at higher levels, as measured in two lines, is also more complicated.

### Table 1

<table>
<thead>
<tr>
<th></th>
<th>( \xi^2 = 2\langle v^2 \rangle ) (km(^2) s(^{-2}))</th>
<th>Percent of total power in range</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>(0 &lt; v &lt; 10^{3} &lt; 1.25)</td>
</tr>
<tr>
<td>Ca II 3933</td>
<td>2.6</td>
<td>6.5</td>
</tr>
<tr>
<td>Ca II 8542</td>
<td>1.0</td>
<td>(18.5)</td>
</tr>
<tr>
<td>Ca II 8498</td>
<td>0.39</td>
<td>(8.2)</td>
</tr>
<tr>
<td>Mg I 5172</td>
<td>0.36</td>
<td>2.8</td>
</tr>
<tr>
<td>Fe I 8514</td>
<td>0.109</td>
<td>7.6</td>
</tr>
<tr>
<td>Fe I 5049</td>
<td>0.078</td>
<td>7.0</td>
</tr>
<tr>
<td>Cr I 5051</td>
<td>0.058</td>
<td>16.7</td>
</tr>
<tr>
<td>C I 5052</td>
<td>0.063</td>
<td>27.6</td>
</tr>
<tr>
<td>Continuum</td>
<td>(\Delta I/I)</td>
<td>0.00026†</td>
</tr>
</tbody>
</table>

*The sources of these data are: For 3933 Orrall (1966); for 8542, 8498, 5172, 8514, Evans, Michard, and Servajean (1963); for 5049, 5051, 5052, and the continuum, Edmonds, Michard, and Servajean (1965).
† \(\sigma^2\) (dimensionless).
Fig. 2. Schematic illustration of the time-development of an oscillation. The initial event is a brightening in the continuum, which is followed by the onset of oscillatory motion in the overlying layers. The phase lag between the oscillation in two overlying lines decreases rapidly to zero.

than the simple picture suggests. Evans, Michard, and Servajeau (1963) obtained the phase spectrum between velocities in the strong Ca$^+$ 8542 line and the much weaker Fe$^{8514}$ line, and found the phase lag to vary with frequency across the 'resonance' peak of the power spectrum. At the low-frequency side of the peak the phase lag is zero, implying a standing wave with infinite vertical phase velocity. At the high-frequency side the phase is about 30°, implying some progressive motion.

In summary, it appears that the 'initial stage' of the oscillation (i.e., that part of the oscillation that occurs immediately after a brightening in the continuum) shows an upward propagation of velocity at or less than the speed of sound. However, the oscillation rather quickly attains very large vertical phase velocity, i.e., a very small phase lag. This phase lag is actually frequency-dependent, varying through the 'resonance' peak from zero on the low-frequency side to about 30° on the high-frequency side. In the 'convective tail' seen at low levels, upward velocities lead continuum brightenings by about 100 seconds.

(iv) Lifetime of the oscillation

We have already mentioned that the lifetime of the oscillation, as deduced from the decay of the time-autocorrelation function of the velocity field, is some 400 seconds. However, several observers have recorded individual oscillations lasting for many cycles. Howard (1965), observing with the Mount Wilson magnetograph in the Doppler mode, has found oscillations lasting at least 4 or 5 cycles with no apparent damping. When the
oscillation does terminate, it is generally by a phase change rather than a decay of amplitude (Howard, 1962, 1965).

Figure 3 illustrates a persistent oscillation observed at a single point on the Sun by Zirker (1964) in the line Fe 5328.

It seems probable, therefore, that the rapid decay of the time autocorrelation is due mainly to phase shifts in the velocity field rather than to an actual damping of amplitude of the motion. No detailed study of the decay in amplitude of individual oscillations has yet been made.

(b) The Intensity Oscillation

(i) Intensity fluctuations in strong lines

Figure 4 shows the statistical relationships between fluctuations in the brightness at the center of a medium-strong (Fe 5171) line and fluctuations in the velocity of the same line (Evans, Michard, and Servajean, 1963). We see the familiar ‘resonance’ peak in the velocity spectrum, common to lines of intermediate strength. In the brightness spectrum, there is also a peak in power at the same frequency \( \nu = 3.5 \times 10^{-3} \) Hz plus a larger peak at very low frequencies.

Thus the chromospheric brightness field consists of a slowly varying rather intense component, plus a less intense but oscillatory component. The slowly varying component is very probably due to the large-scale, long-lived chromospheric emission network, easily visible in spectroheliograms taken in any strong line. It need not concern us for the moment. The oscillatory component is seen to have a high coherence with the velocity oscillations. Moreover, the phase \( \Delta \) shows that the brightness leads the velocity by about 90°, for all frequencies in which there is significant power in the velocity field.

![Graph](image_url)

**Fig. 3.** The variation of velocity with time at a particular point on the Sun, as observed in the line Fe 5328 (Zirker 1964).
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Fig. 4. Spectral properties of velocity and brightness fluctuations in the line Fe 5171 (Evans, Michard, and Servaje, 1963). The top curve is the power spectrum (Fourier transform of the autocorrelation function) of the velocity fluctuations. The second curve is the power spectrum of the brightness fluctuations. The next pair of curves shows the even and odd parts of their cross-power spectrum while the bottom two curves show the coherence $P$ and the phase $Z$, between the velocity and brightness fluctuations.

In other words, a maximum of brightness is followed by a maximum of upward velocity one-quarter period later.

(ii) Intensity fluctuations in weak lines and the continuum

At the deeper levels, the situation is entirely different. Recent analyses (Edmonds, Michard, and Servaje 1965) of the weak C 5052 line yield only marginal evidence for an oscillatory power in the intensity field in the ‘resonance’ region, even though there remains significant power in the velocity field in this region. The power spectrum for the fluctuation $\Delta r$ of the residual intensity $r$ in this line is highly coherent with the continuum power spectrum at all frequencies. The phase relations between $\Delta r/r$ in the carbon line and the continuum reflect two phenomena:

(a) There is a $180^\circ$ phase shift between $\Delta I_c$ and $\Delta r/r$, because increased continuum temperature causes an increased population in the highly excited lower state of the transition.

(b) An additional time lag in $\Delta r/r$ of about 12 seconds appears, possibly due to the finite thermal relaxation time for changes in the radiation field. We shall return to this point in the next section.

In summary, there seem to be two separate regions of the atmosphere in which we encounter rather different behavior of the intensity field:

(a) Above the level of formation of the center of the Fe 5171 line, the intensity field
contains a slowly varying component, which we identify with the chromospheric network, plus an oscillatory component coherent with the velocity oscillations and leading them by about 90°.

(b) Below the level of formation of the center of the C 5052 line, the intensity is mainly non-oscillatory, and is rather highly coherent with the non-periodic continuum fluctuations. This is true even though in the same line the velocity oscillations contain 45% of the total power.

Observations at Mount Wilson indicate that region (b) may be extended upward to the level of formation of the wings ($\Delta \lambda \sim 0.1 \, \AA$) of Ca 6103, where intensity oscillations were sought and not found. Region (a) may perhaps be extended downward to the level of formation of the wings ($\Delta \lambda \sim 0.07 \, \AA$) of Na 5896 where weak evidence for oscillation in the intensity field was found. We of course would expect a continuous transition with height between the intensity fluctuations characteristic of regions (a) and (b).

(c) Interpretation of Velocity and Intensity Data

At this point it is appropriate to try to synthesize the observations discussed above into a coherent picture of the time development of the motion.

It is reasonable to suppose that the oscillatory motion is triggered by some event occurring at a level deeper than the level where we first see the oscillation, that of C 5052 line, or $\tau_c \sim 0.7$ (Edmonds, Michard, Servajean 1965). Whether the triggering event is the appearance of a bright granule at $\tau_c = 1$ or whether the brightening of the granule itself is a result of some triggering event at deeper levels is not evident from the data.

It is also reasonable to suppose that the reason we see no oscillation in the intensity field in the C 5052 line is the short thermal-relaxation time for the photosphere near $\tau_c = 1$. Because we are observing everywhere above $\tau_c = 1$, the thermal-relaxation time should be close to the thin-atmosphere value $\tau = \frac{3}{32} \frac{k}{m_H} \frac{1}{T^3} \frac{1}{\kappa}$, where $\kappa$ is the Planck mean absorption coefficient per gram of hydrogen, and $T$ is the temperature (Spiegel 1957). The main opacity source is the H$^-$ ion, and at $\tau_c \sim 1$, H$^-$ is sufficiently plentiful that the radiative relaxation time is of the order of only a few seconds.

The relative concentration of H$^-$, however, is roughly proportional to the electron pressure, which has a scale height of about 50 km near $\tau_c = 1$. Therefore, a few hundred km above the level $\tau_c = 1$, the radiative relaxation time has increased to hundreds of seconds (Noyes and Leighton 1963).

We thus expect the thermal structure of the deep-lying lines to be firmly locked to the surrounding radiation field, i.e., to the intensity of the photosphere granulation. We do not, for instance, expect to see in these lines any temperature fluctuations due to local changes of pressure and density, such as might be engendered by wave motions. These expectations are in agreement with the observations we have already described, if we interpret the changes in brightness of the carbon line core as directly proportional to temperature changes.

In addition, we would expect the thermal fluctuations in the high-atmospheric layers to be uncoupled to photospheric fluctuations of duration short compared to the very long radiative relaxation time. The gas should behave essentially adiabatically, so that changes in temperature parallel changes in pressure and density. Since for a standing
wave the oscillations of both temperature and density lead the velocity oscillation by $90^\circ$; the source function for a collisionally controlled transition should also lead the velocity by $90^\circ$. Thus, the observed phase relations in the brightness and velocity oscillations in Fe 5171 suggest a standing adiabatic wave motion.

It would appear, then, that brightness fluctuations in collisionally controlled lines in the solar photosphere are of two types: (a) Non-periodic fluctuations in response to fluctuations in the continuum radiation field (i.e., the appearance of bright granules), and (b) periodic oscillations in response to the quasi-adiabatic pressure and density fluctuations that are produced by the velocity oscillations. The former are predominant in weak lines formed at low altitudes, while the latter predominate in strong lines formed at higher levels.

The theory of waves propagating in the Sun has been discussed extensively in the recent astrophysical literature and it is not within the scope of this review to discuss it here. However, we do wish to point out that when the effects of radiative leakage are introduced even into the one-dimensional theory, very encouraging agreement with the observational data is obtained.

(d) Spatial Properties of the Oscillation

Evidence for an increase with height in the characteristic size of elements of the velocity field has been reported often in the recent literature (e.g., Krat 1962; Leighton, Noyes, and Simon 1962; Bernière, Michard, and Rigel 1962; Edmonds, Michard, and Servajean 1965). This increase is illustrated in Figure 5, which is a plot of the half width of spatial autocorrelation curves of Doppler (velocity) spectroheliograms in various lines (Noyes 1963). The horizontal axis is only a crude height scale. We see that the size of the elements more than doubles—increasing from about 1600 km to about 3500 km—in a vertical distance of at most a few hundred kilometers. Since, as we have seen, almost all the power in the velocity field at low levels (i.e., in all lines in Figure 5 except Ca$^+$ 8542 and Hx) is oscillatory, we must conclude that the size of the oscillating elements increases with height.

The increase in size with height is visible not only in the velocity field, but also in normal spectroheliograms of the intensity in spectral lines (dots in Figure 5) and in direct photographs of the granulation. Recent observations by Dr J. Evans in a band of the continuum near $\lambda$ 5300 and simultaneously in a band near $\lambda$ 3800 indicate a considerably larger size of the granulation structures at $\lambda$ 3800. The mean height of formation at $\lambda$ 3800 is considerably higher than that at $\lambda$ 5300, due to the crowding together of spectral lines in the $\lambda$ 3800 region.

The size of 3500 km obtained from measurements of the half widths of autocorrelation curves (Figure 5) refers, of course, to all components of the velocity field. Individual oscillation elements could well be even larger. Evans and Michard (1962) point out the existence of marked coherence between velocities measured as far apart as 10 000 km (see Evans and Michard 1962, Figure 1). Howard (1965) observes strong oscillatory motions with the magnetograph using apertures as large as 20 seconds of arc (15 000 km) square; this suggests that the oscillation could have a comparable scale.

Turning now to the intensity field, it is quite probable that the size of regions undergoing oscillatory intensity fluctuations is the same as for the velocity oscillations. However, the non-oscillatory intensity fluctuations appear to be smaller in area. This may be
Fig. 5. Full width at half maximum (FWHM) of spatial autocorrelation curves of the velocity field in various lines (Noyes, 1963). Crosses refer to data obtained from Doppler spectroheliograms, while dots refer to the original spectroheliograms before photographic superposition into Doppler spectroheliograms. Also shown (circle) is the FWHM for the solar granulation photographed with the same telescope. The height scale (abscissa) is only approximate.

seen from examination of spatial cross-correlation curves between pairs of spectroheliograms simultaneously exposed in either wing of a spectral line (see Leighton, Noyes, and Simon 1962, Figure 9B). It may be shown (Noyes, 1963) that the cross-correlation function $C_{rv}(x)$ between two images is related to the autocorrelation functions $C_{dl}(x)$ and $C_{v}(x)$ of the intensity and velocity fields by

$$C_{rv}(x) = \alpha^2 C_{dl}(x) - \beta^2 C_{v}(x),$$

where $\alpha$ and $\beta$ are constants. The 'W' shape of $C_{rv}(x)$ found by Leighton, Noyes, and Simon (1962), for observations referring to deeper levels indicates that the non-oscillatory brightness fluctuations found at these levels have a smaller size than the velocity oscillations. The absence of such a 'W' shape for an observation at a higher level (Na 5896, $\Delta \lambda = 0.10 \text{Å}$) where intensity oscillations are found suggests that the oscillatory intensity fluctuations have the same size as the velocity oscillations.
This evidence for a difference in size between the non-oscillatory component of the small-scale brightness field and the oscillatory velocity field is admittedly indirect. It draws support, however, from the recent observation of Edmonds, Michard, and Servajen (1965) that the spatial coherence between the predominantly oscillatory velocity field in weak lines and the intensity fluctuations in the same lines is large only for horizontal wavelengths around 3000 km. For larger wavelengths, even though there is still much power in both velocity and intensity spectra, the coherence drops off.

Further, the same authors tentatively conclude from their spatial power spectra that the oscillatory component of the velocity field dominates over the convective component for horizontal wavelengths greater than 4000 km, and that the convective component dominates for shorter wavelengths. The dichotomy in size between these two regimes of flow would therefore appear to be visible in both the velocity and intensity fields.

![Graphs showing mean-square line-of-sight velocity ($V_z^2$) as measured at various values of $\theta$ for three spectral lines. Abscissa: $w = \cos^2 \theta$. Intercepts at $w = 0$ and $w = 1$ are $\langle V_{\text{bort}}^2 \rangle$ and $\langle V_{\text{vert}}^2 \rangle$, respectively (Noyes, 1963).](image)

**Fig. 6.** Mean-square line-of-sight velocity ($V_z^2$) as measured at various values of $\theta$ for three spectral lines. Abscissa: $w = \cos^2 \theta$. Intercepts at $w = 0$ and $w = 1$ are $\langle V_{\text{bort}}^2 \rangle$ and $\langle V_{\text{vert}}^2 \rangle$, respectively (Noyes, 1963).
Finally, several sets of measures have given information about the relative magnitude of the vertical and horizontal components of the small-scale velocity field (e.g., Evans and Michard 1962; Noyes 1963). For example, Figure 6 (Noyes 1963) illustrates measurements of the mean-square line-of-sight velocity in three spectral lines as a function of position on the disk. These measurements were made in such a way as to include only the small-scale (less than 6000 km in size) elements of the field. The data have been plotted in such a way that when least-square fitted by a straight line, the intercepts of that line with the right and left edges of the figure should yield $V_{\text{vert}}$ and $V_{\text{horiz}}$, respectively. From the graph we find the results given in Table 2.

### Table 2

<table>
<thead>
<tr>
<th>Line</th>
<th>$\langle V_{\text{horiz}} \rangle$</th>
<th>$\langle V_{\text{vert}} \rangle$</th>
<th>$\langle V_{\text{horiz}} \rangle / \langle V_{\text{vert}} \rangle$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Fe 6102, $\Delta \lambda = 0.10 , \text{Å}$</td>
<td>0.25 ± 0.02</td>
<td>0.34 ± 0.06</td>
<td>0.73 ± 0.16</td>
</tr>
<tr>
<td>Ca 6103, $\Delta \lambda = 0.10 , \text{Å}$</td>
<td>0.13 ± 0.01</td>
<td>0.21 ± 0.02</td>
<td>0.62 ± 0.04</td>
</tr>
<tr>
<td>Na 5896, $\Delta \lambda = 0.11 , \text{Å}$</td>
<td>$-0.04 \pm 0.03$</td>
<td>0.47 ± 0.05</td>
<td>$-0.09 \pm 0.08$</td>
</tr>
</tbody>
</table>

In summary, the observations suggest that a velocity oscillation begins as a small-scale disturbance, which increases in size as it propagates upward. The oscillatory component of the intensity field is probably coherent with the velocity oscillation and thus also increases in size with height. The non-oscillatory brightness fluctuations appear to have a smaller scale.

The r.m.s. horizontal velocity is comparable to the vertical velocity at low levels, but decreases with height, while the vertical velocity increases with height. At the level of formation of Na 5896 ($\Delta \lambda = 0.1 \, \text{Å}$) the small-scale velocity is almost purely vertical.

**(e) The Influence of Magnetic Fields on the Oscillation**

Magnetic fields seem to alter the character of the oscillations both in plages and within the chromospheric network:

(i) Leighton, Noyes, and Simon (1962) found that the amplitude of the velocity oscillation is decreased by about 20% inside the plage relative to that outside the plage. Orrall (1965), on the other hand, finds no evidence for a difference in velocity amplitude in the Fe 3931 line, in regions of the chromospheric network or in weak plages. This point needs further study.

(ii) Orrall (1966) does find a marginal indication that the period of the oscillation in the Fe 3931 line is slightly longer under the chromospheric network. In the network itself, as seen in the core of the K line, he observes significant and fundamental differences in the oscillation. Individual, regular oscillations of the position of K$_a$ core are observed in the network with periods as long as 600 s. In a plage he found a regular oscillation with a period of 900 s. Such oscillations produce a peak in the power spectrum at about 1000 s, as may be seen in Figure 1.

It is to be emphasized that this long-period oscillation does not reflect simply the
extra power introduced by a broadening of the power spectra at high altitudes. Orrall points out that the long-period oscillations are 'among the most regularly harmonic of all oscillations observed'. In other words, the presence of significant power at these low frequencies is due to the presence of a rather sharply peaked power spectrum, perhaps analogous to that in the 'resonance' range, but shifted to much lower frequencies.

Further, Orrall finds a significant positive correlation between the period and the $K_2$ intensity. Since it is well established that the $K_2$ intensity and vertical magnetic field are very well correlated, we infer that the period is greatest where the vertical magnetic field is strongest. There is also a marginally significant negative correlation between the velocity amplitude in $K_3$ and the intensity of $K_2$, as if the magnetic field tended to inhibit the oscillation just as it does in strong photospheric plages.

3. SUPERGRANULATION AND RELATED CHROMOSPHERIC PHENOMENA

(a) General Characteristics of the Supergranulation

The solar supergranulation is a pattern of cells of moving material, distributed uniformly over the quiet solar surface (Figure 7). The characteristic dimension of these cells is about 30,000 km, the characteristic velocity about 0.4 km s$^{-1}$, and the characteristic lifetime about 20 hours. Suggestions of the existence of such a velocity pattern have been reported in the past (e.g., Hart 1954, 1956)* but it was not until Leighton's 'Doppler Spectroheliographic' method was developed at Mount Wilson that the true nature of the motions became clear (Leighton, Noyes, and Simon 1962). Subsequently Simon, in his thesis, investigated the properties of supergranulation in detail, and his paper with Leighton (Simon and Leighton 1964) is basic to this discussion of the phenomenon.

Figure 7 is a Doppler spectroheliogram in the Na 5896 line, in which lighter-than-average areas indicate velocities of approach, and vice-versa.† The large, quasi-circular cells are plainly in evidence everywhere except at the center of the disk. This implies that the velocities are primarily horizontal. Near the limb of the Sun, the shape of the cells becomes elliptical, presumably due to projection effects. The polarity of the velocity field is always such that the edge of a cell toward the center of the disk shows a velocity of approach, and vice-versa. Thus we infer that the motion is one of horizontal outflow from a center.

(b) Physical Dimensions of Supergranulation

Simon and Leighton (1964) have studied the size distribution of the 'cells' of the supergranulation flow field, using a number of methods:

(i) direct microphotometry of individual cells;

(ii) measurement of the half width of spatial autocorrelation functions of the velocity field;


†The figure is actually a composite of two velocity spectroheliograms exposed 150 s apart, so that the oscillatory component of period 300 s is suppressed relative to longer-lived motions.
FIG. 7. Doppler spectroheliograms showing the solar supergranulation. Lighter-than-average areas indicate velocities of approach, and vice versa.
(iii) measurement of the spacing of cells from the position of secondary maxima of the autocorrelation function, and assumption that the cells are closely packed;

(iv) similar measurement of the spacing of the elements of the Ca+K network, which we will see to be well correlated with supergranule boundaries.

All of these methods converge on a cell size of about 32 000 km. Further, it appears that the distribution of individual sizes about this average is rather narrow, as evidenced by the pronounced secondary maxima seen in the autocorrelation curves. It is interesting that the cell size appears to be so well defined. This size is a fundamental parameter which must be basic to any theoretical interpretation of the motions.

Simon and Leighton (1964) do observe an apparent difference in the average cell size from point to point on the disk. They conjecture that there is a local characteristic size for the phenomenon, but that this local size varies with position on the Sun, perhaps being dependent on latitude, strength of the mean magnetic field, or some other effect. This very important point needs closer study.

The supergranulation velocity pattern has been observed mainly in medium-strong Fraunhofer lines. It reaches its maximum visibility in the Ba+4554 line, although it is visible in both somewhat weaker (Fe 6102, Ca 6103) and somewhat stronger (Mg 5172) lines. The velocity pattern in the still stronger Hα and Hβ lines is, as we shall see, completely different. It has not yet been seen in lines weaker than Fe 6102, but no systematic study has been made.

No evidence has appeared for a change either in cell size or velocity and amplitude with height. However, the spread in individual measures of both these quantities is large enough to mask any reasonable variations.

(c) Velocity Structure and Lifetime of the Supergranulation

Figure 8 is a microphotometer tracing across one of the cells, showing the horizontal velocity variation within the cell. (The ordinate is 1/sin θ times the line-of-sight velocity.) The horizontal velocity has a peak value of about 0.38 km s⁻¹. Tracings by Simon over 45 such cells yielded an average peak horizontal velocity of 0.42 ± 0.13 km s⁻¹.

The up or down motions expected in the supergranulation cells or at their periphery, and required by considerations of continuity, are very difficult to observe. Their magnitude is certainly comparable to or smaller than the noise level in either direct tracings or autocorrelation curves. (Part of this noise is due to photographic grain and errors in registration in producing Doppler plates, but the major part is the presence of the small-scale oscillatory field, with vertical velocities several times larger than those in the supergranulation.)

Nevertheless, suggestions of an upward motion at the center and a downward motion at the edges of large cells appear upon direct examination of the highest quality Doppler spectroheliograms. Simon and Leighton (1964) have verified this velocity pattern statistically by observing a small negative correlation between the velocity field at the center of the disk and the chromospheric calcium emission network, which we shall soon see is well correlated with supergranulation boundaries. From spectra later obtained at Sacramento Peak, Simon (1964) obtained a correlation coefficient of between 0.1 and 0.2 between downward motion in Fe 3931 and intensity in Ca+K. Also, from direct tracings of single cells on velocity spectroheliograms, Simon and Leighton (1964) have determined a peak vertical velocity in the supergranulation of from 0.1 to 0.2 km s⁻¹.
Fig. 8. Microphotometer tracing across an individual supergranulation cell, perpendicular to the solar limb (Simon and Leighton, 1964).

The lifetime of the supergranulation has been crudely estimated to be many hours from comparison of Doppler spectroheliograms obtained from one to several hours apart (Leighton, Noyes, and Simon 1962). A more precise measure of the lifetime of the Ca* network has been made by Simon and Leighton (1964). There are good grounds for equating the lifetimes of the Ca*K network and the supergranulation pattern, as we shall shortly discuss.

The lifetime of the Ca*K network, defined as the time for its correlation with itself to drop to 1/e, is found by Simon and Leighton to be about 20 hours. This is in good agreement with Macris (1962), who followed the time history of individual flocculi. The agreement between the difficult methods suggests that the cells actually disappear or break up in this characteristic time, rather than reduce their correlation by simple changes of position or shape.

It would be most interesting to investigate the details of the velocity field during the birth or death of an individual supergranule. This has not yet been attempted.

(d) The Ca* Chromospheric Network and Magnetic Fields

It is well known that spectroheliograms in strong metallic lines show a 'network' pattern of enhanced emission over the quiet regions of the solar disk (Figure 9). The size of the 'mesh' of the network is quite similar to the size of the supergranulation cells, and thus it is a natural speculation that the two are related. Simon and Leighton (1964)
Fig. 9. Ca$^+K_2$ spectroheliogram showing the chromospheric emission network.
have studied this problem in detail, and find a very good correlation between the network in Ca\textsuperscript{+}K and the supergranulation, in the sense that the 'strands' of the network enclose individual cells of the supergranulation pattern, i.e., the Ca\textsuperscript{+}K emission occurs at the edge of supergranulation cells.

The already well-established one-to-one correlation between regions of enhanced Ca\textsuperscript{+}K emission and regions of magnetic field suggests that magnetic fields are also concentrated at the boundaries of the supergranulation cells. This has indeed been found to be the case, by direct comparison between Mount Wilson magnetograms and Ca\textsuperscript{+} spectroheliograms.

The actual strength of the vertical magnetic field at the supergranulation boundaries is still an open question. Howard (1962) measured fields of strength 10 to 20 G in the supergranulation with an aperture 2 seconds of arc on a side. However, the best Mount Wilson Zeeman spectroheliograms suggest that the width of the magnetic regions between the supergranulation may be smaller than 1 second of arc (Simon and Leighton 1964), so we might expect from Howard's measurements that the true field is at least 40 to 80 G, and could be considerably more.

(e) The Chromospheric Network in Hz

It is well established (e.g., Deslandres 1910) that when seen under moderate resolution, the chromospheric network appears as a dark network in the Hz wings ($\Delta\lambda \gtrsim 0.5$ Å) and a bright network in the Hz core. In other words, dark features in the Hz wings, bright features in the Hz core, and bright features in the Ca\textsuperscript{+}K core are all correlated.

Under similar conditions of moderate resolution, the velocity field in the Hz wings is also correlated with the Ca\textsuperscript{+}K network (Leighton, Noyes, and Simon 1962). At $\Delta\lambda = 0.7$ Å, the velocity field consists of narrow 'funnels' of downward-flowing material located at the position of the K network, while between these regions the material is rather quiescent. Simon and Leighton (1964) have measured typical velocities in the 'funnels' of $-1.2$ km s\textsuperscript{−1}. A similar velocity field appears in Hβ.

When seen under conditions of very good resolution, both the intensity and velocity fields in Hz exhibit great complexity, and the above statements require modification. We shall discuss the point in more detail at the end of the next section.

(f) Spicules and their Relation to the Supergranulation

(i) Spicules at the limb

Spicules are narrow, spike-like features with lifetimes less than 30 minutes, seen outside the solar limb in the Hz line. They have typical lengths of 5000 to 10 000 km and widths of about 1000 km. They are also visible in the other Balmer lines, in emission lines of neutral and ionized helium, in lines of ionized metals (notably calcium), and in the infrared oxygen triplet at $\lambda 7772$.

Observations by Dunn (1960) of spicules with a birefringent filter centered on the Hz line have yielded much information on the geometry and kinematics of spicules at the limb. Direct counts of the number of spicules seen on the limb plus corrections for the overlapping of spicules in the line of sight led Athay (1959) to an estimate of about 10\textsuperscript{6} spicules on the entire solar disk at an altitude of 3000 km. The number decreases by a factor of 10 at 10 000 km, and by a further factor of 10 at 14 000 km. Below 3000 km the overlapping of spicules makes counting impossible.
SOLAR VELOCITY FIELDS

For comparison, the number of granules on the Sun is about $3.6 \times 10^8$ (Rösch 1959; Schröter 1962).

Dunn (1960) measured an average half width of about 800 km for spicules, with some as narrow as 200 km and some wider than 1500 km.

Observations of the kinematic behavior of spicules (Rush and Roberts 1954; Lippincott 1957) show that they appear to rise from the 5000 km level with a velocity of about 20 km s$^{-1}$ to their maximum height of about 12 000 km. After reaching maximum height, 40% fade at that height and 60% appear to recede with velocities comparable to that of the rise. The mean lifetime of the event is about 5 minutes. If the rise and fall is extrapolated to the photosphere, the mean lifetime becomes 10 to 15 minutes. Lippincott observed that sometimes after a spicule descends, another one rises at the same place, as if the phenomenon were repetitive.

Rush and Roberts (1954) suggested that the abrupt stop of the spicular motion at about 12 000 km is only an apparent effect due to the rapid ionization of hydrogen by the corona above that level; the spicular material actually continues streaming, invisibly, outward into the corona at a constant velocity. If the upward flux of matter at the base of the spicule decreases, the spicule appears to fade. If the upward velocity of the material at the base of the spicule decreases, the spicule appears to descend.

Athay and Thomas (1957), on the other hand, showed that due to uncertainties in the observed kinematics of spicules, a constant deceleration of the spiculae under gravity and its eventual return to the solar surface (either before or after fading) could not be excluded.

The question whether spicules do or do not return to the low chromosphere is of great importance to the problem of energy and mass balance of the corona, and yet it has still not been resolved. The only true material velocity which can be measured is the line-of-sight component, which is horizontal at the limb, and which need not bear any straightforward relationship to the more important vertical component. Nevertheless, recent observations have been obtained of the time variation of line-of-sight velocity in spicules, in the hope that they will be of some use in describing the vertical velocity also. Mouradian (1962) has measured Doppler shifts in H$\alpha$ spectra and found that in none of the spicules observed did the velocities change sign. He concludes that the spicular material rises into the corona and does not redescend, at least during the time the spicule is visible in H$\alpha$. Ravi Bhavilai (1965), from simultaneous filtergrams on the red and violet wings of H$\alpha$, also concludes that the line-of-sight component of velocity, and by inference the vertical velocity, does not change sign during the visible lifetime of the spicule. On the other hand, recent observations obtained at Sacramento Peak Observatory (Beckers, Noyes, and Pasachoff, 1966) show several cases of apparent line-of-sight velocity reversals in the Ca$^+\ H$ and $K$ lines and in H$\alpha$. Further observations seem necessary to resolve this discrepancy.

Some additional information on spicule dynamics has been gained from the Sacramento Peak observations. An image slicer was used to obtain spicule spectra simultaneously at two heights above the limb, separated by about 3000 km. Preliminary analysis of the data indicates that in the Ca$^+\ H$ line the line-of-sight velocity at the higher level is almost always greater than at the lower level at the same instant of time (Beckers, Noyes, and Pasachoff 1966). This result is in contrast to that of Michaud (1956) who found just the opposite result in H$\alpha$ over a height difference of 800 km.
(ii) Spicules on the disk

A number of authors have made attempts to identify the limb spicules with features seen on the disk. At the present time there seem to be two schools of thought as to which features on the disk correspond to spicules at the limb. One school holds that spicules are the fine dark mottles best seen in the wings ($\Delta \lambda > 0.5 \text{Å}$) of H$_\alpha$ in filtergrams on the disk. This interpretation has been advanced by de Jager (1957), Macris (1957), Kiepenheuer (1957), Bruzek (1959), Cragg, Howard, and Zirin (1963), and most recently and comprehensively by Beckers (1963, 1964).

The other school holds that spicules are bright structures seen on the disk in H$_\alpha$ in close association with the dark fine mottles, but physically distinct. On the disk they are best seen in filtergrams in the H$_\alpha$ core, but they appear bright relative to their surroundings at all parts of the H$_\alpha$ line. This view has been advanced by Giovanelli (1964) and Rawi Bhavilai (1965).

We do not intend in this review to express a preference for either of these interpretations, for it would appear that we are dealing with an extremely difficult observational problem, and in the author's opinion more observations are necessary to decide the question. However, we do wish to emphasize one aspect the two interpretations have in common, and on which there is general agreement: Spicules, when seen on the disk, do not show a random distribution, but appear in clusters. These clusters invariably coincide with the chromospheric emission network as seen in the K line; by extension, the clusters occur above the boundaries of the supergranulation cells. In the H$_\alpha$ line, the clusters appear as groups of fine bright and dark mottles that are visible only under the best resolution; under moderate resolution, the clusters make up the 'coarse H$_\alpha$ network' that is congruent to the K network.

Under very good resolution, the individual dark and bright fine mottles which make up the individual clusters are found to have a definite morphology. At the center of the disk, both bright and dark fine mottles appear as somewhat elongated structures grouped more or less radially about a bright center. Beckers (1963) has named these clusters 'rosettes'. When seen close to the limb, the mottles are elongated toward the limb, and the clusters appear as 'bushes' in the terminology of Cragg, Howard, and Zirin (1963). There seems to be little doubt that 'rosettes' and 'bushes' are the same structure seen at different angles. Quite possibly they are also identical to the 'porcupine' structure of spicules at the limb, described by Lippincott (1957) as a 'dome-like rise in the general chromospheric level about 18 000 km across, from which spicules protrude radially, as in a porcupine'.

Figure 10 illustrates how the appearance of the fine structure of the chromosphere varies with position in the H$_\alpha$ line (Beckers, 1964). The dark mottles, grouped into 'rosettes', are best visible at $\Delta \lambda = +0.6 \text{Å}$, while the bright mottles, grouped into the same rosettes, are best visible at the line center. The figure also shows how the rosettes group together to form the 'coarse H$_\alpha$ network', which is best visible as a bright network at the line center and as a dark network at $\Delta \lambda > 0.6 \text{Å}$. The size of the network is about 40 seconds of arc, or about 30 000 km.

Beckers (1964) finds that on the average there are about 60 fine dark mottles per rosette. Since bright and dark mottles appear to be grouped more or less alternately, a similar figure should hold for the bright mottles. Using this number, plus the lifetime of the dark mottles (which is observed to be the same as that of limb spicules) Beckers
Fig. 10. Filtergrams exposed at various points in the Hα line profile, showing the variation with $d\lambda$ of the contrast of bright and dark fine mottles (Beckers, 1964).
has calculated the 'birth rate' of the mottles within a rosette, and has found it to be comparable to that of the photospheric granules that underly the rosette. Thus, if limb spicules appear as dark mottles on the disk, granules and overlying spicules have comparable birth rates.

No estimate of the lifetime of bright mottles has been published. However, if they are the same features as limb spicules, they should have spicular lifetimes, and the same argument that Beckers applied to dark mottles should apply to bright mottles also.

In summary, recent observational evidence indicates that spicules, whose properties are rather well known from limb observations, occur on the disk in clusters, whose position coincides with the chromospheric network and hence with the boundaries of the supergranulation and with regions of enhanced magnetic field. The birth rate of spicules within this region is probably comparable to that of granules in the underlying photosphere. It is not yet clear whether the dark or bright fine mottles within the clusters are to be identified with spicules.

4. SUMMARY AND CONCLUSION

(a) Relationships between Velocity Fields in the Photosphere and Chromosphere

In the previous two sections we have discussed recent observations of two apparently rather different velocity fields on the Sun—the small-scale photospheric oscillatory motions and the large-scale supergranulation flow with its concomitant chromospheric phenomena. We shall now briefly summarize these observations and then try to see how they are related.

(i) Granulation and oscillatory motions—Summary

The appearance of the bright granule in the continuum seems to foreshadow the onset of an oscillatory motion in the overlying stable layers. It is not clear whether the bright granule causes the oscillation or whether both events are results of some previous event that occurred deeper in the convection zone. At low levels the motions in the convectively stable zone contain not only predominant power in the 'resonance' frequencies corresponding to a period of 300 seconds, but also significant power in the low frequencies characteristic of the brightness fluctuations in the granulation. As the height of observation is increased, this low-frequency tail of the power spectrum decreases in strength and the 'resonance peak' shifts slowly toward higher frequencies. Also, power increases in the 'high-frequency tail,' corresponding to periods of about 150 seconds.

In plages and the chromospheric network, i.e., regions of magnetic field, the average period for the strong Ca+K line is greatly lengthened, to as much as 15 minutes, without losing its sharply peaked power spectrum. There is a definite positive correlation between period and Ca+K intensity, and therefore magnetic field strength. Weaker lines show little or no correlation between period and Ca+K intensity.

The amplitude of the oscillation increases with height, from 0.25 km s⁻¹ in weak lines formed at the level of the continuum to 1.6 km s⁻¹ in the chromospheric Ca+K line. In plage regions the amplitude is diminished below that of the normal atmosphere.

The brightness fluctuations appear to be made up of two components: a non-oscillatory one associated with brightness in the continuum, and an oscillatory one coherent with the velocity oscillations and leading them in phase by about 90°.
The size of the oscillatory elements of the velocity field increases rapidly with height, from sizes close to that of the granulation for weak lines to several times that size a few hundred kilometers higher. The non-oscillatory (convective) component of the velocity field is smaller in size than the oscillatory component. The size of the oscillating brightness elements seems to increase with height along with the oscillatory velocity field, but the non-oscillatory brightness component does not.

(ii) Supergranulation and related chromospheric velocities — Summary

The large-scale supergranulation flow exists in the same region of the atmosphere in which the small-scale oscillations are found. The velocity amplitude of the two types of motions is similar—about 1/2 km s\(^{-1}\). However, the scale of the supergranulation flow is some ten times larger—about 30 000 km; and the lifetime is some hundred times longer—about 20 hours. The supergranulation flow is well ordered, consisting of horizontal outflow from the center of circular cells, with slowly rising material at the center and/or slowly descending material at the edges. In the medium chromosphere as seen in the H\(\alpha\) wings, the velocity is nearly zero except at the cell boundaries, where a downward motion of about 1 km s\(^{-1}\) is seen. Contrary to the small-scale photospheric granulation, which is seen by virtue of its brightness fluctuations, the supergranulation is revealed mainly by its velocity field. The brightness variation across a supergranule is difficult to observe.*

Photospheric magnetic fields are found to be strengthened to of the order of 100 G at the supergranulation boundaries, i.e., some hundred times the average field in the quiet Sun. Emission in the Ca\(\text{II}\)K line is correlated with magnetic field strength, and forms the well-known ‘chromospheric network’ which traces out the boundaries of the supergranulation cells. In H\(\alpha\) the chromospheric network appears as a bright network in the core and a dark network in the wings. With the best image quality, the H\(\alpha\) network is resolved into clusters of fine bright and dark mottles. The properties of these mottles suggest that either the bright or the dark mottles are the same features as spicules seen on the limb. In either case, the spicules would seem to occur preferentially in clusters that are located above supergranulation boundaries in regions of strong magnetic fields. Birth rates of spicules within clusters and of photospheric granules beneath clusters are comparable.

Spicules seen on the limb seem to rise with typical velocities of 20 km s\(^{-1}\), and then either fade at maximum height or fall back with similar velocities. Recent observations of Doppler shifts show that at least in some cases the line-of-sight motion of the material actually does reverse, but there is some disagreement with earlier observations in H\(\alpha\).

(iii) The relationship between granulation and supergranulation velocity fields

Figure 11 is a highly schematic sketch (to scale) of the various velocity fields and related phenomena we have summarized above. The figure illustrates the two scales involved: On the one hand are the small-scale phenomenon, granulation and oscillations; and on the other hand are the large-scale supergranulation chromospheric emission network, magnetic fields, and spicule clumps. The question naturally arises: Are the phenomena associated with these two scales more or less independent, or are they

*However, a very recent observation by Simon (1965) of the correlation between granulation and H\(\alpha\) spectroheliograms indicates a weak correlation, in the sense that there is decreased brightness of the granulation at supergranulation boundaries.
FIG. 11. Schematic sketch of the granulation-supergranulation-spicule complex, seen in cross-section. A. Flow lines of a supergranulation cell. B. Photospheric granules. C. Wave motions, observable as oscillations of the velocity field in the photosphere and low chromosphere. D. The large-scale chromospheric flow field seen in Hα. E. Lines of force, pictured as uniform in the upper atmosphere (corona) but concentrated at the boundaries of the supergranules in the photosphere and chromosphere. F. The base of a spicule 'bush' or 'rosette,' visible as a region of enhanced emission in the Hα- and K-line cores. G. Spicules. According to the interpretation of Beckers (1964) and others, these are the dark mottles when seen on the disk, while the bright mottles on the disk are simply the underlying base of the spicule bush (F) visible between the spicules. According to the interpretation of Rawi Bhavilai (1964) and Giovanelli (1964), the spicules are bright mottles on the disk, while the dark mottles are loops of material (not shown) arching upwards from regions outside the bushes and then downward again inside the bushes. The scale of the illustration is such that the distance between the bushes is 30 000 km.

different aspects of a single, unified process? It is obvious that there must be some interrelation, for the two velocity fields exist in superposition in the photosphere and low chromosphere. Thus it is not surprising that the granules seem darker at supergranulation boundaries than elsewhere (see footnote, p. 316). The apparent lengthening of the preferred oscillation period in regions of the chromospheric network is another interesting illustration of an interaction between the two velocity fields.

Perhaps the spicules themselves owe their existence to an interaction between the granulation and the supergranulation. We have seen that spicules occur only in regions of strong magnetic field at supergranulation boundaries and may require the presence
of the magnetic field that is there. More basically, they would require the existence of the supergranulation flow field, which probably creates the stronger magnetic field at its boundaries by sweeping the ambient field to the edges (Parker 1963). It is possible that spicules are related also to the small-scale granulation, as the correspondence in birth rates might suggest. Thus it may be that the most characteristic chromospheric phenomena, spicules and the related chromospheric network, owe their existence to the interaction of the granulation and the supergranulation flow fields.

Whether or not such an interpretation proves to be correct, the question arises: Is there a causal relation between the two dominant photospheric velocity fields, the granulation and the supergranulation? Is either necessary for the existence or character of the other? For example, does the supergranulation cause the small-scale granulation (by shear-flow instability, for instance) and thus, indirectly at least, give rise to all the phenomena we have discussed here?

The magnetic field concentration is not the basic phenomenon, for it is almost certainly created by the supergranulation flow (Parker 1963). The granulation is not basic, for it could not create such an ordered flow over a dimension as large as the supergranulation cells. The only acceptable alternative is that the supergranulation and granulation are independent phenomena, which unite to create all the observed velocity fields in the solar chromosphere.

(b) Questions for Future Investigation

Finally, we wish to summarize what in our view are the most pressing questions which have been raised by the observations discussed above.

(i) Observational problems

(a) In order to investigate the relation between granulation and supergranulation, the following observations seem appropriate:

(1) Determine the magnitude of the intensity fluctuation $\Delta I/I$ in the supergranulation. This appears to be surprisingly small—the supergranulation is invisible in a direct photograph of the Sun.

(2) Try to detect whether granules are carried in the supergranulation flow. This is a very difficult observation, since the granules should be carried only a small fraction of their diameter during their lifetime.

(3) Follow the evolution of individual cells of the supergranulation pattern, in order to see the details of their formation or decay.

(4) Investigate the supergranulation velocity field in very weak lines (e.g., C 5052) that are formed at the level of the photospheric granulation.

(b) Much needs to be done in the way of disk observations of the high chromosphere:

(1) The identification of spicules on the disk should be firmly settled; undoubtedly the direct comparison of features outside and inside the limb, as done by Rawi Bhavilai (1965) and Giovanelle (1964), is the correct approach.

(2) Much more study is needed of the properties of disk spicules. For instance, could one, through simultaneous observations of $H\alpha$ mottles and white-light granulation,
hope to observe a direct relation between some event in the continuum and the development of a spicule?

(3) Observations (above the Earth's atmosphere) of the spatial distribution of emission in ions of high excitation—for example C III, O VI, Mg X— are needed, to see if they also outline the chromospheric network. This will tell us much about whether the chromospheric and coronal heating is localized in regions above spicule structures.

(c) Further limb observations of spicule spectra are needed to determine more definitely the dynamics of spicules; e.g., what fraction redescend after rising into the chromosphere, and what fraction 'evaporate' into the corona, depositing their mass there?

(ii) Theoretical problems

(a) We have already indicated our belief that one of the most pressing theoretical problems is to explain the existence of the supergranulation and its relation (if any) to the ordinary granulation. Why is the size of supergranules confined to the range of about 30 000 km? Why is the temperature fluctuation over a supergranule so small (aside from the fluctuation directly related to the granulation)?

(b) Concerning the oscillatory motions themselves, the question of whether motions in the granulation represent the sources or whether the granulation and oscillation are responses to a deeper-lying source is still unsettled. The development of the 'high-frequency tail' of the velocity power spectrum continues to be unexplained. This is important to understand, for the variation of the power spectrum with height may give information about the temperature profile in the atmosphere just above the temperature minimum.

The relation between the predominant period of the oscillation and the strength of the magnetic field (Section II-E) should be investigated.

(c) Concerning the spicules, the paramount question still is, of course, what 'causes' a spicule? We have indicated how the observations might suggest that spicules are caused by the interaction in the photosphere between the granulation and the supergranulation. If so, how are the effects of this interaction transmitted to the chromosphere and how does this create a spicule?

Finally, what is the role played by spicules in heating of the corona? Are spicules instrumental in determining the structure of the corona, or are they after all merely an intrinsically interesting but relatively unimportant phenomenon?

Most of the questions raised here are familiar and have already received considerable attention in the literature. The author hopes, however, that some of the recent observations described above may provide new and helpful insight into old problems.

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References

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Discussion

(Ed. note: Noyes asked that discussion from the floor during his presentation be held to a minimum. What questions there were, were mainly aimed at clarification. Most have been incorporated in the written draft of his talk printed here; those which involved inter-change have been embedded in the discussion following the presentation, in the appropriate place. Michard attempted to direct the discussion chronologically toward the various parts of Noyes' summary-introduction; in editing, I have reordered when useful to accomplish this purpose.).

Athay: Could you clarify your comment that the interpretation of the $\Delta \lambda$ measures in terms of velocities is relatively straightforward, even though the interpretation of $\Delta I_x$ in terms of meaningful physical quantities at the point of origin of $I_x$ may not be so obvious?

Noyes: I can only repeat that the basic assumption underlying the velocity interpretation is that the shifted and unshifted line-profiles are either symmetrical or, if not, that they have the same asymmetry.

Jefferies: What you measure is the difference between the intensities at two different wavelengths. What this means in terms of atmospheric parameters is another question.

Spiegel: Could I ask the non-LTE people whether there is any specific example of a calculation made to show what effect on any observed velocity field these non-LTE effects will have, and whether it is truly appreciable?

Thomas: Such calculations have only been commenced, in a rudimentary way, not sufficiently detailed as yet to be pointed to as guideposts. Kulander wrote a thesis with Jefferies at Colorado recently, in which he began such calculations. I think the only point I would make here, is that, as Noyes points out, one has to assume essentially invariance of profile under displacement due to the velocities for this method to be applicable; and as yet we don't know enough to say whether this assumption is good or bad.

On the interpretation of the intensity, I would only emphasize that even if the line is collisionally controlled, and so $\Delta I_x$ and the associated $\Delta S_x$ at $\tau_x \sim 1$ reflect fluctuations in collisional source-terms, it is not necessarily true that these fluctuations occur at $\tau_x \sim 1$. The value of $S_x$ at $\tau_x \sim 1$ depends upon the distribution of collisional source-terms over a sizeable volume centered at $\tau_x \sim 1$. But this is a point I have continually made throughout this Symposium, so I won't elaborate here beyond simply mentioning it, in the light of Noyes' remark, as a caution.

Krook: I thought the strength of the Leighton technique was that it was independent of all these considerations, because you take a line that is presumably symmetric and just measure the displacement of the line. Isn't it independent of line-formation theory?

Noyes: Only by assumption, thus far. Work remains to be one on the theoretical basis of the assumption, as just mentioned by Jefferies and Thomas.

Spiegel: To serve as orientation between the static atmospheric structure and the kinematic phenomena discussed, could you say where the atmospheric temperature minimum is in terms of the various kinds of lines whose behavior you have discussed?
DISCUSSION

Noyes: Probably the temperature minimum occurs near the height of formation of the line \(\lambda 8514\) of Fe I. This line, I have characterized as a medium-strong Fraunhofer line, which shows the sharp peak in power near \(T = 300\) s.

Lighthill: When we look at the power spectrum of the oscillatory velocities you discuss, are these velocities primarily vertical on the Sun?

Noyes: Yes, primarily vertical, because the observations discussed here were made near the center of the disk.

Athey: Is the power at zero frequency mainly in non-periodic phenomena?

Noyes: There is probably a long-term trend to the velocity field, due to a slow evolution of the velocity field associated with the chromospheric network. Therefore at least some of the power at near-zero frequencies represents non-periodic motion. (The linear part of this velocity trend has been artificially removed from the data for the Ca+K line.)

Evans: This increase in power toward the low frequencies is difficult to interpret in terms of whether or not we have periodic motion here. We assume that this is not a periodic motion; but I have not been able to satisfy myself that we really have a reason for this assumption.

Michard: Well, this is just the power spectrum of the actual time fluctuations. It is a representation of the time fluctuations, by a system of periodic motions with a continuous spectrum of periods, and this is the result. Of course, if you have phenomena which evolve slowly, then you get power at low frequencies. I can only say that I think this is a fair representation of the actual phenomena.

Lighthill: In the case of turbulence, you always have a zero frequency component; so if you are actually penetrating down to the convective layer in your observational material, you would expect to see some turbulence-type spectrum that does not vanish at zero frequency.

Noyes: This last is the kind of an interpretation one would like to make.

Schatzman: What is the range of density from the bottom to the top of the atmospheric region you observe?

Noyes: If we assume the center of the K line to be formed at a height of 4000 km above the continuum (cf., e.g., de Jager 1959), then the density ratio between top and bottom of the atmospheric region observed would be less than \(10^{-4}\), according to most chromospheric models (cf., e.g., Pagel 1964).

Moore: How does the kinetic energy density change with height?

Noyes: Multiplying the density ratio by the ratio of \(\xi^2\) in the K line and in the weak photospheric line (cf. Table 1), we find \(\rho \xi^2(K)/\rho \xi^2\) (phot) \(\sim 0.004\). A similar calculation using the Ca+ \(\lambda 8542\) line (ascribed to a height about 1400 km) gives a value about 0.02 for this ratio. Thus it appears that the kinetic energy density falls off rapidly with height.

Spiegel: In your discussion of the phase differences between different lines, and the evidence for upward propagation of the oscillatory motion, do you have any indications from the observations of any horizontal motions? Does any phenomenon appear to move
along the slit, in addition to whatever vertical motion you infer from the differential behavior of lines?

Noyes: This is a very difficult question, observationally, because small guiding errors can introduce spurious motions along the slit. The Sacramento Peak movies showing the time-development of the oscillation suggest that there is horizontal propagation, with a rather large phase velocity, possibly as large as 50 km s\(^{-1}\). However, the evidence does not as yet appear to be statistically significant.

Lighthill: I have a point on this question of phase differences between lines at different heights reflecting upward propagation. The fact that there is no phase difference between two lines at different heights does not mean that there is no propagation of energy. It merely means that the vertical component of the group velocity is zero. There may be propagation, because these two velocities are at an angle to one another in many types of wave.

Dubov: Do I understand correctly that the differential phase lag between several spectral lines decays monotonically to zero; and once it has reached zero, it stays there?

Noyes: Yes, at least so long as it has been followed in the observations made to date.

Athay: In connection with this question of the duration of an oscillation, and Zirker’s observations in particular, how can you really be sure from the data presented that the long duration is to be associated with a single oscillatory cell? If pulses are initiated at random, there are bound to be many places on the Sun where successive pulses are generated in phase. This will give the appearance of a long-period modulation of a short-period phenomenon.

Michaud: These things are very exceptional. In the case shown in Figure 3, you have a clear change of phase in the oscillation at about half the interval of observation. Oscillations of duration greater than about 2 periods occur very rarely, and only by chance adjustment of the phase in two successive oscillations. I think Howard also believes now that his observed long-duration event was only accidental. I have also made a check of this point on the autocorrelation curves of the velocity, which sometimes show a long oscillating tail, showing very little damping. However, by using sub-samples of our data, we see that the long tails of the autocorrelation appearing in two samples do not correlate at all. They are out of phase completely, which show that these long tails are without statistical significance.

Clauser: If the curve in Figure 3 is not typical, then what does a typical curve look like?

Noyes: Figure 3 is atypical in the regularity of the oscillation; more usually the motion undergoes a change in amplitude or phase after 1 to 3 cycles. Also, the velocity variation during a single cycle is usually not as smoothly sinusoidal as this example. Apparently, from this Figure 3, one observes over a time interval of some 300 s a nice uniform oscillation. It is simply not true, observationally, that all points in the Sun are undergoing such a nice uniform oscillation at all times.

Clauser: It is difficult to reconcile your statements. Such a variation in one or two cycles would show much more energy in the high frequencies, and you don’t exhibit high frequencies in your spectra.
Krook: Is there any correlation between size of the observed area and persistence of the oscillation? Back of my question lies this worry whether some of the observed oscillations are indeed the results of statistics as opposed to single dynamical phenomena.

Noyes: I don't know. To the best of my knowledge, the possible existence of such a relation has not been studied. I would give a high priority to such study.

Michard: I have only a guess, resulting from looking at observed $V(t)$ curves, that when an oscillation actually occurs over a rather large area of the Sun, then it has a tendency to be more persistent.

Jordan: What fraction of the solar surface is experiencing oscillations at any one time?

Noyes: I would guess about $1/3$ of the area.

Clauser: You say these phenomena are hard to understand in terms of statistics. Suppose then you consider a simple mechanical oscillator that is fairly sharply tuned, which you excite with a random force. The result is the spectrum of the random forcing function distorted by the spectral response of the sharply tuned oscillator. Suppose your forcing function is of the kind typical of turbulence—i.e., peaked at zero frequency and dropping rapidly off to high frequencies—and your oscillator is the atmosphere; then the combination will give a spectral function which, I submit, looks like your Figure 1 for the intermediate strength lines. If you try such an experiment in the laboratory, you get curves like your Figure 3, in which it looks as if the phase is changing a bit. You see, because this is a phase incoherent motion (turbulence has no coherence in the phase), the oscillations appear at odd times; so they appear to build up an oscillation, which dies out, and another appears to build up. Now if you indeed had a sharp break appearing, such as you implied a few minutes ago, it would be inconsistent with your observations because it would put more energy into the high frequencies. In fact, any time you do anything abruptly, the result is a high frequency tail—which you don’t have.

Meyer: I do not believe that the observations indicate such a purely statistical mechanism. One finds very high coherence during about $1/3$ of the time and over distances up to about 4000 km, thus much larger than the size of individual granules. $1/3$ of the time there seems to be interference between different oscillations, and $1/3$ of the time no comparably large amplitude is observed at all. This seems to indicate that we have, rather, a superposition of pulsations.

Clauser: What you conclude may be correct. But observationally, here, we have a double oscillatory phenomenon, in time and space. So far, I haven’t been shown that the time and space resolutions are simultaneously good enough to back up your statements.

Noyes: There are observations giving simultaneously good accuracy in both space and time, for time intervals greater than 30 s and space intervals greater than 1000 to 2000 km.

Schatzman: We are also not sure that the solar atmosphere can behave like a sharply tuned oscillator.

Souffrin: It is possible to get the kind of effect Clauser described, the maximum in the observed power spectrum, without any oscillator-like resonance. We start from some kind of turbulence spectrum at some level in the atmosphere. The low-frequencies cannot propagate, and are filtered as evanescent waves. Such a filtering process can give the peaked power spectrum.
Clauser: All these objections are interesting; but my interpretation of the observations is still that they look like a resonant oscillator subject to a random forcing function. In answer to Souffrin, I note only that the time-constant of the filter must be the thing that determines the frequency peak.

Lighthill: Turning to your discussion of the relation between peak brightness in a spectral line and the upward velocity inferred from the line, could you be more specific as to how you interpret peak brightness in terms of temperature?

Noyes: I interpret peak brightness as a peak in the source-function for the particular line we observe. I would prefer to look only at lines that are collisionally dominated, because with them we know the source-function is related to electron density and electron temperature, even though the relation may be to a space-distribution of these rather than to their value at the point at which we want the value of the source-function. For such lines, the only way I know to increase the value of the source-function is to increase either, or both, electron density and electron temperature. I interpret the peak brightness as such an increase. In my discussion of thermal relaxation I tried to interpret how this occurs in its relation to height variation.

Krook: Could you be more explicit in your definition and discussion of radiative relaxation?

Noyes: If a temperature perturbation occurs in an optically-thin atmosphere, then this temperature perturbation relaxes to the undisturbed situation at a rate proportional to the size of the perturbation. Therefore it decreases exponentially, with a characteristic time given by the reciprocal of what I have been calling the radiative relaxation rate. I remarked that this rate decreases as you move upward in the atmosphere because the H⁻ concentration per gram of hydrogen decreases; and it is the H⁻ radiation and opacity which fix radiative cooling in the region of the solar atmosphere studied in these lines. Putting in the numbers, you find that at optical depth unity in the continuum, the relaxation time is about 1 second, which is much shorter than the time for compression by any wave with a period of the kind we discuss. However, if you progress upward a few hundred kilometers to where the optical depth in the continuum is about 10⁻⁵, the radiative relaxation time becomes very long compared with the compression time for a wave.

My personal interpretation of these intensity fluctuations is that, at low levels, the atmosphere cannot respond to any compressional changes in density but follows the ambient radiation field. Because it is firmly locked to the radiation impinging on it, especially that from the granulation, its behavior mimics that of the granulation. But at high levels, the opposite situation holds; the temperature and density are firmly locked to the compressional effects of the wave, in what is more an adiabatic situation. In between these two extreme layers, you should, of course, see a transition region.

Lighthill: Put aside detailed interpretation for the moment, and let me ask if it is correct that there is a 12-second time lag between increased brightness in the continuum and the peak intensity in the weak C line, λ5052, which is presumably one of the lines formed deepest in the atmosphere?

Noyes: Yes.

Thomas: You give the statistics on the increase with height of the mean size of the
oscillating elements. Is there a difference in the distribution function of size at the various heights?

Noyes: Qualitatively, the distribution function in the photosphere appears merely to shift scale with height, without changing shape. For instance, spectroheliograms obtained in weak lines look similar to those in medium strong lines, except for the scale change. The distribution function, however, is quite different for lines formed in even the low chromosphere. A spectroheliogram in the core of the Na D line shows considerable chromospheric structure.

Dubov: What can you say about the K line of Ca\textsuperscript{+}?  

Noyes: I do not have data on the K line, but the Ca\textsuperscript{+} \( \lambda 8542 \) line seems to show a smaller size than deeper-lying lines. This presumably reflects the narrow chromospheric emission network.

Dubov: The position of the \( \lambda 8542 \) line may not be the same as the K line would occupy on your Figure 5? They may originate at different depths, and therefore perhaps reflect a difference in chromospheric structure. But the position of the \( \lambda 8542 \) line is interesting, as it indicates the small size of the elements.

Rybicki: Do you believe your Figure 5 gives the true physical situation, that the curve of size actually increases, then decreases?

Noyes: I think the curve is real; but it probably reflects the narrowness of the emission network rather than the size of the small-scale oscillating features. I should mention, incidentally, that I chose the zero point in height in Figure 5 at the position where a number of guesses place the temperature minimum. Thus, you see, most of the data refer to heights below this temperature minimum.

Biermann: From these observations do you find any evidence for gravity waves?

Noyes: I don’t think it is obvious from what I’ve said that these motions are acoustic waves or gravity waves. The increase of size with height suggests to me that there is evidence for gravity waves; but I think we should discuss this in detail.

Uchida: I have recently discussed this point. In my view, the essential features of this observed oscillatory motion can be explained by considering the standing mode of internal gravity waves trapped in the low chromosphere temperature trough.

Evans: In Figure 6, you have plotted the line-of-sight mean-square-velocity as a function of distance from the center of the disk. In the Mt. Wilson data is the horizontal velocity found to be oscillatory?

Noyes: There is no evidence from the Mt. Wilson data that the horizontal component is oscillating; indeed, there is no evidence either way that it is or is not oscillating. We know, simply, that the line-of-sight component is oscillating.

Evans: There is some small evidence in data from Sacramento Peak that the horizontal component is not oscillating. A rather brief investigation at Sacramento Peak showed that for \( \cos \theta \gtrsim 0.5 \), the observable oscillatory component seems to fade out altogether. There is plenty of random small-scale motion, though. These observations were made in only one line, Fe \( \lambda 5241 \), which is of medium strength. I would say that at least at the center of the disk, this line is formed near the temperature minimum.
Lighthill: In connection with your discussion of the influence of the magnetic field on the character of the oscillations, could you tell me whether the granulation itself exhibits a smaller velocity in the convection zone where the magnetic field is stronger?

Noyes: The only way to measure the granulation velocity directly would be to look into the results from a weak line formed deep, like C I 5505. I do not know of any observations in such lines where r.m.s. velocity is compared with the strength of the local magnetic field. Steshenko (Izv. Krym. astrofiz. Obs., 22, 45, 1959) found in the slightly stronger Fe λ 6302 line that groups of granules exhibited the largest downward velocities in regions of strong (50 G) fields. This phenomenon, however, is quite possibly related to the supergranulation, rather than to the small-scale granulation.

Kraft: In your comparison of the observations in Hα and the Ca+ K line, you make it seem that the velocity patterns are very similar, if not identical. Am I correct in this understanding?

Noyes: The main difference between the velocity patterns in Hα and the emission pattern Kα is that the velocity in Hα appears to be funneled down in more discrete points, whereas Kα is nearly a continuous network.

Kraft: Suppose you plot Hα and the K line in terms of their respective Doppler widths as units, instead of frequency. Now you remark that the Hα and K lines are alike in their behavior in reflecting the chromospheric network, etc., except that it was the K line in emission and the Hα line in absorption. Now, even on this scale of units of Doppler width, is it still all absorption in Hα corresponding to the K emission?

Thomas: If the question is whether you can juggle the scale of representation to get some emission in the Hα core, the answer is unequivocally no. Theoretically, you do not expect emission; nor do you observe emission. The Balmer lines of hydrogen and the H and K lines of ionized calcium are prototypes of two different classes; for the former, an outward increase of Tₑ will not produce emission cores; for the latter, it will.

Kraft: Thus the central core of the Hα line is perfectly smooth, always in absorption, and the line as a whole is approximately as wide, in these units of Doppler width, as just the Kα and Kβ parts of the K line. You don’t have the very broad damping wings in Hα; or if you have them, they are trivial in extent compared with those in the K line. Is this all correct?

Athay: I would not say it is completely obvious that the width of the Hα line corresponds to just the Kα, Kβ part of the K line. Possibly it is crudely true, but even that does not seem obvious.

Weymann: Could you indicate, on your schematic model of Figure 11, where you see the emission Kα component of Ca+?

Noyes: The Kα emission occurs mainly in the chromospheric network, in the quiet Sun; it also occurs, greatly enhanced, in the plages.

Thomas: Do I understand you correctly, that you say if one could isolate the center of these spicule bushes, and remove their emission from what is observed, then the rest of the solar surface would give a calcium line showing no emission core? If so, I don’t believe your model, because the emission core in Ca+ comes simply from the outward increase of Tₑ into the chromosphere from the photosphere. I'll buy different heights
of the emission core, corresponding to different opacities over different regions of the solar surface, but not an absence of an emission core. In our early work, Jefferies and I showed how these different heights of $K_s$ might occur. Could you clarify your model? You certainly don’t assert that there is no outward rise in $T_e$ in points between the bushes?

*Noyes*: No, spectra of points between the spicule bushes almost always show a central, reversed, emission just as the regions of the bushes themselves. However, the intensity of the $K_s$ peaks is much lower than it is inside a spicule bush or in a plage. The accompanying figure (Jensen and Orrall, *Astrophys. J.*, **138**, 252, 1963) illustrates the differences between profiles in the quiet Sun, in plages, and in the chromospheric network or spicule bushes.

*Kraft*: Does $K_s$ correlate in intensity with $K_a$?

*Noyes*: Yes, as $K_a$ increases, so does the residual intensity in $K_s$.

*Jefferies*: Turn to this increased intensity of $K_a$ in plages. I have heard this interpreted in terms of close packing of the spicule bushes. Do you have any comment on this kind of interpretation?

*Noyes*: The spicule bushes are phenomena of the quiet Sun. There is no *a priori* reason to assume that plages, which are associated with active regions, are created by a closer packing of spicule bushes.

*Jefferies*: Let me clarify the observational picture of the spectrum as it appears on a photographic plate with the slit placed across a sunspot. Under conditions of relatively poor seeing, we see two bright features on the Ca$^+$ K line spectrum which correspond to the $K_a$ emission separated by the $K_s$ absorption. Over a plage, the features become more intense and move closer together. I have seen them become as strong as $60\%$ of the continuum. Over the sunspot the $K_a$ peaks merge, although their intensity does not increase greatly above what it was over the plage.

If you now look at the situation under conditions of good seeing, the basic outline of the picture remains the same, except that over quiet regions the $K_a$ emission is very much brighter in small bits of regions, which presumably are the spicule bushes. Over the plage, something different happens. We get bright emission, but it is not now concentrated into small bits, but appears to be spread uniformly over the whole region of the plage.

Again I ask whether this phenomenon of uniform emission over the plage area has anything to do with the bunchedness of the quiet region emission features. If I have to be completely objective in regard to emission from the regions between the spicule bushes, I think I can say that we do not know for certain. We know only that the emission from the spicule bushes is much brighter than from what lies between them.

*Kraft*: If you look at the integrated emission from the whole solar disk, as you do in the stellar case, can you say what fraction of the $K_a$ emission comes from spicule bushes and other disturbed regions like plages, and what fraction from the general disk? Or is this just the same question, with its answer the same uncertainty you just expressed?

*Jefferies*: Weymann and I tried this experiment to some extent by looking at spectrograms of the Moon, thus weaker reflected sunlight, and we saw little $K_a$ emission on 4.5\AA/mm plates with the 100-inch telescope.
Kraft: Wilson found emission on 1 to 2Å/mm plates. But let me ask, in still another way, the question to which I have been trying to get an answer. One has, presumably, an undisturbed Sun in between the spicule bushes, the spicule bushes themselves, and the disturbed regions like plages. Now can I regard an integrated, spatially unresolved spectrum of the Sun as being composed of (i) a smooth calcium profile arising from the undisturbed region, smooth in the sense of no $K_α$ and $K_β$ components; (ii) the $K_α$ and $K_β$ components coming from spicule bushes and plages? Is the observed integrated profile simply the addition of (i) and (ii)?
Michard: We don't know; that is the point of the discussion.

Evans: Certainly if we consider the 20 000 km between spicule bushes it is not true that if we observe the K line in this region, we see no Kα emission. We do see Kα emission, there. But it may possibly be, that if we had high enough resolution, we would find regions showing no Kα. Once in a long while you make a tracing across the K line, and you find such a region; but it is very untypical.

Krook: What scale?

Evans: Of the order 1000 km.

Weymann: This question Kraft asked is important in the interpretation of stellar chromospheres, and you said we don't know the answer to it. As a half-way step toward the answer to this question, could I ask if you know the relative area covered by the spicule bushes? That is, are you uncertain of the answer to his question because of uncertainty on this relative area, or uncertainty on the brightness of Kα in the undisturbed area? From the foregoing, I gather that you are uncertain on this last aspect, but I am not sure how much we know on the first aspect of relative area.

Michard: I don't think this relative area has ever been measured. This description of the coarse network, with inner cells and what is between them, is of course only a very crude approximation which is most apparent when the seeing is not so good. If you took a good spectroheliogram, you wouldn't even see this coarse network unless you knew what to look for. For very good seeing, you have much structure inside the cells also. This fine structure may be connected with the chromospheric oscillations, and when you smooth it out, you see the coarse structure better. There is also a continuous transition between this coarse mottling and the plage—a continuous transition in size, Kα brightness, and in magnetic field. Many of these bright calcium flocculi or coarse mottles are sometimes remnants of plages. It is really not possible to answer Weyman's question, because it suggests too sharp a distinction between different types of structure.

Krook: But isn't there some evolutionary connection?

Michard: Yes, in some sense. The first step in the appearance of a plage is a brightening some place in the network. I suppose that if you would be satisfied with some kind of a guess, you could say that from the disk as a whole, 10% of the emission would be from plages, 10 to 20% would be from coarse mottling, and the rest would be from between these structures.

Evans: And this would be correct at some time during the solar cycle.

Pikelner: I discussed some aspects of the origin of cellular motions and the chromospheric network several years ago. The difference between the chromospheric plages and the undisturbed photosphere lies in the presence of the magnetic field, and this magnetic field increases the flux of energy in the waves. The field over the general solar surface is weak, something like 0.5 gauss; but at the periphery of the supergranulation cells, the field is increased because the magnetic lines of force are dragged by the motion in the supergranulation cell to its borders. The brightness of the emission is comparable in the chromospheric network and in the ordinary plages. The emission is a little stronger in the plages because the magnetic field is stronger there. The network is something like a weak filamentary plage.
In the plages, there is also a network, but of another scale; and the emission in plages is not uniform over their surface. The mechanism of formation of the network is the same; the supergranulation occurs both in the quiet Sun and in plages. The average strength of the magnetic field, however, is higher in the plages. I think that the average strength over the plage as a whole is comparable to the strength in the chromospheric network in the quiet Sun. I think the magnetic forces are important in the transmission of movement from the deeper layers of the photosphere into the higher layers of the corona. I believe the filamentary structure of the corona is also a continuation of the chromospheric network.

Pecker: As you spoke, you drew sketches on the blackboard, and it appears to me that you suggest no $K_2$ emission at all from the undisturbed chromosphere. This practically implies that the undisturbed chromosphere is non-existent; otherwise the Jeffries-Thomas theory would give the self-reversed central emission from these undisturbed areas also. Is this what your picture is, chromosphere not existing over some regions?

Pikelner: I think the answer lies in the observations of spicules. Spicules are denser than the surrounding chromosphere. Spicules are thin, and the sound velocity is high; if there were no external pressure they would be dispersed in 1 to 2 minutes. Also, since the spicules are in the chromospheric network, the magnetic forces are also increased inside the spicules, and act against keeping such thin spicules together. I think the spicules are in quasi-equilibrium because of external gas pressure. The gas between spicules is rarified but hot; and gas pressure is about the same inside and outside them. In terms of their formation, it is possible that when the magnetic lines are compressed together in the supergranulation flow pattern, the density increases, possibly giving thermal instability so that the gas gets cooler, and the surrounding gas acts further to compress it. The condensation propagates upward; and when the gas becomes sufficiently cold, it falls down.

(Ed. note: In private discussions, Pikelner has emphasized that he wants to focus on the question of which is basic: a convection which drags and compresses the magnetic lines of force, or a magnetic force field which controls the gas. In the undisturbed regions, $B \gtrsim 20$ G, Pikelner thinks the former predominates; in the plage regions, $B \sim 50$ to 100 G, the latter. Reference may be made to his article in Vistas in Astronomy, 6, 131, 1965, Pergamon Press.)

Michard: I would like to put some pattern into the discussion. Right now we are trying to discuss the observational situation and problems, as has been done by Jeffries and Pikelner. Later, we shall turn to the problem of going from the observations to the physics, when we observe fluctuations in emergent observed quantities which are not necessarily identical to the fluctuations in the physical parameters characterizing the state of the atmosphere.

We need a procedure for going from fluctuations in intensity of the observed radiation to fluctuations in velocity, temperature, pressure, etc. Since such a procedure requires a solution of the radiative transfer equations in inhomogeneous atmospheres, I shall ask for a report on this problem after the observational discussion. Still later, I shall ask for comments on the interpretation of the derived physical structure. Now we will return to the observational situation.

Krat: I would like to remark that a considerable part of the observational facts reported
by Noyes have been supported by observations made over a number of years by my Soviet colleagues at observatories in the Crimea, in Poulkovo, Uzmjran (near Moscow), and others. They are described in some detail in publications from these observatories and in the Astronomical Journal of the USSR. However, there are also some interesting new facts recently obtained by these colleagues, which I might summarize briefly here, so that you may discuss further details with us.

First, I have taken low dispersion spectrograms of the center of the solar disk at Poulkovo in 1965, and G. J. Vasilieva has made direct photographic tracings of the Sun simultaneously in two spectral regions. We find that sometimes the spatial distribution of brightness in these two regions differs considerably. The series of observations is not extensive, but the fact seems to be well established. On the spectrograms, one can see distinctly that some details of large ($\sim 30''$) or medium ($\sim 3''$ to $5''$) scale are clearly seen only at the one edge of the spectrogram. The whole range of simultaneously registered wavelengths covers 2400Å. Thus some details are distinctly seen in the blue, and others in the red, parts of the spectrum. Smaller details ($1''$ to $2''$) show the same contrast in red and blue.

There are cases where oscillations occur with sufficiently great wavelength to make the convection elements at the base of the atmosphere drop or rise. These oscillations probably have the character of gravity waves. An interesting and strongly anisotropic spreading of a large-scale gravity wave produced by a sudden disturbance in the solar atmosphere was observed in 1961 by Mlle Vasilieva when registering the magnetic and radial velocity fields produced by the Poulkovo solar magnetograph. The oscillation period was 5 minutes. V. A. Krat and G. F. Vjalshin have also observed, in 1963, the large gravity wave oscillations detected simultaneously in the changes of magnetic field and radial velocities coinciding with the supergranulation. Indeed, I think we should conclude that the gravity waves may be responsible for the large-scale formations of the photospheric and chromospheric network.

Finally, I think I should mention that the chromospheric ‘spicules’ as seen on the disk photographs made in H$\alpha$ are not the same objects which are observed at the solar limb at heights ranging from 5000 to 10 000 km. These latter faint objects, with peculiar spectral characteristics, have been studied extensively at Poulkovo and Uzmjran. They are chromospheric—or, strictly speaking, coronal—spicules probably produced by sudden compression and cooling of coronal gases, or by certain processes analogous to those occurring in chromospheric flares. The optical thickness of these objects is small, and they cannot be so distinctly seen on the spectroheliograms as the details of the fine structure of the chromospheric network.

This is a brief summary of the results obtained over the last two or three years that bear specifically on the problems presently under discussion here.

Vasilieva: I might add a few supplementary details to Krat’s summary. (1) Our photoelectric observations in two spectral regions of the continuum ($\lambda 4200$ and $\lambda 5000$) simultaneously, and calculations based on the correlation function, show that the average size of the granulation appears larger in the violet than in the yellow. (2) The cross-correlation coefficient for the brightness fluctuations at these two wavelengths is small, about 0.4. Preliminary results from two days of observations show that this correlation depends very little on the observational conditions. (3) The simultaneous observations of radial velocity and magnetic field, using the solar magnetograph, showed that the
average size of the elements of the magnetic field is apparently 1.6 times smaller than the average elements of the velocity field. These sizes were, respectively, 4" and 6.2".

Evans: I find these results interesting; for we have some results from Sacramento Peak of a similar nature, but unfortunately they lead us to a different conclusion. We have taken spectra of high spatial resolution which show simultaneously two regions: λ 5893, between the Na D lines, and λ 3954, a region between the Ca I H and K lines. The object was to show the difference in the position of the granular features on the Sun in these two wavelengths due to the fact that they are inclined and seem at different heights. The result was negative. The matching of the bright and dark features at the two wavelengths was surprisingly faithful. I have not actually calculated the correlation, but it is certainly very close to 1. The region we sampled has a length of about 135,000 km.

We have a second result in which we compared the continuum at λ 5500 with that at λ 3700 (if you can call the latter a continuum; it's really a broad spectral band of closely-packed lines). Here I have taken actual photographs of the granulation. Because the array is two-dimensional instead of one, we get a great deal more information. We compared the photographs with a stereoscope, again with the idea of seeing whether the granules appear to differ in the two photographs, and indeed they do. In the stereoscope, some of them look as though they were higher than others. Here I would say the correlation is definitely less than 1. Again it is something we have not computed, but I would say that although less than one, it is still high, say about 0.7.

Here we seem to have a conflict in observations for which I have no explanation. I have looked at Miss Vasilieva's tracings. Where she takes her filters out, and lets the two photocells look at exactly the same thing, the curves are identical. But when she puts her filters in, the curves are different. I have no explanation.

Vasilieva: The apparent discrepancy between Evans' Sacramento Peak results and ours is an important point which should be resolved. At this time, I can only make several suggestions. (i) Evans and we observed different parts of the solar surface, the statistical parameters of which may be different. (ii) The different methods of observation by Evans and us may be differently influenced by the terrestrial atmosphere. In connection with the possible influence of the terrestrial atmosphere, we tried to study the problem.

Two points should be made: (a) The different character of brightness fluctuations corresponding to scanning along the solar surface on the one hand, and observing the same point on the other hand, shows that the effect of the trembling, the scintillations and the change in brightness with time are relatively smaller than the fluctuations in brightness due to scanning. (b) The effect of the atmosphere at the time of our observations introduced features of size about 1", and could thus influence only the distribution of elements of the same order of size. But the magnitude of the cross-correlations is mainly determined by the behavior of large elements of order 20" to 30" in size. We do not think that atmospheric effects can significantly influence our results.

Michaud: I would like to add that I, also, think this discrepancy might be connected with the effects of the atmosphere on the different techniques of observations.

Underhill: Could the presence of the filters introduce a certain polarization different from the other situation? Then your receiver might react differently towards differently polarized light.
Michard: I do not see how the polarization can enter, since the real polarization of the normal solar atmosphere is extremely small, of the order $10^{-4}$.

Kraft: What is the band-width of your filters?

Evans: For the $\lambda 5500$ region, we used a Wratten 54 filter which has half-intensity points at about $\pm 200$ Å from $\lambda 5500$. The band-width is less certain for the $\lambda 3700$ filter because of the gradient of the instrumental transmission and film sensitivity, but I think it is quite narrow, about 200 Å wide.

Kraft: The principal difference between the two might be described as a question of the effect of optical depth, since the enormous crowding of lines around $\lambda 3700$ presumably places you somewhat higher in the atmosphere.

Evans: I think that is true, and that is what we were looking for. We have been finding at Sacramento Peak that if we have some means of studying the granules at different heights, we find that they do differ, quite systematically and quite universally. Apparently these bright regions in the solar atmosphere are quite highly inclined, with displacements of the order of $500$ km for height differences of $100$ km. In the work reported, I was looking for this effect in two dimensions, in order that we could be a bit more certain that we weren’t kidding ourselves with the data from the spectral tracings alone. The reported stereoscopic effects seems to confirm our results.

Rybicki: In Noyes’ talk this morning, he showed that the size of the fluctuation phenomena increases with height. Now you are saying that at different levels, one observes essentially the same size. Is this because the things are measured in a different way?

Evans: No, I think we are in complete agreement with Noyes. In the case of our $\lambda 5893$ and $\lambda 3954$ pair, where we do not find any evidence of difference in height, we also do not find any apparent difference in scale of the features. Remember that we are looking at the continuum in both cases. But when we go to $\lambda 3700$, and are presumably looking at a higher level, the structure is quite definitely coarser, and the reason for the coarser appearance is interesting. If you will look at the photographs, you will see that the fine lines that provide the granulation boundaries in the $\lambda 5500$ region seem to become vaguer or disappear completely in the $\lambda 3700$ region. In other words, it looks as though features that are distinct and separate at the lower level, seen at $\lambda 5500$, are coalescing into larger features at the higher level, seen at $\lambda 3700$; this means an increase in the characteristic structure size. If you took the auto-correlation curve, I suspect that the half-width would at least be doubled.

Michard: Yes, but I would like to raise the question of the resolving power on the disk in two different wavelengths and of the correlation between seeing effects in both wavelengths. Now the exposure time on the spectrograms is of the order 10 seconds, and on the photographs is of the order $1/200$ seconds. Now possibly this can explain something, because if you photograph with this time constant, then you have two very uncorrelated seeing effects. Even if the photographs are taken simultaneously, the seeing is not correlated in two different wavelengths.

Evans: To the extent that the seeing is not correlated in these two wavelengths, you are right. But I don’t know how big this effect is.
Solar Velocity Fields

Michard: It can be large, because when the time constant is long, you have a smoothing, but no distortion of the image. When the time is short, you have less smoothing but you have geometrical distortion of the image, which has bad effects on the cross-correlations. I have experienced this also in the study of the continuum. This same thing can explain the difference between your result and Mlle Vasilieva's results because in her photoelectric tracings on the Sun, she probably used a very short time constant, which is equivalent to very short exposure.

Evans: You believe the seeing distortions in these two wavelengths can differ appreciably?

Michard: It is possible; I don't know for sure.

Evans: I agree this must be looked into.

Krat: It is a matter of fact that often the solar granulation in different parts of the spectrum looks somewhat different. On spectrograms covering a wide range of the spectrum (2000 to 3000 Å) we see bands of some dark or bright formations being visible only in the photographic or only in the visual part of the spectrum. This effect is probably due to the wavelength dependence of the opacity. In shorter wavelengths we observe deeper photospheric layers. Of course we have also spectrograms which do not show this effect. I think that the results obtained by Mlle Vasilieva by direct registering of the fluctuations of brightness, using different color filters, are in good agreement with the recent data on the character of the granulation spectrum (Pulkovo).

Pecker: The fact that Evans sees some kind of difference in level when he looks at the two pictures in the two wavelengths means that there is some inclination. Did you measure the angle? If the angle is large enough, we should possibly see some elongation of the granules because their optical depth is large enough to see such.

Evans: We obtained these photographs just before I left, and we made no attempt to measure angles. I have only the roughest notion of the difference in level at which we are looking. I am sure only that the horizontal displacement is about 500 km.

Pecker: Which is larger than the thickness of the photosphere itself.

Evans: Yes, so the difference in depth must be much less than this, probably less than 100 km, maybe only 10 km.

Pecker: This is almost a horizontal phenomenon, and you should see some kind of elongation. Do you?

Evans: I would agree that you would expect to see an elongation, especially in the green picture which is at the lowest level. But this effect is not obvious in the photographs.

Pecker: Along the same summary lines as Krat's comments, I would like to mention some recent work on supergranulation. Roddier has been observing the strontium line at Kitt Peak under high resolution. He finds that elements of size 7" to 8", thus large compared to granulation structure, show the same oscillatory phenomena found by Evans and Michard. Careful analysis shows him that the oscillatory fluctuations arise only from velocity fluctuations; he finds no evidence for change in profile. The profile is simply displaced as a whole. He finds the same period as generally found, and velocities
about 0.3 km s\(^{-1}\). He also finds that the phenomenon decreases considerably when you go from center to limb of the Sun.

**Noyes:** There is no larger amplitude in the core than in the wings?

**Pecker:** This point is not mentioned in his communication.

**Böhm-Vitense:** If you have a phase lag in velocity from the lower regions to the higher regions of line formation, you would expect an asymmetric line profile, wouldn't you? Has this ever been observed?

**Michard:** It is true that there are asymmetric line profiles, and I expect that they are due simply to the changes of velocity with 'depth of formation' of various parts of the profiles. Suppose we measure the velocities in the core and in the wings. We should expect to find different results, but the difference would depend very much on the line we are looking at, because it is not obvious a priori just what difference in height there is between center and wings. For arbitrary velocity field, one can do a variety of things to the relative opacity in core and wings, thus greatly changing their relative position of formation.

**Noyes:** It is a well-known observation that in strong lines such as the Na D line, the r.m.s. Doppler shift increases from the wings of the line toward the core.

**Michard:** The same is true for other strong lines. But it may well not be true for the lines studied by Roddier. I emphasize that a single velocity field can give a variety of possibilities, depending on the line you choose to observe.

**Böhm:** In your pictorial representation, you drew the supergranulation in the deeper layers. Is this really observed? What does one know about the supergranulation in the deeper photosphere?

**Noyes:** To my knowledge, the supergranulation has only been observed in medium-strong or strong upper photospheric or chromospheric lines. I think it would be interesting to look at the velocity in the weak C\(_{\text{II}}\) 5052 line to see if the supergranulation appears.

**Spiegel:** Let me ask for more details on the variation of physical properties along the surface of the Sun on the scale of the supergranulation. At one time Stuart and Rush took the observed brightness fluctuations and did a statistical analysis, obtaining a scale of brightness fluctuation on the solar surface of about 30 000 km. (This was before the observers proposed the idea of supergranulation.) Later, Uberoi criticized the statistical analysis on which this was based. It seems surprising to me that one doesn't see some kind of variation of physical parameters on that scale, and I wonder if it has been looked for again?

(Ed. note: The work referred to included fluctuations in both brightness and velocity, each of which showed the \(10^4\) km scale. There has remained a difference of opinion as to whether this larger-scale phenomenon was legitimately shown by the earlier work. I am prejudiced and believe it was, as well as the suggestion that the \(10^4\) km scale is primary and gives rise to the \(10^8\) km granulation scale as a decay product, probably as a compression wave rather than an eddy turbulence, with an upper boundary on the observed granulation scale placed by the acoustic opacity of the solar atmosphere.)

**Noyes:** I suspect that you are trying to ask more about interpretation in terms of
fluctuations in $T_e$, etc., than about just brightness and velocity fluctuations. To my formal presentation, I can add only a reference to recent work by Simon at Sac Peak.

Simon took simultaneously a white-light photograph of the solar granulation and an H$_\alpha$ filtergram. (You recall that the H$_\alpha$ filtergram shows darker regions at the boundaries of the supergranulations.) When he cross-correlated these data, he found a positive correlation: and this correlation implies that the photospheric granulation at the boundaries of the supergranulation is darker than the photospheric granulation away from the boundaries. This work is so recent that we have no quantitative estimate of the change in source-function or even of emergent intensity at the boundaries of the supergranulation. But a correlation coefficient was sufficiently large to indicate the presence of a real effect. The situation is that the emergent intensity is less at points where the granulation lies at the supergranulation boundaries; and the effect is sufficiently small that in ordinary photospheric granulation photographs it is difficult to see.

**Spiegel:** Even so, you cannot easily tell the difference between the effects of a temperature fluctuation and of an energy flux from below. For example, if there is a magnetic field there that affects the convective heat transport, we can still get a different emergent intensity without such arising principally as a temperature effect.

**Michard:** I would like to comment on the discussion of marginal effects that we can look for in these studies of granulation and supergranulation. We have just heard that in the granulation observations, we may have some traces left of the supergranulation pattern. We might ask if, in the normal granulation—in the photospheric fluctuations of the continuum—we might have some component due to the oscillations? This point was discussed recently by Edmonds, and he comes to the conclusion that the evidence for it is small but significant. He reached this conclusion from a study of the power spectra and correlation curves for the photospheric fluctuations. He finds that in the granulation only about two per cent of the power in the brightness fluctuations is due to the oscillations.

**Spiegel:** Let me ask whether it is possible to detect a difference in oscillatory motions at the boundary of the supergranulation as opposed to the center? I realize that if you look at the center of the solar disk, you don't know exactly where the supergranulation boundaries are, because they are marked by horizontal motions. However, if you could look in the center of the supergranulation and find some sort of variation of the oscillatory motion, and on the 30,000 km scale, that would be convincing evidence to me.

**Noyes:** The question is whether the amplitude or the period or some other characteristic of the oscillation changes across the supergranulation. I don't think this has been looked for. At least, it hasn't been looked for in the photosphere; but there is no reason why it can't be, because we know where the boundaries of the supergranulation are, since they are correlated with the K-line emission.

**Spiegel:** Most of the kinematical and dynamical information has been given statistically. I ask whether more can be said about the detailed history of an individual feature? When you watch developments in a supergranule, can you tell how it appears and disappears?

**Noyes:** This has not been done, to my knowledge.

**Spiegel:** The real question I was coming to is, when the supergranule disappears, does the magnetic field at its edge stay there, and for how long? I realize it is a difficult
question. It would require you to find, for example, a pattern in the calcium network which is not in instantaneous alignment with the supergranule network. In other words, shifts in the calcium network would lag behind shifts in the supergranulation network.

Bohm-Vitense: For all such observational questions, it seems to me the scattered light plays an important role which has not been discussed at all. In connection with this, I recall that at the Fourth Symposium, in Varennna, Rösch showed a movie of the granulation in which the quality of the image was changing with some kind of pulsational period. As I remember, the period was a fairly large fraction of the lifetime of the granule. Does anyone recall what the period of the atmospheric conditions was like?

Michard: A small fraction of a second. The granulation movies were taken at 25 frames per second.

Spiegel: Of course, the oscillation period of the Earth's atmosphere is five minutes.

Spiegel: In connection with this, and also with the comparison of the work by Evans and Mille Vasilieva, I wonder if Evans would let me introduce a report on some observations from SacPeak that have not yet been published or discussed.

When I was at SacPeak this spring, O. R. White and I were discussing oscillations in the Sun. He mentioned that at one time during the past year they were trying to calibrate their instrumental wavelengths by using the Sun. They gathered light from a large fraction of the solar disk into one spectrogram (Evans: From about a tenth of a solar diameter) and found that the wavelength varied periodically with a period of five minutes and with an amplitude corresponding to that of the small-scale oscillations.

The question arises as to whether there may be very large-scale oscillations over the solar atmosphere. White worried that this effect might be instrumental, but no one seems to know what instrument would have such a five-minute period. The SacPeak people feel these results are tentative; one reason for discussing them here is to persuade them to investigate the phenomenon more thoroughly.

Underhill: How do you get your wavelengths, or do you use atmospheric oxygen?

Evans: That is irrelevant; we simply observe the changing position of the lines, which varies sinusoidally.

Underhill: From what point on the photographic plate?

Evans: This is not photographic; we used a scanning photoelectric photometer.

Michard: What surprises me more was that the amplitude of the variation was of the same order as the small-scale features. It is difficult to believe. How large were the amplitudes?

Evans: They must have been in the order 0.4, which is more than we can attribute, I think, to mechanical changes in the instruments. But we must continue to make sure that this same oscillation is not found in an iodine tube in front of the solar spectrograph.

Krook: Over how many cycles was this observed?

Spiegel: I think about 4 or 5 oscillations.

Evans: Let's defer this, now that it has been summarized, until we can check it through more completely.
Mein: I would like to show some two-dimensional power spectra of the velocity field which I have derived from Evans' and Michard's measurements. Curves of equal density have been plotted in the diagram: horizontal wave number versus frequency.

The first diagram refers to a weak photospheric line of C I (5052.16 Å). Here we can see two regions: the 'resonance peak' region, and the low frequency region. In the first one, the maximum of density lies at a period which is roughly 300 s and a horizontal wavelength which lies between 10 000 and 12 000 km. The second region extends up to very short wavelengths; it is limited by the resolution of the telescope, and probably is related to the photospheric granulation.

![Diagram](image)

Fig. 1.

The second diagram refers to an iron line (5049.83 Å) which is stronger and is formed slightly above the minimum of temperature. We can see that the 'resonance peak' region has more or less the same shape. But the energy at low frequencies nearly disappears.

Finally, let us look at a power spectrum of a chromospheric line (8542 Ca II) in Figure 3. Here the formation depth lies around 900 km above the minimum of temperature. The 'resonance peak' region extends now up to higher frequencies. (This corresponds to the high frequency tail of the one-dimensional spectrum.) The maximum is slightly shifted towards short periods. A strong concentration of energy has appeared at low frequencies and large wavelengths (maximum around 30 000 km); it is not correlated with the photospheric granulation but probably refers to the chromospheric Ca II network.
**Bohm-Vitense.** How long were your time-sequences?

**Michard:** Between 15 and 20 minutes. This is the same set of observations used in our one-dimensional analysis summarized by Noyes.

**Bohm-Vitense:** On the figures, the periods are shown as 900 seconds, which is about the whole length of the sequence.

**Michard:** It is true that finite duration limits the resolving power of the power spectra, but you always have some information on the power for each period.

**Clauser:** The information contained is essentially zero compared to what you need to draw a graph.

**Michard:** When you make the Fourier transform analysis, it is quite true that the information is correct only to a limited extent, but those are the results of the calculations. The situation is the same in our one-dimensional analysis summarized by Noyes.

**Clauser:** But consider that basically what we are saying is the following. There is a band across the bottom of these figures presented by Mein, which is no good because at this level the frequency is less than the reciprocal of the total length of your measuring time so that you have no information on what happens at longer times or lower frequencies. Correspondingly, there is a band along the vertical axis for which the figures are no good because it corresponds to wave-numbers less than the reciprocal of the longest distance over which a measurement has been made. That's half the story. The other half is that there is a line above which the graph is no good because time resolution is limited; and the same applies for distance resolution, on a line to the right. We simply say that the region in which the spectrum has usable information is limited.

**Michard:** Then you must object to any results we get. To say that the granulation has a 'lifetime' of eight minutes or to say that we have power at low frequencies of around 1000 seconds, conveys the same information.

**Saffman:** I don't think it correct to say the results are no good. I think what you should say is that the results may possibly have to be corrected because of the finite sample lengths and finite sample times, and there will be some uncertainty. The meteorologists have had to contend with this problem a long time.

**Clauser:** You mean you can correct it when you have no meaningful measures on it?

**Saffman:** You know the length and duration of the sample.

**Clauser:** All you can say is that the spectrum is unknown to this kind of accuracy. Unless you have more information, there is not much you can do in the way of correction.

**Michard:** Yes, there is. We know the pass-band of our spectrum. We compute the power spectrum, and against this result we put the pass band \( \Delta \nu \). Where \( \nu \) is small, \( \Delta \nu/\nu \) is large and the situation is bad. It is effectively the same as observing the spectrum of light with a poor spectrograph. We are trying to improve the situation by taking sequences of longer duration than 20 minutes; Evans tells me that he now has one sequence of duration some 80 minutes. But I would like to emphasize here that whenever we study the spectrum of granulation by any means, we always find lots of power at low \( \nu \). The 5-minute periods are the prominent thing everywhere, of course, and are
DISCUSSION

not affected by the uncertainties we discuss here. What is important in this new result is the quite clear distinction between the low frequencies found for the chromosphere and the low frequencies found for the deep photosphere. This distinction shows that you have to deal with two different physical phenomena, as Noyes, with other evidence, pointed out earlier in this session.

Michaud: Now I will turn the discussion to theoretical interpretations; in particular, I would ask whether we might discuss the problem of the influence of inhomogeneities on the radiative transfer problem in terms of the relation between actual and apparent structure of the atmosphere.

Rybicki: Let me present the results of such a calculation.

I would like to describe an analysis which shows how a simple inhomogeneous structure in the upper part of the atmosphere may be altered in appearance by scattering effects. The model chosen is basically a uniform, plane-parallel atmosphere except for a single vertical cylindrical region which differs in temperature from its surroundings. We shall be interested in the appearance of this structure as viewed from directly above, and in various parts of a spectral line. The frequency dependence of the line opacity in the vicinity of this line is given in Figure 1. It consists of a continuum plus a Doppler-broadened line profile. The letters a through f denote the points at which observations are made, starting in the continuum and proceeding to line center. These observations then refer to regions progressively higher in the atmosphere.

Suppose, first, that the formation of this spectral line could be described by a pure absorption, LTE theory. Then this inhomogeneous structure would show its true horizontal scale for all of these observations, because it is the source-function that we

![Figure 1. Opacity near line center.](image-url)
see, and the source-function in this case is directly tied to the temperature, which differs only within the cylindrical structure.

On the other hand, it is well known that for many spectral lines the LTE theory is inadequate and a more detailed, microscopic approach is required. Taking this broader point of view, (See Pecker and Thomas 1961), we assume that the total source-function is given by

\[ S_T = \frac{\kappa_C B + \kappa_L S_L}{\kappa_C + \kappa_L} \]

where \( \kappa_C \) and \( \kappa_L \) are the continuum and line opacities, and \( B \) and \( S_L \) are the continuum and line source-functions, respectively. We assume that the continuum is formed in LTE, so that \( B \) is the Planck function. At point \( a \) in Figure 1, \( \kappa_C \gg \kappa_L \), so that \( S_T \sim B \), and again we see the true scale of the structure. However, moving toward the line we quickly have \( \kappa_C \ll \kappa_L \) and \( S_T \sim S_L \). The line source-function is not completely locally determined by the temperature, but rather is given by the relation

\[ S_L = \frac{\bar{f} + \epsilon B}{1 + \epsilon} \]

The quantity \( \bar{f} \) is the mean intensity in the line, weighted by the line absorption coefficient and averaged over frequency. It is proportional to the probability of photon absorption at the given point. The parameter \( \epsilon \) is the relative probability that upon absorption the photon will be converted into thermal energy rather than being re-emitted as a new photon. This quantity is generally quite small in the outer parts of the atmosphere. The meaning of equation (2) is that the probability of photon emission depends primarily on the radiation field itself rather than on the local temperature through \( B \). This leads to a scattering process in which a photon is truly absorbed with probability \( \epsilon (1 + \epsilon)^{-1} \) and scattered with probability \( (1 + \epsilon)^{-1} \). By successive scattering events photons may diffuse into regions far from their points of origin. Thus the inhomogeneous structure will appear spread out as soon as one sees the line source-function instead of the locally controlled Planck function.

It is known from detailed solutions in the plane-parallel case (2) that the spreading due to the above scattering process is over a scale that is \( 1/\epsilon \) times as large as the mean-free-path in line center, for Doppler line profiles. This scale is called the thermalization length. The same magnitude of spreading in the horizontal direction may be expected here.

To investigate this effect in more detail, we made some simplifications. We assumed that the opacity and the parameter \( \epsilon \) were independent of height. Also, we assumed that the deviation of the temperature in the inhomogeneous region from that in the ambient regions was a function only of the distance from a line of axial symmetry, but not of height. This simplification seemed to incorporate the main features of the original problem, while allowing much of the solution to be done analytically.

Typical results obtained are shown in Figure 2. The curves are intensity profiles as a function of the distance from the line of axial asymmetry and are normalized to the same value at \( \rho = 0 \). They are labeled by the letters \( a \) through \( f \), corresponding to the frequencies of observation given in Figure 1. The curve labeled \( a \) represents the true scale of the structure. Observing closer to line center, we see that the observed scale
FIG. 2. Intensity change vs. distance for observations in various parts of the line.

spreads considerably. In fact, the scale reaches a maximum at about $\epsilon$, where the spreading is approximately equal to the thermalization length, as expected. As the line center is approached at $f$, the scale decreases somewhat, because we are now so close to the surface that photons may easily escape before they scatter many times in a horizontal direction.

Figure 2 illustrates a case in which the thermalization length is much larger than the true scale of the structure involved. Here, as we observe closer to the center of the line, we see the scale of the phenomena apparently increase, go through a maximum, and then decrease slightly. This is interesting in light of the results presented by Noyes this morning, which show a similar change of scale of inhomogeneities with height in the solar atmosphere. Of course, I am not saying that I believe that the solar scales can be understood solely on the basis of this discussion; clearly, dynamical effects are taking place there as well. However, this may perhaps serve as a warning that what is there may not be what one sees.

References


Evans: Can you give an actual scale of the broadening due to this mechanism to be expected?

Rybicki: This is a question which requires detailed calculation on the basis of the choice of a particular spectral line. It depends on the parameters fixing the value of $\epsilon$, 

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thus the diffusion scale, and those fixing the opacity in the line, thus how high up in the atmosphere you see. Here, I have simply presented the kind of effect which you would expect to arise from these non-LTE effects in an inhomogeneous atmosphere. I worked it out only about three weeks ago, following a suggestion by Thomas, and I have only a simplified analytical solution available at present, just to give you a physical picture. I tried to get it programmed to give you a better numerical solution but have not had time to do this nor follow it through to answer the kind of question you raise.

Jeffries: I thought we worked this out, roughly, and found something like 2 or 3 seconds of arc as an estimate.

Krook: And the size of the actual structure is?

Thomas: That’s the interesting thing here. There are two effects. Rybicki worked the thing out first for an arbitrarily small scale of inhomogeneities. Then clearly the observed scale is fixed by the diffusion length, independently of the size of the actual disturbance. Suppose, then, I consider something of non-zero size. So long as the scale is much smaller than the diffusion length, I still have no relation between actual and observed size. The same holds with respect to the apparent increase in scale with increasing height in the atmosphere; the apparent increase is independent of any actual increase in size of inhomogeneity. It is possible to duplicate these observations of apparent increase, then decrease, in size of granulation as you go from continuum to line center, without any real increase in inhomogeneity size. Of course the actual situation may be a mixture of the actual and apparent size change. This calculation is just a good example of the non-local effects introduced by the non-LTE approach. As Rybicki emphasizes, there remains much to be done in following out the idea in detail.

Noyes: One should note, however, that the size of the elements undergoing Doppler shifts is observed to increase with height in much the same way as the size of the elements showing intensity fluctuations. Thus it is tempting to suspect that the two phenomena are related. While you can possibly explain the change of scale of the intensity fluctuations according to this idea just presented, I don’t see how you can explain the change of scale for the velocity fluctuations. I prefer to think they both reflect an aerodynamic phenomenon, rather than simply an optical diffusion phenomenon.

Let me ask the following question: Suppose you look at lines of different strength, but at different positions within the lines that you select to see the same physical height in the atmosphere. If the spreading is a dynamic phenomenon, it should appear the same size. However, if it is a diffusion phenomenon only, there should be an apparent difference in size.

Rybicki: This remains to be investigated, but it is certainly one way of trying to discriminate between actual and optical effects. But let me emphasize that if you are going to do this, you need a much more sophisticated theory than the one I have presented here. We will, of course, work on this. I just want to emphasize here the presence of this effect, which arises from the dependence of the non-LTE source-function on scattering, as contrasted with the LTE situation where the source-function is a wholly local quality.