PART VII

ENERGY BALANCE, HEAT TRANSFER AND HEATING MECHANISMS IN CHROMOSPHERIC FINE STRUCTURES

THE CHROMOSPHERIC ENERGY BALANCE

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Abstract. The problem of the solar atmospheric heat and energy balances is here divided into three sections.

(i) Basically the problem is an outward temperature gradient involving additional radiation losses whose estimate involves a model atmosphere. Proposed models described are based on optical and radio emissions together with various theoretical considerations such as thermal conductivity. The models are useful in providing estimates of the required flux of mechanical energy from below. However, they are limited by the use of averaging processes, and the failure to take full account of the many structural features which make up the chromosphere.

(ii) It has long been accepted that the main energy input is by acoustic waves generated in the 'small-bubble' Vitense model convection zone. We review recent observations of chromospheric magnetic and velocity fields and their interpretation which strongly suggest that this model is not valid and that the acoustic theory of heating is without basis. At the same time the new data remove the objections of Osterbrock and others to a theory of heating based on Alfvén and/or slow-mode hydromagnetic waves.

(iii) Observations of a variety of individual magnetic-plasma chromospheric structures, together with the adoption of heating by waves which follow the field lines, suggest a new approach to the whole problem of the heat balance. We discuss the heating problem in a number of these magnetic-plasma structures including the emission network, spicules and related disk features, arch filament systems and flares.

1. Introduction

Following the discovery more than three decades ago that the solar atmosphere is hotter than its surface, a clear objective was to determine its temperature and density distributions. Such a model had interest in its own right and also allowed estimates of heat losses and the energy input. Several models were developed using optical and radio data combined with a variety of theoretical considerations, but all were necessarily crude because they were based on measurements of emissions integrated along long lines of sight. Nevertheless they were, and still are, useful in providing averaged estimates of emissions and hence of heat requirements.

Great impetus was given to these studies with the availability of the extreme ultraviolet spectrum where the continuum is orders of magnitude weaker than in the visual spectrum, and the permitted lines correspondingly outstanding. Lines from the upper chromosphere and the transition region are now visible on the disk, and so much more detailed models are available. However, some integration along the line of sight and over a given area is still involved, and in addition there is an internal system of energy transfer in the new models involving thermal conduction and mass gas motions. Thus the new chromospheric models, although much more refined, are still very uncertain.

Meanwhile studies continued of the mechanism of heat input, long attributed to mechanical waves which originate in and below the photosphere. Following the classical study of Osterbrock (1961) acoustic waves have been favoured, and it is now widely accepted that the acoustic theory of solar heating has been established.

Some reservations about this theory stem from the very short wave period (<30 s) which seems to be required, and the very strong concentration of heating observed in

R. Grant Athay (ed.), Chromospheric Fine Structure, 269–291.

magnetic regions. However, it now appears likely that the whole basis of the theory, the small-bubble Vitense model of the convection zone, is in doubt. The discovery of supergranulation has led to the development of quite different convection-zone models, which are unlikely to provide the acoustic flux. The high degree of regularity of the polarization of surface magnetic fields appears inconsistent with tangled fields in the convection zone as required by the acoustic theory. On the other hand, the observations of strongly concentrated surface magnetic fields removes the previous objections to heating by an Alfvén-wave flux and clears the way for such a theory.

All of the above considerations suggest the necessity for an entirely new approach to the problem of the chromospheric heat balance. It is evident that magnetic fields divide the chromosphere into a number of largely isolated magnetic-plasma structures, and it now seems likely that each structure is heated by waves which travel up along the very field lines which define the structure. Some structures which invite study are the network of enhanced line emission, the spicules and their disk counterparts, arch filament systems and, finally and outstandingly, flares.

2. Chromospheric Models

Some two decades ago a popular pastime among solar physicists was the construction of models of the chromosphere. These were based on a variety of observational data and theoretical features, together with unrealistic simplifying assumptions. The most popular, and indeed necessary, assumption was that the chromosphere is a plane stratified atmosphere or, on the larger scale, spherically symmetrical; this implies that magnetic fields are absent or everywhere radial. Pictures of the chromosphere in the continuum and line emissions show that it is highly *irregular*, and the simplifying assumption has been attacked accordingly. In defence one might claim that the models give some sort of statistical average and provide useful estimates of the energy requirement, that the present-day attempts to evaluate the transition zone are based on much the same assumption, and finally that the structure is so complex that one must either adopt this averaging process or else study individual elements one by one. The last method is clearly our ultimate objective, but meanwhile averaged models serve a useful prupose.

Some of the first horizontally stratified models were those of Schatzman (1949) and Giovanelli (1949). In the former, energy was provided by the dissipation of shock waves and was removed by radiation, all other effects being ignored. The latter was based on an assumed constant conductive flux of 6×10^5 erg cm^{-2} s^{-1} through a chromosphere-corona transition region, all other possible sources and sinks of energy being ignored. The conductive model yielded a temperature (T) against height (h) gradient $\mathrm{d}T/\mathrm{d}h \approx 10^{-1}$ K cm^{-1} (near $T = 10^5$ K), and the shock model 10^{-2} K cm^{-1}. A model (Piddington, 1954) based on the interpretation of the observed radio spectrum of the whole Sun, together with some eclipse data, yielded a gradient of only $\approx 10^{-3}$ K cm^{-1}. The extreme case was provided by a model based on the assumption of hydrostatic equilibrium (Pottasch, 1964), for which $\mathrm{d}T/\mathrm{d}h \approx 10^{-4}$ K cm^{-1}. Thus the gradients determined were spread throughout the range 10^{-4}–10^{-1} K cm^{-1}. However, in none of these models were the simplifying assumptions clearly justified, nor was it evident just what was implied by the avaraging processes involved.

The next generation of chromospheric models was based mainly on the extreme ultraviolet (EUV) spectral line fluxes observed on the solar disk, and has been very adequately reviewed by Goldberg (1967), Athay (1971) and Noyes (1971). These data are both sensitive to the nature of the transition region and easier to interpret, because they do not involve very long line-of-sight integrations as do the optical limb observations and the disk radio observations. Because they yield large temperature gradients and consequently narrow transition regions with roughly isothermal regions above and below, the term 'model chromosphere' has been replaced by 'model chromosphere-corona transition region'.

2.1. Spherically symmetric shell models

The classical picture of the transition region is that of a thin, spherically symmetric shell; implicit in this model is the assumption that the magnetic field is either vanishingly small or everywhere vertical. The development of this model from EUV data has been reviewed by Athay (1971), the region being defined by a gradient $\mathrm{d}T/\mathrm{d}h \approx 0.1$ K cm^{-1} and a product of electron density (n_e) and temperature $n_e T = 6 \times 10^{14}$ k cm^{-3} throughout. It is interesting to note that the model agrees with the simple conductivity model proposed by Giovanelli (1949). Athay found that the conductivity flux F_c is large compared with the radiation loss, and that this may explain the near constancy of F_c. The reason is that if F_c varied appreciably then some additional source or sink of energy within the transition zone would be necessary; no such source or sink was then apparent.

More recent investigations (Lantos, 1972; Piddington, 1972b) suggest that even as a first approximation this model may be inadequate, and that there *is* a simple mechanism for varying the conductivity flux as much as desired. This is a continuous upward expansion and flow of thermal energy carried by (a mass motion of) gas which has been heated by the downward diffusion of thermal energy F_c. The equation of energy for a static transition interface is $E_d = E_r + E_c$, where E_d represents mechanical wave dissipation and E_r and E_c radiative and conductivity losses (all per unit volume). If the assumption of a static atmosphere is abandoned we then have

$$E_d = E_r + E_c + E_f, \tag{1}$$

where $E_f = \nabla \cdot \mathbf{F}_f$ represent the upward flow of thermal energy carried by the expanding gas. The model of Lantos (1972) is based on electron densities given by radio measurements. That of Piddington (1972b) uses physical arguments and more clearly reveals the processes involved, and so is described here.

The thermal energy density of a fully ionized gas with n ions cm^{-3} is $3nkT$, and so for an upward expansion velocity $u_f(h)$ we have

$$E_f = \frac{\mathrm{d}F_f}{\mathrm{d}h} = \frac{d}{\mathrm{d}h}(3nku_f T). \tag{2}$$

In the steady state, so that $nu_f = \text{const.}$,

$$F_f = 3nku_f T. \tag{3}$$

At the base of the corona $T = 10^6$ K, $n = 6 \times 10^8$ cm^{-3}, and if F_f balances a downward thermal flux $F_c = -3 \times 10^5$ erg cm^{-2} s^{-1}, then $u_f \approx 12$ km s^{-1} which seems a reasonable flow.

It may be of interest to examine the temperature profile of this model transition region within the limits where we might reasonably assume E_d and E_r unimportant. We then equate F_f to be downward flow of thermal energy, so that

$$3nku_f T - \sigma T^{5/2} \frac{dT}{dh} = 0, \tag{4}$$

where σ is a constant. Integrating again and rearranging we have

$$h - h_0 = \alpha T^{5/2}, \quad \alpha = \frac{2\sigma}{15nku_f}. \tag{5}$$

A reasonable value of σ might be 10^{-6} (see Noyes, 1971), in which case $\alpha \approx 1.3 \times 10^{-6}$. The corresponding parameters of the transition region are given in Table I.

TABLE I

A model corona-chromosphere transition region

T(K)	$h-h_0$(km)	$F_f = F_c$ (erg cm^{-2} s^{-1})	u_f(km s^{-1})
3×10^4	-2.2	10^4	0.4
10^5	0	3×10^4	1.2
3×10^5	640	10^5	3.6
10^6	1.3×10^4	3×10^5	12

These velocities might be varied a little to take account of the energy expended in lifting the corona against gravity. They must, of course, be changed if E_d or E_r are important. As they stand, the temperature gradients are 0.3 K cm^{-1} below the level of 10^5 K and 3×10^{-3} K cm^{-1} above that level. The former is in fair agreement with the EUV value, but the latter is much smaller, and close to the early radio model.

A rather obvious feature of this 'expanding' model is that it cannot be both steady-state and spherically symmetrical. The quiet solar wind represents a continuous mass flow from the base of the corona of $\approx 10^{13}$ atoms cm^{-2} s^{-1} (Kuperus, 1969) while the above flow is some 70 times larger. Presumably, then, most of the gas which moves up across the transition region must return again at another time or place. There seems to be no obvious objection to such irregular motions and, indeed, spicules themselves may represent an upward flow of $\gtrsim 10^{16}$ atoms cm^{-2} s^{-1} averaged over the Sun (Athay, 1971) which is an order of magnitude larger than the above requirement. The model simply provides an alternative mechanism for heat disposal to that of Kuperus and Athay (1967) and Kopp and Kuperus (1968) which invokes intermittent eruptions of gas instead of the stedy flow.

2.2. Magnetic field effects

The more-or-less spherically symmetric chromospheric models discussed above are useful in providing semi-quantiative estimates of the various physical factors involved. However, observations reveal that the chromosphere is highly irregular and that the irregularities are caused mainly by magnetic fields which protrude through the solar surface. These fields introduce new density gradients, horizontal as well as vertical, they introduce new temperature gradients partly because the thermal conductivity remains appreciable only along the field lines, and they introduce a complex velocity field. In addition to all of these effects, magnetic fields may play a major role in transporting and guiding the mechanical-wave energy from the convection zone into the chromosphere.

On the basis of these considerations it seems likely that the chromospheric structure and energy balance may be described only in terms of numerous models of individual magnetic-plasma structures. Meanwhile, in reasonably quiet regions we may adopt an averaging process based on assumed vertical fields in the photosphere and the known supergranule motions which push the fields into the boundary regions.

The resulting magnetic structure is shown schematically in Figure 1 where the photospheric fields in the supergranule boundary regions (SBRs) have strengths of many hundreds of Gauss (Livingston and Harvey, 1969). Before reaching the corona, the field lines diverge to provide a uniform vertical coronal field of a few gauss. As the diameter of a supergranule cell is $\approx 3 \times 10^4$ km, the transition region is a relatively thin

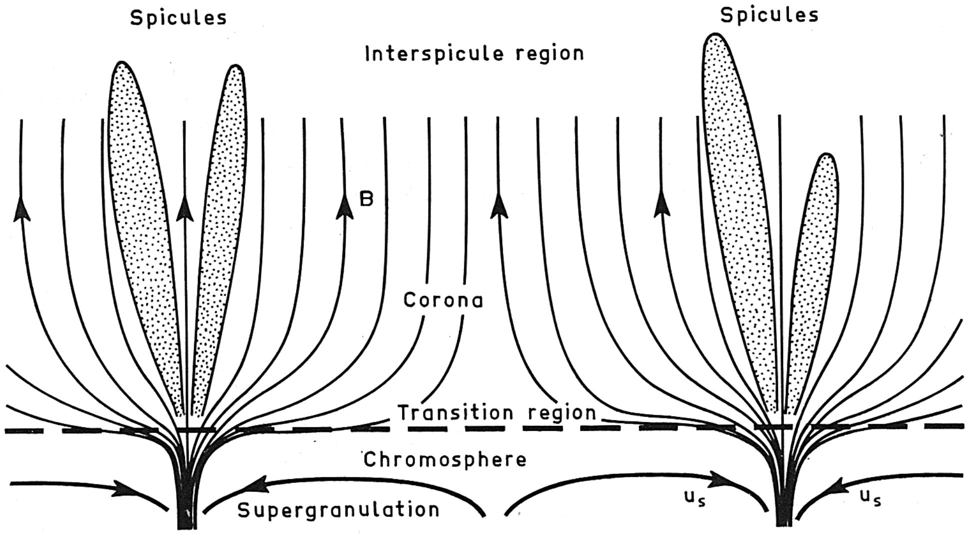

Fig. 1. Schematic cross section of the solar atmosphere over a supergranule cell, showing the fluid flow lines u_s, the consequent bunching of the field lines at photospheric levels, and the locations of the chromospheric-coronal interface and the spicules.

sheet as shown by the dashed line; field lines traverse this sheet at a variety of angles. Spicules are confined to the SBRs as shown; theories of their origin are discussed in Section 4.

Kopp and Kuperus (1968) have used this magnetic model to determine a model transition region based on the simplifying assumptions of negligible wave dissipation and radiative heat loss. However, they also neglect the possibility of upward expansion of the plasma (E_f in our Equation (1)), and in addition their model provides a very thick transition region (average $\approx 10^4$ km) with most of the emission from regions above the centres of the supergranule cells, which is contrary to observational evidence (Tousey, 1971). For these reasons it seems that this model and that of Dubov (1971) must be rejected.

More recently Kopp (1972) has developed a quite different model based on the observational evidence (Tousey, 1971) that emission from the transition region is concentrated into a network which is the upward projection of the Ca emission network. If emission is confined to a fraction q of the disk then the transition region should be correspondingly thicker in order to provide the same total emission over the disk. This means that the temperature gradient must also be reduced and the thermal conductivity flux reduced by a factor q^2 to 5×10^4 erg cm^{-2} s^{-1}. Kopp finds a radiation loss which considerably exceeds this conductivity flux and concludes that the dissipation of mechanical wave energy must play a dominent role in the local energy balance. He invokes slow-mode hydromagnetic waves which have degenerated into shocks.

In summary, we consider that these various one- and two-component statistically averaged models have served a very useful purpose in indicating roughly the energy radiated, and hence the input required. However, as far as internal energy transfers are concerned, there are too many unknowns to make the models very useful. For example, if upward or downward gas drifts are possible, as seems likely, then they may settle the thickness of the transition zone. As seen in Table I, the temperature gradient below the 10^5 K level may be large, as in some of the earliest models. On the other hand, if the transition region proves to be very thin, then at any point on this thin sheet the direction of the magnetic field is the same at the top and bottom surfaces; there is no convergence of conductive heat flux as envisaged in the models of Kopp and Kuperus (1968) and Kopp (1972).

We consider that future study of this problem might best be aimed at *individual* magnetic-plasma structures, each of which must have different temperature and density structures. This approach is discussed and used in Section 4 below.

3. The Source of Thermal Energy

The only major source of the chromospheric heat input which is worth consideration is the dissipation of mechanical waves generated in and below the photosphere. There are three basic wave types, acoustic, Alfvén and gravitational, but the last two have been mainly rejected and it is now usually taken as established that the primary source of heating of the solar atmosphere is acoustic energy (for a review, see Kuperus, 1969), and that we are 'reasonably certain that there is a mechanical flux of at least 2×10^6 erg cm^{-2} s^{-1} associated with acoustic noise from the granulation' (Goldberg, 1971). The theory was developed by Lighthill (1952, 1954, 1967), Proudman (1952), Stein (1967, 1968), and others using the convection zone model of Vitense (1953) and Böhm-Vitense (1958). In this model hot bubbles of dimensions equal to a scale height move upwards continuously while cool gas moves downwards. The acoustic power so generated has been computed by Osterbrock (1961), de Jager and Kuperus (1961) and Kuperus (1969, where other references are given). The upward propagation of the sound waves and their dissipation has been discussed by Osterbrock, de Jager and Kuperus, and more recently by Ulmschneider (1967) and Kuperus (1969) who agree on a flux of mechanical energy of about 3×10^7 erg cm^{-2} s^{-1}. The energy required to replace radiative and other losses has been computed by Osterbrock (1961), de Jager and Kuperus (1961), Athay (1966), Ulmschneider (1970, 1971) and others with more recent estimates in the vicinity of 5×10^6 erg cm^{-2} s^{-1}, which is comfortably smaller than the acoustic flux available.

In the above theory, magnetic fields play important, but strictly secondary, roles. The observed enhancement of heating in regions of stronger magnetic fields is attributed to the tangling and amplification of the fields in the convection zone, and to the consequent increased efficiency of sound production (Kulsrud, 1955; Osterbrock, 1961; Kuperus, 1969). The theory also takes account of the conversion of sound waves to the fast hydromagnetic mode in those parts of the atmosphere where magnetic forces equal or exceed compressional forces.

The basis of the above acoustic theory is the Vitense convection model with rms bubble velocities ranging up to 2.3 km s^{-1} (see Osterbrock, 1961, Table 3) and assumed magnetic fields of strength $\lesssim 50$ G. More recent measurements of the surface magnetic and velocity fields seem incompatible with both of these features and suggest the necessity of a complete review of the theory of the origin of the chromospheric heat input.

3.1. Velocity and Magnetic Fields in the Convection Zone

The acoustic theory of solar heating depends on gas bubbles moving upwards with velocity u; they have dimensions equal to a scale height H and so produce a spectrum of sound waves peaking at frequency $\nu = u/H$ (Osterbrock, 1961). Magnetic fields are 'passive' in the sense that they are too weak to influence gas motions, and so become tangled and thereby increase acoustic output by an order of magnitude in the regions of strong fields.

The above 'small-bubble' model of the convection zone was developed before the discovery of the supergranule motions and appears incompatible with those motions. There is now little doubt that the true convective motions extend over a number of scale heights, with consequent increase in efficiency (Simon and Weiss, 1968; Wilson, 1972b, who also reviews other models). These large-scale, much slower motions would not produce an acoustic flux comparable with that of the small, fast-moving bubble model. Thus the theoretical basis of the acoustic theory of solar heating is removed.

Observational evidence against the Vitense model and the acoustic theory is provided by the observed surface magnetic fields. The criterion for equipartition of magnetic and kinetic energy within the convection zone is

$$4\pi\varrho\langle u^2\rangle = B^2, \tag{6}$$

where ϱ is the gas density, $\langle u^2\rangle$ the mean square convective velocity and B the field strength. This yields $B = 450$ G at a depth of a few hundred kilometres (Osterbrock, 1961), so that weaker fields are tangled and fields up to $\lesssim 1000$ G should be strongly twisted. Also, since the observed surface fields have scales ranging from ≈ 1000 km up to $> 10^5$ km, they must have the same pattern as fields only $\lesssim 500$ km below the surface.

The observed surface fields are completely incompatible with the convection model and the acoustic theory. Most of the flux is concentrated in more-or-less vertical flux tubes with dimensions ≈ 1000 km and fields $\lesssim 1000$ G (Harvey, 1971, Vrabec, 1971 and others). To some extent these are controlled by the supergranule motions, but there is no evidence that they are tangled or even strongly tilted by any smaller-scale motions. One might argue that there are weaker fields, not observable, that are tangled; this is not possible, because after tangling they would be squeezed and amplified by the supergranule motions and so become visible. Finally, we refer to the largest-scale surface magnetic feature, a field of a single polarity extending for $\gtrsim 10^5$ km (Vrabec, 1971, Figure 4).

These results appear to rule out completely the assumption made by Kulsrud (1955), Osterbrock (1961), Parker (1971) and many others that the fields in the convection zone are passive and are tangled by the gas motions. It also invalidates the claims of Parker (1971) and Weiss (1966) that after the fields are tangled, the small-scale components are eliminated by magnetic annihilation across neutral sheets. Even the earliest stage of tangling would be observable in the surface fields at some period or other in the life of an active region. Also, as shown earlier (Piddington, 1972a, 1973a), only components of scale $\lesssim 100$ km can be eliminated by magnetic diffusion, which leaves all larger fields to be eliminated by the ubiquitous neutral sheet – a highly improbable assumption.

As far as the acoustic theory of solar heating is concerned, there remains the possibility that it may be modified by basing it on the observed granule motions rather than the Vitense model. However, this suggestion meets two major difficulties: it still leaves unexplained the observed close correlation of heating with magnetic fields, and it is quantitatively inadequate. The observed velocities ($\lesssim 1$ km s^{-1}), cell sizes ($\gtrsim 1000$ km) and periods (≈ 300 s) yield acoustic waves which are too weak by a factor of > 100 (Piddington, 1973c) and with much too long a period to suit Ulmschneider's (1970, 1971) requirement of $\lesssim 30$ s.

3.2. The source of chromospheric heat

From the above arguments it appears that the theoretical basis of the acoustic theory of chromospheric heating no longer exists, and that the theory is also incompatible with a great deal of observational evidence. Accordingly we consider the possibility of heating by one of the other wave modes (Piddington, 1973c).

Gravitational waves may only develop in stably stratified regions, which excludes the convection zone (Lighthill, 1967). It is possible that some convective motions project locally into the overlying stable regions as 'tongues of turbulence' and that these generate gravity waves. However, as these tongues have densities orders of magnitude less than the convection zone gases, and velocities equal to or less than those of the Vitense model, it seems unlikely that the necessary power would be provided. A further argument against gravity waves is the observed close relationship between heating and magnetic fields. This has no obvious explanation in terms of gravity waves alone.

This brings us to the Alfvén mode which was suggested some years ago (Piddington, 1956) as a source of chromospheric heating. The mode was rejected because it was generally accepted that magnetic fields at the solar surface are generally weaker than 50 G in which case the waves would be mainly absorbed (Osterbrock, 1961). In addition, the Alfvén speed in the convection zone is $\lesssim 0.2$ km s^{-1}, or 0.1 of the Vitense turbulent speed, in which case the predicted Alfvén energy flux is negligible (Lighthill, 1967). It is now known that the first of these objections is not valid and that the second is most doubtful, so that Alfvén waves (and slow-mode waves) should be reconsidered.

A satisfactory theory based on these modes requires an established convection zone model and this is not yet available. This deficiency may be partially rectified by the use of observed surface motions which have been used to provide two sub-models of chromospheric heating. The first, described below, is based on observed granule motions; the second (Section 4.2) is based on observed supergranule motions. In addition to a velocity field, the heating model requires a magnetic field structure, and here we are on firmer ground. It is now known that almost all surface flux is concentrated into small ($\lesssim 700$ km), more-or-less vertical flux tubes with field strength of order 1000 G (Chapman and Sheeley, 1968; Livingston and Harvey, 1967; Frazier and Stenflo, 1972, and others).

The sub-model based on granule motions is illustrated in Figure 2a where a single granule is shown in vertical section. The vortex motion V_g distorts an originally straight, vertical flux tube B as shown and an Alfvén wave must propagate upwards. Values of V_g have been discussed by Bray and Loughead (1967) who point out that the measurements give at best only a rough indication of order of magnitude of V_g. For the purpose of illustration we assume an rms velocity of 1 km s^{-1} in the low photosphere ($\tau_{5000}=1$) in the presence of a field of 500 G. With density $\varrho = 2 \times 10^{-7}$ g cm^{-3} we have $V_A \approx 3.2$ km s^{-1} and an energy flux $F = \varrho V_g^2 \, V_A \approx 6.4 \times 10^8$ erg cm^{-2} s^{-1}. This flux is some 200 times the heat requirement of the average chromosphere, and so takes account of the fact that fields of 500 G occupy only a corresponding fraction of the surface.

The dissipation of Alfvén waves provides two distinct problems. In and below the chromosphere the flux tubes which transport the energy are of relatively small cross section, and are separated by regions of much larger extent. The flux tubes move like taut wires vibrating in a compressible fluid, and so are likely to cause strong localized heating outside the tubes. In the corona the field lines spread out and merge to form a more-or-less uniform medium as shown in Figure 1, and strong dissipation is likely only after the formation of shocks (Osterbrock, 1961).

Different aspects of the vibrating-wire mechanism are illustrated in Figure 2b

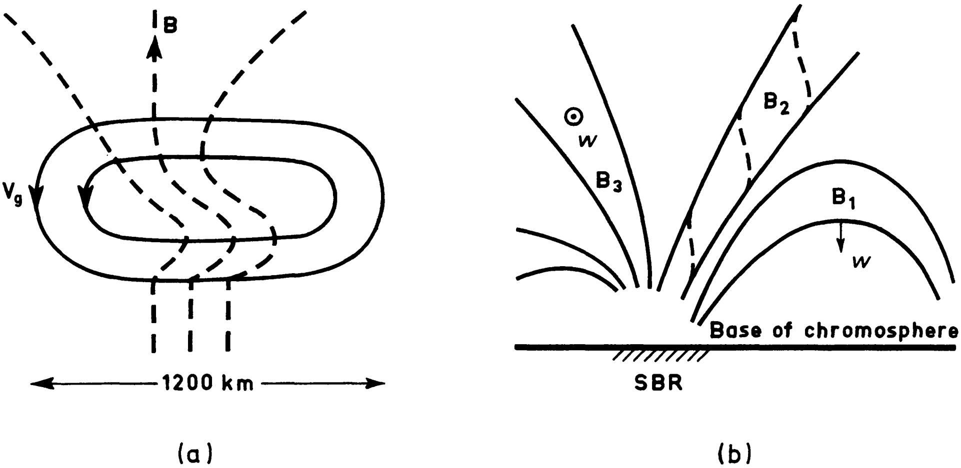

Fig. 2. Solar heating by Alfvén waves. (a) Generation of waves in mainly vertical magnetic field lines B by the rotation of a granule cell. – (b) Several flux tubes emerging from a supergranule boundary region (SBR), and providing optical emission patterns which depend on the direction of the perturbation velocity u.

where three flux tubes are shown emerging from the photosphere. It may be assumed that they have been concentrated so by the supergranule motions. The tube B_1 loops back into the photosphere and at the moment illustrated the upper part of this loop is moving downwards with velocity u. The gas below the tube will be compressed and will provide enhanced emission; later, when the tube moves upwards, the gas above the tube will emit more radiation. The tube B_2 illustrates the possibility of a helical twist, strongly suggested by the forms of some chromospheric features, and capable of energy transmission by a twist Alfvén wave. The tube B_3 is moving into the paper and the enhanced emission will show a red Doppler shift; during the second half of the cycle this will change to a blue shift. Other possible effects include the formation of a shock on one side of a tube when u exceeds the sound velocity, the collision of adjacent tubes and, finally, different gas densities inside and outside a tube. Each of these effects may show up as more, or less, complicating factors in the interpretation of line profiles.

4. Structural Components of the Chromosphere

The evolving picture of the chromosphere is that of many individual structural components, each of which is undergoing continuous changes. It follows that the chromospheric models discussed in Section 2 are statistically averaged models whose physical nature is not clear. A true physical picture of the chromosphere and a knowledge of its energy balance thus requires satisfactory models of each of the various structural components.

The basic structural components seen on the disk have a variety of names including bright and dark, fine and coarse mottles, fibrils, filaments and threads, and there is not always agreement about just what object is referred to. Without entering this controversy, we suggest that the vibrating wire model described above is capable of providing a variety of bright and dark structural components either elongated if the field is orthogonal to the line of sight or small blobs if along the line of sight. Collectively, these components define the supergranule boundary regions (SBRs) and are termed the emission network. The energy balance of the network is discussed in Section 4.1.

The most notable structural component seen on the limb is the spicule and this, with its related phenomena, is discussed in Section 4.2.

Turning to active regions, we again find that the various structures correspond with and are presumably determined by magnetic fields. Again we find the various network components (mottles, fibrils and so on), but with sufficient magnetic flux these may fill supergranule cells instead of being mainly confined to the SBRs. Another difference is that while the magnetic ffelds in quiet regions are predominantly vertical, those in active regions may have substantial horizontal components. The latter form closed loops within the chromosphere and low corona, often referred to as arch filament systems and discussed in Section 4.3.

Finally, in Section 4.4 we discuss the two most notable of all structural features: sunspots and flares. The sunspot energy deficit and the flare energy requirement constitute the two largest components of the energy flux.

4.1. The emission network

The fact that the lower chromosphere, as observed for instance in the H and K lines of Ca II, exhibits a complex network of enhanced emission has been known for many decades. On the other hand, the extension of this network into the chromosphere-corona transition region has been observationally established only recently by high-resolution EUV spectroheliograms (Tousey, 1971). From such data it appears that the network persists at least to the 2×10^5 K level, and an analysis of OSO-4 data by Reeves and Parkinson (1972) suggests that it may extend into the low corona where $T \lesssim 2 \times 10^6$ K. Confirmatory evidence of coronal enhancement at a much lower level of resolution is provided by the earlier results of Hansen *et al.* (1971) who measured the K corona above plages.

These results are extremely important in the theory of the heat balance because they prove that the energy emerging from the photosphere is dissipated mainly in the SBRs and the regions of strong, more-or-less vertical magnetic fields (Figure 1). This result is entirely consistent with the main conclusion of the last section that the energy concerned is carried by Alfvén and/or slow-mode hydromagnetic waves, both of which tend to follow the field lines.

The energy requirement of the network must exceed that of the average chromosphere by some factor, and using He 10830 spectroheliograms de Jager and Loore (1970) found a factor of 7. As this seems to agree with the ratio of the areas concerned it is acceptable and gives an energy requirement for the network of roughly 3×10^7 erg cm^{-2} s^{-1}.

However, this requirement is insignificant in comparison with that of the usually neglected photospheric or white-light faculae. Their requirement was recently confirmed by Wilson (1971) as about 10^{10} erg cm^{-2} s^{-1} or some 3000 times that of the higher levels, so that a theory of heat balance may be dominated by these faculae. It is unlikely that they are a direct result of enhanced convection because, as is well known, gas motions in the network are generally downwards. If they are caused by mechanical waves then they pose a new problem in the determination of the energy balance. The energy flux required is some 300 times that estimated for acoustic waves (Kuperus, 1969), and so provides further evidence against that theory. However, as shown in the following subsection, this flux is not beyond the capabilities of a magnetic flux tube with field strength of order 1000 G.

Wilson (1972a) has already proposed that some of these faculae may be provided by an Alfvén energy flux through sunspots. This will not account for all of them because some are present in active regions before and after the spots, and some occur in polar regions where there are no spots.

4.2. Spicules and Related Phenomena

The solar disk features discussed above are likely to have equivalent limb features, and of these the spicules are the most notable. Beckers (1968, 1972) has provided a comprehensive review of their properties, the first being an upward growth rate of ≈ 30 km s^{-1} and Doppler shifts with rms values $\lesssim 20$ km s^{-1}. This suggests that spicules are upward jets and may be an important factor in the energy balance, either directly by the upward transport of kinetic energy or indirectly through control of the plasma mass balance. As seen in Section 2, there appear to be upward plasma streams representing $\approx$ 100 times the solar wind loss and so there must be downward streams representing an almost equal flow. These streams are also likely to be very important in the heat balance.

Models of spicules have been reviewed by Beckers (1968, 1972) who rejects those not depending on magnetic fields because of the compelling association. All of the models attempt to describe an upward-moving supersonic gas jet, but such a model meets difficulties when spicules are identified with disk features. There is little doubt that during different phases spicules correspond with both dark and bright fine mottles (Beckers, 1968), so that one should see blue Doppler shifts of $\lesssim 20$ km s^{-1} from many of these features. Observational results have been reviewed by Beckers and further analysis made by Grossmann-Doerth and von Uexküll, 1971) which show line-of-sight velocities in mottles of only ≈ 4 km s^{-1}, the average being slightly red-shifted.

A possible explanation of this discrepancy may lie in the Doppler-shifted line profiles (Beckers, 1968, Table XII). The large rms values found are largely due to the high-velocity tails to the distribution. If we exclude the measurements made in active regions, because these may involve a somewhat different phenomenon, the *mean* velocities lie in the range 2.5–8.0 km s^{-1}. Now the disk measurements of mottles must refer to the lower, denser part of the spicule, and this moves up or down with mean velocity ≈ 4 km s^{-1}. We suggest that the high-velocity tail seen at the limb relates to a higher level, where the gas is moving upwards or downwards with velocities $\gtrsim 25$ km s^{-1}. The actual direction depends on whether the spicule is tilted away from or towards the observer and is not known. Accordingly, we have suggested (Piddington, 1972b) that spicules involve an upward gas motion averaging 2×10^{15} atoms cm^{-2} s^{-1} averaged over the solar surface (200 times the solar wind) followed by an equivalent *downward* flow. The latter comprises a flow of ≈ 20 km s^{-1} in the upper parts of a spicule (density 10^{11} atoms cm^{-3}) falling to 2 km s^{-1} in the lower parts. There is also evidence of associated wave motions propagating upwards with speeds of 400 km s^{-1} (Beckers, 1968).

These and other considerations have led to a model of spicules, polar plumes and quiet-region faculae (Piddington, 1972b) illustrated in Figure 3. The flux tube *B* is

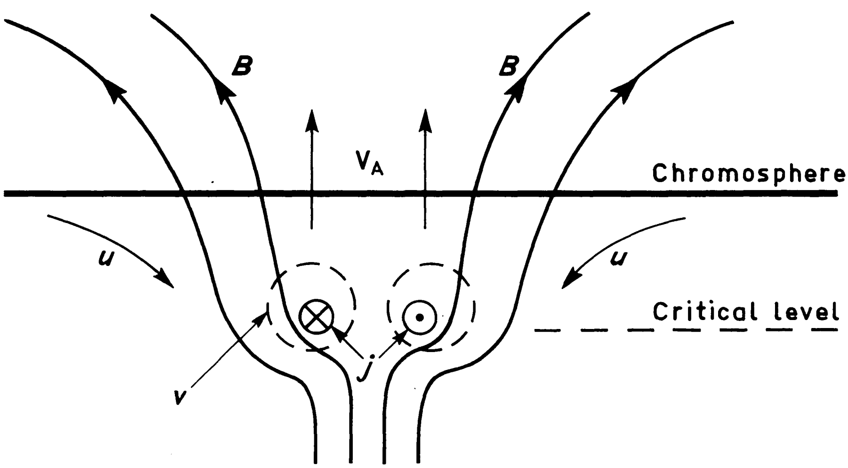

Fig. 3. A magnetic field configuration *B* following hydromagnetic collapse caused by converging gravitational-acoustic waves *u*. Above a critical level the field resists the plasma pressure, but a few scale heights lower it is strongly compressed. An inner current loop *j* introduces a Lorentz force with a vertical component. As shown by Altschuler *et al.* (1968), a vortex ring *v* moves upwards with the Alfvén velocity. It is invoked to explain spicules, polar plumes and heating of the quiet chromosphere and corona.

located in an SBR and is a detail of the field of Figure 1; the likely field strength indicated by observations is $\lesssim 1000$ G. The velocity field has a variety of components including the supergranule cell motion, the 300-s motion (Frazier, 1968), and the granule motions which may combine to provide localized velocities with peak values equal to the sum of the components, perhaps as much as 3 km s^{-1}. The vertical flux tube will easily withstand these motions in the chromosphere, but below a critical level given by Equation (6) the motions shown will compress the tube. For a field of say 500 G the critical level is at $\varrho \approx 2 \times 10^{-7}$ g cm^{-3} a little below that of unit optical depth. Since the scale height is only ≈ 200 km and the horizontal scale much larger, the transition layer is relatively thin and the field lines are bent sharply.

The hydromagnetic configuration becomes non-linear and extremely complicated so that only a qualitative discussion is attempted. However, there appear to be some interesting effects involved. First, there is the nature of the gas compression. which depends on the radiative relaxation times. At the lowest levels concerned, these times are less than 1 min (Ulmschneider, 1971) so that compression is nearly isothermal and the ratio of magnetic to thermal energy density increases in proportion to B. The relaxation times increase rapidly upward, so that above the critical level (Equation (6)) compression is adiabatic and the gas resists compression more effectively. This further narrows the region of transition from gas to magnetic domination.

Gas flowing into the SBR is blocked above the transition region but continues onward at lower levels. The likely result is that some gas flows downwards from the upper layers through the transition layer. For a fall of only 200 km the gravitational energy gained per unit volume of the original gas (mass 2×10^{-7} g) is 10^5 erg which is that of a field $B = 1600$ G. The fall may well be several hundred kilometres, particularly in view of the fact that the original supergranule motion is mainly downward near the boundaries. Thus fields of several thousand Gauss seem possible, particularly as we envisage inflow of gas from all sides to compress a cylindrical force tube.

This hydromagnetic collapse can neither continue indefinitely nor attain a stable state. The deformation implies the introduction of a current loop j shown in section in Figure 3, and when combined with the local, mainly horizontal, field the result is a mechanical force of density $f = j \times B$ which is mainly upwards. In that direction the inertial force opposing expansion is provided by a layer of thickness only one scale neight, and so an eruption appears likely.

A similar non-linear hydromagnetic disturbance has been analysed by Altschuler *et al.* (1968) as a model of surges. The model is necessarily simplified by the assumption of incompressibility, and a numerical solution is obtained over the period taken for the transfer of magnetic to kinetic energy until equipartition is achieved. They show that the magnetic perturbation propagates upwards at approximately the Alfvén velocity V_A. The whole perturbation corresponds to two current rings, one of which is shown in our Figure 3. This ring moves upwards and at the same time contracts towards the axis of symmetry. The second current ring is not shown here, but it is obvious that it must lie below and outside the ring marked j, so as to account for the lower bends in the field lines. This ring moves away from the axis as the field lines straighten, and needs no further discussion here.

At the base of the photosphere ($\varrho \approx 2 \times 10^{-7}$ g cm^{-3}) a field of 500 G provides upward velocities of $V_A \approx 3$ km s^{-1}. For corresponding gas mass motions, $u = 3$ km s^{-1}, the upward energy flux is $F_d = \varrho u^2 V_A \approx 5 \times 10^9$ erg cm^{-2} s^{-1}. This is roughly the requirement of the photospheric faculae, which may absorb most of this energy flux. A small residual is ample to account for the estimated spicule kinetic energy of $\lesssim 10^7$

erg cm $^{-2}$ s^{-1} (Athay, 1971) and the remainder of the network radiation and other losses.

4.3. Magnetic-plasma structures in active regions

Observational results have been summarized and extended by Frazier and Stenflo (1972) to show that almost all of the non-spot magnetic flux in active regions is concentrated into thin filaments of extent $\lesssim 1000$ km and strength $\lesssim 1000$ G. When the field strength is substantially greater, perhaps 1500 G, pores and spots form.

The extension of this pattern to three dimensions is effected by measuring both longitudinal and transverse magnetic fields (Severny, 1965) and by observing the evolution of an active region from its first hours (Weart, 1970; Harvey, 1971; Frazier, 1972 and others). It appears that flux tubes erupt through the photosphere over a period of a day or so, and organize themselves into an east-west bipolar configuration. Each flux tube forms an arch filament with two feet held firmly by the gas in and below the photosphere. The supergranule motions move the feet about, and if they separate further then the filament grows accordingly; their dimensions range threfore from zero up to the size of the whole active region. Thus the arch fllament system (AFS) appears to be the main structural feature of a spot-free active region, and so is likely to be the controlling factor in the energy balance.

A fully developed arch filament system constitutes a dome-shaped magnetic field which projects into the chromosphere and corona. As the strands emerge from the photosphere they lift cool plasma through the chromosphere, but at the same time plasma appears to flow down the field lines (Frazier, 1972) and presumably some collects at the feet of the arch. A second feature of such a group of AFSs is that some are more luminous than the surrounding chromosphere (Weart, 1970) and so may represent concentrations of hotter or more dense gas. A third feature of the dome field is provided by radio observations of the slowly-varying thermal component (Kundu, 1965) which indicate localized concentrations of very hot, dense gas. Such gas must be contained by a magnetic field, presumably of the dome form. Finally, such dome fields are often the sites of flares which develop from the pre-existing plage; these are discussed in the following subsection.

It would appear therefore that an arch filament system is likely to have its velocity, density and temperature distributions determined mainly by its magnetic field. The heat balance in such a system may have no relationship to that of the surrounding chromosphere, and must be studied quite separately.

4.4. The heat flux of sunspots

Apart from the general optical emission, the two surface phenomena which involve the largest heat fluxes are *flares* and the *sunspot energy deficit*. During a major flare, energy of unknown form is converted to thermal and mass kinetic forms at a rate of $\approx 3 \times 10^{29}$ erg s^{-1}. Because of its lower temperature a sunspot radiates less than the photosphere, the deficit for a large spot being about the requirement of a major flare and, incidentally, of the entire quiet atmosphere.

The flare energy source is usually assumed to be a store of energy located in the atmosphere before the flare commences, but a recent investigation (Piddington, 1972c) suggests that corresponding flare models are unsatisfactory. Accordingly an old suggestion (Piddington, 1958) is revived: that a flare is the result of an enhanced flux of Alfvén-wave energy through the solar surface during the flare (Piddington, 1973b).

Several writers (Danielson and Savage, 1968; Musman, 1967; Savage, 1969) have shown that 'overstable' oscillations are likely to occur in spot flux ropes. Savage has shown how their upward escape may be prevented, and Wilson (1972a) how their leakage may account for umbral dots and other effects.

We suggest that they may normally escape *down* the flux rope, but that this energy sink is sometimes blocked for some minutes to reverse the flow and cause a flare. The blockage may result from the collision of the spot flux ropes following the reversal of spot polarity; it is well known (Zirin, 1970; Sakurai, 1972) that such reversal is very effective in flare production.

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MAGNETIC FIELDS IN THE CONVECTION ZONE

Addendum by J. H. Piddington

The main problems of the heat and energy balance in the solar atmosphere might be listed as follows.

(1) The nature and energy flux of the mechanical waves responsible.
(2) Radiation losses and other energy transfers, up and down.
(3) A model of the atmosphere – which divides into three sections as follows.
(4) Models of small structural features – spicules, mottles, etc.
(5) Models of intermediate features – arch filaments, X-ray knots, flares, etc.
(6) Models of large features – active regions, quiescent prominences (filaments).

There is no doubt that magnetic fields play a major part in all of these problems, and that lack of an understanding of the fields *below* as well as above the surface has been the main cause of lack of progress. The major theories relevant to fields and heating may also be listed as follows.

(1) The dynamo theory of the origin and form of the magnetic field and of the 22-yr cycle of activity.

(2) The acoustic theory of heating.

(3) The theories of the concentration and strengthening of subsurface and surface fields by the various convective motions.

(4) Magnetic field annihilation across neutral sheets as a cause of flares, etc.

These four basic theories are rather closely related in that they invoke the control of fields by subsurface gas, which results in varying degrees of twisting and tangling of field lines. The theories have largely dominated solar physics for a decade or so. I believe that they are all incompatible with observations and invalid – I believe that collectively they have provided a major blockage to the advance of solar physics.

I will suggest an alternative picture of large-scale, enduring, subsurface fields and give a brief description – more details will be published elsewhere.

Following many earlier workers, let us assume a substantial flux rope which has been created by differential solar rotation *below* the level of convection. Let us assume that for an unknown region this rope develops an inverted U section which projects upwards and approaches the surface. The rope is not affected by convective motions and remains intact until its upper, horizontal surface nears the photosphere. The rope might have a diameter of 2×10^4 km which is about 100 scale heights at that level; its total flux might be 10^{21}–10^{22} Mx.

When the upper surface reaches a critical level where the gas pressure outside the rope equals the magnetic and gas pressure inside the rope, drastic effects must occur. The *upper* surface fields of the rope cannot be retained and must billow upwards and become weaker; fields at lower level are still intact. Small flux tubes lift from the upper surface of the rope and are then lifted through the photosphere by the supergranule motions. These are observed as arch filaments and as the main rope rises further, innumerable such filaments provide the arch filament system. Because the horizontal section of the rope spans several supergranule cells, the arch filaments are rather disordered, but a degree of order is imposed by their connections to the rope.

Finally, when the horizontal section of the flux rope emerges through the critical level there must be another rather drastic change in the surface pattern. The feet of all of the arch filaments will move towards one or other of the *vertical* flux rope sections and the whole pattern of the arch filament system will become ordered. The two vertical flux ropes will cause two spots to form, and if any of the arch filament magnetic fields are strong enough to provide spots or pores, then these must all flow into the main spots. This curious effect is entirely consistent with observations.

Spots endure sometimes for periods of weeks, and so must have some coherence to resist the convective motions. A likely explanation is the presence of a helical twist which will provide cohesive magnetic stresses. When the rope untwists it is likely to 'fray' by the loss of 'strands' or small magnetic tubes. This is also observed, because a spot may divide into smaller spots or may decay by the loss of small magnetic elements which are carried away by the supergranule motions. The original flux rope will fray to steadily increasing depths but it endures for months or years. The individual flux tubes may separate into smaller tubes as a result of untwisting, but there seems to be no reason why tubes of diameter $\gtrsim 100$ km (in the convection zone) should not last for a few years. They will be spread by differential rotation and by supergranule motions and eventually provide large 'preceding' and 'following' unipolar magnetic regions.

Each such region is under the control ultimately of the original subsurface flux rope, and the whole provides a magnetic structure which may extend below to $\approx 10^5$ km and across the surface for a few times 10^5 km. Such large structures appear to be essential to account for the observed stability and long duration of large quiescent prominences and of the large decaying magnetic regions with a single polarity observed over enormous areas.

Such a picture is, of course, quite incompatible with the dynamo theory of solar fields.

DISCUSSION

Sturrock: I want to comment on the comparison of the acoustic wave hypothesis of heating to the Alfvén wave hypothesis. I don't think it is fair to quote Osterbrock, who used the Lighthill-Proudman estimates for the generation of energy, because their formulation is based on the hypothesis that the wavelengths of the waves being generated are small compared to the scale height of the atmosphere. This simply isn't so.

Piddington: I think it's all right. I can quote 175 km for a certain depth, and that is exactly the scale height. I am fairly sure he took the scale height.

Sturrock: But the wavelengths of the waves being generated have wavelengths much larger than that, so the use of the Lighthill-Proudman formula is entirely inappropriate. One should use formulas that are based on the assumption that the scale height is in fact less than the wavelength. Under those conditions you will get much more efficient conversion of photospheric motion into propagating acoustic energy. It seems to me that one of the difficulties with the assumption that the energy transfer is due to Alfvén waves is that one would then expect a very strong dependence of the heating of the solar atmosphere on the magnetic field strength. In fact one would then get an energy flux depending upon the cube of the magnetic field strength. That means that if you went from 1 G to 10 G you would go up by a factor of 1000 in the energy flux, and 100 G would give you a factor of one million. It is my impression that the temperature of the corona goes up with the average field strength of the photosphere but not all that rapidly. It goes from one or two million to three or four million. This is not a tremendous variation and argues against the Alfvén wave hypothesis.

The other point I wish to comment on is the flare hypothesis, which is due to the conversion of energy that should come out of sunspots. The point here is that you have simply taken one fact about flares, namely the energy conversion, per unit area per unit time. Lots of other facts about flares against which you could compare your theory and our theories exist. I think it is very unfair to compare flare theories on the basis of only one factor.

Piddington: In answer to the first comment, I agree that attacking other people's theories is not very rewarding but I think that it is necessary if one is going to put forward an alternative theory, particularly with such a strongly entrenched theory as the acoustic theory. Even as late as 1967, Ulmschneider used the Böhm-Vitense model and as far as I can see much the same theory to develop his acoustic theory. Since then it seems that he has more or less postulated the existence of the waves needed to account for the heating that is observed. That is not a very sound basis and it leaves a critic in a very difficult position. What do you criticise about such a theory?

To your second question, I would answer that perhaps the energy does go up as the cube of the field strength, but that doesn't mean that it is all dissipated. In these loop structures, which are very common, the Alfvén waves could just propagate back and forth. A second answer to that – and there might be others – is that very strong fields will tend to suppress the motions that generate Alfvén waves, so it's not entirely clear-cut. Finally, if we invoke the kind of model that I have suggested, within flux tubes, the cubic dependence is not at all obvious because a very strong flux tube simply resists the flow of gas around it and the development of Alfvén waves then is very rare.

In connection with flares, naturally I did not base my theory upon mere coincidences with the sunspot energy deficit. Perhaps I should not even have introduced flares in this review. I think perhaps the strongest point in favour of the Alfvén wave theory arises from the difficulties met by the magnetic storage theories. This comes down to examining the magnetic configuration in many, many flares, in particular in the simplest, smallest flares. I agree with Kiepenheuer and de Jager that these occur as very, very small domes, which seems to exclude the neutral sheet. I don't think the Alfvén wave theory is more than a hypothesis but it is an interesting one. I hope the observers will look for motions of a few kilometers per second in sunspots.

Thomas: Could you give just a rough sketch of the energy dissipation as a function of height? It seems to me that you have concentrated mainly on the corona so far as the energy input is concerned and one worries very much about having all the energy put in way above the region where the conduction comes in. Could you sketch on the board some idea of how the dissipation varies with height?

Piddington: No. I think that's far too detailed a question. After all, you have a flux tube that you might imagine as a taut string without changing its cross section. This is vibrating in an atmosphere with a density scale height that is small compared to the height of the flux tube, and it's a very, very difficult question.

Thomas: But if you are going to criticise other people's theories you ought to at least make some kind of a rough idea as to the rate of energy dissipation as a function of height. That's the one thing that we have some kind of an observational handle on, even though it may not be very accurate.

Piddington: I think it's very inaccurate. I think it's also sufficient that with a little bit of very rough arithmetic I can show that the input from granulation with a photospheric speed of 1 km s^{-1} is more than 10^8 erg cm^{-2} s^{-1}.

Thomas: It seems to be a characteristic of theories for heating the chromosphere that at least six mechanisms can be shown to have potentially the amount of energy that one needs. The question is, do you really get the energy where you need it. That is the big point.

Piddington: You get it where the magnetic fields are. That seems to be overwhelmingly important.

Schmidt: I have a number of small comments. First your comment that the follow-up work of Parker has made much out of a factor of 10. Basically Parker showed that there is a parametrical change. The exponent in the mach number of the convection which comes into the sound production goes up by two, not by a factor of ten but by an addition of two. But this happens to be a parametrical change and can be very large, depending upon the details of the convection in the sunspot which we do not know. We have seen in this symposium some evidence presented by Giovanelli which seems to indicate that we might just have in the umbra of sunspots mechanical acoustic flux of several times 10^8 erg cm^{-2} s^{-1}. I admit that there are certainly difficulties with the production of acoustic flux and difficulties with the production of Alfvén waves. For the latter case I would only mention one. You really have to get the magnetic flux into the granules, and this is a clear-cut question of observation. It is terribly important to learn whether the average magnetic flux enters the granule or not. If it does then certainly your proposal has a very good chance. If it does not, it has none at all.

Piddington: Let me answer your comment on Parker's work. I would not have raised that point at all had it not been for Peter Sturrock. I raised it because I was afraid that he would if I didn't. I don't take much stock of it myself.

Schmidt: There is still a good chance that we might have much acoustic production in the model using strong fields, which might be sufficient, and I think Parker's argument is well taken that the acoustic flux wouldn't grow so terribly and would grow in proportion more or less to what we observe, that is by a factor of ten or so in active regions.

Newkirk: In connection with waves in the corona that are responsible for heating, I would like to remind people that the observations so far are completely inconclusive. Observers at HAO, Sacramento Peak Observatory and the people at Max-Planck Institut have collaborated to look for both compressional waves and Alfvén waves in the corona in both the green line and in white light. So far these efforts have shown no positive evidence. We do have the possibility in the Skylab experiments and of two successful experiments directed towards these ends carried out during the last eclipse; also Dr Liebenberg has reported that he sees changes over a period of several hours, so this point may be resolved perhaps during the next week.

Gabriel: I have a question about your rising flux tubes and the critical line that you have drawn. Would you not expect the pressure balance to be in a quasi-steady state with the external gas pressure balancing the magnetic pressure in the flux rope with the tube expanding so that you would always maintain equilibrium? In that case what is your critical line?

Piddington: Yes, I agree with that, but the scale height is only a few hundred kilometers and the thickness of the tube is perhaps 10000 km. It projects through several scale heights. The disparity becomes very acute and I mentioned that it should be a big expansion. Assuming that the field is weak enough, the supergranulation motion can take control.

Zirin: I like the idea of some lower influence on the magnetic field because I think that any model of the supergranulation must take account of the fact that the lifetimes of the magnetic structures in the quiet region are of the order of one day, whereas the lifetimes of plages are several weeks, with little apparent change. It is only on the edge of the plages where the magnetic fields are somewhat weaker that one sees evidence of the supergranulation and even there the lifetime is of the order of three or four days. We still don't understand how the large unipolar regions arise. We only see fields originating in the form of sunspots and arch filaments. Although you have drawn a very large flux tube you haven't shown how the rest of the group might come up in this weak field configuration. Secondly, do you have any idea about the asymmetry between preceding and following spots. I am referring to the well-known fact that preceding spots are bigger and live longer than following spots.

Piddington: So far as the arch filament is concerned, I have drawn the three supergranule cells at the top. I imagine that the flux loop at the top expands and weakens considerably so that the supergranules can get hold of part of the magnetic field and give a collection of arch filament systems that will tend to be oriented with the flux rope but also controlled considerably by the supergranule circulation. I would expect a rather random collection.

Zirin: That's great for sunspots but one does not observe a similar phenomenon contributing to the large unipolar regions. That is what bothers me with your picture.

Piddington: Surely that would be in the form of coronal arches or very enlarged coronal structures?

Beckers: When I look at your model of the upward-moving flux tubes and the similar one presented by Vrabec yesterday, and when I put in numbers for time-scales and distances, I get an upward flow of about $1\frac{1}{2}$ km s^{-1}. I expect that the Evershed effect might result from this upflow, resulting from the upward motions of the flux tubes. However a general upward motion of $1\frac{1}{2}$ km s^{-1} is well within observational capabilities and I am not aware of any observations that indicate such motions. Do you have any estimates as to what we might expect to observe?

Piddington: The flux rope breaks through the photosphere with the first arch filament and from then on it is continually shedding arch filaments. When they have all moved through the photosphere the flux rope is more or less vertical, and at that stage I cannot see why there should be any particular flow of matter. Every arch filament goes upward but it sheds its excess downward.

Bracewell: Could you make it clear to me why the flux loop rises in the first place. That is a difficult point. Schatten has mentioned that he is trying to revive Alfvén's old theory in which a toroidal loop propagates up parallel to the main magnetic field. This would fit in with the theory that I have. However I can't see a field of 3000 G propagating up a field of 1 G. I simply do not understand this and I don't think anyone understands. Buoyancy seems to be negligible since the gas pressure is so enormous and the magnetic pressure is negligible.

Wilson: Let me comment in answer to Bracewell. If you believe in giant cells – and I believe this requires a certain amount of belief – then it is natural to think of the flux ropes emerging in the region where the upflow in the giant cells is concentrated most, and one could develop a sunspot theory also related to the motion in the giant cells.

Piddington: But you do have to believe that the giant cells exist in the first place.

Bracewell: I wonder whether negligible buoyancy is a correct concept. If you have something deep down in water at immense pressures and the density is a little bit less than that of water, it will come up even though the buoyancy is very small. So if we had some way of explaining how the magnetic pressure could form a little below the equilibrium value, that would be enough. Perhaps it could occur that the flux rope would develop some instability that would cause it to be locally curved. Then if I use physical intuition with a little hand-waving, there is a longitudinal tension in the rope in the region of the bends. If there is any friction then you would expect a slightly smaller flux density in the region where you develop a loop, so the thing would expand a little with the same flux but a little less flux density. It would seem, therefore, that a random variation would lead to an instability and cause perturbations to grow. Perhaps by the time you have a situation where the flux rope was actually vertical, and especially if there is another bend in the flux rope below similar to that you have shown at the top, I can see very well that the tension in the lines of force would not be able to maintain the same flux density around the corners, and perhaps there is an increasing tendency for the rope to become more buoyant.

Piddington: If you limit the field to 5000 G, at some depth where the flux rope exists the gas pressure will exceed the magnetic pressure by 1000 times and the buoyancy factor is 1 part in 1000. If you squash the field and make it stronger and stronger, you can of course get any buoyancy you like, but I hate to admit fields of 100000 G because these have to be generated.

Smith: You have said that you think flares occurring low down cannot be due to magnetic field annihilation. Yet if you think of a flux rope coming up it has to interact, as Hal Zirin showed yesterday, with the flux that is already sitting there. Very possibly the flux coming up could indeed cause reconnection. Is there anything wrong with the idea that reconnection occurs low down by the inclusion of flux from below coming up to the flux that is already there?

Piddington: No, there is nothing wrong with that, but some of the pictures we have seen here as well as in the past few days showed a very marked neutral line separating regions of rather uniform polarity, so flux loops of opposite polarity would be very unusual and I cannot image them carrying very much energy.

Brown: In the absence of reconnection, how do you suggest getting such a large proportion of the energy into energetic particles?

Piddington: Are you referring to type III bursts?

Brown: No, I am referring to hard X-ray bursts. I don't know that the X-ray particles are accelerated as fast as the type III's, but the type III's pose a problem of very rapid accelerations and I am willing to give the type III's to the magnetic neutral sheet picture. The time-scale I am talking of is of the order of 1–10 s.

Piddington: That is a very difficult thing to describe.

Brown: The reason I mention the X-rays rather than the type III's is that they involve very much more energy.

Athay: I hate to bring up the subject again but I would like to make a comment about spicules in relation to the comment that Dr Schmidt made on Monday. We have all been guilty of ignoring the work done by the spicules in overcoming the gravitational energy. In making an estimate of the amount of work done in this process, I find for a typical spicule density of 6×10^{10} particles cm^{-3} and a vertical velocity of 25 km s^{-1}, the rate of doing work against the gravitational field is about 5×10^{8} erg cm^{-2} s^{-1}. This is a very large amount of energy. If you average it over the solar surface by assuming that the spicules cover 1% of the surface, you still find an average energy flux of 5×10^{6} erg cm^{-2} s^{-1}. This is an order of magnitude more than is required to heat the corona and is even somewhat larger than the energy required to heat the chromosphere. If this estimate is correct, then the major part of the mechanical energy coming into the atmosphere from below is going into the spicule motions and is used to overcome the gravitational energy.

Piddington: I would agree with that but I would put it in a different way. The spicules are driven by hydromagnetic forces in nonlinear Alfvén waves.

Schmidt: May I comment on this last discussion. I agree with the estimate; however part of the energy is regained reversibly. The spicules are basically a recurrent phenomenon and the energy put into them when they go up, to a large degree, but certainly not fully, comes back into the magnetic field and the matter falls down again.

Giovanelli: I would like to return to the emerging flux region. It seems that if you have an emerging flux region which is going to give rise to ropes that come out, then gather themselves together by some handwaving, as Piddington describes, these have to be twisted, and indeed that is what has been shown on the board. I am going to assume that a wreath of twisted rope can have little pieces that will shred off, but if this is the case, they will emerge with a particular orientation. When the top of that ropes comes up, the orientation will be in one particular direction, so we should see emerging flux regions with a typical orientation which reverses or at least twists around as the bottom of the flux rope comes through. This is something that should be observable, and I would like to ask the observers who study these flux regions whether they have found such an effect.

Frazier: That is exactly what I have observed. In a *Solar Physics* article several months ago I pointed out that one sees a sequence of arch filament systems over several days and one can trace the plane of rotation as successive loops come up. The top loop is rotated, but when you get down to the bottom it is indeed oriented at the proper angle.

Martin: We have observed the arch filament systems to rotate as much as 120° during the first day or two of the development of the region.

Zirin: I understand that when people study laboratory magnetic fields there are something like 132 instabilities that are always occurring. This doesn't seem to happen on the Sun. Except for flares, things seem to be surprisingly stable. Is there any reason for this?

Schmidt: Nature looks for the stable solution.

Zirin: Yes, but one should see it going from the unstable towards the stable. Do you think it has had enough time to relax by the time that we observe it?

Piddington: I think it is very strongly relaxed. There is really no difficulty here, it just provides coherence.