PART IV.

Considerations on Localized Velocity Fields in Stellar Atmospheres: Prototype — The Solar Atmosphere.

C. - Transient Velocity Fields in the Lower Solar Atmosphere.

Discussion.

Chairman: R. LÜST

— R. LÜST:

We add first a few observations which may be useful for the aeordynamicists; data on the spicules and on the different types of radio bursts. Then we consider the motion in sunspots; it is important to know how sure we are that the motion is along the magnetic lines of force and to discuss the problem if there are motions across the magnetic lines of force. Second, we should discuss the motion in prominences. We are faced with the problem of how important is the magnetic field in the motion of quiescent prominences and in the eruptive prominences. As a third subject we have the flares. Finally, I would emphasize that we should also discuss the spicules, and return to the question whether the spicules are related to the granulation, and are the same phenomena we see in different layers. We see the granules in the photospheric layers, and we see the spicules in chromospheric layers. I think the main problem on flares is their eruption, what kind of forces are involved, and which are the most important features we can explain.

— R. N. THOMAS:

Refer to the accompanying schematic representation of the properties of the spicules, and where they occur on the surface of the sun. For orientation, I have included the height parameter, the temperature parameter, and optical depth parameters—in the continuum at 5000 Å, in the Lyman continuum, and in H_α of hydrogen, so you have an idea where things occur relative to rocket spectra as well as the visual spectral region. First point on spicules —there are a great number of them; they are small objects moving with relatively high speed. Number is something like 10^4 at the solar surface at any one time. Speed: ranges between 20 and 100 km per second; 100 is for an abnormal spicule; a spicule reaching the height of 20000 km is an abnormal spicule. Most of them get to around 10000 km. The maximum height reached shows a good correlation with speed. An original impulsive motion decelerated under gravity is as good a representation of time, distance data on spicules as is any. Density in a spicule, we can guess roughly to be 10^{11} protons per cm^3 down at the height where they first appear around 4 000 km. All observations of the spicules refer to heights of 4 000 km and above. The theories which have been made on spicules refer to 4 000 km and below, where there are not spicule observations presumably because there is so much obscuring material. I don't cover theory here, I simply mention there has been some attempts at constructing several.

TABLE I. – *The spicule system and its environment.*

Height	0	500	1000	1500	4000	20 000
← 500 km to photosphere			← inhomogeneous region begins →		spicule structure appears in broad-band H_α filter	
T_e	4000° ·|· 4500°	6000°	8000°	cold - $1\cdot 10^4$ hot - $(2\div 5)\cdot 10^4$	proton flux upward in spicule system $\sim 10^{38}$ s^{-1}	
n_H	10^{15}	10^{14}	10^{12}	10^{11}	↑ 20 km/s $< V < 100$ km/s	
τ (λ 5000)	.01	10^{-5}	—	—	$n\sim 10^{11}$; $T_e\sim (2\div 5)\cdot 10^4$	
τ (LyC)	—	10^4	100	<1	*spicule* height $\sim 2\cdot 10^4$ km	
$\tau(Ly_\alpha)$	—	—	10^6	10^3?	$< 10^8$ km width spicules cover 1% of surface	
τ (H_α)	—	$50\div 100$	20	—	↓ 10^4 total over sun	
					n (interspicule) $\sim 10^9\div 10^8$	
					T_e (interspicule) $\sim 10^5\div 10^6$?	

There are only observations at the limb—nothing on the disk which has been unambiguously identified as a spicule. At 4 000 km the typical spicule has an upper limit of 1000 km in diameter. With the density already given, 1% of the solar surface or less is covered by spicules. Yet, if you compute the total flux of material in a spicule, using a typical velocity of about 30 km per s, you get roughly 10^{38} protons per s ejected into the solar atmosphere. Warning: one sees spicules going up and coming down again. The figure given is only the number of atoms going up, so that the net number of atoms supplied is something less than that. But there is probably not a factor as large as 10 in the difference, if one takes the number of spicules going out, minus those coming in. Roughly, there are 10^{42} protons in the entire corona; so roughly in three hours the spicule system feeds in enough material to replenish the corona. The uncertainty in this conclusion is that in the figures given. Character of the medium through which the spicules go, uncertain. It looks very much as if at the height of 10 000 km, the interspicular medium is at 10^6 temperature, rather than a small number. I want to emphasize this point relative to the diagram distributed the other day. Basis: observations of the coronal lines at the eclipses, show that Fe X exists at heights of 10 000 km, maybe lower. Maybe already at 4 000 km we have such temperature in between spicules. The situation between 1 000 km and 4 000 km is uncertain. Note that the density of this region is near 10^9, so we have a temperature of 10^6 with 10^9 protons per cm^3 between spicules. In a rough way, the problem then is to tie what I summarize here with the structure of the chromosphere and with the structure of the photosphere discussed in the preceding sessions. I will not attempt to talk about the interrelation of structure now, however. Note also, there is a variation in distribution of spicules from pole to the equator, and possibly a variation in orientation; I do not think the question is well-enough settled that I would like to say something about that. I just want to stress, however, the great importance of spicules for the solar astrophysicist in terms of loss of material from the solar surface; the velocities involved; and the interrelation to the medium we have been talking about. From the point of the aerodynamicists, if I look at these velocities and take a thermal velocity corresponding to the table; I see that if these spicules do indeed extend downwards to the lowest chromosphere or photosphere region, I do indeed have a superthermic phenomenon which maybe is similar to a supersonic jet. If the spicules only extend as low as 4 000 km, and between the spicules is just the coronal medium, then the spicules are subsonic phenomena. But this is a point where one has to tie a theory to the structure of the medium. I would like to stress this uncertainty on the medium, and this is the reason I make such a point here of what the character of the interspicular medium is relative to the character of the spicules in terms of the things that one wants to interpret. For more details, refer to the book *Physics of the Solar Chromosphere* by R. G. ATHAY and myself, and the thesis by R. B. DUNN of the Sacramento Peak Observatory.

— R. B. LEIGHTON:

Are spicules not often observed going out at a considerable angle with the vertical and then coming back along the same line?

— R. N. THOMAS:

Yes, the polar spicules are more vertical than the spicules at the solar equator.

— R. LÜST:

Polar spicules sometimes can be seen to have a tilt connected with the tilt of the polar rays.

— K. O. KIEPENHEUER:

If you look at the spicules on the disk, then it appears that the spicules are not distributed homogeneously over the solar surface. On the disk, the spicules can be seen in the H_α-pictures as small dark spots, which seem to be arranged in a kind of network, which obviously is coinciding with the network we observe in the calcium flocculi; the Ca^+ pictures in the undisturbed region of the sun. The network can last for several days. The spicules seen on the disk seem to have the same lifetime and total number as those seen at the limb.

— R. N. THOMAS:

May I emphasize that the spicules are really only defined on the limb; if I interpret a disk observation and identify it with a spicule, I have already introduced an interpretation of the data. I do not disagree with you that the disk observations may be spicules, I want only to emphasize this point of comparison between observation and interpretation of observations.

— K. O. KIEPENHEUER:

I will survey the different velocities which occur in the disturbed parts of the solar atmosphere.

1) *Flare regions*: We have learned already, that velocities between 0 and 600 km/s have been observed in the bright parts of the flares. From the flare region so-called « surges » or « flare surges » are ejected (appearing dark against the disk and usually brighter than prominences against the sky) with velocities between 50 and 250 km/s. They seem to follow the magnetic lines of force. Most of them return along curved paths to the sun.

2) *Prominences* are cool formations ($T \sim 5\,000$ to $10\,000°$) in the corona ($T \sim 10^6$ degrees). They show internal motions of 10 km/s or more, have a lifetime of weeks or months, they can rise with velocities up to 700 km/s (thermal velocity in the corona ~ 200 km/s). There is no observational evidence, whether the corona is moving with the prominences. There are different types of effects of flares on prominences (filaments), which work up to distances of several hundred thousand km with velocities of 20 to 100 km/s. An unknown agent is being radiated away from the flare, affecting form, stability and internal motion of the prominence (filament). This effect is obviously

TABLE II. – *Velocities and energies of active sun.*

I) *Velocities*

Life	Phenomena	Velocity	Data
Weeks	Plage regions	~ (0.5 ÷ 2) km/s	Doppler
Days	Sunspots	(0.5 ÷ 8) km/s	(Doppler, away from center of disk)
Months	Quiescent prominences	Internal motions 10 km/s	(Doppler and visible displacement)
Hours	Ascending prominences	(50 ÷ 700) km/s	Displacement
Hours	Coronal motion (internal)	10 km/s	
Minutes	Coronal whip	~ 600 km/s	—
Minutes	Flares	Internal motions (0 ÷ 600) km/s	Doppler
Minutes	Flare surges	(50 ÷ 250) km/s	(Doppler and visible displacement)
Minutes	Steady streams and flows of gas producing sequences of terrestrial disturbances	⩽ 1000 km/s	
Minutes	Effect of flares on existing prominences	(100 ÷ 1000) km/s	
Minutes	Effect of flares on triggering other flares	(1000 ÷ 1500) km/s	—
Minutes	Radio bursts type II (flare associated)	~ 1000 km/s	—
Seconds	Radio bursts type III	$\sim \frac{1}{3}c \div \frac{2}{3}c$	—
Hours	Radio bursts type IV (flare associated)	Highly correlated with ensuing geomagnetic storms, $V \sim 500$ km/s	
Hours	High speed gas generating magnetic storms ⩽ 1500 km/s		

II) *Energies*

Large flare: $> 10^{32}$ erg (radiated energy)
$> 10^{30}$ erg (particle emission, 10 MeV ÷ 30 GeV per proton)

Radio emission from large flare $\sim 10^{25}$ erg
Ejected mass $\sim 10^{19}$ g (total $\sim 10^{34}$ erg)
Total energy content of quiet corona $\geqslant 10^{32}$ erg implies annihilation at 500 G in the flare

being propagated through coronal volumes with densities of 10^8 to 10^9 protons and electrons/cm^3.

There is another effect of importance:

3) When a flare is occurring, the probability that other flares will occur in the neighborhood is larger than random. This can only be explained by saying that one flare is triggering another flare. And this triggering effect has been observed all over the hemisphere, mostly by BECKER. The velocity of this triggering effect, which is propagating along the surface of the sun, probably in the corona, is about 1000 km/s. It could be occurring in a lower layer but then it is more difficult to understand this velocity. It might be a « solar quake ».

4) Then we observe after the flare, on the earth, a geomagnetic storm, and this storm obviously is produced by clouds of corpuscles which are ejected somehow from the flare. The travelling velocity of this cloud of corpuscles, deduced from the fact that the geomagnetic storm starts about one day after the flare, turns out to be of the order of 1000 to 2000 km/s.

5) Now I come to the radio bursts, which give us the possibility of deducing some velocities. Let me, in a few words, explain radio burst: It is assumed that something is travelling through the corona upwards from the flare, or in any direction from the flare, exciting plasma oscillations in the corona, the frequency of which depends on the electron density in the corresponding path. So some agency moves through coronal regions of decreasing density if upward, increasing if they go down, and therefore the frequency of the emission will change correspondingly. If we observe the change of frequency as a function of time, we will be able to deduce the velocity in the corona. From this simple idea the type II and the type III radio burst can be understood. For the type II radio bursts, obviously something must move away from the flare again with a velocity of the order of 1000 km/s. This velocity, at least for the type II burst, has been confirmed by interferometric observation; so they could follow the transmitter through the corona and they were able to tell something about the orbit. This phenomenon takes a few minutes.

6) There are shorter living phenomena called type III bursts which correspond to velocities of $\frac{1}{3}$ or $\frac{2}{3}$ of the velocity of light. These phenomena which are much faster last only a few seconds. The type II and type III occur after or specifically during flares. We must imagine that something is moving through the corona with this speed; I have to add that type III bursts occur not only with decreasing frequency, *i.e.* with increasing height, but also are observed coming down. So some of the bursts are started in the high corona and then move downward; and in other cases this burst has the shape of a U, called

U-bursts, which means that the travelling agent is coming up and then coming down again. This may be interpreted saying that something is moving along the lines of force.

7) Then there is another motion in the corona which has been observed at Sacramento Peak with the coronagraph; there was a kind of « whip » motion of a streamer changing in a few minutes from one static configuration to another. The velocity which is necessary to explain this deformation turned out to be of the order of 600 km/s. This is the only case, to my knowledge, that a motion of such a high velocity of the coronal matter in the corona has been observed optically. There is no evidence if this is a motion of matter or a motion of an excitation wave or something like that.

— A. B. SEVERNY:

I would like to add other phenomena connected with the flares. The first one is the outburst of cosmic rays with energy of about 10 GeV; and the second one is the outburst of more slow protons, which was measured during the IGY, protons with energy of about 10^6 eV. One more phenomenon is the measurements made by LYOT, ROBERTS, WALDMEIER *et al.* connected with the motions in the green corona. This is specific coronal emission, and the motion of the knots which may be observed in the green line give a velocity which is not very high, something around not more than $(10 \div 30)$ km/s. This may be interesting because some people tended to identify the motions in the corona with the corpuscular stream itself.

— R. B. LEIGHTON:

In studying the effects of flares at some distances away from where they occur, there is one effect which appears in some motion pictures taken at the Lockheed Observatory (*) and which seems quite striking.

There is a flare at some point on the solar disk, and some distance from it there is what is called a disk filament, which is dark and narrow region (as seen in H_α), which remains quiet for many days, perhaps for several solar rotations. Then it often occurs that shortly after the eruption of the flare, a certain part of this filament will be « evaporated » and suddenly disappear from the disk filament. Many examples of this have been observed in the Lockheed Observatory film and characteristic propagation speed is about 1000 km/s. So I add an eighth point here: flare effect on filament—about 1000 to 1500 km/s. I may mention also that for the same flare where the filament disappeared, one sometimes can also see one or more other flares at some distances away which are sometimes called « sympathetic » flares; as

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(*) R. G. ATHAY and G. E. MORETON: *Ap. J.*, in press.

Kiepenheuer said, these are statistically unlikely to occur independetly and certainly they suggest that somehow, whatever instability made one flare also made the other one, possibly by a propagation of the « trigger » in each direction from some point which could be remote from both flares.

— K. O. Kiepenheuer:

I want to add another point, namely, that the effect on filaments from flares is not isotropic, it can happen that one filament which is close by the flare is not affected at all, and another in another direction which is far away, is strongly affected. From that one may possibly infer something about the nature of the force acting.

— R. B. Leighton:

I am sure it is connected with the problem of propagation along magnetic field lines.

— H. Petschek:

Why associate radio frequency bursts with plasma frequency instead of with cyclotron electron frequency?

— F. Kahn:

Radiation can be propagated in an ionized gas only if its frequency ν_{rad} exceeds the plasma frequency ν_{pl}. For radiation of a given frequency, the opacity of the medium increases with ν_{pl}. The maximum contribution to the energy in a given ray therefore comes from the level where ν_{pl} is largest. For a ray leaving the solar corona radially this occurs where ν_{pl} and ν_{rad} are equal.

— F. H. Clauser:

If we have a non-linear wave propagation outwards, and if it decreases in intensity as it moves outward, then you should observe a change in speed. Do you observe such change in these observations of triggering action?

— K. O. Kiepenheuer:

I think it is impossible to get such information from the few data existing, but from the radio bursts it turns out that the velocity does not change in spite of the fact that the density change is a factor 10^3 along the path of the burst. Such information is really important for fixing the nature of these phenomena.

— M. Minnaert:

There is a narrow relation between items 4 and 5; so that we see that the geomagnetic phenomena are closely related to type II bursts.

— K. O. **Kiepenheuer**:

This is very probable; however, there are more features than type II bursts. It could be indeed that even 3, 4 and 5 [3 (triggering of flares), 4 (travelling speed of corpuscles from sun to earth), 5 (type II burst velocity)] might be the same because of the same velocity of about 1 000 km/s. We do not know for certain yet.

— W. H. **McCrea**:

Are there other regions except flares from which particles are coming?

— K. O. **Kiepenheuer**:

To my knowledge, there is nothing else besides a flare which does such a thing; because all of the bursts are correlated with flares.

— M. **Krook**:

Type III bursts are very important in solar activity, and they very often occur when there is no visible activity on the sun; so one can assume that there are small, very very small flares which are not visible in H_α, but which will trigger type III bursts.

— R. **Lüst**:

Could you comment about polarization of the bursts?

— M. **Krook**:

The information is complicated and inconclusive.. Neither Wild nor Maxwell is very keen on committing himself at the moment about polarization information. It is a very difficult measure to make.

— C. W. **Pecker**:

Are not type IV bursts important in this matter?

— K. O. **Kiepenheuer**:

Type IV bursts are supposed to be clouds of matter floating in the corona; going up with the velocity of several hundred km/s; radiating because of the synchrotron mechanism; so it is assumed that there are electrons of some million volts in these clouds and there must be a magnetic field of the order of a few gauss in order to explain the intensity of the radiation and the observed strong polarization.

— R. **Lüst**:

This type IV bursts are really very strongly polarized, nearly 100%.

— M. **Krook**:

They are a continuum as compared with the comparitively narrow types II and III.

— C. **de Jager**:

It is good to remark that it is not the type II bursts which are principally correlated with geomagnetic storms, but rather the type IV bursts.

— F. H. **Clauser**:

You observe things moving along a magnetic field under gravity; if you try and deduce a magnetic field of the whole sun, do you get consistency from day to day; or do the magnetic patterns change so much from day to day that there is no pattern?

— K. O. **Kiepenheuer**:

In the outer part of the corona, there are very few observations so that we have not yet direct knowledge of the variation from day to day. In the inner part, around the sunspots, there are evident variations from day to day in the shape of prominence motions, in the shape of the ejection of surges, and also from the change of polarization from day to day in certain radio observations.

— H. **Liepmann**:

In the same line as Clauser's question, can one say anything about inconsistency between the steady magnetic field of the sun, and a field produced by fluid motion? *I.e.* are the observed field lines and the observed streamlines free of contradiction?

— R. **Lüst**:

If somebody has a good answer to this question, we would already be near the solution to a number of these problems.

— J. C. **Pecker**:

I have two feelings when looking at prominence motions. Very often you have a prominence flowing in a certain way, apparently following magnetic lines of force, or approximately so, and the prominence disappears after some time; quite often it reappears later, showing the same pattern of motion. The second point is that a very common thing is that knots in prominence motion are spiralling, possibly around the magnetic lines of force. Evidence for following the lines of force does not seem to me conclusive.

— L. BIERMANN:

With regard to the spiralling motions, or what appears as a spiral motion in the prominence, which sometimes occur, we have directed our attention in Göttingen and in Munich to the resemblance of the observed pattern to the force-free fields which have been investigated by LUNDQUIST, LÜST and SCHLÜTER, and others. We would like to suggest the possibility that the observed motion in such prominences, which appears as helical, implies that the motion is guided by magnetic fields of the force-free variety. I think this observation is one of the indications that such fields really do occur in nature.

A second point is Liepmann's question as to whether or not in general one could speak of consistency between what one could derive from the motion themselves and from the different information about the magnetic field from spicules, spots, etc. As far as I know there is consistency in the sense that we are unaware of any violent discrepancy between the information which could be derived from the several sources, which are available. But we have to keep in mind that the observations regarding the magnetic field are of such a kind, that we get no unique information about geometrical properties. We can only put together different pieces of evidence, make a reasonable guess, and compare again with what we see. As far as I know, no obvious inconsistencies appear to exist.

— F. H. CLAUSER:

Does anything like a dipole structure appear in the steady state?

— L. BIERMANN:

For instance, we know the following: the field of a sunspot is what you would expect if you would regard the spot as a source or sink of magnetic lines of force and arrange the currents in some way along and below the surface. I think what you would see in the prominences is reasonably consistent, with the picture that one gets from the photospheric observations. Ordinarily it looks as if there would be no strong currents above the surface affecting the magnetic field. Exceptions, of course, are the indications for fields of the force-free variety; the force-free fields are not potential fields because of the currents which flow along the lines of force.

With regard to the dipole character of the « general » magnetic field of the sun, things have become very complicated since the recent discovery by BABCOCK of two years ago, which revealed that the pole fields of the whole sun have reversed their polarity. This makes somehow doubtful the earlier suggestion that this polar field has to be regarded as dipole field of the whole sun. Now there are some suggestions as to the answer, but this should perhaps be excluded from the present discussion.

— E. SPIEGEL:

A question to SEVERNY—about the azimuthal field you described in sunspots. I wonder if you would elaborate your argument for their existence, and describe their distribution if you know anything about that, and suggest the magnitude of the azimuthal component in sunspots?

— A. B. SEVERNY:

I am sorry, I wish I could do that but we do not have any method to measure the azimuthal field.

— E. SPIEGEL:

You suspected the existence of a non-radial component?

— A. B. SEVERNY:

It is just a guess. I find some indication of possible existence of this azimuthal field, and this indication is the following: We tried to establish this azimuthal field by scanning the spots near the very border of the sun. If we have an azimuthal field, then we must have a radial, for instance, a north component at this border of the spot in an upward direction, and from the scan through the south border we should get a downward direction of the field. Thus, by observing the spot at the borders of the sun, we can try to establish this component. I have some indications of the existence of this field, but they are not conclusive as yet. This is what I mean; but I am sorry we don't yet have methods permitting us to determine the azimuthal component of the magnetic field; this is a very hard job. Now, I would like to add a few words regarding Clauser's remark.

There were observations made at Boulder which show that the assumption of dipole field for spots is in satisfactory agreement with the observed motion of prominences. As far as I can remember the picture, it was considered theoretically that the dipole field is a little below the surface; and they calculated the field lines picture at different inclination of the dipole to the solar surface. The observed motions were compared with the location of dipole field lines and they found pretty good agreement between the two.

— J.-C. PECKER:

J.-L. LEROY in Meudon made very nice measurements of the transverse field in sunspots using polarimetric measurements. This, of course, is very important because, between the time it is at the center of the disk and the time it is at the limb, the spot and the magnetic field of the spot can be changed. If you have a method to measure transverse magnetic field, then you can measure topography of the field at the same time. Actually, many spots have been measured this year, and I think the fine resolution of the magnetic field is quite good. I don't have here many details.

— A. B. SEVERNY:

The problem is not to measure the inclination of lines of force, because we can measure inclination of magnetic force just using the simple Seares' formula. This was applied at Mount Wilson Observatory since 1920; your method is, of course, better; but how to determine the azimuth of the projection of transversal field, this is the question, that has not been solved as yet; that's what I mean.

— J. TUOMINEN:

The basic problem of the appearance of sunspots is more a problem of the solar interior than one of the solar atmosphere. Now I should like to ask especially SEVERNY: Do the velocity and magnetic fields in sunspots give any indication on 1) whether a long-lived sunspot is a phenomenon which is continuously coming out from its source beneath the photosphere? or 2) is the sunspot a phenomenon which once comes out from its source beneath the photosphere and then lives a shorter or longer time at the solar surface? This question is important, for instance, when movements of sunspots are studied.

— A. B. SEVERNY:

Except for the Evershed effect I mentioned in my talk, and disregarding fine structure, we observed sometimes a lifting of the whole region connected with the spot; but, owing to the bad seeing, we don't have indications on fine structure of this motion. And, of course, in most of the cases these motions are also masked by the usual Evershed pattern, and we cannot distinguish clearly the motion of the spot as a whole and the Evershed pattern.

— R. LÜST:

As far as I remember, the measurements from the Evershed effect of the motions in sunspots had given velocities of the order of one to two km/s. Now SEVERNY has reported velocities up to about 8 km/s. Could he comment?

— A. B. SEVERNY:

These velocities are higher than the velocities reported in the usual literature, because they refer to a fine structure, and are determined by careful study of the line-profile.

— R. B. LEIGHTON:

Is it not true that the 7 or 8 km/s refers to a very small region at the outer boundary of the penumbra and not to a velocity distributed over the disk of the sunspot as found by Evershed? Second, does the velocity correspond to outward flow or inward flow of matter?

— A. B. Severny:

These high velocities correspond to the outer boundary of the penumbra. The sign of the velocity follows precisely that found by Evershed. The U is the umbra in the spectrum and P is the penumbra (see diagram). Bumba found that if we try to draw the form of spectral lines, something like the following picture called by him the flag phenomenon takes place. He has not been able to measure the motions in the umbra itself and his results refer only to the penumbra. This is what I am speaking about.

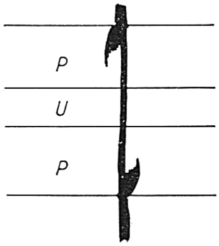

Fig. 1.

— J. Rösch:

Concerning the Evershed effect—you may have noticed that on the film by Spiegel several days ago there were motions just at the edge of the boundary between the penumbra and the umbra. We have also found in our first attempt to have moving pictures of the granules and sunspots, something of this sort. I think this is a point which must be looked into very carefully, because my feeling up to now is that one will find here, just on this edge, velocities higher than the average velocities inside the granules and inside the penumbra. I should not be surprised that here on this edge we find velocities of several km/s and this will be quite consistent with the velocities indicated by Severny.

— J.-C. Pecker:

Either there is a correlation between thermodynamic structure of the spot in penumbra and umbra, and the motion, or there is not. Now from the recent measurements made in Meudon, I think that the pattern of Evershed motions measured by Servajean show little correlation, for complex spots, whatsoever with the shape of umbra or with the penumbra also. There is a very strange pattern which looks more like the maps of the magnetic fields that Severny showed which also show no strong correlations with shape of the umbra or the shape of the penumbra.

— R. B. Leighton:

We have also studied motions around sunspots with our Doppler device. If we observe a spot with both umbra and penumbra near the center of the disk, it is invariably true that the velocities from H_α, the Na D-lines, etc., are essentially zero within the outer boundary of the penumbra. We find no evidence for motions on a scale larger than $1'' \div 2''$, which would exceed 0.1 km/s. However, just *outside* the penumbra one finds very pronounced radial lines of flow, starting at the boundary of the penumbra and shading off as we go away. If the sunspot is seen foreshortened near the limb, one sees the same thing. However, in the λ 6103 line of calcium, there may be indications of an outflow or inflow of matter—generally the former from our observations. In H_α there will be opposite directions of motion on the two sides of the spot; we interpret this to mean the gas moving in toward the sunspot is moving very nearly *horizontally*, along the surface of the sun. Schematically, I visualize a picture of sunspot structure consistent with this, as follows. We have the granulation outside the spot, the penumbral region following the converging lines of force toward the umbra, the penumbra making a small angle with the solar surface. The hydrogen, being free to move along lines of force, moves essentially in a vacuum outside the penumbra, but is suddenly stopped when it strikes the denser atmosphere at the boundary of the penumbra.

Note that we sometimes find an eruptive prominence coming from some point on the edge of the penumbra. MICHELSON observed these first and you will find accounts of them in the early issues of the *Astrophysical Journal.* He pointed out (from his visual observations) that these start from the outer boundary of the penumbra. But we also find that in an eruptive prominence not only is there an outward motion, but also often a component of *rotation* so that one edge of the prominence may be moving toward us and the other side away, as if there were spiralling around lines of force. This has been seen several times, although it is by no means a universal property of eruptive prominences.

Continuing to another point, we have obtained some interesting results on the K line of Ca^+ which, although the interpretation is not completely clear, seem sufficiently striking to be pointed out here. We took a spectroheliogram of the solar surface; however, as the slit scanned the solar surface, we also scanned the slit past the spectral line, so that we obtain a combination spectrogram and spectroheliogram. It has been known for a long time that the K_2 emission is stronger in the *violet* component of the K_2 line. I think we have tracked this down to a difference in the *kinds of features* which produce the emission. We see many sharp, bright points of emission scattered about the disk, and these are more numerous and brighter in the violet component of K_2 than in the red. The slide shows the effect. One-half was centered on the red component of the spectral line and the other one on the violet; they are otherwise (with respect to the exposure and all the subsequent photographic treatment) identical, except in being different regions on the sun. You see that there are many more regions per unit area (many of them very tiny) which emit ligth on the violet side of the line than are present on the red side of the line.

I suggest that the calcium emission comes from regions which may be very small, in many cases rather point-like, and which are preferentially moving upwards as they emit. I think these might have something to do with spicules.

— J.-C. PECKER:

I want to draw attention to one point, which must be considered when discussing the dynamics of a sunspot; *viz.*, empirical determination of the gas pressure at a given geometrical level. Because there is a magnetic field, the gas pressure within and outside the spot may of course differ. The point is, that present measures of this differential gas pressure do not seem conclusive. The measurements by MICHARD of some years ago gave a pressure in the umbra which was equal to about 0.2 that in the photosphere; a rough estimate of magnetic plus gas pressures gave them equal to the gas pressure in the photosphere. From more recent measures by LABORDE, it seems that this was wrong, and that the pressure in the umbra is actually much bigger than it was thought before, at the same level, and is almost of the same size as the pressure in photosphere. These types of data are important to the problems being discussed today; I want to emphasize how difficult it is to obtain results.

(*Ed. note*: There followed an inconclusive discussion between ELSTE, PECKER, MINNAERT on reliability of relative geometrical scales within and outside sunspot.)

— E. SPIEGEL:

I'd just like to ask how LEIGHTON envisages the general mass flow around a sunspot; in particular, how does he satisfy the conservation of mass, what are the sources for the inflow, where does the mass go, and such questions.

— R. B. LEIGHTON:

When I discussed our Doppler effect measurements I showed a plate of the motions far from the center of H_α, all over the disk, and these were predominantly downward motions. I think that when one looks at limb prominences with Lyot filters, one sees predominantly downward motions in the quiescent prominences. I would regard the inward flow, seen in H_α, to the outer boundary of the penumbra as being merely another example of the general downward flow of the hydrogen gas from the corona. Now the reason motion stops at the boundary of the penumbra, it seems to me, is that the density increases so greatly that to keep the conservation of mass the velocity correspondingly decreases and goes below our resolving power.

— E. SPIEGEL:

Would you think then that the penumbra is basically cool with bright streaks resulting from the inflow of hot material?

— R. B. Leighton:

No, I don't think the bright streaks have anything to do with the part of the material that is coming in. I think these are probably convective cells that are seen edgewise. The material has to move along the lines of force; I'm assuming that the lines of force go in that direction, which seems likely. So I think of the bright lines being convective cells and, as the lines of force leave the surface, you just have a boundary region to which the hydrogen can come more or less freely from the relative vacuum in the chromosphere and corona outside. This is a qualitative picture, and there may be some quantitative difficulties with it which I haven't yet discovered.

— E. Spiegel:

If you're thinking of convection, I can't understand why it circulates in that way. The convection in the model for sunspots that I've mentioned previously by Danielson is supposed to be occurring in rolls, the rolls having their axes radially in the penumbra, and these correspond to the filaments in the film that was shown. And here now, we're confronted with another kind of flow, transverse to that presumed convective flow. This has also been called convection and it is, I think, confusing.

— A. Unsöld:

Perhaps just a word of explanation. Looking at the motions in a sunspot from a hydrodynamical viewpoint, one should be aware that the outward motion which one sees in the Evershed effect of the usual metallic lines is really the main phenomenon and comprises by far the largest mass. What one sees as inward motion in H_α and in H and K takes place at about the level of the spicules; it is only a secondary phenomenon. Observations also show that the whirls which one sees on the H_α-spectroheliograms have nothing to do with the magnetic field but are simply determined in the same way as the circulation in terrestrial cyclones and anti-cyclones by the Coriolis force.

In a cross-section things would look approximately like Fig. 2. In the main level of the photosphere, the motions must have come up near the umbra and then go outwards. And only in the high level the spicules—or one may say just as well very small prominences—move inwards. You should compare my remark to the well-known observations that also large prominences are frequently drawn into the sunspots. The sunspots exerts—we don't quite understand how—an attractive force on prominences, and what we see as inward motion in H_α and H and K is evidently a phenomenon of the same nature. This is something quite different from the outward motion in the ordinary Evershed effect, which gives the $(6 \div 8)$ km/s near the outer edge of the penumbra and velocities of about 2 km/s nearer the umbra. If we try

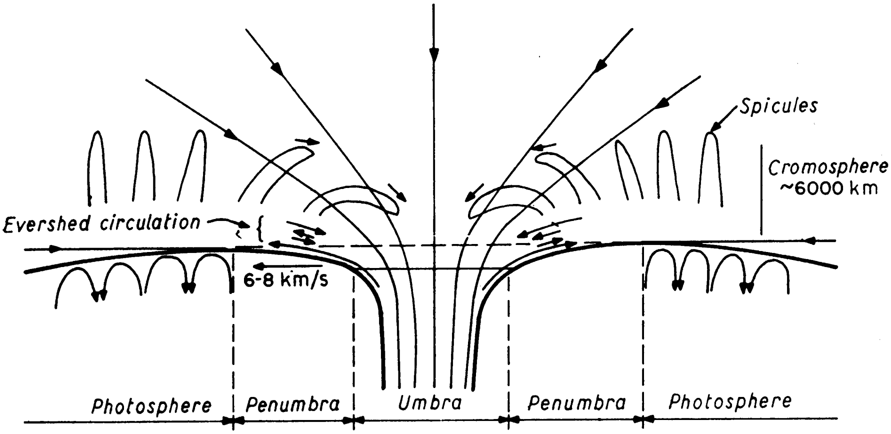

Fig. 2.

to collect our ideas into a hydrodynamical theory of sunspots, we should in any case begin with these outward motions and try to complete this circulation somehow. The inward motions are evidently a secondary phenomenon. But this is a wholly theoretical matter.

— A. J. **Deutsch**:

Do you ascribe the vortical patterns that we see in the H_α-spectroheliograms to Coriolis force acting on those outward motions?

— A. **Unsöld**:

The spicules are pulled in by the same force which pulls in the large prominences, and that motion is accompanied by a Coriolis force giving the right curvature. There is no connection with the sign of the magnetic field.

— R. B. **Leighton**:

It seems to me that what is primary and what is secondary in importance depends upon which part of the sun you're interested in. For many purpose we're interested in what happens outside, where the prominences are, and it seems to me that what the hydrogen clouds are doing out there makes a difference. Also, we are very much interested in establishing, as far as possible, the relations—if any exist—between these motions in outer and lower regions. So first, let us be a bit more definite on the motions in the lower regions.

(*Ed. note*: There now followed a disordered discussion trying to establish which lines showed outflow, which inflow, and the place of origin of these lines. Unsöld's diagram has been expanded a bit following Leighton's suggestions and the « consensus » from the floor. Mrs. **Böhm-Vitense** estimated mean depths $\tau_0 \sim 0.1$ for the lines showing the Evershed pattern; no estimate was placed on differential depth between lines showing inflow and outflow in the Evershed pattern. Again, the question of differential height within and outside a sunspot of the same line was raised and not answered.)

— R. B. LEIGHTON:

With this information on hand, I would now remark that the motions in H_α that we observe occur at a much higher level and presumably correspond to motion downward along lines of force that aren't just the ones that go out of the photosphere, but the neighboring ones as well. It is, however, significant, I think, that the boundary of the motion for the inward-moving H_α is geometrically the same, as far as we can tell, as the boundary of the sunspot as seen in integrated light, which perhaps implies only a small height difference in the atmosphere for these two things—much less than indicated in the figure. Prominences may come in along the lines of force as well.

— A. B. SEVERNY:

In this connection, I would mention that together with BUMBA we measured magnetic fields in the chromosphere above the spot. We reported in *Observatory* two years ago that from measures of the Zeeman effect in the center of H_α, H_β and Ca^+ H and K we found an upper limit of some 500 gauss (as compared with some 3000 gauss in the spot—*ed.*) My results this morning showed records of the field in the chromosphere above the spot of some 60 gauss. So if we compare the kinetic energy in the chromosphere with the magnetic energy in the field above the spot, the former is a little smaller. So above the spot, we have a picture in which the magnetic field organizes the motion in some way.

— C. DE JAGER:

In drawing the magnetic field lines, they are assumed parallel throughout the star body and then they diverge suddenly close to the surface. Drawings like this are often found both in scientific and in popular accounts. It should be made clear, however, why we do it like that. Intuitively, one would think that the lines diverge at the limb because the sun « ends » there. But it is clear, of course, that the limb of the sun is only the point where the solar body changes from opaque to transparent; this has nothing to do with the magnetic field, and the density decreases continuously outward there as smoothly as it does 1000 km higher or lower. From that point of view there would not be the slightest reason to assume the field lines parallel just to the sun's surface and to let them diverge higher up. If there is a reason to make them divergent in the chromosphere they must be divergent too in the lower regions.

The only reason I see for making them divergent just at the limb is because there the solar matter is mainly neutral.

— R. B. Leighton:

De Jager's point is well-taken, but it seems to me that the visible boundary of the sun represents more than just the place where the light comes from. It also represents the place where the density is increasing so rapidly downward that it and the pressure go up to enormous values only a few hundred km below the visible surface, to values which can very well provide the pressures which it takes to constrain lines of force of the order of 3 000 gauss strength. So, while it is quite true that we don't know within several hundred km just the height at which this can take place, several hundred km in height is very small compared with the many thousands of km size of a sunspot.

— L. Biermann:

I would like to make three points connected with the discussion thus far. First, relative to the Evershed effect, it seems to me that after the report of Severny, there is no reason to believe any more that there is a sort of average motion across the magnetic lines of force. Such motion would contradict the constancy of the magnetic flux, which is to be expected from the value of the electric conductivity.

Second point was the inhibition of the convective energy flux by the magnetic field: that came up already in the discussion. I think the current picture of why a sunspot appears dark is that outside the spot, underneath the photosphere, the energy is carried largely by convection whereas in the spot the convection is affected strongly by the magnetic field, which is strong compared to the kinetic energy of turbulence. It is gratifying to see from the discussions which we had in the last few days that while the whole theory of convective energy transport has become considerably more complicated than astrophysicists usually believed, it is obviously now well within the range of theoretical possibilities to have motions—certain types of convection—of several km/s in the spots as well as outside the spot, but—in the presence of the strong magnetic fields—no energy transport in the spots, but effective transport outside the spots. This is, of course, no answer; but simply an emphasis on a problem which is essentially a theoretical problem, which is very important in the theory of sunspots.

The third point has, as far as I recall, not come up in the discussions. It is the following: As was discussed already a long time ago by Cowling, the apperance of a spot on the solar surface must mean that the magnetic field is carried to the surface by mass motion. I don't know whether any observations which were discussed, or which are possible now, give any indication of mass motions connected with the appearance of a spot on the surface or

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the disintegration of a spot to a magnetic patch, or the disappearance of both to the normal quiet state of the photosphere. One would not expect very large mass motions; just as a guess I would expect something of the order of 0.1 km/s or so, so it might well be below the level of observation. But in addition to all the observations which have been and are being carried out, I would suggest that particular attention be given to events of this kind, that is to say to phenomena which are appearing during the birth of a sunspot and during the later stages in which it changes its large-scale structure.

— H. LIEPMANN:

Why do you say the convection is inhibited? I can see that turbulence is inhibited, but if you have a large mass motion from the center on up, should that motion be inhibited as well?

— L. BIERMANN:

I think the convective motion that shows up an Evershed effect is probably connected to a very thin layer, and so it is not at all obvious that the mass is really considerable, and that the energy which is connected with it plays any particular role. The idea I discuss is connected with the state of observations of about 10 years ago. At that time the observers told the theoreticians that in a spot there was no turbulence. And on that basis it was suggested that the absence of turbulence was brought about by the magnetic field, and therefore no energy transport by convection. Now, recently, it has become apparent that in the umbra there are both structures and motions, and therefore we have the somewhat more complicated problem that we have a type of motion which probably differs inside and outside the spot. The theoretical problem which I emphasized was to get more insight into the conditions under which convection—in the presence or otherwise of a magnetic field—can or cannot carry energy.

— F. M. CLAUSER:

If you carry this 8 km/s motion back along a magnetic line this indicates that material is being brought up from below. This is convection.

— L. BIERMANN:

Well, this may be suggested tentatively, but I think everything we know is consistent with the possibility that actually this outflow is a phenomenon in quite a thin layer as compared with the dimensions, and therefore the velocity which we have inside the spot—from the divergence, from continuity, is very much smaller than you would infer from what you observe here on the edges.

— F. H. Clauser:

Yes. It may be small, but it is still a velocity from in to out and doesn't this carry hot material out?

— L. Biermann:

Yes, but the density is so small that for this purpose it can be neglected. But I must confess I have not made this estimate. Anyhow, it is not available.

— E. N. Parker:

One can begin with Biermann's point, that the strong magnetic field in a sunspot inhibits the convection so that the convective transport beneath the spot is at a somewhat lower rate than in the normal convection zone. Since this gives a lower temperature in the interior of the sunspot, it is easily shown that the field is further increased, and convection inhibited even more, etc.

Consider a spot which is perhaps 10 000 km across and presumably therefore of comparable depth. Now, 10 000 km at the temperatures you see on the sun is something like 30 or 40 scale heights, the scale height being the vertical distance over which the pressure drops by a factor of *e*. Suppose that I have a very weak column of magnetic flux within which it is slightly cooler than outside because of convection inhibition by the magnetic field. Thus, the scale height inside will be less than the scale height outside. Now the pressure inside drops off rapidly, starting down at some base level deep in the sun. And so 30 scale heights or 50 scale heights above the base level, the gas pressure inside the field is considerably lower than the gas pressure outside, even though the temperature difference may be very slight over the entire range. Suppose the temperature is only 1 or 2% different between the inside and outside. In 50 scale heights, you still get a factor of 2 between the pressures. We must have, of course, hydrostatic equilibrium. That is, pressure outside (if you neglect curvature of the lines of force) must be equal to pressure inside plus $B^2/8\pi$. Now if you have too low a pressure inside, the pressure outside merely caves in the tube of flux squeezing the material out along the lines of force and increasing the field intensity B. The outward flow of gas goes on until the increasing $B^2/8\pi$ makes up for the deficit. Increasing B further inhibits convection, cools the gas inside the field, so that the pressure drops still more, etc. In this way the spot develops. This is the extension of the arguments which Biermann began with his remark that the strong field inhibited the convection.

— E. Böhm-Vitense:

I would just like to make the point that this is, of course, only right as long as the temperature in the spot is lower than the surrounding photosphere.

But if there is really no convective energy transport, or at least much less than in the surroundings, you can calculate very easily that already in relatively high layers one gets a temperature which is higher in the spot than in the surroundings.

— E. N. PARKER:

If convection was stopped completely you are right, but presumably it is only partially inhibited.

— E. BÖHM-VITENSE:

In any case, I think this is what you would expect, because if the convective energy transport is stopped somewhere below, then you would expect the temperature to rise below this level, because the flux gets stuck there and will heat the layer below the sunspot. Nevertheless, I would think that the Evershed effect might be a non-stationary phenomenon because I think the sunspot cannot ever reach a stationary state. One can calculate the time which is needed for a disturbance in temperature to reach the higher layers of the spot. If the spot would be a few thousand km deep it would be nearly 1000 years. And since the spot only lasts but a few weeks, I think the spot cannot be in a stationary state. So I think the Evershed effect may very well just show that the spot is not in a stationary state. If the gas in the deep layers of the spot gets heated, then, of course, you have to push up material in order to keep the equilibrium of pressure in those deep layers.

— V. D. SHAFRANOV:

It seems to me, that there are some arguments in the theory of hydromagnetic equilibria, which support the idea of an azimuthal magnetic field in sunspots, developed in the report by A. SEVERNY.

Let us assume, in accordance with the Alfvén idea, that the magnetic field of a spot is a ringformed toroidal flux-tube, emerging out of deep layers. As is known, the magnetic force lines tend to contract, so a force of attraction F_1 arises which is $F_1 = (B_\parallel^2/8\pi)\cdot\pi\alpha^2/R$ per unit length. In this case the radial velocity of the ring must be of the order of

$$-v_r \sim \frac{B_\parallel}{\sqrt{4\pi\varrho}}\cdot\sqrt{\frac{\alpha}{R}} \gtrsim 1 \text{ km/s} ,$$

i.e. much higher than the observed velocity of approach of spots. So

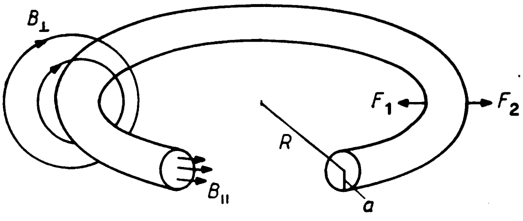

Fig. 3.

it is necessary to have for equilibrium a current along the ring, which produces an azimuthal field $B_\perp$, and the force F_2, opposite to the force F_1.

I have proposed (*Žurh. Ėksp. Teor. Fiz.*, **33**, 710 (1957)) that there may exist an equilibrium stable configuration in a fluid, having just such structure. It is reasonable to suppose, that a spot represents a cross-section of this configuration.

The connection between azimuthal and longitudinal fields in this configuration is as follows

$$B_\perp = \frac{B_\parallel}{\sqrt{\ln(8R/\alpha) - \frac{1}{2}}}, \quad \text{for } B_\parallel^2 \approx \text{const over cross-section},$$

$$B_\perp = \frac{B_\parallel^{\max}}{\sqrt{2[\ln(8R^3\alpha) - \frac{1}{4}]}}, \quad \text{for parabolic distribution of } B_\parallel^2.$$

The difference between gas pressures outside and inside the ring is positive, and less than the pressure of the longitudinal magnetic field:

$$p_e - p_i = \frac{\overline{B_\parallel^2}}{8\pi} - \frac{B_\perp^2}{8\pi} > 0.$$

— R. Lüst:

This ends the discussion on motion in sunspots, and we change now to the subject of the flare phenomena.

— C. de Jager:

I comment not so much on the observations as on their interpretation. Often a flare is pictured as a region where suddenly much heat is released. But I think the most important phenomenon which we observe in a flare is the sudden and large increase in density. Let us picture here the situation. The chromosphere has an electron density of 10^9 or 10^{10} particles per cm^3. In the corona it is 10^8. The flare arises in a few minutes, and we observe it to have a density of the order of 10^{13}—so you see in a very short time interval the density exceeds that of the surroundings by a great factor. I think this phenomenon to be the most fundamental one of a flare. It is true that a flare may also have a higher (or even lower, depending on whether it is formed in chromosphere or corona) temperature than the surroundings. Different values are quoted in the literature; depending on the way it has been found one gives 10 000°, 50 000°, even 100 000°. But that is not the main point. I think the essential point is that in a very short time a region of the corona or chromosphere, depending on where it is, collapses to a very high density. A secondary aspect is that such a collapsed region can emit more radiation: the number of particles per cm^3 is greater, there are more recombinations, etc.

— F. H. CLAUSER:

When you say collapse, this would imply the particles come from outside. Are you sure they don't come from below?

— K. O. KIEPENHEUER:

There is some evidence against this idea, because the structure of the chromosphere underneath the flare doesn't change at all. So there can't be a big flow of mass. I think this is quite crucial.

— ZD. SVESTKA:

I should like to mention one observation which may have a connection with the velocity field in flares. SEVERNY mentioned here this morning that according to the observations made in the Crimea for flares on the limb, the Balmer lines in these flares are broadened by some flare motions. I am not sure that all flares can be interpreted in this way, because many flares can be described also in terms of Stark or damping broadening. But there are several flares quite certainly where this explanation in terms of Stark broadening is not possible because, first, the wings of the Balmer lines do not follow the law of the Stark broadening, second, because these flares are evidently optically thin. We can observe the absorption lines through the emission of the flare. And because we need for the Stark effect a great number of atoms in the line of sight, we must explain the broadening of the lines in these « thin » flares by means of some velocity field. We observed one large flare on the solar surface on July 20, 1958, where we took a series of spectra during the whole development of the flare. This flare is optically thin, and it seems that the lines there had to be broadened by some velocity field. If the Balmer lines were broadened by Doppler effect, then if we plot $\log \tau$ ($\tau =$ optical thickness) *vs.* $(\Delta\lambda)^2$ we should get straight lines. We get such straight lines only just for three minutes in the flash phase of the flare, not for the parts before and after this. We can get such straight lines, however, if we plot $\log \tau$ *vs.* $(\Delta\lambda)^m$, using $m < 2$; this same effect was already observed by JEFFERIES and SMITH at Sacramento Peak. They tried to explain it by non-Maxwellian velocity distributions in flares. But there are some difficulties. The first is that we do not observe corresponding broadening of the lines of calcium and the lines of helium which would

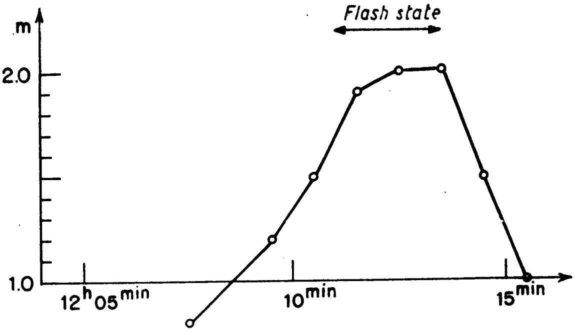

Fig. 4.

give much smaller velocities than those from hydrogen. And second it is rather curious that just in the flash phase we get these Maxwellian distributions of velocities and not before and after. The value of the quantity m changes as shown in the figure. There would be another possible explanation; namely, that there appears a change of the Doppler width; namely, of $\Delta\lambda_D$ inside a flare even if the velocities were Maxwellian. Such a change could give rise to such fictitious straight lines for other values of the power of $(\Delta\lambda)$, than 2. Then this graph would mean that this change in $\Delta\lambda_D$ was very large in the beginning and end of the flare. But, during $(2 \div 3)$ minutes, there was no change at all. The situation 4 minutes after the maximum of the flare was roughly the same as 4 minutes before. The velocities, of course, were rather large, they were higher than 200 km/s in the flash phase. This cannot be explained as due to temperature, because this would require temperatures of the order of coronal temperatures, more than one million degrees. And, therefore, we are obliged to assume some microturbulent motion inside the flare. And I should ask SEVERNY how was the situation in the limb flare he observed at the Crimea; were the Ca^+ and helium lines inconsistent with the hydrogen lines with respect to velocity or were they not?

— A. B. SEVERNY:

There are some cases in which we observed them. Now we are observing flares with echelle gratings, which permit us to obtain the whole spectrum in the range from 6 800 up to 3 200 Å, in the form of strips corresponding to the different regions of the spectrum. And sometimes from the observational standpoint it is most important to fix the flare on the slit. And we do really observe sometimes this same picture in the Ca^+ lines H and K and in the helium lines—not only for D_3 of He I but also in λ 4686 of He II. We really observed the same wings—very broadened wings as in « mustaches », in these lines simultaneously. But there are some flares in which you can only observe hydrogen emission, but no broad calcium emission and helium emission appear. Sometimes you can observe very strong emission in metallic lines, but emission in hydrogen is comparatively weak.

— A. UNSÖLD:

I was struck by seeing one of the spectrograms taken by SVESTKA, that on this particular spectrum, the metallic lines were quite narrow. How is the situation in your case? Do the metallic lines also show these high velocities of the order of 1000 km/s?

— A. B. SEVERNY:

In some of my cases, the metallic lines are broad.

— A. J. **Deutsch**:

In this Symposium I have heard of many things that I do not understand. But now I wish to inquire about one point that I do not understand more thoroughly than any of the others. This is the equation which governs the time rate of change of the magnetic field in a conducting medium,

$$\frac{\partial \boldsymbol{H}}{\partial \tau} = \frac{1}{4\pi m \sigma} \nabla^2 \boldsymbol{H} + \nabla \times (\boldsymbol{V} \times \boldsymbol{H}) .$$

If one computes the order of magnitude of the right member for the case of a static medium, in order to find the time of rigid free decay for a field comparable in size with a typical flare, he gets, conservatively, 10^4 years or more. The observations of **Severny** indicate that, in the course of a flare, the magnetic field in the reversing layer changes drastically, at least in some parts; and this has also been supported by theoretical arguments advanced, I think, by **Parker**. A flare typically releases most of its energy in about 15 minutes. If I did my arithmetic right, the ratio of these two time intervals is of the order of 10^8. Moreover, if fluid motions exist, they cannot accelerate the dissipation. The Laplacian alone gives the dissipative part of the time change of $\boldsymbol{H}$. The curl term dissipates nothing; it just convects the lines of force some place else. How is it done?

— L. **Biermann**:

The problem of how this can be reconciled has been thought of in several steps—and can be found in a paper by **Sweet** as a contribution to the Stockholm Symposium on Electromagnetic Phenomena in Cosmical Gases. The main point is that this equation has to be applied with some care if mass motion along the lines of force occurs. The second point is that the resistivity in certain layers is very greatly increased by ambipolar diffusion. That plays a great part as has been pointed out an several occasions by **Schluter** and myself. When you combine all these factors and take into account the special factors introduced by neutral lines in neutral surfaces, you approach an answer. Since at least 1948, it has been recognized that these neutral lines play a serious central role in the discussion of what happens in a flare.

— E. **Schatzman**:

I would like to say a few words more on the question of the origin of flares associated with neutral points. **Severny** this morning has already reminded us that when we have on the surface of the sun « hills » of opposite polarity, we have in between these hills a region where the magnetic field vanishes, and that is what is called a neutral point. This is not to be confused with the regions along which the magnetic field, being transverse, is not seen on the magnetograms. It has been proved already by DUNGEY on one hand, by SWEET on the other that a neutral point is a region of instability, and it was appealing then to try to explain the appearance of the flare at the neutral points by a special kind of instability. For that purpose, I have studied a magnetic field of a much simpler nature—that is, a magnetic field which is periodic in x and y and decreases exponentially in z. Or is constant in the z direction. I won't draw the picture of the magnetic field, I just want to mention that there is a periodic structure of neutral lines of force—that means neutral lines along which the magnetic fields vanish. The magnetic field which has been used is a so-called force-free magnetic field. We have the advantage that there is no magnetic force so that the pressure equilibrium is realized with a constant pressure and that simplifies the calculations. With that special choice of the magnetic field, I have tried to see whether there was stability or not. It can be seen that there exists perturbations which are unstable if some characteristic value of the magnetic field is greater than a constant times the gas pressure: $B^2 > P_G \cdot \text{constant}$. The constant is of the order of unity but has not been found by the theory. So, though I think it is an oversimplified problem, I think it goes in the line of the observation of SEVERNY and worth mentioning briefly here (paper to be published in *Rev. of Mod. Phys.*).

— E. N. PARKER:

I want to call your attention to some of the numbers characterizing solar flares. Let me restrict my remarks to a large solar flare—they come in all smaller sizes so you can scale down my arguments as much as you like. The energy from the large flare (the radiant energy, the visible energy) is not less than 10^{32} erg. LÜST suggested the number 10^{33} erg, and I think that is quite a reasonable estimate. The flare does a number of things, most of these large flares now are observed to emit protons with energies anywhere from 10 MeV up to as high as 30 GeV. Most of them do not emit energies much above 100 MeV, but the total energy in this particle emission is 10^{31} erg, or even 10^{32} erg. Now I might also add that from the gas which blows past the earth a day or so after the flare, you deduce that there must be something of the order of 10^{18} g of matter ejected with a kinetic energy of about 10^{34} erg. Now the first and obvious question is where does the energy of the flare come from. The thermal energy of the entire solar corona is only about 10^{32} or 10^{33} erg. Even if you could bleed the corona on all sides of the sun and feed it into the flare spot, you would probably not have enough energy to run things and, of course, there is no known mechanism for doing this. The observations show that the corona is unchanged during a flare, even fairly close to the flare. People have therefore been forced to the idea, that you have heard frequently expressed today, that the flare energy source must be a magnetic field. Well, a big flare might easily be 10^4 km high and it might be 30 000 km on a side—

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you find that if this entire volume is filled with a field of 500 gauss and if the onset of the flare completely annihilates that field, then you will have enough energy to perhaps account for the flare. There has been so far suggested no other answer to the riddle of the energy of a solar flare.

— H. **Liepmann**:

It is certain that the energy comes from the flare?

— E. N. **Parker**:

No, but the flare is what is making all the noises and waving all the flags and one assumes that it is the center of the energetics. There is no other disturbance that can be seen on the sun at the time of the flare, so it would be even more mysterious if the energy came from a quieter region.

— H. **Liepmann**:

No, but it could be the same cause that causes the flare to produce itself.

— E. N. **Parker**:

You are correct. The visible energy is in fact perhaps only a small portion of the total energy, and therefore might be a decoy.

— I. K. **Csada**:

I would like to comment on some statistical evidence for the general magnetic field of the sun.

In the following a statistical method will be proposed for evaluation of Babcock's magnetograms in order to study the structure of the general magnetic field of the sun. As the local fields show random fluctuations and suggest the existence of magneto-hydrodynamic turbulence the usual representation for the local field is as follows

$$H = \bar{H} + H' ,$$

where the mean value $\bar{H}$ may be considered as the general field and may be supposed to be governed by the differential equation deduced for the averages.

In the magnetograms the component of the magnetic field in the line of sight is recorded *i.e.* we may write

$$H_x = \bar{H}_x + H'_x$$

and the determination of $\bar{H}_x$ may be carried out planimetrically. As it seems theoretically possible to suppose that the symmetry of the magnetic field is axial (magnetic axis) the field may be represented by the vector potential

$$A_\varphi = \sum \frac{h_n P_n^{(1)} \cos\psi}{r^{n+1}} ,$$

where ψ is the polar distance related to the north magnetic pole. Simple deduction show that the x component of the mean value of the magnetic field along constant θ is

$$\overline{H}_x = \sum \frac{2nh_n}{r^{n+2}} P_n(\cos q) P_{n+1}^{(1)}(\cos\theta) ,$$

where q denotes the distance of the magnetic axis from the axis Z.

Let us introduce the following notation

$$A_n = \frac{2h_n}{r^{n+2}} P_n(\cos q)$$

then the magnetic strength on the solar surface is

$$\overline{H}_x = \sum A_n P_{n+1}^{(1)}(\cos\theta)$$

which will be considered as an interpolation formula.

This expression will be used for the model of a two-term potential function (DO model) in the following form

$$\overline{H}_x = A_1 P_2^{(1)}(\cos\theta) + A_3 P_4^{(1)}(\cos\theta) .$$

As the calibration factor is not given for all magnetograms, we must reduce the analysis to the non-dimensional parameter $h = A_3/3A_1$ being independent of the calibration, that is

$$c\overline{H}_x = P_2^{(1)}(\cos\theta) + 3hP_4^{(1)}(\cos\theta) ,$$

where

$$h = \frac{h_3}{h_1} \frac{1}{r_0^2} \cdot \frac{1}{2} (5\cos^2 q - 3) .$$

In the statistical analysis h was determined for 250 disc recordings (magnetograms) which were made in the Mount Wilson Observatory and were sent by Babcock to the University of Szeged. Values of h were found between 0.0 and 1.0, but a well defined grouping appears at 0.4. The explanation of the spread is as follows:

i) It is possible that the structure of the magnetic field characterized by h_3/h_1 would be a random function of the time and we can mention « general field » in the statistical sense.

ii) From the statistical point of view h_3/h_1 may be nearly constant in time (its random fluctuations are very small), but the orientation of the magnetic axis varies in the system XYZ.

A periodical variation of h seems to appear from time to time syncronously with the synodic rotation of the Sun. This fact suggests a deviation of the magnetic axis from the rotational axis. As q is given by the expression

$$\cos q = \cos s \cos v + \sin s \sin v \cos \omega(t - t_0)$$

(where s is the distance of the axis of rotation from Z and v that of the magnetic axis from the axis of rotation) it is clear that q and also h must show two periods: annual period and rotational period. Both periods are found and shown in Fig. 5 and 6. From the rotational period it was possible to estimate the co-ordinates of the north magnetic pole

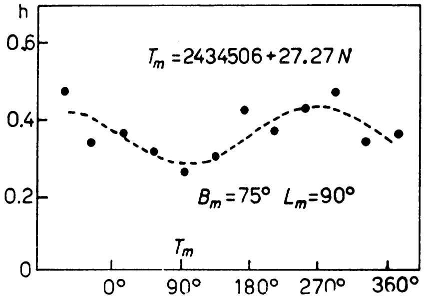

Fig. 5.

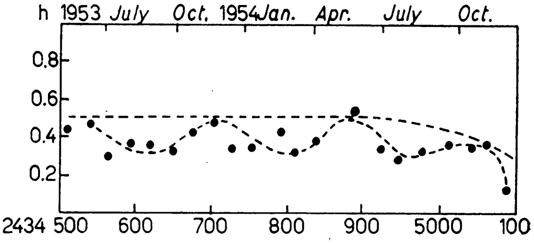

Fig. 6.

$$B_m = 75° \qquad \text{and} \qquad L_m = 90° .$$

Now, I shall show some consequences of these results relating to the solar atmospheric phenomena.

The disturbances which are generated in the photosphere propagate through the chromosphere into the corona as Alfvén waves (magnetohydrodynamic waves) and magneto-acoustic waves.

It is possible to limit areas in the field of the DO model in which no Alfvén waves can proceed radially. From this point of view the discussion like the one published by BILLINGS at the High Altitude Observatory seems to be important. This paper contains observations of some wave motions in the corona, and the anomaly is pointed out at $B = 50°$ where no Alfvén waves can proceed in DO model.

Finally, I should like to mention that the orientation of the spiculae seems to follow the lines of force of the DO model better than that of a simple dipole field. Fig. 7 shows the orientation of the spiculae (derived by LIPPINCOTT, published in the *Contribution of the Smithsonian Institute*) and the line of force of the DO model at $h = 0.4$. I think such an interpretation of spiculae may be important to determine the solar magnetic field during the maximum activity when Babcock-magnetograms are not evaluable in this way as the local fields are too large compared to the general field.

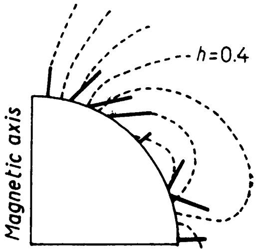

Fig. 7.