Some Problems in Coupling Solar Activity to Meteorological Phenomena

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The development of a theory of coupling of solar activity to meteorological phenomena has to date foundered on the two difficulties of (1) devising a mechanism that can modify the behavior of the troposphere while employing only a negligible amount of energy compared with the energy necessary to drive the normal meteorological system, and (2) determining how such a mechanism can effectively couple some relevant magnetospheric process into the troposphere in such a way as to influence the weather. If such a mechanism exists, it appears that we may not be able to define it without understanding much more than we do now about long-range weather behavior. A clue to the nature of the interaction between the weather and solar activity might be provided by the fact that most solar activity undergoes a definite 11-yr cycle, and meteorological phenomena undergo either no closely correlated variation, an 11-yr variation, or a 22-yr variation.

It is safe to suppose that the search for correlations between occurrences in the heavens and events on the Earth dates into prehistory. Many such efforts come to mind, including, for example, the hypothesis that the position of the Sun, Moon, and planets controls human destiny (for example, astrology), or the practice of beating tom-toms during an eclipse to restore the Sun (a correlation that has been conclusively verified by thousands of independent experiments). Some such searches lead to fruitful outcomes. For example, the connection between sunspot number and geomagnetic activity was suggested almost as soon as both phenomena could be clearly identified. Schwabe's discovery of the sunspot cycle was announced in 1851 after he personally had collected two cycles of data. The next year, Sabine (1852) reported results showing that geomagnetic activity appeared to vary cyclically as did the sunspot number. There was a setback to this line of research when Kelvin (1892), who at the time held the powerful position of president of the British Royal Society, denounced this correlation (illustrated in fig. 1) as a "mere coincidence." The concept that this correlation exists, survived, however, because the result could be reproduced cycle after cycle.

After the discovery of the correlation between sunspot numbers and geomagnetic activity, there were attempts to establish a relationship between sunspot number and a variety of items, such as the occurrence of the aurora, animal and plant growth, stock market prices, the temperature of the thermosphere, the frequency of volcanic outbursts (see fig. 2), cosmic radiation, suicide rates, variations in the solar constant, and, of course, the subject of this conference—the weather. Of these items, only the aurora, the temperature of the thermosphere, and the solar-cycle variation of the low-energy component of the cosmic radiation are accepted and generally understood. It appears that correlations in geophysics are not easily established.

CORRELATIONS IN GEOPHYSICS

Why is it that, with few exceptions, one finds such difficulty in establishing a causal relationship between two geophysical phenomena, or even in saying what regularity might govern the
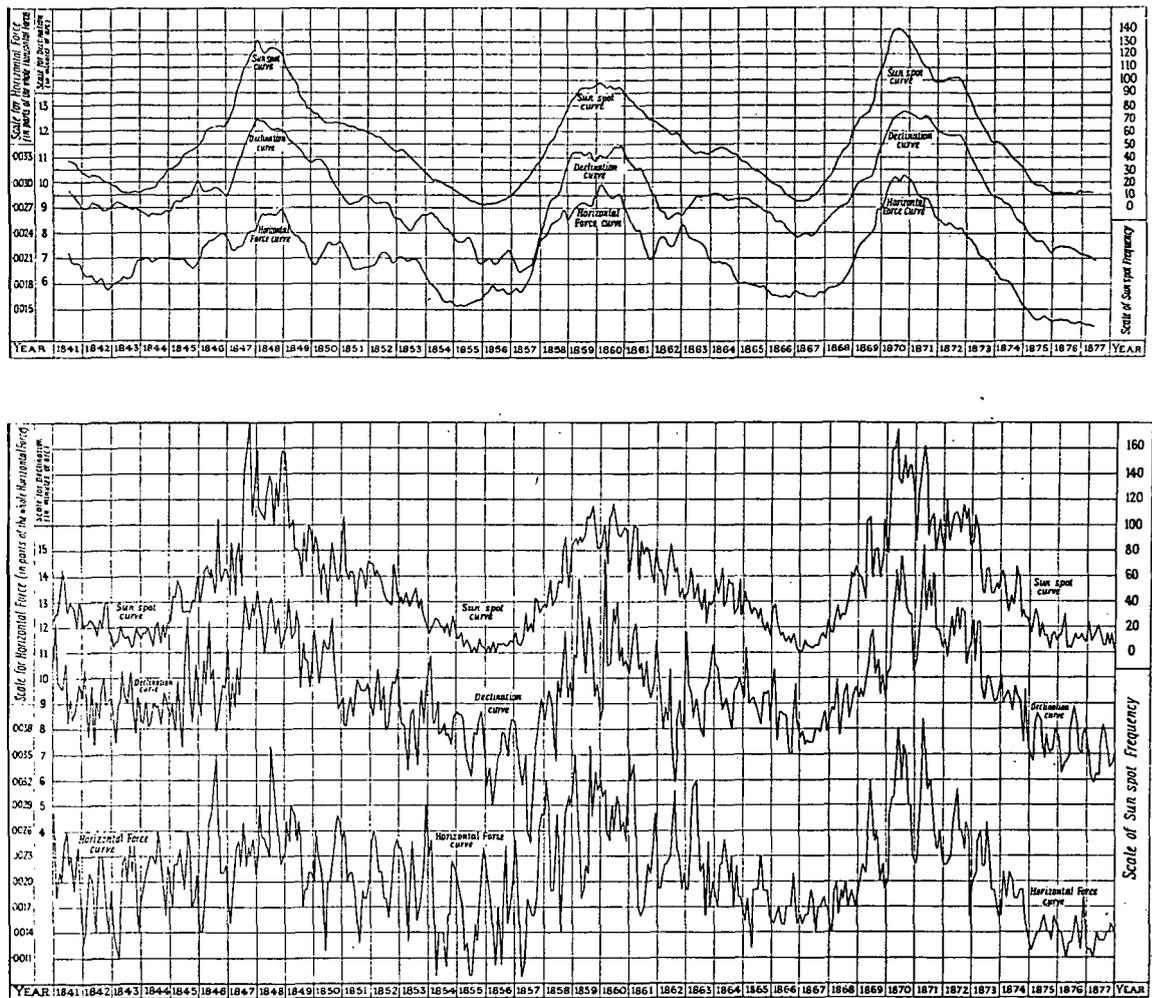
FIGURE 1.—Sunspot number and geomagnetic activity plotted for the time period between the years 1841 to 1877 (after Chapman and Bartels, 1940). The correlation between solar and geomagnetic activity is obvious.

time-dependent behavior of a single variable? There are several factors we must consider:

(1) Most geophysical phenomena have a high intrinsic noise level. Their first-order behavior is erratic. The phenomena we are looking for change slowly with time. For example, Schwabe was well into his second solar-cycle data set before he could demonstrate the systematic cycle. It took 20 yr to establish the 11-yr sunspot cycle.

(2) There usually is no acceptable theory to help organize the data into a manageable search. The theory usually follows the recognition of the phenomenon from observations. One must have great patience and perseverance. A good

example is Kepler's work that resulted in his laws of planetary motion. Kepler had the data that Tycho Brahe had gathered with painstaking observations over his lifetime. Kepler labored for more than six solid years. By trial and error he groped in the dark, with no possible glint of theory to illuminate his search until, finally, he chanced on the correct relationships. Patience, hard work, and extensive runs of reliable data are necessities.

(3) Finally, there are scoffers, like Kelvin, who delight in strangling new hypotheses in their infancy. The record shows, although Kelvin was often wrong in his prolific criticisms, he was
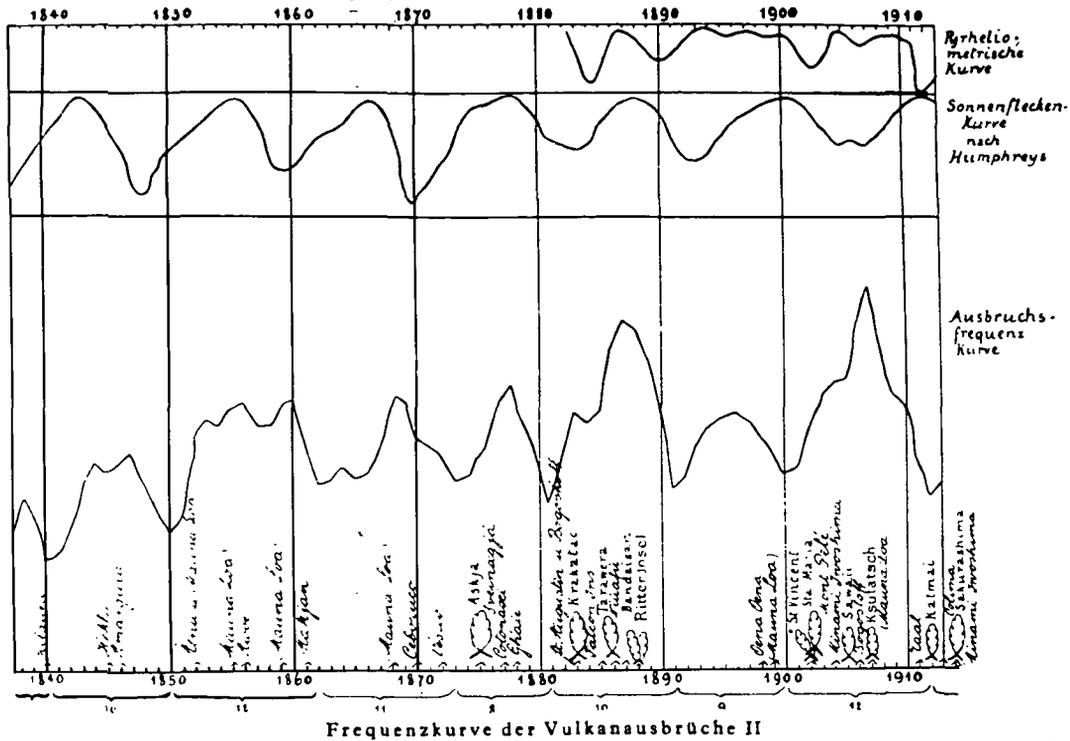
FIGURE 2.—Sunspot number and frequency of volcanic outbursts plotted for the time period between the years 1838 to 1914. The correlation between solar and volcanic activity, while not as obvious as the correlation of figure 1, is "not bad."

quite influential in slowing progress in several fields of research. Such people often rely on what is sometimes referred to as Bates' Principle, "Never believe an observational result until it is confirmed by theory" (Bates, 1974).

COUPLING BETWEEN SOLAR ACTIVITY AND THE WEATHER

I am not aware of any present viable theory that proposes a coupling between solar activity and some meteorological phenomenon. However, there is much good, relevant data at hand. Researchers in this field thus need only the patience of Kepler, a good sense of humor to handle the Kelvins among us, and a little luck to lead them to the right parameters.

Let us look at a few of the difficulties a theory must overcome before it can be regarded as a hopeful candidate for explaining a relationship between solar activity and some meteorological phenomenon.

Energy

The energy source for meteorological phenomena is (virtually) entirely provided by sunlight absorbed by the Earth's surface. This energy flux is $U_{EM} = \pi r_E^2 F(1-A)$, where r_E is the radius of Earth, F is the solar constant, and A is the Earth's albedo. If we assume the Earth has an albedo $A = 0.5$, we find that $U_{EM} = 8.9 \times 10^{16}$ W $= 8.9 \times 10^4$ TW, where TW signifies a terrawatt $= 10^{12}$ W. Essentially all of this energy is ultimately radiated back out into space. But much of it first becomes involved in the tropospheric weather system, where it establishes temperature differentials to drive convective systems and evaporates large quantities of water to provide for interesting instabilities within these convective systems.

To compare this energy flux with the solar-wind energy flux, we note that the solar wind, carrying an embedded magnetic field, strikes the geomagnetic field with a total energy flux of U_s,

where

$$U_s = \pi r_M{}^2 \left(\frac{1}{2}\rho V_s{}^2 + \frac{B^2}{2\mu_0}\right) V_s$$

where r_M is the radius of the magnetosphere, ρ is the mass density of the solar wind, V_s is its velocity, and B is the strength of the interplanetary magnetic field. μ_0 is the magnetic permeability of free space. Calculations made using various space and ground based observations indicate that less than one percent of this energy, on the average, penetrates the geomagnetic field. Let us estimate U_c, the value of the corpuscular and magnetic energy flux that is pumped into the geomagnetic field. We will assume $U_c = 10^{-2}U_s$. For $r_M = 12\,r_E$, $\rho = 8 \times 10^{-21}$ kg/m³, $V_s = 400$ km/sec, and $B = 10$ nT (that is, 10 γ), we find that $U_c = 5 \times 10^{-2}$ TW, and the ratio $U_c/U_{EM} = 6 \times 10^{-7}$, where U_{EM} is the solar electromagnetic energy flux. Thus the available energy flux of the solar wind and interplanetary magnetic field is less than one millionth of the solar electromagnetic energy flux absorbed by the Earth.

One can improve this ratio quite a bit by choosing conditions when U_{EM} is small (for example, wintertime or nighttime) and when U_c is magnified by short but intense bursts of geomagnetic activity that draws on stored energy within the geomagnetic tail. Snow and cloud cover may cause the average albedo on the illuminated portion winter hemisphere to reach 0.9, and the winter polar cap is not illuminated at all. For the winter hemisphere, $U_{EM(min)}$ might drop to 6×10^3 TW. If we wish to raise the corpuscular energy flux to a maximum, we should consider the period during an intense magnetic storm, when energy that had been stored in the geomagnetic tail by the solar wind is dissipated, so that, in the order of 10^4 seconds, approximately 10^{18} J of energy is fed into the magnetosphere in the form of aurora, ionospheric currents, ring currents, and particle energization. Thus, during a magnetic storm, U_c could increase to $U_{c(max)} = 10^2$ TW. This leaves us with

$$\frac{U_{c(max)}}{U_{EM(min)}} = 1.7 \times 10^{-2}$$

which might be just barely large enough to do some good.

These calculations indicate that, unless there is

some energetic component in the solar wind of which we have no knowledge, we should look for ways to use the energy of the solar wind and interplanetary magnetic field as a trigger that subtly switches the lower atmosphere from one quasi-stable mode of operation into another. This approach is, in principle, feasible, since weather systems, once started, run largely on internal energy derived from heat of condensation and crystallization.

In a paper presented elsewhere in this meeting, Hines (1973a) has proposed a theoretical model that may well be the breakthrough we have been looking for. It is energetically feasible. (But, as we shall see later, the coupling is weak.) The idea is that magnetospheric convective motions, which are intensified during magnetic storms, change the vorticity of the lower atmosphere at or near auroral latitudes by viscous coupling. This theoretical suggestion is directed toward explaining the observations of such vorticity changes as reported by Roberts and Olson (1973a).

The change in vorticity is characterized by an increase in the angular velocity of the air at and above the 300-mb level following certain geomagnetic storms. The rate at which energy must be supplied to accomplish this change can be estimated as follows: Assume a disk of air above the 300-mb level with a radius $R = 500$ km whose angular velocity, ω, increases from 4×10^{-5} rad/sec to 6×10^{-5} rad/sec. (These parameters are typical of the observed vorticity changes (W. O. Roberts, private communication).) The moment of inertia, I, of the disk is $\pi R^4 \rho/2$ where ρ is the column density of air above the 300-mb level, $\varrho = 3 \times 10^3$ kg/m². Substituting these values we obtain $I = 2.9 \times 10^{26}$ kg m². The energy of the rotating system is $E = \frac{1}{2} I\omega^2 = 5.3 \times 10^{17}$ J for $\omega = 6 \times 10^{-5}$ rad/sec. This energy is comparable to the energy of a magnetic storm. The power input U_r required to increase ω from 4×10^{-5} rad/sec to 6×10^{-5} rad/sec in 24 hr is

$$U_r = \frac{dE}{dt} = I\omega \frac{d\omega}{dt} = 2.7 \text{ TW}$$

The increase in energy of rotation is 2.3×10^{17} J. This power value is to be compared with $U_{c(max)} = 10^2$ TW, derived earlier, dissipated within the

magnetosphere during a magnetic storm. Thus there appears to be enough power within the magnetosphere to cause such changes in vorticity if the power can be directed and coupled effectively. We will now discuss problems with this and other processes.

Shielding

The troposphere is well shielded by the Earth's magnetic field from particle bombardment by the magnetosphere (except in auroral and polar regions) and by the overlying atmosphere (even in auroral and polar regions). For example, at an altitude of 16 km (the top of the tropopause at low latitudes), the shielding is 100 g/cm². Electrons or protons would require energies greater than about 10^8 eV to penetrate this barrier. The flux of particles either in the solar wind or within the magnetosphere having such energies is negligible. Direct measurements of X-ray fluxes beneath auroral displays show that the flux of auroral X-ray that penetrate to 16 km altitude is seldom detectable above cosmic-ray background. Again, the atmospheric shielding, roughly equivalent to a lead shield 9 cm thick, effectively screens out any penetration. The shielding problem is actually more critical than discussed above because in auroral and polar latitudes, where we might expect more effective particle penetration, the top of the troposphere drops to an altitude of about 10 km. Here the atmospheric shielding is nearly 300 g/cm². Thus, if we wish to suggest direct particle interaction, or even the less efficient X-ray conversion interaction, we must propose that it is the stratosphere, extending up to about 50 km (or perhaps it is even higher levels such as the ionosphere), that provides the link to meteorological phenomena.

It has been well established that auroral and geomagnetic activity cause marked increases in the temperature of the atmosphere above about 120 km altitude (for example, Jacchia et al., 1967; Newton et al., 1965). A significant portion of the heating is accomplished by direct particle bombardment in the auroral zone. An intense auroral beam has an energy flux of only about 1 W/m² or less than 1/1000 that of sunlight. The heat capacity of the upper atmosphere is so small that the effect of absorbing this energy flux

is profound. However, the upper atmosphere is thermally isolated from the lower atmosphere by two temperature minima, one at an altitude of 80 km and the other at about 15 km. Some energy, such as infrared radiation and infrasonic noise, is converted to forms that can penetrate through these temperature minima to the troposphere. But with a power input of only 1/1000 that of sunlight, it is hard to imagine that the small fraction of this energy that would go into either component would provide a significant perturbation to the tropospheric system.

Finally, to return to the mechanism suggested by Hines in which ionospheric winds might set the lower atmosphere in motion, we find the coupling is too weak. There are two ways to calculate the drag that the upper atmosphere exerts on the lower. They give similar results, so only the simplest one will be shown.

The convective motions in the magnetosphere encounter a drag motion in the ionosphere that produces ionospheric currents. These currents, which may reach an integrated value of $J = 10^6$ A as an upper limit, exert a force $\mathbf{J} \times \mathbf{B}$ per meter of length on the neutral atmosphere. For the polar value of $B = 6 \times 10^{-5}$ T,

$$\mathbf{J} \times \mathbf{B} = 60 \text{ N/m}$$

If this force is integrated over the diameter of the disk of air that was discussed earlier and applied in the most favorable way to this disk, an angular acceleration of

$$\frac{d\omega}{dt} = \frac{2JBR^2}{I} = 10^{-13} \text{ rad/sec}^2$$

is the result. This acceleration is to be compared with the acceleration of 2×10^{-10} rad/sec² that is necessary to make the process fit the phenomena reported by Roberts and Olson (1973a). While there is enough available energy, there is not enough coupling force to utilize this energy by a factor of about 10^3. C. Hines (private communication) has calculated the magnitude of this drag force by a different method and arrived at an answer in reasonable agreement with the one presented here. The more optimistic tone in his abstract reflects a more hopeful view of the serious nature of this discrepancy and slightly different assumptions.

Climate Theory

The two points discussed above have implications that are relevant to theories of climate. We wish to develop a theory in which some particle effect in the stratosphere (or perhaps even in a higher region?) somehow couples to the troposphere to cause a significant change. It is here that we appear stuck for the time being. Present theories of climate are quite primitive. For example, there is no accepted theory for the ice age, which, geologically speaking, occurred only yesterday. Nor is there an accepted theory for the quasi-stable states of the troposphere, with the required trigger mechanism, that was alluded to earlier. This lack of theoretical groundwork would seem to me to present a formidable handicap to anyone who wished to propose a detailed solar activity/meteorological coupling mechanism. It would seem that, at a minimum, it would be necessary to be able to forecast weather one or two weeks in advance with reasonable reliability. Then changes triggered by solar activity would be detected by matching the "bad" forecasts against unusual solar activity. The next step would be to postulate something about the trigger mechanism and the nature of the bistable states of the troposphere and devise experimental tests of the hypotheses.

But I have gone too far. We do not know if there is a bistable atmosphere of the type described, or even if we need one. The point is, we know so little about these aspects of the meteorological system that we find it hard to ask good questions. Asking good questions is essential to the development of a reasonable theory. This last point can be illustrated by pointing to the aurora, a phenomenon which, in recent times, has had no shortage of theories because the phenomenon is reasonably well defined in an input-output sense. The task of the auroral theorist is to explain something of what is going on in a well-defined black box. Solar activity as related to meteorology has not reached this stage of definition yet.

Correlations With Geomagnetic Activity

Figure 1 shows that solar activity (as indicated by sunspot number) and geomagnetic activity are correlated. The search for a similar correlation between sunspot number and the weather has been carried on up to the present time. The principal problem encountered was that there is apparently no consistent 11-yr cycle in the weather. Reports of either no sunspot correlation or a 22-yr cycle have tended to confuse the issue. That is, rainfall, winds, and temperatures vary from year to year, sometimes showing persistent behavior (as in an ice age or a long drought), but these parameters do not consistently exhibit an 11-yr cyclic pattern. There is presently a claim that 3 rings show an 11-yr pattern. If this is true, the 11-yr, rather than a 22-yr, pattern would be established. Trees respond principally to springtime rain, temperature, and sunshine. (See Fritts (1971) and Fritts et al. (1971) for a review of the uses of tree rings in climate research.)

Recently Shapiro (1972) and Wilcox et al. (1973) have presented results showing a correlation between geomagnetic storms and winds and pressure troughs. These papers are reviewed by Roberts and Olson (1973b).

There is perhaps a clue to a possible mechanism arising from this work. If there is no 11-yr cycle in the meteorological phenomena they are testing, perhaps there is a special type of geomagnetic storm that should be sought that also does not have an 11-yr cycle. For example, recurrent geomagnetic storms do have a much smaller variation over the sunspot cycle than do the great storms. According to Newton and Milsom (1954), the frequency of recurrent storms varies by a factor of 2.5 over the solar cycle while the large storms vary by a factor of 7.3. If meteorological variables could be correlated against only recurrent geomagnetic storms, we could see if the basically different nature of these storms was important to meteorological phenomena.

The existence of an unvarying base frequency of a special type of geomagnetic activity might explain why Shapiro (1972) found an improved correlation when he eliminated the years of sunspot maximum from his data—if there is no 11-yr variation in his meteorological data, elimination of the geomagnetic data from sunspot maximum would tend to eliminate the 11-yr cycle in geomagnetic activity. This point has been taken up by Hines (1973b) who points out that

the remaining correlation may actually be caused by the meteorological phenomena sending energy to the ionosphere (Bauer, 1958) by means of gravity waves (Georges, 1973). These waves will cause currents to flow in the ionosphere, which can be detected as geomagnetic activity (Hines, 1965). Thus Hines suggests that cause and effect are reversed. (See also Shapiro, 1973.)

The approach of Wilcox et al. (1973) is different in that they have chosen the sector boundary structure of the interplanetary magnetic field to correlate with a vorticity index derived by Roberts and Olson (1973a) for pressure troughs in the northern hemisphere. The number of sector boundary crossings per year should show an 11-yr cycle. Does the vorticity index show a similar 11-yr variation? If not, it would be important to learn which sector boundaries at sunspot maximum were not effective in causing a change in vorticity index. The answer to this question might lead to an understanding of what is essential and what is not in order for the interplanetary medium to affect the troposphere.

CONCLUSION

As Roberts and Olson (1973b) have pointed out, "it has now become a matter of high scientific priority to develop and test working hypotheses for the empirically established (solar-activity/meteorological) relationships." But nothing viable seems to be forthcoming from the theorists. This lack of theoretical development may be caused by our lack of understanding of how the weather really works on time scales of a week to ten days. On the other hand, we may be in much the same predicament as the unfortunate Lord Kelvin who was completely unaware of the existence of dominant physical processes (such as the solar wind, which could transport energy from the Sun to the geomagnetic field). Perhaps the developments of the next few years in determining why there is no pronounced 11-yr cycle in meteorological phenomena while there is one in geomagnetic phenomena will provide the clue we need to establish some hypotheses that can be tested.

ACKNOWLEDGMENTS

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DISCUSSION

DELAND: Ray Deland, Polytechnic Institute of New York. I would like to defend the statistical approach a little bit, because this is my own approach. Certainly if you correlate A and B, you find A is correlated with B, as so many of these studies have shown. One does not know whether A is causing B in the sense of fluctuations in A propagating some energy that is transferred to B or vice versa. Neither do you know whether something else is causing both A and B.

One approach applies, I think, in this situation—based only on the hypothesis that if you have a transfer of energy from A to B there is usually some sort of signal velocity involved, and there is a time delay of the effect on B compared to A—is lag correlation studies.

That is, correlate A delayed by plus or minus a few days with B. My own experience with this, unfortunately, is that, when one does that, one finds the best correlation usually when you take zero lag which makes it very difficult.

Again, gradually building up some experience that most things go up rather than come down in terms of the correlations between weather changes and what is upstairs, you get the better correlations with a delay of what happens upstairs compared to what happens downstairs.

LONDON: In the magnetosphere observations, is there any way that you can recognize one cycle from another except for changes in polarity, supposing you were given a long trend and asked to identify them?

DESSLER: That is a good point, because in geomagnetic activity, auroral activity, and things like that, there is no trace of the 22-yr cycle that I am aware of. Solar wind interaction with the geomagnetic field is beginning to be understood, and there in no way do appearances depend on the spot wave.

So that is something we have not thought of yet, and this recalls again what happened to Lord Kelvin. In each case, the mistake he made was based on insisting that he knew everything. But there were things he did not know about, like the atom is not indestructible, and there were other things along that line that he didn't know about, and he was wrong on the age of the Earth. He didn't know about radioactivity, and he didn't know about solar winds and made a mistake on the correlation.

So there is something in the solar wind, the component of the solar wind we do not know about, that somehow depends on the polarity of the sunspots going wild like that. Then maybe it will do something to the weather, but it sure doesn't do anything markedly significant that is observable and detectable and noticeable, either in the aurora or geomagnetic storms. They have an 11-yr cycle, not a 22-yr cycle.

QUESTION: Can you describe in a few words what actually happens when the boundary sector passes the Earth, from the standpoint of physics?

DESSLER: I will give you the party line, and the evidence for it is reasonable enough but a lot of it is circumstantial: there is a connection between the interplanetary magnetic field and geomagnetic field, which draws a lot of magnetic field into the tail. And magnetic pressure builds up in the tail. The plasma sheet which has separated the two halves of oppositely directed field in the tail disappears, and all of a sudden you get a lot of magnetic field being annihilated.

Net energy from annihilating the magnetic field drives the remaining plasma sheet into the geomagnetic field where it causes the auroral ring current. The plasma moves in so far before it creates the ring current, and it energizes the particles by betatron acceleration, so then they can precipitate as the aurora. And so it is a pretty straightforward chain. A lot of details need to be explained.

HUNDHAUSEN: This question is really addressed to two members of the audience. I think it is appropriate at the moment. The persistent change in the sector pattern has been inferred for several solar cycles from ground-based measurements.

However, is it now true that this pattern develops in the same way in all cycles? In other words, there is not a change in interplanetary polarity pattern with the major and minor solar cycles, so if we emphasize the use of solar sectors in studying these effects we seem to be limiting ourselves, therefore, to the 11-yr and not the 22-yr cyclic phenomenon.

ROSNER: You are quite correct. There is no 22-yr variation in the sector.

PARKER: How is it known, insofar as the polarity is concerned, though?

ROSNER: Well, we can determine what the polarity is since on any given day by looking at geomagnetic polar disturbances, and so we know what the polarity is. There is no 22-yr cycle.

DESSLER: Again, I do not believe the sector structure's peak will occur coincident with the solar cycle's peak. I think there will be a 4-yr displacement, because they are the source of recurrent storms, and recurrent storms peak 4 yr later.

NORDBERG: Let me try another elementary freshman-class magnetosphere question. What is the cycle of the sector boundary sweep across here? I assume there are about 4 sectors, and so it is 4 divided into 27?

DESSLER: Either 2 or 4, yes, and it would be 2 into 27 or 4 into 27. Now, at times it gets more complex when the solar structure gets complex. During some intermediate stages, as new sectors are being created, you may not have such a simple division, but generally, that is right, either 2 or 4 divided into 27.

NORDBERG: In that case, since you raised the question of what to look for in 22 yr, 11 yr, 3 days, or what not, I have a wild idea here. If it turns out 4 into 27, then it just falls right that you have about 6- or 7-day passages of the sector boundaries. That is very closely coincident with the life-cycle of planetary waves, or the generation cycle of planetary waves. How about some kind of a resonance mechanism here?

Whenever a sector boundary happens to sweep when condition are ripe for cyclogenesis, that one old wave has just died and you generate a new one, that could match that vorticity correlation with the sector boundary sweep. And it is understandable that sometimes and in some places it works exceedingly well, and in other places it does not work where you have a mismatch.

DESSLER: So I guess you could take the time when there are only two sector boundaries and see whether every other vortex that was generated was weaker or later or somehow showed the effects and noneffects of the vortex.

QUESTION: Considerations of both energy and momentum you have shown as weakly coupled to the atmosphere, and one has to consider them as triggering mechanisms.

DESSLER: Well, there are other things that could serve as triggering mechanisms, for example, like volcanic eruption.

QUESTION: I was wondering, could you give for comparison the energy involved in volcanic eruption— what is the correlation between, say, volcanic eruptions and weather phenomena?

DESSLER: I am afraid I do not know offhand. The volcanoes are very, very energetic, and at the time I knew it I was impressed at how powerful they were. But I showed you a slide that showed what I thought was not a bad correlation between frequency of volcanic outbreak and sunspot number. Did you not like that result?

HEPPNER: I think you may have confused our nonmagnetospheric physicists here when you related sector structure to rate of reconnection. Sector structure is the east-west component, reconnection is usually attributed to the north-south component. I do not know of any theories that relate sector structure to rate of reconnection. I think you called that the party line.

DESSLER: Yes, that's why I said that, because geomagnetic activity rises at the sector boundary crossing. And, as you said, it is a north-south component that explains the rate of reconnection and geomagnetic activity. So I was going through a real weak point there, which is true. As you know, I am not very sympathetic with the party line, but I feel obliged to follow it at the present time.

ROBERTS: This is on your comments about, for example, *trying to distinguish between an 11-yr cycle and a 22-yr cycle in the vorticity index, particularly if* it is integrated up over the northern hemisphere, as we did in sector boundary studies. This probably isn't going to be a terribly fruitful way to go.

First of all, it is going to take a long time to get enough data on the vorticity index to be able to do something that will satisfy Lord Kelvin. And moreover, we have a tremendous wealth of variation of much shorter term between various types of magnetic disturbance and sector boundaries and vorticity in particular areas, and so on. But it does seem to me that the emphasis on the difference between the 11- and 22-yr cycles might be a fruitful thing to look at in terms of some kind—as Bill Nordberg suggested—of resonance in the terrestrial system. Because it is perfectly possible, for example, that due to time constants and ocean temperature changes or something like that, a 22-yr cycle could be driven by an 11-yr forcing function.

QUESTION: You brought in one pseudocorrelation with no explanation, that is, solar relation to volcanic activity. But you ignored one suggestion which has been made a number of times, namely, that the cosmic-ray change, which is really due to solar activity, could in turn change the magnetic field, and this could relate to weather.

Remember that the ionization change due to the cosmic-ray change is something like an order of magnitude. As you go up in the atmosphere it's around the tropopause, or around 20 km. So this is a good relationship and I would like to hear your comment.

DESSLER: I was very brief in discussing the cosmic-ray variations in the soft component for cosmic-ray energies of a few billion electron volts. And it comes into the polar cap where its ionization peak is at about 22 km altitude. The tropopause in the polar cap is at 10 km, and at this altitude there is just no change. There is almost nothing reaching there now.

If you have an effect where you can use production of ions or maybe some gas chemistry 10 km above the tropopause, then that would be great. But, unfortunately if the cosmic rays come in at the equator where the tropopause is higher, amplified through maximum, then you would be in business. But I see the shielding layer above the polar cap tropopause, and I do not see any good way to get around this fact.

PARKER: At middle latitudes we are talking about 10 percent variations in the cosmic-ray intensity. The other thing you might suggest, along this same line, is that there are occasionally enormous proton flares, which every few years at least produce rather enormous amounts of energy, of ionization, sometimes down to at least middle latitudes if not low latitudes. But, again,

there is the same question as to elevations at which you produce the ionization.

DESSLER: Now, those unusual events will just do everything, but they are once every 5 yr. They are a funny kind of flare that, in my opinion, show no relationship to the solar cycle. They just appear once every 3, 4, 5, 6 yr. There is some evidence that they avoid solar maximum and minimum, but it is not that clear, there have been so few of them. You can't have a weather effect of the kind that has been talked about in the meeting that relies on a rare event like that.

WOODBRIDGE: You mentioned that in the sector structure that we have four or two sectors, except at times when we have changes. Has anybody looked at what is occurring at these times? If geomagnetic storms are associated with the sector boundaries, then *when* these changes are occurring—it seems like everyone has passed over this point—may be the most important times.

Are they associated with the 11-yr cycle? How often do they occur? How violent are they? Or are they associated with the 20-yr cycle?

DESSLER: I think that clearly these changes are associated with an 11-yr cycle.

WILCOX: In the first approximation, one has two or four sectors all the time coming around very clearly. Now, having said that, we can say that during the time observed by spacecraft in part of 1965, this pattern was not quite as clear. It was somewhat more broken up. But I think, in terms of trying to understand the weather, we shouldn't worry about those few months but should consider the 10 yr in which just very regularly the boundaries sweep past the earth.

VOICE: Why?

WILCOX: Based on the work of Leif Svalgaard, it seems that around sunspot maximum there may be a tendency to have two boundaries per rotation for a few years. And the rest of the time, particularly, say, going into minimum, it is four. As to why, we do not know.

HUNDHAUSEN: In fact, as you all know from my talk yesterday, I am no foe of simplification to try to understand some basic physical phenomena. But I think we have to be very careful here and not talk about interplanetary space as though such a structure were the only thing present. Now in fact, during this period in early 1965 when the sector structure seemed to appear, and at least for one month, there were no sector crossings, there were still geomagnetic disturbances. And in that case, as I showed at the Chapman Symposium in June, there were high-speed solar windstreams, and the geometric changes were pretty well correlated with the stream structure that remained even when there were no sector boundaries.

During the period of the solar cycle, when there may be two sectors, there are often two streams per sector, and in most cases there still were back in the Mariner 2 data geomagnetic peaks when the different streams came by, even within a sector. So the sector structure has proven very useful in many ways, both in relating inter-planetary phenomena to the Sun, and in doing superimposed epoch analyses with the terrestrial phenomena. But let's not regard all of interplanetary space as organized purely by the sector structure. There are other obvious influences on geomagnetic activity, and one should not ignore the fact that there may well be other important physical driving mechanisms for the rest of the atmosphere.

DESSLER: That is why I wanted to see what happens with the nonsector boundary, to remove the sector boundary storms, because most of the storms are not sector boundary storms. I want to repeat the total of the storms from max to min, varied by a factor of 7.5 in number of currents per month. Whereas the sector boundary storms, which would be presumably the recurrent storms, vary by a factor of about 2.5 from sunspot maximum to minimum. So, most of the storms are not sector boundary storms.

DELAND: The sector boundaries, in fact, seem to be fairly periodic. Bill Nordberg suggested that I say something about a 7-day periodicity in planetary waves. However, if you look at them carefully, you find there is a whole spectrum of frequencies, just as there is a whole spectrum of wavelengths. I want to really emphasize that anything involving the planetary waves is very far from periodic. This is partly because people have jumped to that conclusion at times. And in looking for resonances, we had better be very careful.

LONDON: Since we are talking about mechanisms, it might be important here to mention an idea that has been advanced by Ruderman and Chamberlain on a solar-weather relationship and the mechanism by which this could be caused. This has to do with cosmic rays being modulated in a solar cycle period, coming down to a meteorologically important level. That is, down to about 20 to 30 km, and there exciting nitrogen and thus lead to the local formation of nitric oxide. We know that nitric oxide can be deleterious to the ozone concentration at these altitudes.

At 20 to 30 km, the ozone concentration has its maximum. It also has its maximum in high polar latitudes. If cosmic rays, therefore, in an indirect but understandable way, can affect the ozone concentration at, let's say, 25 km, this effect can affect the radiation budget at that level. The difficulty is to find out whether there is sufficient energy in the cosmic rays to produce enough NO, which will produce enough destruction of ozone. Here is something that can be very easily tested by numerical models.

However, a countermechanism has been suggested, also invoking cosmic rays. And that is, if there is ionization of O_2 at these levels, then there can be dissociative recombination. And in that case one can produce atomic oxygen. As everybody knows, it's atomic oxygen that then forms ozone.

So we have two counterprocesses. One can put both of these into a numerical scheme, knowing what the relaxation times or kinetic rates are for these reactions, and get some kind of approximate solution.

DESSLER: This procedure would take a long time to carry out. It would not be a geomagnetic storm effect.

DELAND: Goodwin and Chamberlain used this mechanism for a so-called, or presumed, solar cycle variation in ozone. We are not sure that there is one, but if there were to be one, then they have this mechanism to account for the 11-yr period.

MARKSON: I would like to discuss Kellogg's and London's suggestion about the importance of cosmic rays, because I agree that you have to look for something that gets down to meteorological altitude. And the ion⁻ production maximum is at 16 km. I think some numbers that would answer an earlier question about looking into this are that at 10 km the variation from solar minimum to solar maximum, between 1954 and 1958, was 30 percent. At 15 km it was 50 percent. Now, what I would like to have meteorologists consider is whether, assuming thunderstorms are modulated in the way I suggested yesterday, the energy released by thunderstorms contributes to synoptic scale meteorological variation.

JOHNSON: Concerning London's suggestions about Chamberlain's work on the chemistry being involved, I would just like to comment that the ionization produced by the bremsstrahlung from energetic electrons also comes down to altitudes of, say, the order of 30 km. That is a significant fraction of the cosmic-ray ionization rate. Therefore, one could tie this in to the magnetic storm effect.