SOLAR EVENTS AND EFFECTS ON TERRESTRIAL METEOROLOGY

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I feel a little uneasy about speaking on the subject I have been asked to—"Solar Events and Effects on Terrestrial Meteorology." This subject involves two branches of science that are somewhat sophisticated by themselves, but, when brought together, they unfortunately have somewhat of an unsavory reputation. Much of this has come, I think, from a great desire to improve weather forecasting by any means available. Admittedly, much of the effort to improve forecasting by this particular means has been of questionable merit. I suspect that one reason sunspots were used is that their remoteness makes them a little difficult to check up on. There are those who are determined to blame everything that varies cyclicly on sunspots. For example, you can find in the literature claims for strong correlations between sunspots and such events on the earth as birth of babies, growth of wheat, and stock market fluctuations. There is no doubt but what these claims have contributed quite a bit to the somewhat poor reputation of the field of solar weather relationships. If I felt that the subject merited this reputation, however, I would not be speaking here. We are beginning to make some progress and I shall spend my time this evening outlining the basis of this progress.

While sitting in the audience the last few days, I have learned a lot and enjoyed the meeting very much, especially the informal atmosphere, and I hope it will be preserved tonight. I have also cultivated a sympathy for those sitting in the audience who do not know what the speaker is talking about. I would like to spend most of my time trying to explain to you what I am talking about. Toward the end we can get around to the relationships between the sun and weather.

In starting I would like to say that in looking for a relationship between the variable sun and the weather, we do not have in mind explaining the general atmospheric circulation. I do not think anyone seriously maintains this idea. What we do have in mind are some particular anomalies of the circulation that appear to be related to events on the sun.

In making the step from the sun or events on the sun to anomalies in atmospheric circulation, we are taking a step that most meteorologists would prefer not to take at this time. I would not advocate that we make the additional step of attempting to relate the anomalous features of the sun to oceanographic anomalies, which in effect is making two steps beyond our knowledge and which would not be justified.

I came here with the impression that it is very fortuitous that the period of time we have covered in our studies coincides with the period that you are interested in. After listening to the discussion, I almost wish that it had occurred at some other time, and I am a little afraid it might detract from the central issues here. I sympathize very much with Charney in the attitudes that one has first to consider the relationships between atmosphere and the oceans. I hope that anything I say will not detract from this main idea.

The whole field of solar weather relationships is very complex at both ends of the problem. The atmosphere is a very complex medium. The circulation of the atmosphere depends on many factors, all of which must be taken into account before we can fully explain the circulation. The Sun, at the other end of the problem, is at least as complex, if not more so. As in the study of oceanography and meteorology, we cannot really control the experiments. We take what we observe and interpret. We have the apparent handicap of being further away, but, after listening to your discussions, I am not sure it is a real handicap.

I would like to summarize some of the evidence for variations on the sun. I will talk specifically on only those we are interested in tonight. There are a great variety of features observed on the sun that are variable in time and that may result in important perturbation in the terrestrial atmosphere. There are two in particular which I would like to talk about, both basically related to sunspots, with which I am sure you are familiar.

Sunspots as you know, come and go in an eleven year cycle, and are currently at a maximum. Actually, we are not certain whether they are past or approaching the maximum. The evidence seems to be that they are very near the maximum at this time. The last year, 1957, and the beginning of this year, represent the greatest sunspot activity that has been observed in the last 2½ centuries. The last two years, therefore, have been of extreme importance to us, as well as to you. Other solar activity associated with sunspots is also apparently at an all time high. The rise to maximum activity has not been monotonic. It has fluctuated a good deal, which is also typical of other sunspot cycles.

The visible part of the sun we refer to as the photosphere. We may think of the photosphere as a surface, but it is actually the visible layers of the radiating solar gases. Above the photosphere there is a chromosphere and corona, which we can observe at the time of a total solar eclipse. Or, we can observe them with a coronagraph, which is a special telescope for making an artificial eclipse of the sun. We use coronagraphs at high altitude mountain observatories, where the air is exceptionally clear. The chromosphere lying just above the photosphere, and the corona overlying the chromosphere are anomalous features of the sun, as I will now illustrate.

The surface temperature of the sun is of the order of 6,000°C. The corona, however, has a temperature of above 1,000,000°C. We have not yet accounted for this spectacular increase in temperature. It is in itself a departure from the normal idea of a sun that can be represented by a blackbody at 6,000°C. As a consequence of the high temperature in the corona, there are strong solar radiations beyond the visual part of the spectrum, some of which have been observed and some of which have not been observed.

The corona varies markedly during the sunspot cycle. Generally over sunspot groups we see regions of the corona that are very active and have abnormally high density and high temperatures. These regions carry the implication of a strong excess of ultraviolet and X-ray radiation. How strong and how much in excess, we cannot really say. I will mention in a minute the sort of estimates we can make. First, however, I should remark that the corona itself does not vary rapidly in short periods of time. If we pick out a particular active region, the life time is of the order of weeks to perhaps months, and we do not expect really rapid variations.

However, there are features apparently associated with the corona that exhibit rapid variations. The most important of these is the solar flare. Flares usually lie near the upper chromosphere or lower corona and are sometimes called chromospheric flares. The flare has a life time on the order of half an hour. They usually take something less than 10 minutes to reach maximum brightness and then fade out, in something like a half-hour's time. We customarily observe these flares by their increased brightness in the cores of the strong Fraunhofer absorption lines in the visible spectrum. Normally, we cannot observe them in the white, undispersed light of the solar spectrum. Three or four have been observed in this way, but they are the exception rather than the rule. Therefore, flares have no appreciable effect on the visible radiation from the sun. In the cores of the strong Fraunhofer lines and in the ultraviolet and X-ray spectrum however, flares have a pronounced effect.

Let us consider the specific changes that are known to occur in the solar spectrum. The solar constant has been measured over a period of several years by workers at the Smithsonian Institution. These data seem to indicate that there is no significant variation in the radiation from the sun in the visible part of the spectrum. There have been reports in the literature of significant periodic variations in the solar constant. In fact, several periods have been ascribed to these data by the questionable technique of somewhat arbitrarily selecting a period and amplitude, subtracting this periodic component from the raw data, then picking other periods and amplitudes from the remaining data until a reasonable representation is obtained. One can always find periodic components by doing this, but, in a power spectrum analysis of the same data, no significant periodic components are evident. Summarizing the data for the visible part of the spectrum, one can say that there are no variations larger than .1 of 1 percent. This is about the limit of the observational accuracy, and is an upper limit to any possible variations.

On the other hand, if we go the extreme limits of the spectrum, we observe very violent variations in the spectrum. In the radio end, which I will mention just briefly, there are variations of many orders of magnitude in the intensity of the radiation. So far as we know, these variations have absolutely no effect on the terrestrial atmosphere. They carry practically no energy and do not seem to be of any particular interest to us as far as terrestrial effects are concerned. Going in the other direction, however, we observe large variations in the ultraviolet and X-ray part of the spectra, and we suspect that there are still larger variations which have so far escaped detection. We have observations beyond the visible range down to about 1,000 angstroms. The spectral region beyond 1,000 angstroms is unobserved down to 100 angstroms, but below 100 we again have observations. This is because of the absorption spectrum of the earth's atmosphere, which will not let the radiation between 900 and 100 angstroms penetrate far enough for us to get rockets and other observing equipment into the proper altitude, so we simply have not observed it. In the parts we have observed, we detect variations that are at least of the order of 100 percent. However, we have every reason to suspect that we have come nowhere near to observing the extreme variations.

You can imagine the difficulty we have trying to observe the radiation from a flare. The radiation reaches maximum intensity in about 10 minutes time. Flares are totally unpredictable insofar as our present stages of sophistication are concerned. In order to observe ultraviolet and X-ray radiation from a flare, we must constantly watch the sun; and when a flare begins, we must somehow get instruments into the upper atmosphere to a height of at least 70 kilometers to observe the effect we are looking for. This must be accomplished in about ten minutes time at the most. There have been deliberate attempts to do this by the Naval Research Group, in the summer of 1956, I believe it was. A ship was stationed off the west coast of Central America with several pre-equipped rockets ready to fire. The rockets were carried aloft in balloons to get them to altitude faster and make them a little more efficient. The rocket crews on shipboard were in radio contact with our observatory, and they had flare detectors of their own. Because of mechanical failures on shipboard the flare detectors did not work. When flares occur the excess radiation often disturbs the ionosphere, disrupting radio communications. Thus, to some extent, this entire operation was plagued with difficulties to start with. One flare occurred that did not disturb the ionosphere too much, and a radio message got through. The rocket was fired and reached altitude during the waning stages of the flare. The rocket instruments showed a rather large increase in X-ray radiation, but in the wave lengths around the resonance line of hydrogen where we expected to observe variations, no variation was evident. However, the visible flare was practically gone and we cannot really state conclusively that the resonance line of hydrogen did not change in intensity. In fact, we know that it has to change since the lines of hydrogen in the visible spectrum change rather remarkably, and with any reasonable model the resonance lines have to change by a much larger factor. Another attempt to observe the ultraviolet and X-ray radiation from flares was made from San Nicolas Island, with similar failures. One rocket reached altitude during late stages of a flare and again indicated a strong increase in X-ray radiation. However, the ultraviolet equipment did not operate properly. Our real hope for observing the ultraviolet and X-ray radiation from flares lies in artificial satellites. This is why there has been a vigorous effort to have satellites in orbit during IGY. We must have an observing station that is up there a long time so we can catch flares during the initial stages and observe them throughout their life history.

Returning now to the corona, I should point out that coronal radiations are more or less steady on a time scale of hours and days. However, they do vary over periods of weeks to months. Just to give you a picture of what is happening on the sun, I have prepared a chart (Fig. 103) showing the variation in monthly means of coronal radiation and flare activity. The time in years is plotted along the bottom. For comparison with the sunspot cycle I have also plotted the sunspot numbers. The sunspot curve rises rather irregularly from 1954 on and varies considerably from month to month and even more so from week to week. In 1957 you see a very high intensity of coronal radiations, which we have integrated to provide a global value. The observed coronal radiation is in the visible part of the spectrum. We have reason to think that the ultraviolet radiation will be somewhat proportionate. We are not certain in exactly what way, but certainly the two vary in the same direction. You can also see an overall increase in the coronal radiation along with the sunspot cycle. In fact, there is a quite good correlation between the two curves. On the bottom of the chart, the dashed line, indicates the flare activity. This is an index that involves the number of flares. In some ways this curve is misleading. I could multiply the index by a factor and get something like the curve for the coronal radiation. I plotted the curve this way in order to get the very large peak during 1957 on the chart, but it has made the rest of the graph very misleading. In general flare activity increases along with sunspot activity in rather direct proportion to it. Late in 1957, September and October, there was a very large rise of the order of a factor of five to six in the flare activity index. As far as I know, this was the greatest flare activity that has ever been observed on the sun.

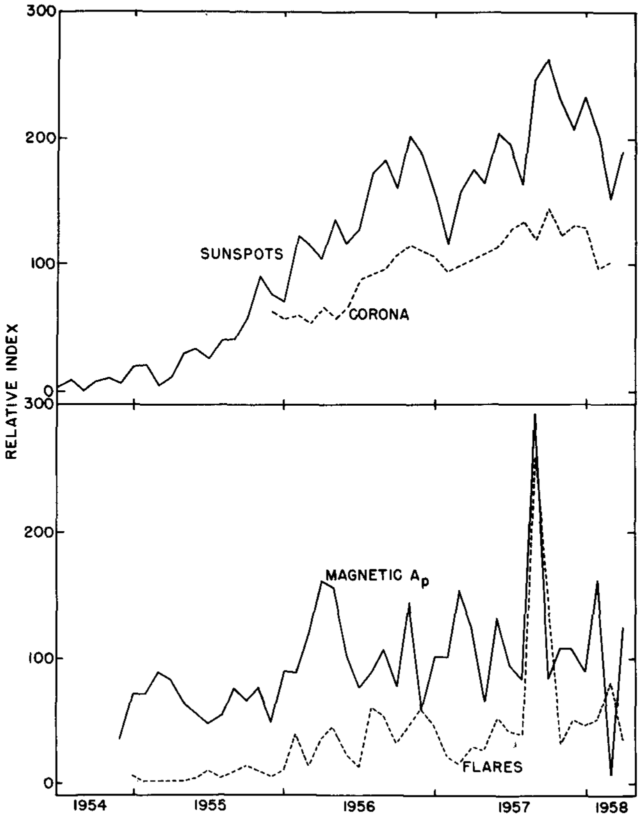

FIGURE 103. Monthly means of sunspot number, coronal radiation, geomagnetic A_P index and flare activity.

Essentially this summarizes what we know about the ultraviolet ray and X-ray radiation from the sun. I could say more about it, but it is not the particular feature I want to talk about tonight. There have been attempts to correlate some of these features to weather on the earth, for example, the work by Clarence Palmer. I think it rather difficult to evaluate what success he has had, so I will say no more about it.

There is another feature of solar radiation which we cannot observe but which seems to be extremely important, and it shows up in a variety of phenomena on the earth. This is what we call corpuscular radiation, actual radiation of matter from the sun. We have never observed a single case of matter actually leaving the sun and impinging on the earth. We have observed solar events that are suggestive of matter leaving the sun, and a variety of terrestrial events that imply that matter has left the sun and bombarded the earth, but it is a problem in which we must rely on indirect evidence, circumstantial evidence in a sense, because of the very nature of the solar atmosphere.

Let me first tell you about the evidence from the events in the sun's atmosphere. If we look at the sun in the light emitted by hydrogen, we observe huge clouds of material often shooting toward outer space from the sun. In many cases the matter is not decelerating as it would if it were moving only under the force of the gravitational field of the sun, and as far out as we can observe it we have every reason to assume that it will continue to escape the sun and fly off into space. We see a variety of such features in association with flares. Surge-type prominences, which are characteristically associated with flares, usually decelerate and fall back into the sun. Many, however, shoot up at constant velocity and gradually fade from view. The implication is that they shoot out entirely away from the sun. There are other times when comparatively small globules of material (and when I say small, I mean small in the sense that they are 90 million miles away and they look small) shoot out at high velocity from the sun with no deceleration. There are still other prominence types, which you have undoubtedly seen pictures of, that move out from the sun with rather high velocity.

Of particular interest is the phenomenon associated with the chromosphere that we call spicules. The whole surface of the sun is literally covered with spicules, which number of the order of 10,000 at any one time. As near as we can tell, they move upward with constant velocity and one can stretch his imagination a little bit and say that they continue to move on at this rate right out into space. They are only visible through a path extending about 10,000 kilometers into the outer atmosphere. We are not certain why they fade from view, but the general indication is that they simply become too hot to radiate in the visible part of the spectrum where we can see them. As far as we can tell, they do not decelerate so we can therefore infer that they may escape from the sun.

With radio telescopes we have observed events on the sun which indicate that material is moving outward at high velocity by the characteristics of their spectrum. These disturbances generate random noise at radio wave lengths and the frequency of maximum noise decreases with time. This implies that the disturbance is moving outward through the atmosphere. If we interpret this in terms of our conventional pictures of the solar atmosphere, we find that some types of disturbances move outward at a velocity of the order of 1,000 kilometers per second. In others, the velocity is very nearly the velocity of light.

This pretty well summarizes what we know from the sun itself about matter moving outward, with one exception. When we look at the spectral lines emitted by flares, they have a depression on the violet side. We can account for this if we assume that there is absorbing material between us and the flare that is moving toward the earth. The velocities of this material are consistent with the other velocities we have mentioned.

When we come to consider the evidence of solar corpuscles impinging on the earth, the evidence gets somewhat better. The very first flare observed was followed by a strong storm in the earth's magnetic field. It was also followed by an aurora or the so-called Northern Lights. The second flare observed, which was some thirteen years later in 1872, was also observed to have a magnetic storm following it with an associated auroral display. In a sense these were very unusual flares. They had to be in order to be observed with the crude observing techniques that were being used. However, since that time there has become a rather well-known relationship between sunspot activity and geomagnetic disturbances and auroral displays. That solid curve in the lower part of figure 103 indicates the variations of the magnetic Planetary A index through the sunspot cycle. The Planetary A index is essentially a linear measure of the variations in the earth's magnetic field weighted over several observatories throughout the world. When we look at the general sunspot cycle, there is a very pronounced correlation between variation in the earth's magnetic field and sunspot activity. Eleven year cycles go hand in hand and over long periods of time, so there can be no doubt that the two are very closely related. You certainly cannot claim a detailed correlation however, as there is really no clear-cut well-established association on a short time scale. Some flares seem to cause violent magnetic storms. Other flares of equally great size seem to have absolutely no effect on the earth's magnetism. Similarly some sunspots seem to produce magnetic disturbances, while other sunspots do not. This does not say that there is no relationship; it might simply imply a complicated relationship.

I hope you realize that when we talk about features of the sun, we are talking about symptoms of a disturbance on the sun and not necessarily the main disturbance. It is not even obvious that we have observed the main disturbance. All the phenomena we observe are correlated with each other. They all occur on the same areas on the sun and are all pretty much symptoms of a common disturbance. Just because we find a correlation of one particular solar event with a terrestrial event does not mean that that particular solar feature is causing the terrestrial event that we are observing. This seems to be the case with flares and solar corpuscular emission. We have to consider flares as an indicator of some more basic disturbance, which, in turn, is perhaps what has caused the magnetic storm and the aurora. Aurorae show pretty much the same correlations with solar activity that the magnetic storms do. On a long time scale they correlate very well. However, if we look very carefully at the data on a short time scale, say on a daily or weekly basis, there is no clear correlation, all of which indicates a complex relationship.

If you see an auroral display in Southern California, it is almost a foredrawn conclusion that there has been a flare on the sun. These low latitude auroral only come with, or after, rather large flares on the sun, and you observe them only near the maxima on the sunspot cycles.

The theory of geomagnetic storms and aurorae was really the first indication we had of the sun shooting matter into outer space. In both these events we have to pick something associated with the sun that interacts with the earth's magnetic field. The only logical choice is electrically charged particles that move from sun to earth. Now we can in a somewhat satisfactory manner present arguments to show that protons and electrons are shot out from the sun in electrically neutral streams. With a somewhat reasonable interpretation of the effects of these streams on the earth, we can account for many of the features of magnetic storm as well as the auroral features. Incidentally, the auroral zone seems to be very narrow, but, even so, we can predict it in the right place.

In the case of the aurorae, we have a little more direct evidence to go on. Since the streams of solar particles are interacting with the earth's magnetic field, we know that they are charged particles, probably protons. If protons come into the earth's atmosphere, they will capture electrons, and emit the spectral lines of hydrogen atoms. When we examine the light of the aurorae looking along the magnetic zenith, we do indeed see radiation emitted by hydrogen atoms that are moving toward us at rather high speeds of up to 3,000 kilometers per second. Now it takes roughly two or three days following the flare before an aural display occurs. If you interpret this in terms of the time it takes for the particles to come from the sun to the earth, they traveled something around 1,000 kilometers per second, which is consistent with the sort of velocity we observe for these incoming hydrogen atoms. We also observe, perhaps in the most direct way, radiation from the sun in the cosmic ray spectrum. At the time of large flares, there have been observed on several occasions large increases in cosmic ray flux. The largest increase occurred in February 1956, when the cosmic ray intensity in the lower energy part of the spectrum increased by several hundred percent and came from the direction of the sun. Cosmic rays traveling almost at the speed of light seem to come with almost all large flares. This is the most direct evidence we have of the sun emitting streams of particles.

Another piece of evidence comes from a somewhat unexpected source, the study of comet tails. When we look at the tails of comets, we observe matter moving out from the head of the comet away from the sun. In some cases this matter accelerates away from the sun. Initially, we thought this was caused by ultraviolet radiation from the sun, simply as a result of radiation pressure, but, having observed the part of the spectrum where we expected this to occur, we now know that the radiation is not of sufficient order of magnitude. The only other way we can account for this phenomenon is to assume that there is matter coming out of the sun that is pushing matter out from the comet. There have been attempts to correlate the motions in comet tails with geomagnetic storms with enough success to suggest that there is a relationship between the two events.

So all in all, the assumption that streams of particles leave the sun makes a reasonable picture. However, I would like to put in a word of caution. That is simply that we have no direct evidence of these particles coming from the sun to the earth, with the single exception of cosmic rays.

Now then, in looking for specific association with weather, we have taken a somewhat different philosophy from that which is usually taken; the usual one being an attempt to correlate some weather parameter averaged over a large area of the earth and over a large period of time, say a year, with sunspot activity. No real positive correlation has been demonstrated by this approach. Since the particles coming from the sun are funneled into a narrow belt in the polar regions, it seems to us to be a logical approach not to take space averages, but to look for specific events, specific occurrences, of the weather, and particularly for things that vary in some areas but not in others. Therefore, we will not consider space or time averages, but we will look for specific events.

All of the solar variations that I have mentioned are observed in the high atmosphere on the earth—at seventy kilometers or above where they are absorbed. The pressure at this height is about one quarter of a millibar compared to 1,000 at sea level. Our normal weather observations are restricted to the atmosphere below 100 millibars or so. Therefore, any attempt we make to correlate events on the sun with events on the earth has to leave a large gap in the atmosphere. In a sense this makes the study difficult. Even if we know what the solar event is that we are looking for, it is difficult to say what the resultant atmospheric events are going to be, because there must be some intermediate mechanism connecting the high and the low atmosphere.

Many arguments can be advanced against any relationship between the sun and terrestrial weather mainly on the basis that all of the known changes in solar radiation are absorbed in the high atmosphere and that they make up a small fraction of the total solar energy. The first objection is certainly true. If there is to be a solar weather effect, it has to be a large effect in the outer atmosphere in order for this tenuous tail to wag the big dog down below in the lower atmosphere, which has a great deal more mass.

When it comes to the question of just how much of solar energy changes, we find it difficult to give exact numbers. In fact, we have become suspicious of limitations that have been placed in the past on these variations. We can make some guesses as to how much it changes by extrapolating from those events which we have observed. In general, these estimates lead to the conclusion that the energy changes are small. The solar energy comes into the earth's atmosphere at the rate of about 10^6 ergs per sq. cm. per second. We observe variations on the sun, which suggest variations at the earth of the order of a few ergs per sq. cm. per second, probably about 10^{-5} of the total solar energy. But, this does not rule out the possibility of much larger variations. One recent suggestion that this is really the case comes from Professor Wenkler of Wisconsin, who has been using high altitude balloons to study high energy radiation and has picked up, quite by accident, very strong X-ray radiation at an altitude of 70,000 feet. If one extrapolates this energy back to the top of the atmosphere, the implication is that the X-rays are generated by high speed electrons coming into the atmosphere with an energy of something like 100 Mev. There are enough of these electrons to be equivalent to about 3 ergs per sq. cm. per second. From all indications about the nature of the streams of particles coming from the sun, protons come along with electrons at the same velocity. They, therefore have about two thousand times the energy that the electrons do. If we accept this, then these streams feed in energy at the rate of about 10^4 ergs per sq. cm. per second. Admittedly, this is in the realm of speculation. However, other ways of estimating particle density and the energy carried by solar corpuscular streams lead to similar results. To be perfectly honest, we can not restrict the sort of variations that occur in the solar energy received locally at the top of the earth's atmosphere to anything less than 100 percent of the solar constant itself.

Another question that I would like to raise is simply how much do we have to change the energy we put into the atmosphere before there is a noticeable effect on atmospheric circulation? I am not certain this is a question that can be answered. If it can, then I would like to have the answer. I do not think we really know whether it is one percent of the energy which normally comes in, or 10 percent, or 1 part in 10,000.

We have adopted the attitude of simply looking for a possible connection between the solar particles coming into an atmosphere and some atmospheric response. The work I would like to report on tonight stems from work done earlier by Shapiro of the Air Force Cambridge Research Center, in which he made a study of the persistence of atmospheric circulation. What he did was to lay out a grid of latitude and longitude over the United States and then study the time correlation in the heights of the constant pressure surfaces at the grid points. What he found in doing this sort of correlation was that the heights of the constant pressure surfaces were very persistent in time for periods of several days, or, in other words, the height contour pattern at a particular time correlated very strongly with the contour pattern a few days later. However, he also found that there were specific periods in which the persistence or correlation was not as good as it was at other times. These periods of breakdown in the persistence of atmospheric circulation were apparently correlated with geomagnetic storms. Since then, he has extended his studies to Europe using surface weather maps that cover a period of forty to fifty years. Some features of the initial correlation show up in all of his studies; some do not. Those that show up commonly in the studies indicate a very significant relationship between atmospheric circulation and geomagnetic storms.

The implications from Shapiro's work are that magnetic storms are followed a few days later by changes in atmospheric circulation. The nature of the change is such as to indicate that the primary change is in the long wave hemispheric circulation.

In our study, we have deliberately started with this point in mind, that is, we started with the hypothesis that there is an association between wave disturbances in the atmosphere and geomagnetic storms. To carry out the study we have used data supplied by the Department of Air Transport of Canada because we wanted data at the highest altitude where a sufficient area of the Northern Hemisphere was covered. The charts furnished by the Canadians were for the 300 mb level.

For purposes of this study, we chose the region from zero to 180 degrees west longitude. As one index we used the length of the contour line defining the position of the 30,400 ft height of the 300 mb surface between the extreme longitudes. This is a convenient index to use since it is quantitative and objective. We also defined another index intended as a measure of trough development. For each trough, we picked the points of inflection in a fixed contour. We then defined a trough index based on the ratio of the distance between the inflection points to the distance measured along the trough line from the point of maximum cyclonic curvature to the line connecting the inflection points. This is one way to get a measurement of the development of a trough and its intensity. There is, of course, considerable uncertainty and arbitrariness in such an index. The only saving point in this case is that the amount by which you can force the index for any particular trough is considerably smaller than the range you observe from trough to trough. One could always re-draw the chart in a somewhat different way, of course, but even by doing this you cannot force the index for any particular trough nearly as far as you can the indices for different troughs. In that sense, it has some useful characteristics. For the geomagnetic index, we took the Cheltenham "A" values simply because they were the most readily available. (All magnetic indices correlate very well.)

Since we were looking for a specific feature of the circulation as I have already indicated, we decided to look for trough development. Somewhat arbitrarily we chose the region between 180° and 120°W longitude and north of 40°N latitude as the test area. Actually, we picked this region with the thought in mind that it is a region well-known for being a more-or-less semi-permanent area for generation of troughs, and because it is the western boundary of our charts.

Using the magnetic data, we picked out certain key days on which the magnetic index was greater than 23, and on which the increase in the index from the preceding day was greater than, or equal to, twelve. In other words, on key days the Cheltenham A index, A_{ch}, had to be greater than 23 and on the preceding day it had to be less than eleven. The reason for this selection is that there are long-term variations in the magnetic indices characterized by a slow rise in intensity with subsequent decline, which need to be distinguished from the variations of a distinctly different type that represent a sudden onset of magnetic activity. It was the latter we wished to use. There were nineteen geomagnetic key days during the period of study running from October 1956 to March 1957.

We also found it desirable to define key days in terms of troughs. We did this by picking those troughs that were first observed in the test area three or four days after an A_{ch} key day. The day on which the trough was first observed then became the key day.

The average trough index for the 54 troughs studied is about 0.6. If we pick large troughs for which the trough index reaches a maximum value greater than 0.7, which is somewhat above the average value, and do a superposed epoch analysis of the number of troughs first appearing in the test area versus magnetic key days, we find the results shown in figure 104. Evidently there is a significantly larger number of these troughs that first appear in the test area three to four days following the magnetic key days than on other days. If we take all troughs, we do not find any such relationship. There is still a peak three to four days following the magnetic key days, but there are other peaks equally as large.

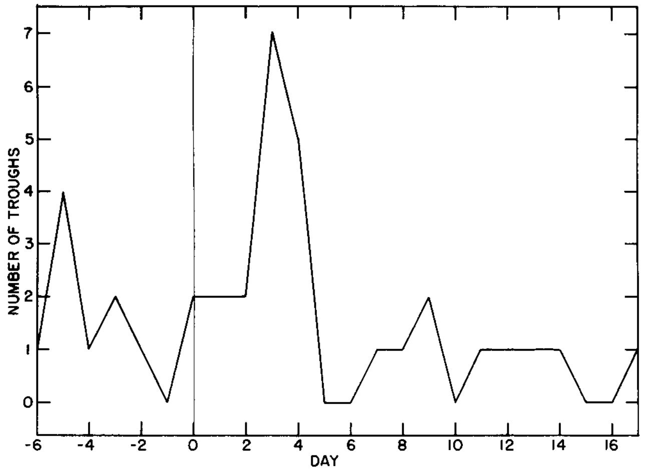

FIGURE 104. Number of large troughs appearing in the Alaska-Aleutian area before (—) and after (+) days of geomagnetic disturbances.

The next step in the analysis is to look at the development of the key troughs following their appearance in the test area. Figure 105A shows the trough index plotted against time for days following the time when the trough first appeared. The solid curve at the top is for the key troughs. In this case there were sixteen key troughs. That is, of the nineteen magnetic key days, sixteen were followed three to four days later by the appearance of a trough in the test area. The trough index, $\overline{I}_t$ increases to a much higher value for key troughs than for the rest of the troughs. The trough index is essentially a measure of cyclonic curvature of the trough, and does not necessarily measure the amplitude of the trough. Figure 105B illustrates the same plot with the three largest and six smallest troughs removed. This is used as a test to see if the result is determined by just a few extreme troughs. Again the same effect shows up. If we consider the fact that the troughs first appear three to four days after the magnetic storms and then take something like five days to develop, we expect to see an effect in the circulation something like a total of eight to nine days after the magnetic storm. I should point out that not all the troughs that show up three to four days after the storm get to be large troughs, and not all of those that follow at other times in the test area are small troughs. There are large troughs that are not key troughs, as I will point out later, but figure 105 definitely shows that those troughs that appear three to four days after a magnetic storm tend to become troughs of large cyclonic curvature.

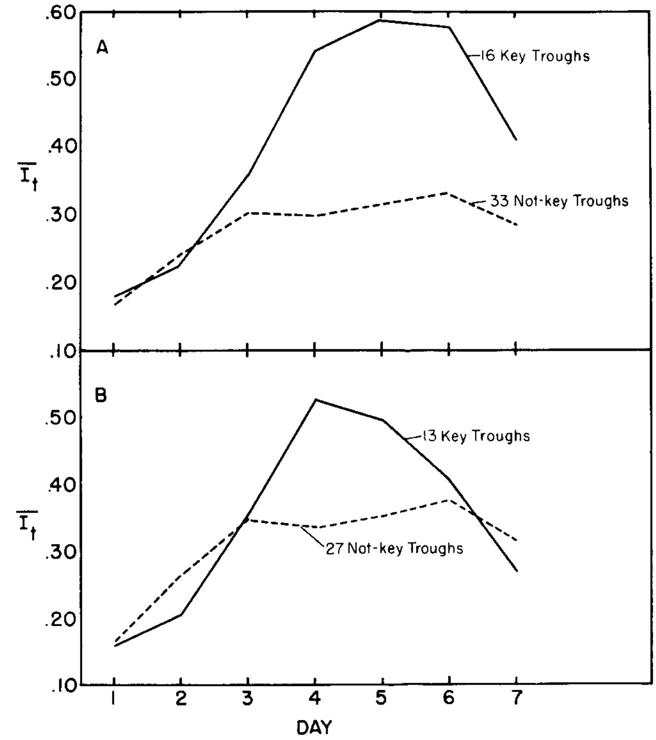

FIGURE 105. Average values of the trough index ($\overline{I}_t$): A. For 16 "key troughs" that followed magnetic disturbances and 33 non-key troughs. B. With three largest key troughs and six smallest non-key troughs removed.

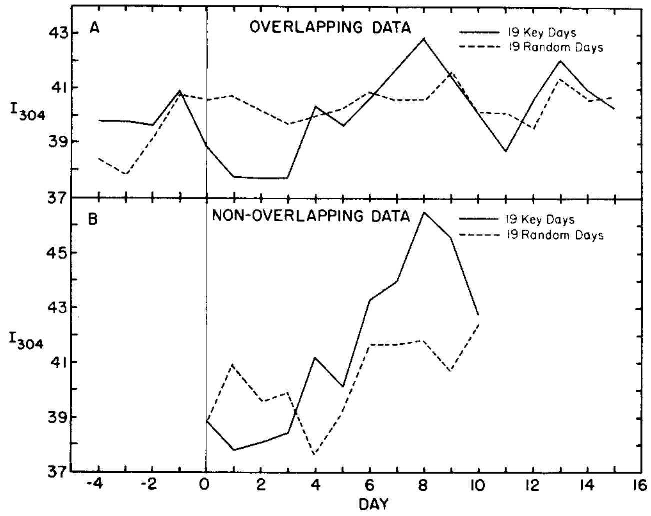

FIGURE 106. Average value of the counter index (I_{304}) for days before (—) and days after (+) magnetically selected key days. The broken curve represents means using randomly selected key days.

The next step in this study was to look at the length of the 30,400 ft. contour versus the magnetic key days. Figure 106 exhibits a superposed epoch analysis using the eighteen key days for the solid curve and nineteen random days for the dashed curve. The top set of curves in figure 106A use the data just as it was collected. The average period between key days of about nine days is less than the period of the analysis, so in many cases the same data were entered twice. In other words, the magnetic storm will occur, a trough comes into the picture and before it has time to develop another trough has occurred. Since this will tend to smooth out any correlation that is present, we separated the cases in which there was this overlap and plotted the curve for the non-overlapping days in figure 106B. Unfortunately, when you do this you cannot really define an average so it is difficult to test significance. However, the peak at about eight days is still very much evident in the non-overlapping data.

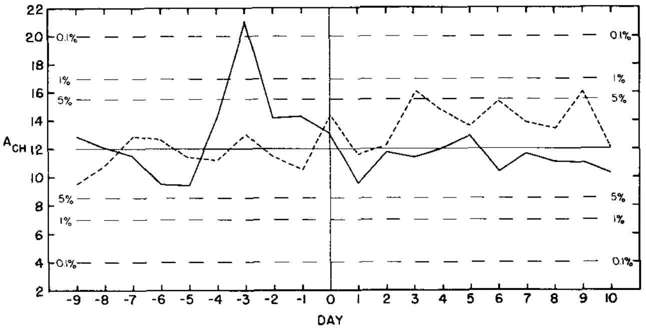

FIGURE 107. Mean value of the geomagnetic index (A_{CH}) for days before (−) and days after (+) the 25 days when trough type A or B (solid line) and the 28 days when type C (broken line) first appeared in the test area.

At least it is suggested rather strongly that this eight-day lag between the magnetic event and the development of the trough is really there.

In inspecting the synoptic maps it is quite evident that the trough that developed into a larger trough exhibited tendencies to have a preferred position with respect to the surface of the earth at maximum development. These preferred positions seemed to be over the West Coast and East Coast of the United States. Therefore, we deliberately divided the troughs into three categories. Those troughs that develop to maximum intensity over the West Coast of the United States were called type A troughs; East Coast B; all others C. Type C again will include some of the large troughs. We are not throwing all the large ones into A and B when we make this separation.

If we then pick out key days for troughs defined in terms of troughs first appearing in the test area three to four days after the magnetic key day, the A and B troughs exhibit the behavior illustrated by the solid line in figure 107. The peak comes at minus three days, as you would expect it to. For the type C troughs, which are the group that do not reach maximum intensity over the East and West coasts, there is no such relationship. Alternatively, we may illustrate the difference between A and B and C troughs by means of a contingency table.

TROUGH TYPE	A_{CH}	NO A_{CH}	TOTAL
A & B	14	11	25
C	4	25	29
TOTAL	18	36	54

CHI-SQUARE TEST $P = .001$

The key troughs, which are those that first appear in the test area three to four days after the magnetic key day, are placed in the column labelled "A_{CH}". All other troughs are placed in the column labelled "No A_{CH}". The first row contains the A and B troughs lumped together. From a total of 25 A and B troughs, fourteen first appear three to four days following the magnetic key days. There are eleven that appear in the test area at some period other than three to four days. I might point out that we have nineteen key days. We are taking two days for each key day so there are 38 days out of six months, and we get

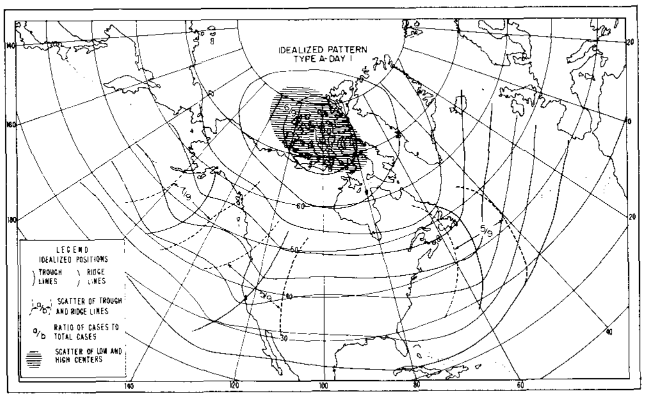

FIGURE 108. Idealized 300 mb chart, Type A, Day 1.

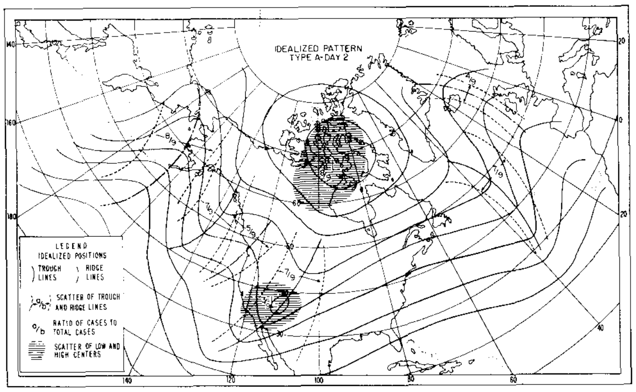

FIGURE 109. Idealized 300 mb chart, Type A, Day 2.

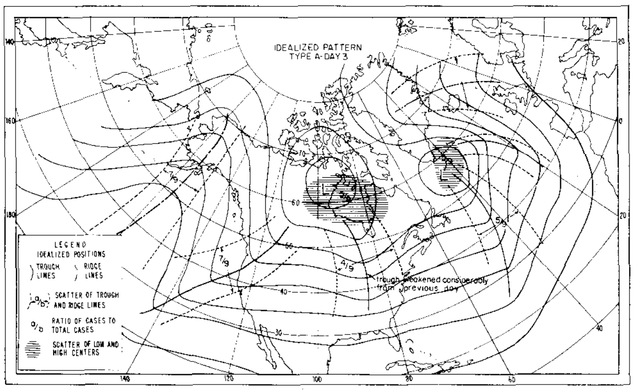

FIGURE 110. Idealized 300 mb chart, Type A, Day 3.

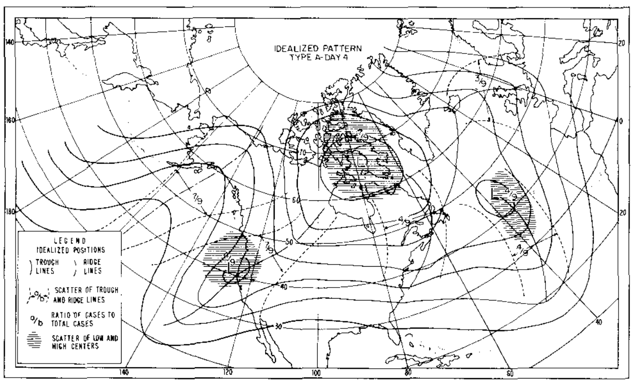

FIGURE 111. Idealized 300 mb chart, Type A, Day 4.

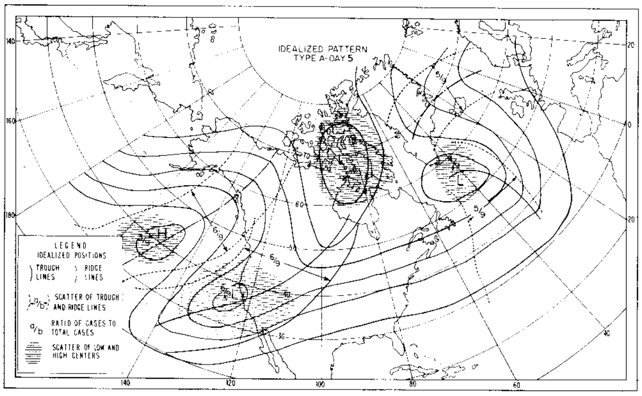

FIGURE 112. Idealized 300 mb chart, Type A, Day 5.

something over half of those troughs showing up in the test area within those 38 days. For the type C troughs, the corresponding numbers are 4 and 25. The probability of such an arrangement of numbers is something like 1 part in 1000, which is significantly different from a random relationship. We probably have not proven anything conclusively by these arguments. I, myself, am not a statistician, and I am in general quite skeptical of statistics because of my lack of understanding. However, I think it is at least very suggestive from this analysis that, following an abrupt magnetic storm, the troughs at the 300 mb level that appear in the Aleutian Island area three to four days after the magnetic storm, tend to develop into large troughs, become major perturbations in the circulation and come preferably over the West and East Coasts.

We have attempted in the series of idealized charts shown in figures 108-112 to show a typical development of a type A trough. These charts were drawn by superimposing the actual synoptic charts. A total of nine cases of type A troughs were chosen and the fractions entered in figures 108-112 give the number of cases in which the trough and ridge lines fell within the indicated boundaries. These charts presumably show the typical development of any one of these troughs. If you focus attention on the two troughs over the West Coast and Gulf of Alaska on day one, you will notice that the trough over the West Coast deepens some as it moves eastward on day two but subsequently fills. The trough over the Gulf of Alaska, however, deepens steadily into a cut-off low. The first trough is considerably east of the test area, and the latter is in the test area on day one. The troughs that are in the test area on day one deepen markedly as the statistical analysis suggested. We have constructed similar charts for the type B troughs and these show similar developments over the East Coast rather than the West Coast of the United States. As I have said before, we have not really proven anything, but the consistency of the results when looked at from several standpoints, at least suggests that the relationship is real. I think it is further supported by the fact that we started out expecting to find such a relationship. We deliberately chose the magnetic index and the circulation indices in order to test the hypothesis that following magnetic storms there is a marked perturbation in atmospheric circulation. This is just what we found, which in some ways adds confidence to the result.

DISCUSSION

Saur: Did you look back to see whether these troughs were unrelated when the storms actually reached the earth?

Athay: Our maps do not extend far enough to show this. There is one thing I should point out, however. We have picked troughs in a particular area, and this is not to be interpreted as necessarily meaning that the development of these troughs is the major effect. As Namias pointed out, this could be a result of developments someplace upstream or downstream. If indeed we are detecting anything, it would be going too far to say that we have actually found a cause and effect relationship.

Schaefer: In regard to indices, did you inspect this particular series of data before you made these indices, or did you make the indices first from prior data?

Athay: Before I answer I should say I did not do the work I am reporting on. It was done by someone else, and it has been checked independently by other people. The particular indices used were chosen after a preliminary inspection of the first three months of data.

Namias: Is it possible, if your results are statistically significant, that you are getting an evolution via the connection between magnetic activity and circulation features of the atmosphere? The magnetic index is not a unique characteristic of the solar activity as I understand it.

Athay: This is always a possibility. All I can say here, is that Wolf's theory has not been generally accepted by geomagneticians. Furthermore, the large geomagnetic storms are indisputably of solar origin, and the general solar control is evidenced by the correlation with the sunspot cycle. It may be a backwards sort of correlation, but the fact that we are finding atmospheric effects *following* magnetic storms implies that the relationship is the other way around is the reverse of what Wolf suggested. Whatever is causing the magnetic storms produces changes in the circulation, rather than the reverse. If it were as Wolf suggested, we should expect to find changes in circulation coming before the storm instead of behind the storms.

Revelle: Not necessarily.

Athay: Perhaps, but it is difficult to visualize any mechanism that would lead to such a relationship.

Charney: Do you ascribe any unusual significance to a three to four day lag, or is this the lag that gives you the best correlation?

Athay: This lag may be caused by the test area, and it is not necessarily a significant result. It may be that the event is occurring down or upstream.

Revelle: I am surprised at the conservative figures of the radiation flux.

Athay: I was conservative deliberately. Frankly, the estimates range through several orders of magnitude. It is not obvious now just where you should place the order of magnitude. One can give arguments, as Chapman did, that the flux is of the order of one-tenth of the solar constant. Others may raise the estimate up to the solar constant or lower it several orders of magnitude. We hope that the satellites will be able to furnish a conclusive answer, but it has not been done yet.

Namias: There must be a test of this hypothesis. This type of trough development into the southwest, is frequent enough so that it might be worthwhile going back in the past records for special cases. There are certain months when it has a tendency to occur, and other months when it does not occur at all. By separating these months, perhaps you could clarify the relationship with solar activity.

Athay: Would there be data available? We have done the same thing for the past winter '57 and '58 and we generally find the same result. There are some differences in detail, but the same general results are still found.

Namias: In that location, or did you change the location? How many troughs occurred?

Athay: The same test area was used but I do not know how many such troughs there were. I have not followed the later part of the analysis.

Namias: At M.I.T. a student tried to correlate the success or failure of forecasts with solar events, but I do not believe anything conclusive has come of it. Of course there are times when a trough of this nature develops which is not forecast so there is plenty of room for another element not presently considered to come into the picture. On the other hand, many successful predictions of such trough developments are made routinely.

Athay: Especially on a time scale of the order of the one we are considering.

Namias: Just before I left, I had the occasion to see some work prepared by Teweles of the Weather Bureau, in which some rather extensive maps have been constructed up to the 25 millibar level. Some of these involve cases where there are major changes in the high levels of the atmosphere including cases where there was very rapid warming. As Charney indicated, I think that the coupling between whatever goes on below and with events at high altitudes is very important. Up to now this has been more or less neglected because of lack of data.

Isaacs: The index of magnetic change is purely a terrestrial thing, isn't it?

Athay: It is a terrestrial effect, but it is related to solar activity both empirically and theoretically. We are using it as a measure of solar corpuscular radiation.

Isaacs: How does this correlate going back in time—the solar activity that you consider in something like cycles if you consider the sunspots?

Athay: It correlates very well on a long time scale. If you look at short term correlations, it is not nearly as good, however, except for the large storms.

Isaacs: But how can you go back to this kind of weather condition?

Athay: The charts that are available for this elevation do not go very far back.

Isaacs: But can you recognize these troughs from sea level data?

Athay: Presumably there is an association between the 300 mb trough and features on surface charts. Shapiro's work went back some forty years using surface charts and 700 mb charts. He was not looking for particular features; he was looking for any change in circulation. He found that if he deleted the years around sunspot maxima the correlation between the persistence of the atmospheric circulation and geomagnetic storms was enhanced.

Namias: It is rather difficult to relate simply sunspots to the sea level pressure patterns. As Athay indicated, all sorts of studies have been made not revealing very much.

Charney: What is the chance that disturbances in the lower atmosphere can produce magnetic storms?

Athay: One reason it is not believed is that many of the storms of the particular type we are considering occur simultaneously over the earth, which suggests that they are caused by something a long way away from the earth. The theory of solar particles interacting with the earth's magnetic field indicates that the initial disturbance occurs at about five earth radii out. This is one reason. Another reason is that many geomagnetic storms are so closely associated with solar phenomena as to immediately dismiss any alternative hypothesis.

Fleming: In your study criteria, Shapiro selected these events which are related to magnetic storms. What sort of events would lead up to these situations? This is not the beginning. There is nothing very difficult to explain about the indices. These are something to merely measure. What might be concurrent with your magnetic storm and increase the indices by eight days?

Athay: I cannot answer that.

Revelle: The underlying physical assumption is that when the magnetic storm occurs, something also happens that initiates the wiggles in the contours. The question is, if they are not due to magnetic storms, what other types of event could happen eight days before this happening?

Schaefer: Revelle, if it is not the magnetic storm, what could it be?

Revelle: The magnetic storm can only act at the time it exists. It can not act at some other time.

Fleming: I am thinking of something that leads up to these meteorological features. This is not the beginning. What could precede these features?

Revelle: How does a magnetic storm act? Either both the magnetic storm and this event are caused by something else, or the existence of a magnetic storm brings about this event.

Namias: During certain months the monthly mean planetary wave patterns favor a strong ridge over the eastern North Pacific and an associated strong trough over the southwestern United States. In such cases the type of development indicated by Dr. Athay will be frequent. Each daily trough and accompanying cyclone will plunge into the Great Basin, deepening as it moves. In other months such trough developments may be absent. If this is a solar effect, it should be shown perhaps most strongly by a study of monthly means.

Revelle: There is no observed immediate relation between magnetic storms and the visible radiation penetrating in the lower atmosphere. Wolf has claimed that there is, but no one else has been able to find it. We can correlate visible radiation with magnetic storms that occur two or three days later. It is not the radiation that produces it but something else. All we can observe is something we see visually. What Wolf did was try to correlate the geomagnetic storms with the occurrence of an active region just coming around the sun. He claimed to find such a relationship but no one else has been able to verify it and it is generally discounted as a theory for storms. There are recurrent storms—magnetic storms which occur over a 27-day period that have defied any attempt to correlate them with any visual features. It might very well be that you get a greater or lesser absorption of a visible radiation in the high atmosphere. I am bothered about the machinery, the means of getting the energy from the high atmosphere to the low atmosphere. This has been one of the arguments against such a relationship since no one knows the height of the atmosphere. Winkler suggests one possible mechanism which you can perhaps settle by radiation.

The presence of noctilucent clouds may indicate the same high altitude process of some nature.

Athay: The first column of the contingency table contains essentially the key troughs which determine the three to four days lag after magnetic lag days. The other column is simply the remainder. The point is that most of the A and B troughs come after magnetic key days, but very few of the C type do. We have not said anything about energy mechanisms, we just assume that particles are radiating from the sun, and we look for some effect on atmospheric circulation.