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Relationships Between Solar Activity and Climate Change

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Climate change is of extreme importance in world affairs. Therefore, we should forcefully pursue all avenues of research leading to improved understanding of the underlying causes of climate change. One such avenue involves the possible effects of variable solar activity on regional and world climates. A major obstacle to theories that seek to relate climate to varying solar activity is the extremely small energy involved in such variations. Thus "trigger mechanisms" will have to be invoked if progress is to be made.

Vast numbers of apparent solar/climate relations have been advanced. I have chosen to review only a few, including one that apparently relates recurrent droughts in the High Plains of the United States to the double sunspot cycle. Some of the pitfalls of such analyses are reviewed. There is a common thread emerging in research, however, that suggests that high solar activity is generally related to an increase in meridional circulation and blocking patterns at high and intermediate latitudes, especially in winter. I offer a speculative suggestion that the effect is related to the sudden formation of cirrus clouds during strong geomagnetic activity that originates in the solar corpuscular emission.

Climate changes vitally affect world affairs. One need only consider the "domino effect" of the summer droughts of 1972 to realize the dependence of humanity on seasonal weather anomalies. The intense Moscow area drought and heat in the spring and summer of 1972 was serious enough to compel the Soviet Union to purchase wheat from Canada and the United States. This unusual need coincided with new demands elsewhere that conspired to wipe out our surpluses. The result was skyrocketing domestic and international grain prices, with dire consequences for meat and poultry prices. The impact in India, the sub-Sahara, and elsewhere was far more tragic: millions of people went hungry because of the exhaustion of world grain reserves at the same time as their own fields dried up with spreading droughts.

It is therefore not surprising that there is an increased interest in climate research the world over. The need to predict and to plan is just too important to world welfare for us to leave any new research leads unexploited. And, indeed, the time is now scientifically favorable for new initiatives in climate research. There have been great improvements in the understanding of the general circulation of the atmosphere and the oceans in recent years. These findings have come at a time when Earth-orbiting satellites have given us new means for observing the global behavior of the oceans, the atmosphere, the land cover, and the ice; these factors, together, hold genuine promise of advances in the understanding of climate changes. So for the years immediately ahead, it is a matter of urgency to find the people who will do this climate work and to give them the support that the problem deserves. In our country, climate research had been an underdeveloped science in recent years. The time has come for us to become a rapidly developing Nation in this field of research that so critically applies to human needs.

One of the many contending theories of climate change involves variations of the solar input

to Earth's atmosphere and surface. This is the subject of my paper. There are, of course, many different ideas about the origins of climate change. Many factors have been looked upon as potential causes: vulcanism, sea surface temperature changes, changes in CO_2 content of the atmosphere, oscillations in Arctic ice and sea depth, and atmospheric turbidity changes due to man-made dust or wind-blown soil and sand.

These theories, including the solar one, share the difficulty that they have not yet reached the stage where convincing experimental verification is possible. It may turn out that climate changes occur for a number of different reasons and that more than one theory will ultimately be verified. Meanwhile, it is important to follow forcefully all promising leads that have any chance of advancing our climate forecasting skills and to devise critical experiments and analyses to determine which leads are the most significant.

Probably the reason so few talented scientists have worked on climate theory is that real progress appears to most meteorologists to be very difficult. Success has seemed unlikely until other tasks have first been accomplished in short-term weather forecasting research. But this may not necessarily be so. It may be that the atmosphere responds sensitively to long-term changes in rather small forcing functions, such as increased ocean evaporation due to anomalously warm sea surface temperatures over large areas. In such a circumstance it may be easier to make progress by looking at monthly, seasonal, or annual mean circulations than it is by examining day-to-day meteorological changes. In another research area, it may be possible to do explicit numerical dynamical modeling of climatic properties effectively, and this may be a more fruitful approach to climate modeling than integration with the usual general circulation models over long periods of time. Be that as it may, my purpose here is to look at one aspect of climate theory, namely that having to do with the effect on climate of variable solar activity, if indeed there is one. For this paper I will confine my attention to climate changes that manifest themselves as anomalies of meteorological parameters of seasonal, annual, or decadal time span. I shall not look at climate changes in the time frame of centuries or millenia, important as they may be. Nor will I look at day-to-day solar/weather effects; that is the subject of the next paper. Indeed, I suspect that the most important climate effects are simply the aggregations of persistent day-to-day weather effects, as Prof. Hurd Willett pointed out long ago.

Finally, let me say that I do not plan to do a comprehensive survey of the vast literature on the subject of suspected influences of variable solar activity on climate. There are good summaries of this available (Lamb, 1972; Pokrovskaya, 1970). My purpose, instead, is to discuss critically a few selected findings that seem to show a real effect of variable solar activity.

OBSTACLES

The subject of solar/weather relationships is spiced with strong language. To be sure, a great deal of uncritical work was done, especially in the 1950s, by workers whom Lamb (1972, p. 441) had characterized as "over-optimistic or naive amateurs working in isolation and without adequate criticism of either data or results." Andrei Monin (1972) has some sharp words for "heliogeophysics enthusiastics" working on suspected influences of solar activity on weather:

> Most of the information concerning such an influence fortunately produces only an impression of successful experiments in autosuggestion; the hypotheses proposed concerning the physical mechanisms of the influence of solar activity on the weather lack convincing substantiation.

He says "fortunately" because he thinks that to find such a result would be almost a tragedy for meteorology because then one would have to predict the solar activity to predict the weather, and he thinks we have enough problems without that one.

But the matter will not go away that easily; and if indeed solar activity is a significant factor in weather and climate, it will not advance research progress simply to wish it away. If it is not a significant factor, we will be better off to know that as we seek theoretical explanations for climate change. I am convinced, however, that there is good evidence, on some occasions at least, that certain weather and climate phenomena are significantly linked with solar activity or with

upper atmospheric phenomena generally considered to be caused by solar activity.

It must, nonetheless, be frankly stated that the literature of efforts to find links between variable solar activity and meteorological phenomena is spotty. Many of the publications in this field are vague and sketchy. Some are very poorly done. We have more than the normal share of such papers I fear. They hurt the reputation of all workers in the field. Few of the published research works deal effectively with the physical mechanisms that must, sooner or later, be subjected to critical tests if we are to develop confidence in our understanding of empirically discovered connections. In my view, the most important step that must now be taken by those seriously interested in the solar/weather field is to generate some plausible physical explanations and then to test them quantitatively against observational data. I hope that this symposium will be a step in this direction.

A serious obstacle facing hypothesis makers in this field is the energy problem. It is a hangup that has been recognized for a long time. The problem, simply stated, is this. There are large potential and kinetic energy transformations involved in changes of the large-scale dynamical features of the general circulation of the stratosphere and its interactions with the troposphere. The changes in solar energy incident upon the atmosphere as a result of changes in solar activity, on the other hand, are orders of magnitude smaller. It is hard to imagine a plausible scheme to have this tiny tail wag the huge dog. But that is the essence of the problem. Many authors appeal to "trigger mechanisms," but these are, of course, very difficult to deal with quantitatively and logically. If trigger mechanisms are at work (and unless I am wrong about the reality of solar/weather influences there must be such processes going on), then we have a serious responsibility to find ways to assess the collateral consequences of any given trigger mechanism, and to use them to increase the susceptibility of the hypothesis to quantitative test. That is the most important item on our agenda now, as I see it.

It is obvious that variable solar activity controls many important ionospheric phenomena. In some instances very high ionospheric winds are produced. But these offer no easy solution to the energy problem because the atmosphere at the levels of solar control has so little density that its kinetic energy is still trivial, in spite of the high velocity, in comparison with that needed to push around the lower atmosphere.

When one is addressing questions of solar activity and climate, still another obstacle must be faced. This is, in brief, the very unsatisfactory state of affairs with regard to theories of climate change. Only in most recent years have we begun to give explicit attention to the forcing mechanisms that are almost certainly involved in climate change even though their short-term weather implications are small. Atmospheric scientists are now beginning to give the appropriate attention to the radiative balance implications of increased atmospheric CO_2 or scattering aerosols. They are now also starting to look carefully at the interactions between polar ice, ocean flow, and the atmosphere. These are examples of important steps in climate research. Only when our general understanding of climate change improves greatly, I suspect, will we make substantial progress in understanding the true role of variable solar activity as an influence upon climate. It is, moreover, likely that climate change is not uniquely determined, but that different or even contrasting initial influences may alter world climates in similar ways. This will not simplify our task!

REVIEW OF SELECTED SOLAR-CLIMATE EFFECTS

There is an enormously abundant literature dealing with research work purporting to relate changes of solar activity to various aspects of climate change. I shall select only a few of the published works to discuss critically. My choice is designed to concentrate on just a very few items from among the many that are probably relevant, and I have selected those research findings that seem to me to provide the securest empirical-statistical evidence for an influence of solar activity on climate change.

Recurrent Droughts in the High Plains Area of the United States

The best-established result of statistical studies showing apparent effects of variable solar activity

on climate, so far as I am aware, is that relating solar activity to severe droughts in the High Plains of the Central United States in the first 500 or 600 km east of the Rocky Mountains. Various authors have called attention to this coincidence (Borchert, 1971; Marshall, 1972; Thompson, 1973). There is a striking tendency for the droughts in this region during the last 150 yr to recur with a periodicity of about 20 to 22 yr, and with a reasonably constant phase relationship to the alternate minima of the solar activity cycle.

The easiest representation on which to visualize this cycle probably is that used by Thompson (1973). Figure 1, adapted from his paper, shows the sunspot numbers for this century plotted in such a way that the alternate maxima are plotted as negative numbers. There is no physical reason to interpret alternate cycles as negative numbers, but it has long been known that there is a very real sense in which the "true" sunspot cycle is about 20 to 22 yr rather than 10 to 11: The magnetic fields of the leader spots of sunspot pairs are opposite in the opposite hemispheres of the Sun during a given 10-yr spot cycle but both reverse at the start of a new cycle. This fact was noted many years ago by the solar physicist G. E. Hale, and the 20- to 22-yr quasi-cycle of sunspot activity is often termed the "Hale double sunspot cycle" or simply the "double sunspot cycle." The physical reason for this behavior is still a matter of speculation.

For illustration, in the cycle from 1934 to 1944, the leader spots in the solar northern hemisphere

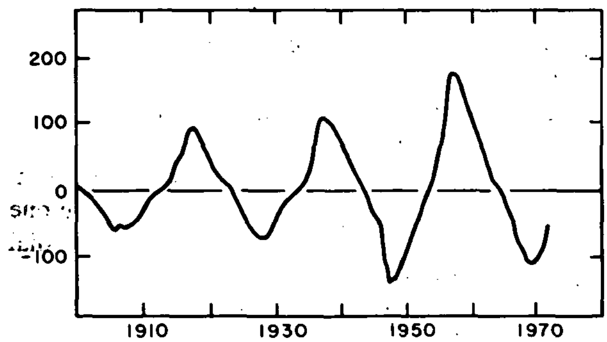

FIGURE 1.—The Hale double sunspot cycle. The alternate maxima in the 10- to 11-yr sunspot cycle are plotted with opposite sign. Plotted below the horizontal zero line are the alternate maxima whose amplitudes have tended to be smaller.

were north seeking; in the cycle from 1944 to 1954, the leader spots in this same hemisphere were south seeking. It was not until the cycle beginning after 1954 that the spots had the same polarities as they did after 1934. Things were exactly opposite in the solar southern hemisphere. Thus, there is a very real sense in which the behavior of the Sun may be considered quasi-cyclical with a period of approximately 20 to 22 yr. Drawing the sunspot diagram as Thompson has done in figure 1 simply calls attention to this fact.

The polarity of the magnetic field of the Sun near the poles (sometimes loosely called the "dipole field" because it roughly resembles a dipole in shape near the poles) is generally believed to reverse each 10 or 11 yr, but there is great irregularity in the time of reversal and uncertainty regarding its relation to the sunspot cycle. Sometimes both poles carry the same sign for extended times, as one polar region lags the other in reversing. There are also surprisingly substantial day-to-day changes in the poloidal fields. During the sunspot maximum of the international geophysical year, which occurred in 1958, the solar poloidal field was antiparallel to Earth's, having reversed in 1957.

There is, moreover, a tendency in the recent spot cycles for the alternate halves of the 20-yr cycle to be systematically different in magnitude. This can be seen in the fact that the spot numbers plotted negatively in figure 1 are slightly smaller, on the average, than those plotted positively. It is customary, then, to refer to the 11-yr cycles plotted negatively as "minor" and those plotted positively as "major." It would perhaps be better to call these "odd" and "even" cycles because before 1880 some of the negatively plotted maxima are larger than the positive ones.

Figure 2, reproduced from Thompson (1973), shows the sunspot numbers plotted as above, but carried all the way back to about 1750. It also shows by horizontal bars the years from 1800 onward for which the tree growth ring analyses of Weakly (1962) indicated droughts in Nebraska. It is rather striking that there is evidence for a drought at eight successive times very close to the sunspot minimum that follows the minor sunspot maximum. It is also notable that no severe

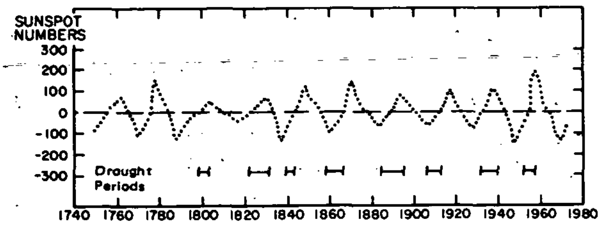

FIGURE 2.—Solar cycle and drought in western Nebraska (Thompson, 1973). Drought periods in Garden County, Nebr., are shown by horizontal bars below the sunspot numbers plotted as in figure 1. All droughts published by Weakly (1962, 1943) are included.

droughts occurred in this region as the major maximum drew to a close.

To illustrate the matter with independent data, I have adapted figure 3 from the Ph.D. thesis of Marshall (1972). A vertical line is drawn at the center date of each of the droughts in his analysis, which was based on drought data from other workers. Figure 3 shows that all of the major droughts of the available time period came remarkably close to the solar activity minima that followed the minor peaks. Moreover, there were no major, extensive droughts at dates other than the ones shown, giving us a one-to-one correspondence during the period under study.

Two nagging questions come to mind: (1) are these coincidences since 1800 accidental and simply the result of selection due to a long search for a correlation in a vast body of global weather records, and (2) are the droughts related to the 20-yr solar activity cycle, or are they evidences of a natural terrestrial oscillation of about 20-yr period that happens by chance, just now, to hold an approximately constant phase with the solar cycle? The distinguished climatologist, J. Murray Mitchell, Jr. (1964) has given serious attention to both questions, and has also given us some very apt warnings about the many pitfalls of seeking periodicities in climate records. He even has some pungent words about the subject: "Hasty and uncritical acceptance of the reality of evidence of cycles in climate has evidently been the source of more waste effort in meteorology than any other kind of scientific misjudgment." And a very similar criticism could be levelel at solar activity versus climate correlation analysis, as Mitchell so cogently points out.

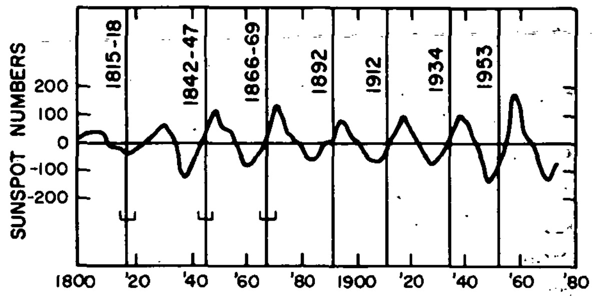

FIGURE 3.—High Plains droughts. This figure is adapted from the Ph.D. thesis of Marshall (1972). The vertical lines correspond to the center dates of all droughts cited by Marshall from rainfall data over the High Plains region. The three earliest droughts are less reliably determined; for them the horizontal bars show approximate beginning and ending dates. Note that every drought occurs near the sunspot minimum following the negatively plotted sunspot maximum.

At my suggestion, Mitchell recently resurrected some drought data for eastern Kansas developed by Wayne C. Palmer, his former colleague in the National Oceanic and Atmospheric Administration (NOAA). He has now plotted severe drought years on two types of harmonic dials: (1) a strict 20-yr recurrence dial and (2) a dial based on the double sunspot cycle (of approximately 20- to 22-yr length). The data embrace nearly the full time span of available records, reaching back to about 1850 and forward to 1960, with some serious uncertainties about the earliest data. The region was chosen by Palmer because he believes it partook in each of the major High Plains droughts since 1850. The data are taken from mean climatological division statistics developed by NOAA. Except for the earliest drought (1852), for which there are some uncertainties in the data, all of the worst years of the severe droughts have tended to cluster near the rising branch of the sunspot cycle following the minor cycle. Figure 4 shows the harmonic dial for these data, which I have adapted from the one given me by Mitchell. Note that half of the dial is completely free of drought indications. The worst drought years listed here tend to cluster slightly later in phase than those in the results which I showed in figures 2 and 3; but this is not surprising because I suspect that the extreme years of a given drought period are likely, other things being equal, to come near the end of the

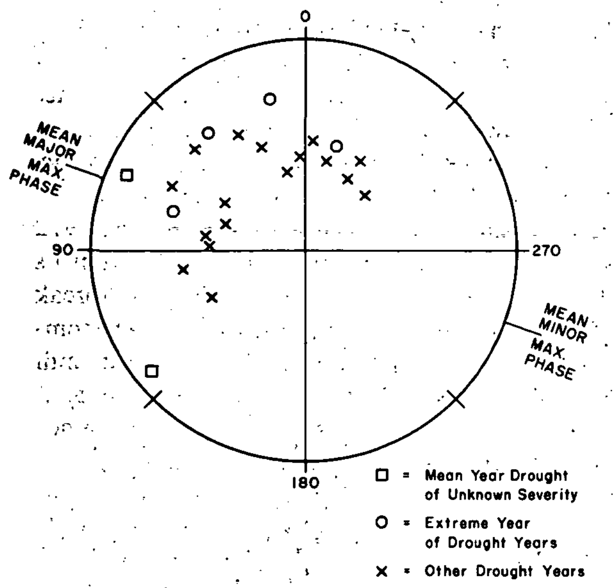

FIGURE 4.—Harmonic dial showing drought dates of differing amplitude for western Kansas as measured by Palmer (Mitchell, 1964). The minimum sunspot phase following the negatively plotted sunspot maximum is designated as phase 0°. Note the clustering of droughts on one-half of the dial around the axis between phases 45° and 225°.

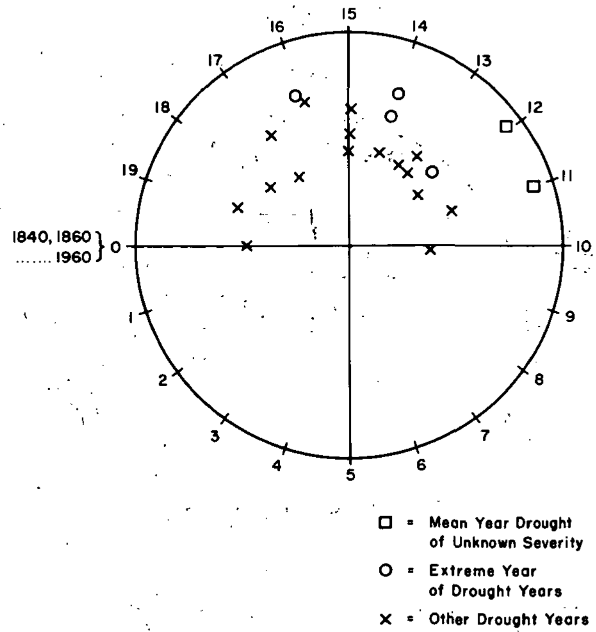

FIGURE 5.—Harmonic dial showing drought dates in figure 4 compared with 20-yr periodicity (Mitchell, 1964). Note absence of droughts in alternating decades 1840 to 1850, 1860 to 1870, . . . , 1960 to 1970. This figure and figure 4 illustrate that western Kansas drought recurrence since 1840 can be explained equally well by association with the double sunspot cycle or with a 20-yr recurrence tendency.

cumulative effect of several successive dry years.

Mitchell next asks whether it is possible, with these same drought data of Palmer, to discriminate between a strict 20-yr recurrence and the double sunspot cycle. Figure 5, also adapted from one given me by Mitchell, shows a harmonic dial to test this. The clustering tendency is approximately the same, except that the 1852 drought falls better in line. One must not forget, however, that in choosing a strict 20-yr period because it seems to fit the data, he has taken advantage of one additional free parameter for the analysis. Nonetheless, the dial shows that one cannot, with the available data span and with these data, safely discriminate between the hypothesis that the double sunspot cycle associates with the droughts and the hypothesis that the droughts are approximately 20-yr recurrent.

To bring to bear the data in figure 2 on this question, I have made two additional harmonic dials. I have plotted points from Weakly's original data and represented them in figure 6, which shows the drought years in Nebraska according to phase in the double sunspot cycle, just as is done in figure 4. I have picked the middle year of the drought and weighted it according to the indicated length of the drought to give the amplitude in the harmonic dial.

It is clear that the harmonic dial for the phase relative to the double sunspot cycle, figure 6, has a significant clustering near the minimum after the minor sunspot maximum. This is what one would expect from figure 1. The double sunspot cycle orders the data slightly better than does a 20-yr cycle, although I have not reproduced the 20-yr harmonic dial here. A cycle slightly longer than 20 yr would organize the data just about as well as does the sunspot cycle. So, once again, it is not possible to distinguish with these data between a periodic recurrence of Nebraska droughts with a cycle length of about 22 yr and a recurrence in phase with the double sunspot cycle. On the other hand, we have no good reason to suspect any physical process of purely terrestrial origin that would produce a periodic fluctuation of High

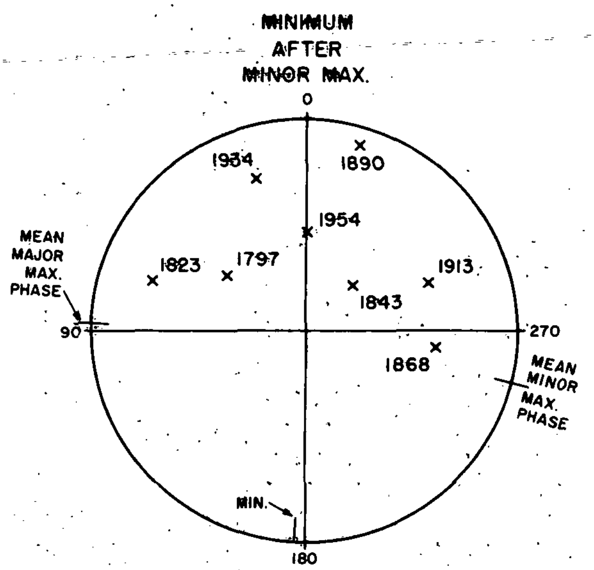

FIGURE 6.—Harmonic dial: western Nebraska droughts reported by Weakly versus double sunspot cycle, 1800 to 1970. Note significant clustering in the upper half of the dial, corresponding to a centering on the minimum following the negatively plotted sunspot cycles. This graph agrees approximately with figure 4, although the droughts lag slightly in phase compared to figure 4. Amplitudes correspond to drought duration. Drought dates are shown beside drought points.

Plains droughts with a 22-yr period. We do have, on the other hand, a valid a priori reason to look for the double sunspot cycle, namely our suspicion that some feature of the quasi-cyclical behavior of solar activity causes the drought.

Other High Plains parameters show a similar 22-yr recurrent behavior. For example, Thompson has reproduced July–August temperatures in the "corn belt" of the United States; these data show a warming trend in the same phase as the drought years (Thompson, 1973). I do not think, however, that it is worthwhile to spend any major effort to do additional statistical-empirical searches for connections to this drought region though I am sure there are many. What is far more important is to search for possible physical mechanisms to explain the apparent effect in terms of variable solar activity—and then to test candidate mechanisms against available observations.

I would like to make some additional points before leaving this discussion of the High Plains. First, it will be extremely interesting to see what happens in this region in the period 1974 to 1978. In recent years the double sunspot cycle has averaged a bit under 21 yr. A 21-yr recurrence would place the start of a High Plains drought right about 1973; none has occurred, and in fact the spring of 1973 was a growing season of abundant moisture. On the other hand, in August 1972 solar activity took a sharp spurt upward from its decline toward minimum, with a large outbreak of flares, sunspots and other active Sun phenomena, and substantial activity has continued until this writing (Oct. 1973). It looks, therefore, as if the solar activity minimum after the recent minor maximum may be delayed. If the drought is correspondingly delayed, this will be a strong boost to the hypothesis that the droughts are causally connected to solar activity.

Second, I want to comment on the earlier western Nebraska drought data of Weakly (1962) not analyzed by Thompson. Sunspot data are available back to the time of Galileo's discovery of the phenomenon around 1610, although reliable and regular sunspot measurements date only from about 1700. In figure 7 I have reproduced a harmonic dial like that of figure 6 for the period 1610 to 1800. I have assumed, in making this dial, that the double sunspot cycle alternated as it has in more recent times. This is not an entirely safe assumption because there are some indications that long-term phase anomalies in the spot cycle occur; and, of course, no spot magnetic field observations or other direct solar activity records exist for these earlier periods. The dial does not lend any very strong independent support to the hypothesis of a relationship of the double sunspot cycle to droughts in western Nebraska. It is not a clear negation of this hypothesis, however, because there is some clustering near and after the minimum that follows the minor maximum. Moreover the anomalously long 1698 drought, which was over 20 yr in duration, is the one latest in phase. Late phase relationships for center dates or long droughts also showed up in the more recent data, as was discussed earlier.

The data do not, however, show that a distinct drought accompanies every minima following the minor sunspot maxima, as was the case for the period from 1800 on. We are probably straining too hard, however, when we try to push both the

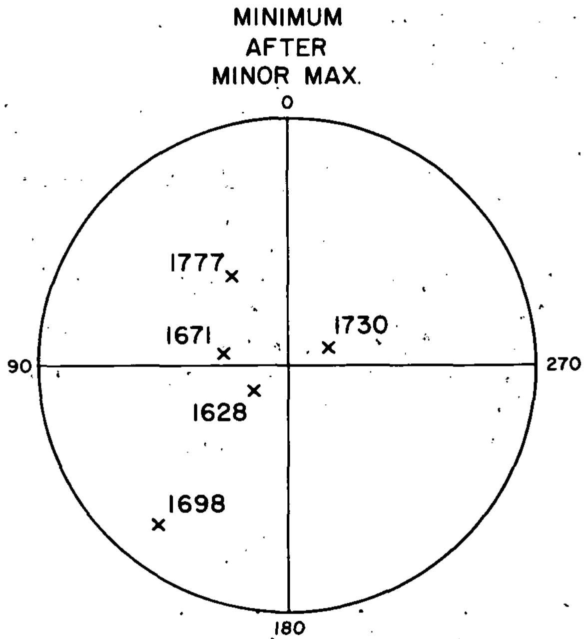

FIGURE 7.—Harmonic dial: western Nebraska droughts reported by Weakly versus double sunspot cycle, 1610 to 1800. Drought data for 1610 to 1800 plotted on same basis as figure 6, but with expanded amplitude scale. The 1698 drought, which is late in phase, was also very long (20 yr), necessitating the expanded amplitude scale compared to figure 6. Paucity of data leads to inconclusive results regarding double sunspot cycle association with droughts in this time period.

sunspot and drought data all the way back to the discovery of sunspots, especially in view of the fact that the distinction of major and minor maxima is not clear in these earlier periods. In any event, we cannot draw from these earlier data much evidence for or against the apparent High Plains drought relationship to solar activity that is so marked from 1800 on.

Finally, I would like to comment about what the climatological picture for a High Plains drought might be, in the hope that it will contribute to the search for a mechanism. My concept is perhaps too simple, and therefore I would be glad to have some more sophisticated experts shoot it down. My reasoning goes as follows. For a spring or summer drought to occur in the High Plains of the United States, it would seem to me reasonable that the large-scale circulation should have a persistent anomaly that would lessen the prospect for warm moist Gulf of Mexico air to penetrate northwestward to the lee of the Rockies where its contact with cold Canadian air thrusting southward results in precipitation.

A likely mechanism for this would, in my opinion, be a strengthening of the jetstream westerlies over the Colorado Rockies so that there would be a relatively warm, strong, dry, west wind on the lee side of the mountains. In this case, the Gulf air would be pushed appreciably farther east and its precipitation would occur perhaps 1000 km or so downwind from the Rockies, say from St. Louis eastward. On this assumption, one might search directly for a solar activity correlation in strong winds at the troposphere and at the surface and for a corresponding reordering of precipitation patterns eastward. If this were verified, it would focus attention on a strong westerly wind as a step in the explanation.

Reliable wind data for this region over any appreciable time span may be hard to come by. It is certainly true, within the memory of present-day farmers of the region, that the "dust bowl years" of the 1930's and the drought years of the 1950's were characterized by high surface winds, and no one contests that this greatly promoted soil erosion in spring and early summer. Weakly (1962) reports that in the extreme drought that ended in 1564, the trees in his test area of western Nebraska were buried in nearly 3 m of windblown soil. Even though long-term wind data are hard to acquire, it may be possible to find jetstream wind and rainfall associations with solar activity that are operative on a short time scale of perhaps week-to-week changes; such findings encourage us to surmise what would happen if the changes were persistent in one pattern or another over a season or a year or a series of years.

In fact, it was in hope of finding such a lever to understanding climate changes that I decided, many years ago, to look at short-term changes in the 300-mb circulation over the North Pacific and North America to see if they were connected to changes in the geomagnetic disturbance activity. The findings from that work appear generally to support the notion that low solar activity is a time of stronger and less meridionally perturbed westerlies, but it says nothing about the difference between the two minima of the double sunspot cycle. I suspect that it should be possible to look

more directly at the Rockies and the High Plains, and from data covering as few as 30 yr to produce differential 300-mb circulation maps for 2-week or 1-month periods characterized by different phases of the spot cycle and also characterized by differing aspects of other features of solar activity or geomagnetism. Such a study will be especially attractive a few years hence when we pass through the coming minimum of the double sunspot cycle because it is the one for which we have some empirical reasons to expect a High Plains drought to recur.

Solar Activity and Warm (Cold) Periods

There are numerous studies of solar activity indices and their possible relation to the occurrence of colder or warmer climates. These are summarized by H. H. Lamb (1972, p. 443) and I shall not go extensively into detail here. However, Lamb is of the opinion, in spite of the welter of complex and often confusing results, that warmer weather in most regions appears to have occurred significantly more often during the years of high solar activity. He quotes J. R. Bray (1968), one of the most active workers in the field, as believing that "75–80 percent of all known glacier advance events and other indicators of cold climate in late glacial and post-glacial time occurred during intervals of weak solar activity, and a similar percentage of glacier recession and warm climate indicators occurred with high solar activity." Bray's results cover a wide range of latitudes and data from both hemispheres.

There are, however, very great complexities in long-term temperature trends as related to solar activity. Work of Suess (1968), for example, illustrates this. Over very long periods, Suess determined solar activity from the cosmic ray production of natural radiocarbon deposited in wood samples of known age. His results show suggestive relationships with temperatures in some regions and periods, but very confusing results, and unlikely time lags in others. The story is obviously far from simple, and it is no wonder that results of this character have caused many workers to shy away from the field, believing the evidence of real solar-climatic relations insufficient to merit major research effort on their parts.

Pressure Pattern Differences Between Solar Activity Maximum and Minimum

Many investigators have sought sunspot-cycle-related features of regional or global pressure patterns and circulation systems. Wexler (1950) did a thorough study seeking mean surface pressure differences between maximum sunspot years and minimum for the northern hemisphere over a 40-yr period and confirmed an earlier finding of Clayton that high latitudes show higher average pressures at spot maxima than at minima. Wexler did not, however, consider the results conclusive. In today's context they appear more significant, perhaps, than he thought.

Willett (1965) did perhaps the most extensive modern study of the matter, using several indicators of solar activity, such as geomagnetic storm activity. He concluded that at high solar activity there is a mass displacement of air toward high latitudes, consistent with Wexler's and Clayton's findings. He also found abundant but complex evidences, especially in North American climate data, for the effects of the double sunspot cycle in temperatures, rainfall, and other phenomena. Abstracting his findings, Willett has said,

> . . . analysis of the double sunspot solar-climatic cycle indicates that this cycle is . . . pronounced in middle and high latitudes, particularly in the winter season. It is suggested that this cycle probably reflects a change of the transmissive properties of the atmosphere, i.e., a greenhouse effect, in such a manner as to sharpen or suppress the relative heat and cold sources of the continental-maritime monsoonal cells of the general circulation.

Willett suggested varying atmospheric ozone as the causative factor, a notion that has gained some support from recent work of Angell and Korshover (1973).

Schuurmans (1969) has carried out an interesting study of the relation of solar activity to the relative frequencies of different types of weather patterns over Western Europe. He used the "Grosswetterlagen" classification system introduced in 1952 by the German meteorologists Hess and Brezowsky in their *Katalog der Grosswetterlagen Europas*. In this system there are three principal types of circulation: Z = zonal, H = half meridional, and M = meridional or blocking. Schuurmans found that the meridional circulations

are most frequent and the zonal types least frequent during highest solar activity. The effect is strongest in winter and spring. Moreover, he finds that both the frequency and duration of the meridional or blocking circulation increases at maximum solar activity. He concludes as follows:

> Therefore we might say that increased solar activity, quite apart from having an influence on the development of meridional type circulations, strengthens the persistence (i.e., continuation tendency) of meridional or blocking type circulations, while on the other hand it interrupts spells of westerly zonal circulation, which are normally quite long.

There is much supporting evidence for these conclusions of Willett and Schuurmans, but this will not be discussed in this paper.

A FEW WORDS ABOUT MECHANISMS

Other parts of our symposium will deal with the search for mechanisms. My job was to lay out some evidences for the reality of effects in climate and to discuss these critically. However, I would like to say a few words about mechanisms.

My first comment stems from the work of Schuurmans (1969, p. 114) which suggests that the atmospheric reaction to solar activity (in his case, solar flares) shows a maximum at the tropopause and that it "is not propagated downwards from a higher level in the stratosphere but is initiated *in situ,* most likely through a cooling mechanism near the tropopause level." As he points out, if an effect originates near the 300-mb level, it can propagate downward, causing the circulation to become more meridional after a few days. It is not surprising, if such a mechanism is operative, that the magnitude of the reaction is, as Schuurmans and others have observed, dependent on the initial atmospheric conditions at the time of the solar activity intervention.

To me the most promising place to search for mechanisms operative at the tropopause is in modification of the atmospheric radiation budget through the sudden formation of cirrus clouds following solar activity. It seems reasonable to expect that a cirrus cloud could produce, near its level, either a heating or a cooling. As Olson and I (Roberts and Olson, 1973) have pointed out, for example, a reasonably solid cirrus deck overlying a relatively warm ocean surface during high latitude winter could easily lead to a heating of 1° C per day, enough to be dynamically significant.

What evidence is there to suggest that solar activity could produce such cirrus? The evidence is slender, but not totally lacking. A. von Humboldt back in 1845 called attention to an apparent connection between the polar aurora and subsequent cirrus clouds in a paper now mainly of historical interest. More recently, Danvillier (1954) wrote:

> It is invariably found that after the phosphorescent final stage of an auroral storm the sky rapidly loses its limpidity and that it becomes covered with a light veil of cirrostratus giving rise to lunar halos.

Dauvillier also states the following:

> Tromholt found that observations at Godthaab from 1857–1873 showed a strong correlation between the number of halos observed and the number of aurorae. At dawn the sky is seen to be full of cirrus. These clouds always follow auroral display.

I have some personal doubts about the "invariably" and the "always," but perhaps these questions should be reexamined by modern techniques. I find this particularly so in the light of the provocative but very short-term study of Barber (1955) that suggested a light-scattering layer over England following magnetic storms. I am also impelled in the same direction by the analysis by Vassy (1956) of Danjon's analyses of the shadow of Earth on the eclipsed Moon that led Vassy to conclude that there is an increase in light-scattering aerosols in Earth's atmosphere during periods of strong solar corpuscular emission associated with high solar activity and strong auroras. Finally, there is the work of Tilton (1934), based on a long series of observations beginning in 1844, purporting to show a change in atmospheric refractive index as a function of solar activity.

Perhaps satellite IR data will give us an opportunity to settle definitively, in a few years, the existence of this kind of a solar-modulated IR budget from high terrestrial latitudes that might account for the climate phenomena that apparently display a measure of control by variable solar activity. Be this as it may, there is sufficient evidence, in the light of our compelling need to understand and predict climate change, to justify

greatly enhanced research attention, by scrupulously critical workers, to study of the effects of variable solar activity on climate phenomena.

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DISCUSSION

ROBERTS: The question was could I give some numbers about the disparity in energy from the Sun and the energy required, through a brute-force mechanism, to produce some kind of circulation change in the lower atmosphere. All right. I hope there will be some further discussion of this later because I know some people here have done some new calculations on this. But the work that I did, of a very qualitative sort, some years ago, shows that while the solar constant produces a flux into the top of the atmosphere in a direction normal to the direction of the Sun of the order of 10^6 ergs/cm^2 · s, the features of variable solar activity precipitate into the atmosphere something only of the order of the few ergs, or a few tens of ergs, occasionally maybe as high as 1000 ergs/cm^2 · s.

It is very difficult—at least, for a nonmeteorologist like me—to calculate the amount of energy that would be required in a brute-force way to produce, for example, a substantial trough amplification over one of the large-scale planetary wave types of circumstances or a large blocking high like those that produce themselves in the winter season in certain regions of Earth. But it looks to me as if the energy required to do this in some brute-force way is of the order of 10^4 ergs or greater.

QUESTION: You mentioned the drought and you seemed to emphasize spring and summer in that discussion, but later you seemed to think that the place to look is in the winter. Now, in those drought years, is there a variation with seasons?

ROBERTS: I think I mentioned late winter and spring, for the most part, but I am not sure. In any event, in this region, from the Rockies to about 1000 km to the east and from South Dakota down into the Texas Panhandle, the drought appears to be well established in the early spring or late part of the winter, and to continue right on into the summer. So if you take whole growing season integrated drought data, or if you take data for the period March, April, and May, you get about the same results. This has been done month by month by Marshall, for example.

RASOOL: One thing that bothers me in these correlations between solar activity and the phenomena on Earth is why should we take only the local phenomena, as in Kansas. If anything is going to happen because of the solar activity, it should be planetwide, so why not take the measurements from all over the globe?

ROBERTS: First of all, there is very abundant literature on searches for drought in various latitudinal and longitudinal regions all the way from, for example, looking at something like the level of Lake Victoria as an

isolated instance in Africa. I chose to pick just a few selected instances and it seems to me very, very clear that the influence on climate is one that has a very regional character.

It seems clear to me, for example, that if there is any systematic change in the wavelength of the Rossby waves as a function of solar activity cycles, then in some regions it will produce drought and in other regions it may produce increased rainfall. Therefore, it seems important to concentrate your studies in a particular region that has some particular relationship to these circulation features. But I could have picked regions showing different kinds of relationships. This seems to me to be the one that is most clear cut and the most pronounced.