

# Can solar variations explain variations in the Earth's climate?

## An editorial comment

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There is a huge literature on the claimed correlation between solar variations (and cosmic rays) and climate variability on Earth. I wrote major critical reviews of this subject in 1978 and 1983 (Pittock 1978, 1983), both of which found numerous errors and omissions in the literature and suggested some simple ground rules for future work that might improve the standard.

Common problems included poor data quality (especially poor dating of non-instrumental data), data selection, data smoothing and autocorrelations, and post-hoc elaboration of hypotheses to explain discrepancies. The latter often introduce additional degrees of freedom and thus require higher correlation coefficients for statistical significance. For example, Xanthakis (1973) claimed correlations of rainfall with solar activity at various locations. He found conflicting signs of the correlations at different times and places, and postulated that this was due to a correlation of rainfall sometimes with an index of solar activity,  $I$ , and sometimes with a constant minus  $I$ , with the reason for this peculiar behaviour not stated (see subsequent discussion by Pittock 1979 and Xanthakis 1979).

My 1978 review concentrated in detail on claimed correlations between the 11-year and 22-year sunspot cycles and tropospheric or surface weather patterns. After reviewing more than 100 papers I came to the conclusion that: "... little convincing evidence has yet been produced for real correlations between sunspot cycles and the weather/climate on the 11- and 22-year time scales, although evidence for correlation with solar events on time scales of days appears to exist."

I went on to say "the evidence to hand suggests that if in the future more data and better analysis enable the detection of statistically significant relationships, these will account for so little of the total variance in the climatic record as to be of little practical value". I concluded that "The state of the literature in this particularly controversial area must raise doubts as to the prevailing standards of objectivity and critical analysis in other areas of science as well. Clearly, in the case of sun-weather

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relationships, further research requires much higher standards of objectivity, with the rigorous and critical application of statistics, and step by step investigations of hypothetical mechanisms.” My suggested ground rules were taken up in a review by Brian Tinsley in his appendix to a NOAA *Special Report on Solar Influences on Global Change* in 1992 (NOAA 1992).

Although I have not personally followed the more recent literature in detail, more recent reviews, notably by the Intergovernmental Panel on Climate Change, which focussed on longer-term relationships, suggest that regrettably not much has changed (see for example Burroughs 1992; IPCC 2007; Duffy et al. 2009; Pierce and Adams 2009; but also Prikryl et al. 2009).

My 1983 paper examined another 100 papers published since 1980. It noted a bit more emphasis on mechanisms, and looked in addition at discussions of possible longer-term data connections between solar variability and climate.

This latter review found some evidence that solar ultraviolet variations may affect middle stratospheric temperatures and ozone content and tropospheric planetary wave energy. However it stated that these are complex phenomena which had only been modelled in very simplistic terms. It noted that satellite observations of the magnitude of solar variability placed firmer limits on the theories and mechanisms available to explain any relationships with the weather. It also suggested that the approximately 20-year cycle in some surface climatic variations might be related both to the 22-year solar cycle and the lunar synodical cycle of period 18.6 years, but noted that mechanisms were still not established. The review further noted that on timescales shorter and longer than two decades the balance of evidence was swinging against significant effects of solar variability.

My review concluded with a philosophical discussion of the subject as one that is ‘trans-scientific’ in the sense elaborated by Weinberg (1972). “Science is at the one time both a purely intellectual quest for truth and understanding, and also the necessary prerequisite for practical applications and decision-making. Many questions can be asked of scientists, often with very practical applications, but which cannot be answered with the degree of certainty with which they would state a ‘scientific fact’ or regard a theory as in some real sense ‘proven’.” In Weinberg’s terms such questions are not scientific in that they cannot be answered with certainty due to inadequate data—they are therefore beyond science and can only be answered subjectively in terms of likelihoods or value judgements.

A more important such case, in my view, is human-induced climate change. We cannot dogmatically and conclusively state what will happen in the next 50 or 100 years, but we can look at the limited data and theories, including modelling results, and estimate the probabilities and associated risks.

Thus we have, in the two cases of claimed solar–weather relationships and human-induced climate change, estimated probabilities that the theories are true, with various value-laden consequences. Risk is the product of probability times possible outcome. In the case of a connection between solar variations and weather, there is a probability of a connection, and an outcome which is the potential value of using such a connection to make predictions of weather or climate. The value of this connection is related to the percentage of variance explained by the solar–weather connection. If that is large, we might accept as potentially useful a lower probability that the connection is real, but if the variance explained is small, the value of using the theory to predict the weather would also be small.

In the case of the theory of human-induced climate change, on the other hand, while there may be only a limited probability of disastrous consequences, the large magnitude of those consequences makes the risk large. Thus we might well decide that we should take a precautionary attitude and act as if the theory were true. This is especially so if we can act on the theory without bringing about other dire consequences. Action thus depends not only on the predicted consequences if the theory is true, but on the cost of such action.

Given the limited potential value of solar variations as a partial explanation for climatic variations on Earth, why are such explanations so popular? At the risk of some repetition, I will now look at several reasons and discuss their possible validity.

1. *The first appears to be axiomatic: the Earth's climate is driven essentially by the Sun's energy output, so if that varies, ipso facto, the Earth's climate must vary accordingly.*

The trouble with this argument is that the Sun's energy output varies very little (some  $0.12 \text{ W m}^{-2}$  from the Maunder Minimum in the 1750s until now, according to the IPCC 2007). On the other hand the Earth's surface climate has varied a lot over geological time, and even the change in radiative forcing from anthropogenic gases since 1750 is estimated at about  $1.6 \text{ W m}^{-2}$ . Natural climatic variability is due to all sorts of other drivers such as variations in the Earth's orbit around the Sun, volcanic eruptions that put clouds of particles into the upper atmosphere, continental drift, and internal fluctuations such as natural quasi-cyclic events like El Niño, and positive feedback processes that amplify small variations.

It is therefore the relative magnitude of these various climate drivers and feedbacks that is critical as to whether solar fluctuations are important at the Earth's surface. Well established mechanisms are observable on short time-scales in the upper atmosphere where solar variations demonstrably drive changes in the ionosphere and even in the stratosphere, but these rapidly diminish with magnitude as we descend to the surface. In order to credibly claim that solar variations have noticeable effects at the surface, relevant positive feedback effects (which might magnify solar energy driven variations) must be postulated and proven via observations of intermediate effects and quantified energetics. Many have been suggested, but few if any have been well established (for recent examples see the arguments and references in Duffy et al. 2009; Pierce and Adams 2009; and Prikryl et al. 2009).

2. *Solar variations provide ready explanations of otherwise poorly understood variations in climate observations or data.*

This is a tempting way to round off an otherwise quite valid set of observations with an interesting hypothesis. But is it justified or is it pure speculation? Here again, to be convincing a linking physical explanation or mechanism is needed, with quantification of the eventual effect. Otherwise, if it is published, it becomes a precedent in the literature which can be cited to justify another similar hypothesis grabbed out of the air. One unjustified hypothesis can easily lead to another being built on the first. In my experience this is common in the solar–weather relations literature.

3. *It is tempting to justify one's disciplinary interest or specialty, e.g., solar or ionospheric physics, by appealing to a hypothesis that suggests that one's discipline*

*has practical application for weather forecasting. It may help get funding, and it might indeed prove to be valuable.*

Such a reason is tempting but it is not rigorous science until mechanisms are established. But it can be better justified if others before you have made the same claim. What if there is something in it? Maybe it is worth pursuing just in case, but until there is convincing evidence one really ought to emphasise the speculative nature of the link. Several people making the same speculation does not make it correct.

William Burroughs (1992), in his book *Weather Cycles Real of Imaginary?*, discusses the question of the value of cycles in the weather as an aid in weather or climate forecasting. He states that these goals “more than justify the growing efforts to identify semblances of order in our climate.” However, he goes on to say “But this progress will be scant consolation to those enthusiasts who over the years confidently predicted that reliable patterns had been found and would enable forecasts to be made years or even decades in advance. In truth, almost everything they have studied so closely over the last few centuries is little more than the random noise of an immensely complicated physical system—full of sound and fury, signifying nothing.”

4. *Solar influences might be suggested as an alternative explanation of climatic change because that might undermine some unwelcome hypothesis such as that recent climate variations are due to human emissions of greenhouse gases.*

This is one reason why the IPCC has had to devote a lot of effort to discussing solar variability. But then, the advocates of solar causation just might be right, so it must be taken seriously, as the IPCC has. The real problem here is that some self-styled sceptics use such suggested solar variability explanations, which are poorly established, as reason to dismiss, and to not act on the much better-established alternative. Scepticism needs to be even-handed. If it is only applied to one side of an argument it is not objective and can be downright misleading.

This is where rigour can help. Methodology and the quality of the data must be critically assessed, and statistics taken seriously. Could an eye-balled correlation be explained by chance? In order to deny that possibility we need to be sure of the dating and quality of the data, and understand any auto-correlations or effects of data smoothing that might affect the statistical significance of the result. Do we have a hypothesis that links the solar variations to the hypothesised climatic effect? And if so, can we document that it is operating?

In my experience any cyclic behaviour is unlikely to be established as statistically reliable, say at the 95% probability level, unless there are at least four or five complete cycles of well-dated data. That is especially difficult to come by with proxy data for cycles having periods of hundreds of years (Wu 2009).

It behoves authors, referees, editors and readers to bear these considerations in mind when dealing with claims in an area of science that has been hotly debated for decades. How much influence do solar variations have on surface weather and climate? And does the present claim, whatever it may be, bear close examination?

Quite early in my career as a climate change scientist it was seriously put to me that as I was a well-known sceptic re solar–weather connections, naturally I would be sceptical of human-induced climate change. I thought I was, but the growing body of evidence convinced me that my scepticism was unjustified and that I needed to get into the details to make this knowledge useful. Maybe that will happen in due

course with solar–weather/climate variations, but I doubt such relationships will ever explain a useful amount of the variance. If it did, the correlation would be so large that it would already be well established, mechanism and all.

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