The sun plays only a very minor role

Before discussing any detail, there are a few general points that I do feel need to be made. Please forgive me if writing these down appears patronising for, indeed, these points are well known and I have never seen them contested in any rational argument - which makes it all the more surprising (and alarming) how often one or more of the following fundamental considerations is overlooked.

1. The difference between global mean climate and regional-and-seasonal climates is significant. Both data and models show that there are many locations and seasons for which there can be sustained trends that are quite different from the trend in the global mean whatever the cause in the latter. Thus taking temperature data or proxies from restricted locations and a certain time of year (or with a seasonal bias) can be misleading.
2. Following on from point (1), it becomes entirely possible for there to exist genuine solar signals in regional and seasonal climates that do not have any significance for the behaviour of global average climate.
3. Detection of trends and cycles requires long data sequences (such that the effect of the trend exceeds the noise or several cycles are detected: how many cycles depends on the application of a detection of seemingly cyclic behaviour).
4. Our data series on both climate and the Sun are not homogeneous, particularly the longer ones. Instrument location (or distribution of locations for networks), sensitivity, measurement technique, calibration, noise levels, time resolution all change over time. Too often a composite time series is used because it is the best available, but the effects of its limitations are rarely investigated.
5. Significance tests of apparent correlations and relationships are absolutely vital – and they must be done properly and thoroughly. Because natural internal variability of the climate system causes noise in any data series, coincidences do and will occur and the onus is on anybody proposing any connection to provide evidence that the relationship did not occur by chance. This requires comparison against a suitable noise model to show that the probability that it was not a chance occurrence was low. In precise laboratory sciences like high energy physics they generally work to the “3-sigma” level for which this probability must be below 0.3%. For areas like geophysics, where it is not generally possible to separate variables, and natural variability always gives noise, a strong result is at the “2-sigma” level for which this probability is below 5.5%. Many reported connections do not even pass at the “1-sigma” level for which the probability limit is 32%. Multivariate fits can often look convincing, but drilling down can reveal that it has been arrived at by an unphysical or unlikely combination of the input parameters. This is referred to as “over fitting” and features that are, in reality, due to the internal variability of the climate system can be incorrectly fitted by a feature, or combination of features, in the inputs to the fit. Proper significance tests to check this has not occurred must allow for the length of the data series, the persistence in both data series and the numbers of degrees of freedom employed in making in the fit. How data gaps are handled is also important and checks are needed that they have not had an effect. But even when a significance test is passed (at a certain level), the connection may still be a “selection effect”, whereby it works with one data series but others that do not work are dismissed with hand-waving arguments or, more commonly, just ignored. Science has to fit all the known facts, so such selective reporting is, quite simply, bad science.

6. Solar radiative forcing F_{RS} and change in total solar irradiance dS are different things and the difference needs to be clear when putting TSI changes into context. The two are related by $F_{RS} = dS * (1-A)/4$ where A is Earth's albedo and the factor 4 arises because TSI is power per unit area of the disc presented to the Sun by the Earth and radiative forcing in per unit surface area of Earth. The albedo is not known well (let alone its variation with time) and is impossible to measure properly because every point of the Earth's surface/atmosphere reflects and scatters the incoming sunlight incident upon it in every direction back into space. Our best estimate of A is somewhere near 1/3, which means $F_{RS} \approx dS/6$. The largest estimates of dS since 1750 are around 6 Wm^{-2} [Shapiro et al., 2011] which therefore corresponds to $F_{RS} \approx 1$ Wm^{-2} . However, most recent estimates of dS are in the range 0.6-0.9 Wm^{-2} which corresponds to F_{RS} between about 0.1 and 0.15 Wm^{-2}. In contrast the radiative forcing caused by changes in well-mixed greenhouse gasses over the same interval is relatively well known at 3.2 ± 1.2 Wm^{-2}. Thus the largest estimate in TSI change is about 30% of the known greenhouse radiative forcing, the mean of a basket of recent estimates is about 4%.
7. Logic based on the name given to a phenomenon, interval or feature is bad science, because the name is often inadequate and misleading.

Having made those general points, let me go on to discuss a couple of specifics in relation to solar influences on global and regional climates.

TSI changes and their effects

The one fact that everyone can agree upon is that, if there were sufficiently large changes in Total Solar Irradiance (TSI), then there would be changes in the global terrestrial climate. The problem is that changes in TSI are limited by the massive thermal time-constant of the solar convection zone. The question is how limited are they on centennial timescales?

Until towards the end of solar cycle 23, our TSI observations only covered 3 solar cycles that were rather similar in amplitude and were all part of a grand solar maximum [*Lockwood et al.,* 2009]: this meant there were great uncertainties in what the TSI level would be when solar activity was weaker. Subsequently, solar activity has declined rapidly, through the so-called "exceptional" (it is only exceptional for the space age) solar minimum 23-24 and the weak current solar cycle 24. For the first time, this has given us some dynamic range in the data and starts to constrain the centennial TSI reconstructions.

I will here use the composite of TSI measurements constructed by WRC/PMOD [*Fröhlich*, 2000]. Before continuing, I must acknowledge that this is not the only such composite. The main difference between the various composites arises from a difference in philosophy. Because absolute radiometry from space is an extremely difficult measurement to make [*Fröhlich*, 2003], the PMOD composite is based on the concept that continuous corrections and adjustments are required. An alternative philosophy, adopted in particular in the construction of the ACRIM composite [*Willson*, 1997] , is to trust the instruments to remain constant in calibration and sensitivity. This is clearly dangerous but, to be fair to the ACRIM composite, there is also a danger that with too many and/or invalid adjustments, the trend in any data sequence could be made into anything one wanted.

That having been said, the vast majority of the difference between ACRIM and PMOD comes down to just one event: a known pointing direction glitch by the oldest satellite deployed (Nimbus7) which has to be re-used in an interval known as the "ACRIM gap" after the loss of the ACRIM1 instrument on SMM and the start of operation of ACRIM2 on the UARS satellite [*Lockwood and Fröhlich*, 2008]. Despite some published claims to the contrary, modelling of TSI using data from solar magnetographs strongly supports the PMOD composite [*Krivova et al.,* 2009] and only the PMOD composite has a consistent variation with other solar activity indicators such as sunspot number and cosmic ray fluxes [*Lockwood and Fröhlich*, 2008].

Hence I regard the PMOD philosophy of constant checking and re-calibration of data as the right one to adopt and I regard the PMOD composite as by far the best and so employ it here.

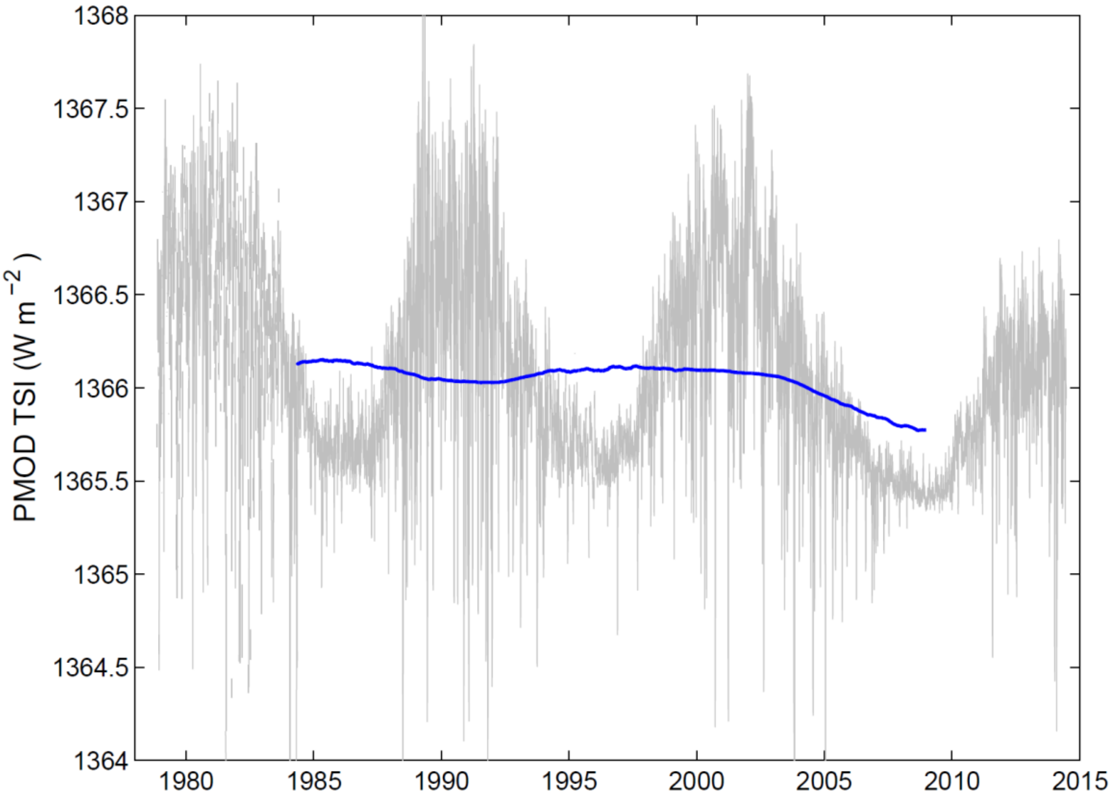

Figure 1: The PMOD composite of total solar irradiance (TSI). Daily values are shown in grey, 11-year running means in blue. Data were downloaded from the WRC/PMOD website on 6th September 2014 (http://www.pmodwrc.ch/pmod.php?topic=tsi/composite/SolarConstant).

Figure 1 shows daily values of TSI from the PMOD composite (in grey) along with their 11-year running ("boxcar") means (in blue). The last available value is for the 11-year period centred on decimal year 2009.03 and is 1365.77 Wm^{-2}, which is 0.38 Wm^{-2} lower than the peak running mean value of 1366.15 Wm^{-2} which was observed at decimal year 1979.9.

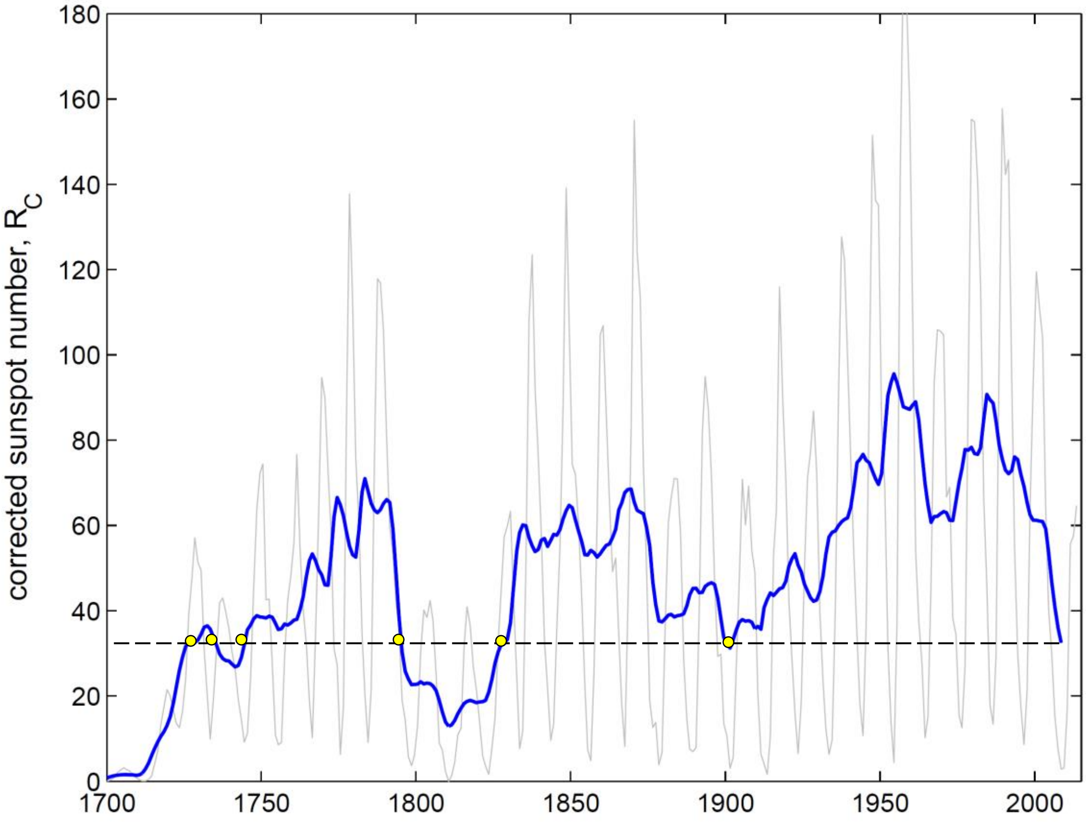

Figure 2: Corrected sunspot number, R_C. This is a composite of international, Zurich, Wolf and group sunspot numbers with implementations of corrections as discussed by Lockwood et al. [2014]. The grey shows annual means, the blue 11-year running means. The horizontal dashed line shows the last available value of these 11-year means (for 2002-2013, inclusive) and yellow dots show when average sunspot activity was at this level in the past. The Data available from *http://onlinelibrary.wiley.com/store/10.1002/2014JA019972/asset/supinfo/JGR_Lockwood_OSF2_supplementarydata.txt?v=1&s=9889b7655c22cc249a9f6e2f43aa76305125cbdd*

Because TSI is undoubtedly related to sunspot number, it is instructive to do the same analysis for sunspot numbers. Figure 2 shows annual means (in grey) of extended and corrected sunspot number, R_C. This sequence is very similar to others and its precise derivation is discussed in *Lockwood et al.* [2014]. The blue line shows the 11-year running means of these sunspot data. The value for 2009 is 32.4, and the last time we saw such low average levels of sunspot number was 1900. This value was also seen in the fall into, and recovery out of, the Dalton minimum and also applies around 1750.

In general, we cannot say that TSI is a function of sunspot number only; however, that is an assumption (implicit or explicit) we are forced to make if we use sunspot numbers alone to reconstruct TSI back to the Maunder minimum. This being the case, we would expect the average TSI around 1900 and around 1750 to be very similar to the 2009 value, i.e. 1365.77 Wm^{-2}, which gives a change in TSI between 1750 and 1980 (the peak of the running-mean of the TSI observations) of (1366.15 –1365.77) = 0.38 Wm^{-2}.

We can compare this expectation to the reconstructions. The largest drift in TSI is the *Shapiro et al.* [2011] reconstruction for which the average TSI around 1900 is 1362.5 $\mathrm{Wm^{-2}}$, i.e. the change in TSI between 1900 and 1980 is predicted to be $(1366.15 - 1362.5) = 3.65\ \mathrm{Wm^{-2}}$. Hence comparison of figures 1 and 2 indicates that the long-term drift in the Shapiro [2011] reconstruction since 1900 is overestimated by a factor of almost 10.

Looking at 1750, the difference is not quite so great: the *Shapiro et al.* reconstruction gives around 1364. 5 $\mathrm{Wm^{-2}}$ which is 1.3 $\mathrm{Wm^{-2}}$ lower than we would estimate from the recent TSI data. Hence the *Shapiro et al.* reconstruction appears to overestimate both the long term trend in TSI after 1900 and the fluctuation level before 1900. The value of TSI for 1750 and 1900 of 1365.77 $\mathrm{Wm^{-2}}$ inferred here does agree very well with many other recent reconstructions of TSI [for example, *Wang et al*., 2005; *Foukal et al*., 2006; *Krivova et al*., 2011; *Schrijver et al.,* 2011].

Referring back to general point 6, the change in TSI between 1750 and 1980 of $dS = 0.38\ \mathrm{Wm^{-2}}$ is a radiative forcing of $F_{RS} \approx 0.06\ \mathrm{Wm^{-2}}$. Hence the recent downturn in solar activity is giving us direct evidence that the contribution of TSI is small, of order a few percent of the effect of well mixed greenhouse gases. This agrees with modelling studies that have predicted that even a return to Maunder minimum TSI values would only give a minor slowing of greenhouse gas driven warming [*Feulner and Rahmstorf* , 2010; *Jones et al.,* 2012]. The inferred radiative forcing of $F_{RS} \approx 0.06\ \mathrm{Wm^{-2}}$ is very similar to the IPCC estimate and the main change to the IPCC consensus values that I would suggest is needed is that the level of confidence in the value should, in the light of the recent data, be changed from “medium” to “high”.

There are many other observations consistent with the above conclusions because the nature of the warming is not consistent with a rise in TSI. For example, a TSI rise would mean that the stratosphere, like the troposphere below it, would have warmed. This is not what is observed: the stratosphere has cooled which is a characteristic signature of enhanced greenhouse trapping in the troposphere. In addition, a rise in TSI would give a rise in average day-night temperature differences which has not been observed and summer temperatures would have risen more than winter ones – in fact the opposite is true and winter temperatures have risen considerably more than summer ones.

Multiple Regression fits to Recent Decades

Thus latest evidence strongly points to some role of TSI variability on global climate change, but only a very minor one. The question then one asks is: what about other sun-climate interaction possibilities? The most well-known of these is cosmic-ray modulation of cloud cover, but some others are mentioned in the next section.

I have long been a supporter (and actually a member) of the CLOUD experiment at CERN: not because I think it will revolutionise our view of radiative climate forcing, but because I am sure it will make really valuable contributions to our understanding of cloud microphysics. I do think there are some really interesting indications of solar influences (via cosmic rays and other energetic particles) on atmospheric electricity phenomena and consequent effects on cloud edges and polar tropospheric pressures. This is a vibrant area of research and I will do it no favours by making a hurried review of it here. However, I do have reasons for thinking that the net effect on the terrestrial global radiation budget will be small, and I discuss those reasons here.

Multiple regression fits of global mean temperature have been made using known inputs into the atmosphere at earth's surface. Similarly global multiple regression maps of the air surface temperature have been generated, revealing influences on regional climates. These have been carried out using a number of different procedures and datasets [*Lockwood*, 2008; *Lean and Rind*, 2008; *Folland et al*., 2013 ; *Kaufmann et al.*, 2011], but the conclusions are always very similar [*Imbers et al*., 2013] that contributions to global mean air surface temperature changes are in very close proportion to the estimated radiative forcings (see figure 3).

It is important to bear in mind that these are multiple regression fits and so can lack statistical significance for the reasons presented in general point 5 – in particular they are prone to "over fitting". Although tests had been applied in the above studies, the most rigorous testing was carried out by *Imbers et al.* [2013]. These authors showed that the key conclusions of these fits were robust to the inclusion of additional signals and also to the characterization of internal variability using both a short memory AR1 ("auto-regressive 1") process or a long memory FD ("fractionally differenced process" or "Fractional ARIMA model") noise models. In addition, these authors extended the sensitivity analysis to show that 20- and 60-year oscillations that have been proposed to explain recent apparent changes in rates of warming [e.g. *Loehle and Scafetta*, 2011] do not change the conclusions.

The key point is that these multiple regression detection/attribution fits all show that the influences are broadly proportional to their estimated radiative forcings, in the case of solar variability that is estimated from TSI observations (as discussed above), which leaves little room for any other solar factor or mechanism. Furthermore, any proposed mechanism must explain all – and I stress all - the data, not just the global means air surface temperature: such constraints include the latitudinal profile (why the Arctic has warmed most), coherent longitudinal variations, the altitude profile (the cooling in the stratosphere), the seasonality (why the warming is greater in winter), the lack of a diurnal variation increase. All these features are well explained by the observed rise in well-mixed greenhouse gases and so to be considered a serious alternative, any proposed mechanism must also explain all these observations.

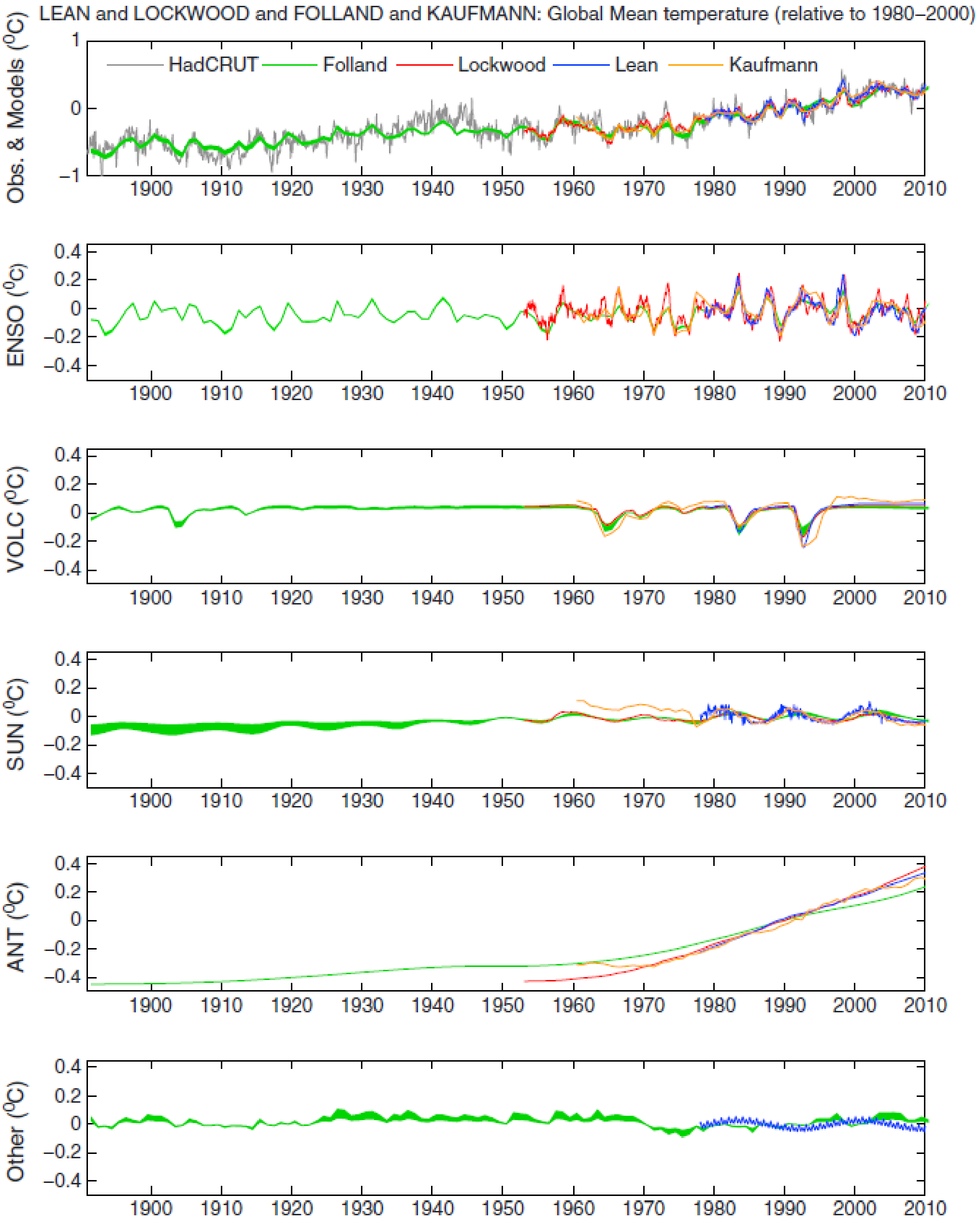

Figure 3: (top) The observed global mean air surface temperature anomaly fromHadCRUT3 (in grey) line) and the best multivariate fits using the methods of Lean and Rind [2008] (blue), Lockwood [2008] (red), Folland et al. [2011] (green), and Kaufmann et al. [2011] (orange). Note that in the case of Folland et al. data are plotted with 94.4% confidence interval based on the ensemble standard deviation. The remaining panels show the individual temperature contributions from multiple regression fits to the data in the top panel : from ENSO (second panel); volcanoes (third panel); solar variability (fourth panel); anthropogenic contribution (fifth panel); and other factors (sixth panel) that include the AMO for Folland et al. [2011] and minor annual, semi-annual, and 17.5 year cycle identified in the residuals of Lean and Rind's [2008] model. In the case of Folland's data, the figure shows the mean ˙1 standard deviation of the 120 time series ensemble. For the Lockwood [2008] fits, solar variability was quantified using cosmic ray flux measurements and each input was convolved with a response time-constant which was a fit variable in each case. From Imbers et al. [2013].

Solar Influences on Regional/Seasonal Climates

This is an area of some academic debate, but there is a growing body of evidence that there are effects of solar changes on some regional and seasonal climates [see reviews by Gray et al., 2010, Lockwood et al., 2012]. The mechanisms largely involve modulation of the jet stream (particularly in the northern hemisphere which is different from that in the south because it passes over much more complex ground orography than the southern jet stream which is largely over ocean).

The jet stream straddles the boundary between the troposphere, responsible for weather and climate, and the much less dense stratosphere above it. The modulation appears to be driven by changes in the stratosphere caused by solar UV irradiance changes and/or by the catalytic destruction of high-latitude stratospheric ozone by energetic particle fluxes that are modulated by solar activity. These effects matter much more in winter when the driver of the jet stream, the equator-to-pole gradient in lower stratospheric temperatures is much greater.

Both data and models indicate a “quadrupole pattern” whereby lower solar activity drives lower winter temperatures in central and northern Eurasia and the central and northern USA, but higher winter temperatures around the Mediterranean and over Greenland and northern Canada. These ideas are increasingly supported by numerical models that extend up into the stratosphere and so can capture stratospheric dynamics and temperatures [e.g. *Ineson et al.,* 2011]. Thus far models have no self-consistent treatment of the ozone abundance and this is likely to yield an amplification of the predicted effect. Models that do more than employ “fixed ozone” by including the chemistry and the dynamics will be needed to evaluate the relative effects of UV and energetic particles.

An example of results for the winter North Atlantic, and which can be associated with solar modulation of jet stream “blocking events” is shown in figure 4. Therefore, in global average temperatures these tend to cancel out and there’s no evidence that they don’t cancel out completely. However this effect is still relevant to long-term analysis of global climate change because the distribution of observations is not homogeneous and has changed over time.

In particular as we go back in time, observations become increasingly dominated by central Europe. Many pieces of evidence for solar influence on climate relate to observations made in Europe in winter from before the mid nineteenth century. Treating this evidence as indicative of global temperature variations is therefore a major error. The situation is better with some paleoclimate data when one has a wide variety of locations from what to take observations, such as from tree ring data. However note these data are often biased to the growing season, i.e. summer.

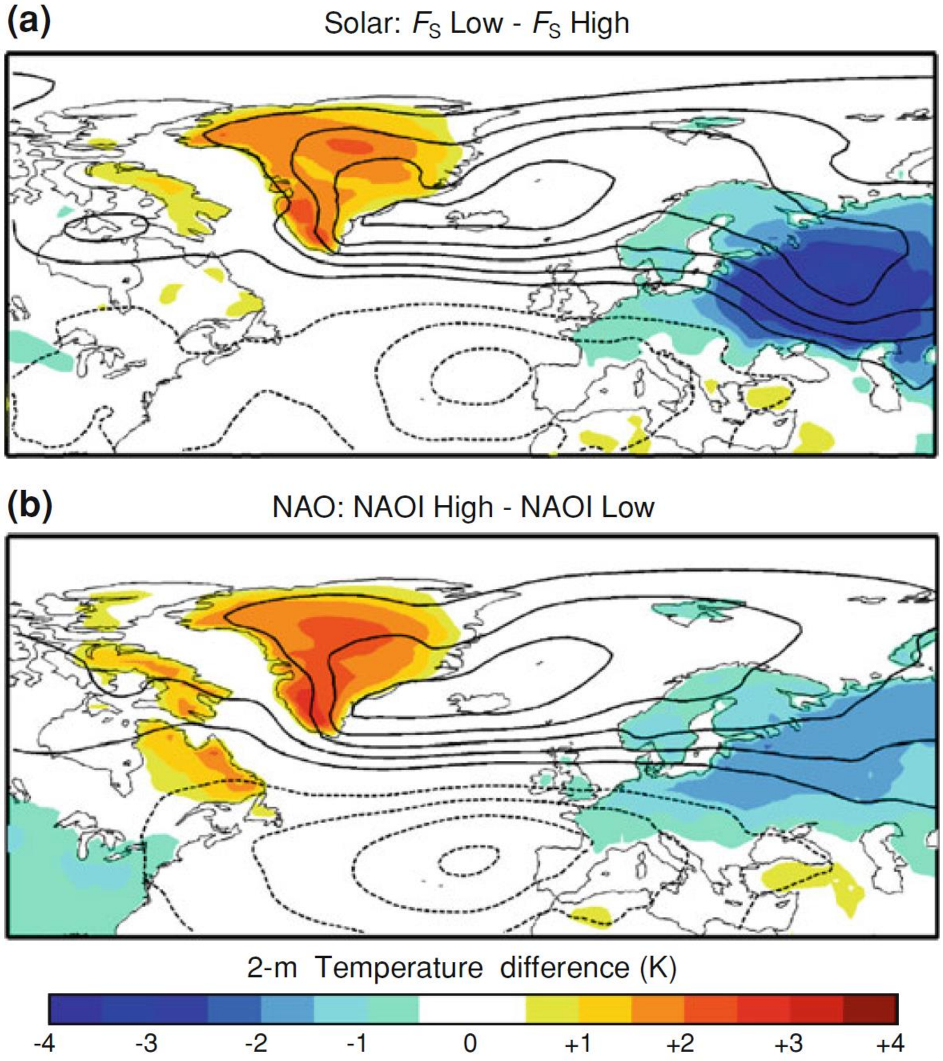

Figure 4: (a). Composite difference maps of winter (December/January/February) means of mean surface level pressure (MSLP, contours 1 hPa apart) and 2-m temperature (colour map) between low- and high-solar conditions defined by the lower and upper terciles of the open solar flux (F_S). (b). Scaled anomalies associated with the NAO for comparison, in this case the two subsets are for the lower and upper terciles of the NAO index. Data are from the European Centre for Medium-range Weather Forecasting ERA-40 reanalysis data set (Uppala et al. 2005) extended to cover 44 complete winters (1957/1958 to 2000/2001) using operational data. A bootstrap test was employed, resampling the sets of winters in order to estimate the sampling uncertainty, to test whether the solar pattern in a was different from that associated with the NAO in b: this found that the two patterns are significantly different at the 93% level [from Woollings et al. 2010].

The “Little Ice Age”

I dislike this name as it has been used to build arguments that rely on the name which, as mentioned in point 7, is inherently bad science. Many paleoclimate records show an interval of lower temperatures but the start and end dates are not well agreed. For example it has been claimed to correspond to the Maunder minimum (i.e. 1645-1715) , such that in many arguments the terms “Maunder minimum” and “Little Ice Age” are even taken to be synonymous! This is undoubtedly invalid as most paleoclimate records indicate that the lower temperatures began at least 50 years before the start of the Maunder minimum.

But I also dislike the name as it is inherently misleading: this period was not an ice age in any shape or form. For example, the Central England Temperature (CET) is the longest series of temperature measurements in existence, starting in 1659 and continuing to the present day. The CET data for the Maunder minimum show that there was a higher occurrence frequency of cold winters in this interval. However, the term “little ice age” implies that they were unremittingly cold. This is not the case. For example, the coldest winter in the CET record is 1683-4, right in the middle of the Maunder minimum; but the winter just two years later was the fifth hottest in the whole 350-year CET record. Furthermore, there are many examples of hot summers during the Maunder minimum. Hence, in central England at least, it was not unremittingly cold – far from it!

Biosketch

Prof. Mike Lockwood is a professor of space environment physics with the Department of Meteorology, University of Reading, UK. He has received five international academic awards and in 2006 was elected as a Fellow of the Royal Society of London.

Prof. Lockwood studies variations in the Sun on all timescales up to millennia and their effects on near-Earth space, Earth's atmosphere and climate. His quantification of the variation in the open solar magnetic flux using geomagnetic activity observations provided a constraint which allowed physics-based reconstruction of total solar irradiance over the past 10,000 years.

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