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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Solar cycle signals in sea level pressure and sea surface temperature" *by* I. Roy and J. D. Haigh

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We thank the referees for their careful reading of the paper and acknowledgement that we have an original angle (especially Fig.3). We intended the paper to be a short note identifying a new perspective on observations of solar signals in surface climate. The referees, however, consider we should include discussions about the mechanisms of the processes involved. We are quite happy to do this (as outlined below) in an expanded paper if the editor feels that is desirable.

Replies to referee's comments: Referee #1 (Meehl)

General comments

This paper presents a multiple regression analysis of reconstructed sea level pressure

and sea surface temperature datasets, along with stratospheric aerosols, the sunspot cycle, and Nino3.4 time series. The authors show, for peaks in the solar cycle, positive sea level pressure anomalies in the North Pacific, thus re-confirming results from other studies, but the story for SST is somewhat more complicated. This level of complexity with regards to the timing of forcing and response, shown in detail in a number of previously published papers, has been missed by the authors in the present paper. However, the fundamental aspect of this timing is actually described by the authors as noted below, and needs to be clarified.

Reply: We entirely agree that the timing is crucial, and an important aspect of our paper is to point out and clarify this between previous analyses, in the context of ENSO variability. In a revision of the paper we would include reference to/discussion of the more recent relevant papers suggested by the referee.

Comment: Though the authors state that their Fig. 2 shows "a weak El Nino-like pattern", what Fig. 2 actually shows is no consistent signal across the equatorial Pacific. The very small anomalies of about +/- 0.1 K show no coherent pattern in the equatorial Pacific. There are negative values close to South America, weak positive values from about 90 to 130W, small negative values from 140W to 170W, and very small positive values west of the Dateline. In my view this is not El Nino-like or La Nina-like, but a mixed neutral pattern

Reply: It is correct that in our analysis of the 155 year dataset the signal does not present a clear ENSO-type signal but the warming in the Eastern equatorial Pacific is El-Nino like and, furthermore, an El Nino pattern is clearly portrayed by White et al (1997) in their analysis of data from 1955-1994. The point here is that the results are sensitive to the time period chosen and that any mechanisms proposed to explain a solar influence should be consistent with the long dataset unless there are reasons to think otherwise.

Comment: The reason these SST anomalies are neither El Nino- or La Nina-like is

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summarized best by the authors themselves in section 25844, and this accurate insight goes to the crux of the solar forcing/response problem and probably should be the main result of the paper: “peak years tend to occur very soon after the solar cycle becomes more active...at least a year before.... the peak of a broader decadal variation”. My guess is that the multiple regression is picking up this broader peak (as did White’s filtering). Therefore, the early La Nina-like response is blurred into the subsequent El Nino-like pattern a year or two later, producing virtually no distinguishable pattern from the multiple regression.

Reply: This is exactly what we intended to express in section 25844, lines 11-15.

Comment: Since the work by van Loon, Meehl, Arblaster, White, Liu and others have shown an oscillatory response to solar forcing in the equatorial Pacific, timing is everything. It would appear that the initial response to the ramp-up of solar forcing leading to the peak would be a La Nina-like response as documented for two mechanisms (one working top-down, the other bottom-up) that produces such a response in observations and models (Meehl et al., Science, 2009, not referenced in the present paper). Then the system responds to that impulse (working on the timescale of ENSO, namely months, as shown in detail by Meehl and Arblaster, 2008) since coupled ocean-atmosphere ENSO-like dynamics are involved, to produce a lagged El Nino-like response. Again, this was shown for observations and models by Meehl and Arblaster, 2008 (the text states erroneously that study only showed model results), as well as by Meehl et al. (2009). Thus the multiple regression is picking up an intermediate time period in the broad decadal peak as the system is transitioning from the initial La Nina-like response (as documented in their Fig. 3b) to the more El Nino-like pattern a year or two later, but still in the broad decadal maximum.

Reply: The problem with this argument is that while the tropical SSTs behave in the ENSO-like way described by Meehl and Arblaster (2008) the SLP signal in the North Pacific is not consistent maintaining a positive anomaly (La Nina-like) throughout the period of higher solar activity.

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Specific comments

Section 25840:

Comment, Line 5: Meehl and Arblaster (2009) was a combined modeling and observational study, not just a modeling study as stated here; the combination of two observational datasets and two models makes a strong case for the oscillatory response of the tropical Pacific to solar forcing; also note this reference is cited with incorrect authors in the reference list.

Reply: Yes, we should have made it clear that both observational analysis and modelling are in the Meehl and Arblaster (2009) study. The error in the citation was introduced by the ACPD office on the day of publication and we pointed it out immediately but were advised that they would leave it until the revision stage.

Comment, line 13: Meehl et al (Science, 2009) is not cited here, but should be, since it makes a process-oriented argument for two mechanisms that would work in the same sense to produce the initial La Nina-like response, which stimulates the coupled dynamics documented in Meehl and Arblaster (2009) that transitions to an El Nino-like pattern along the equatorial Pacific a year or two later.

Reply: Meehl et al (2008 & 2009) discuss a peak in irradiance at the peak of the decadal solar oscillation producing the La-Nina-like response. They show this is lagged after a year or two by an El-Nino-like event (although van Loon and Meehl (2008) show a weaker cold event at one year lag). We agree that there is often a cold event at peak sunspot number but this occurs a year or so in advance of the peak in irradiance so that the subsequent warm event coincides with the solar maximum in irradiance, as shown by White (2007) and White and Liu (2008). This difference in timing is important in considering mechanisms which involve forcing by solar irradiance. Another consideration is the SLP signal in mid-latitudes: associated with the cold event, as is standard with ENSO negative phase, is a positive anomaly in the N. Pacific and Meehl et al discuss how the two are physically related. However, in the observations this

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positive anomaly is maintained for several years and continues through the following "warm event" which would normally be accompanied by a negative SLP anomaly. This raises questions with regard to the coupled ocean-atmosphere bottom-up mechanism outlined by Meehl and Arblaster (2009). In our paper we intended to publish a simple observational study and leave detailed discussion of the implications for mechanisms to another time but we would be happy to expand along these lines if the editor feels this would be desirable.

Section 25841:

Comment, line 13: Vecchi et al is apparently cited incorrectly; in the reference list it is Vecchi and Soden. Additionally, the authors need to cite Vecchi et al (2008) who provide an enlightening discussion of inconsistencies of the various SST reconstructions in the equatorial eastern Pacific that provide different results between the NOAA and HadISST data.

Reply: Thank you for pointing these out.

Section 25842:

Comment, line 6: Meehl et al (2008) provide compelling evidence that convective heating anomalies associated with the SST anomalies in the tropical Pacific force the SLP pattern characterized by anomalous high pressure over the North Pacific with peaks in the solar cycle.

Reply: As discussed above.

Comment, line 15: Actually, the biggest and most dominant signal in the observations shown by van Loon et al (2007) is exactly the positive SLP anomalies in the North Pacific shown here again, though the authors do not mention this.

Reply: Yes, we should refer to van Loon et al (2007) SLP signal at peak SSN, and following years.

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Comment, line 27: White et al show a much more consistent El Nino-like pattern than that shown in Fig. 2 of the present paper which shows basically no consistent pattern in the equatorial Pacific.

Reply: Yes. A multiple regression using only data over the second half of the 20th century, as used by White, shows a similar strong El Nino pattern when we extend the data to 155 years this signal becomes much weaker. We should make this clear in the paper.

Section 25843:

Comment, near line 20: The authors have identified, in another way, what van Loon and co-authors have shown in multiple papers, that the response to peaks in solar forcing resemble weak La Nina conditions; however, the authors should mention that van Loon and Meehl (2008, in the reference list) showed that the peak solar conditions are different from La Nina events in the Southern Oscillation mainly in the equatorial stratosphere, where the latter have the opposite sign zonal wind anomaly compared to the former.

Reply: Van Loon and Meehl (2008) do show a different sign but only right at the top of the data domain. If it is not an ENSO-like signal how do the mechanisms relating to ENSO-like atmosphere-ocean coupling, Walker cell etc hold up?

Comment, line 24: I would argue that the pattern in Fig. 2 is not a “weak WE-like response”, it’s more of a neutral response; see discussion in general comments above.

Reply: Discussed above.

Section 25844:

Comment, lines 9-15: I suggest that this could be the main result of this paper with regards to timing and the oscillatory response of the equatorial Pacific to solar forcing.

Reply: Yes. This figure makes the association between peak sunspot years and nega-

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tive ENSO index very clear. We do not think this result has been published before.

Comment, line 28: The authors have presented no results regarding physical processes; they only present statistical results from a multiple linear regression.

Reply: We entirely accept this. Our intention in this paper was only to seek clarity between apparently confusing messages from previous data studies and to point out new ways at looking at these results. However, as discussed above, we could add material on the implications for mechanisms if desired.

Section 25845:

Comment, lines 1-5: As shown in great detail in Meehl and Arblaster (2009) and discussed above, the timescale of the response to peak solar in the equatorial Pacific is the same as ENSO dynamics, namely a timescale of months, certainly not centennial to millennial!

Reply: Of course we are not suggesting that the effects take hundreds of years to develop just that paleoclimate studies have suggested that solar variability is manifest in similar overall changes in the tropical ocean circulations in response to longer term solar variability. (As well as Clement et al, which discusses idealised forcings in a simplified model, see Emile-Geay et al ("El Niño as a mediator of the solar influence on climate", Paleoceanography, 2007) which specifically discusses solar influences).

Comment, line 10: Indeed a limitation of the observations is a finite number of solar cycles, but results from two different global coupled models shown by Meehl et al (2008), using multiple ensemble members from 20th century simulations and thus on the order of 40 to 50 realizations of peak solar forcing for each model, show a similar pattern to the shorter observational record, and Meehl et al (2008, 2009) provide mechanisms to explain it.

Comment, lines 15-18: the Meehl et al (2009) paper uses versions of models with both the topdown stratospheric ozone mechanism and the bottom-up coupled ocean-

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atmosphere mechanism to demonstrate how the two mechanisms mentioned in the comment above could work.

Reply: The model studies, and analysis, are very intriguing but the statistical robustness of the response to variations in solar irradiance remains to be established in a context of internal model (ENSO and other) variability and the uncertainties in timings discussed above.

References

Meehl, G.A., J.M. Arblaster, K. Matthes, F. Sassi, and H. van Loon, 2009: Amplifying the Pacific climate system response to a small 11 year solar cycle forcing, Science, 325, 1114-1118.

Vecchi, G.A., A. Clement, and B. J. Soden, 2008: Examining the tropical Pacific's response to global warming. Eos Trans. AGU, 89, 9, doi:10.1029/2008EO090002 Interactive comment on Atmos. Chem. Phys. Discuss., 9, 25839, 2009.

Replies to referee's comments: Referee #2 (Anonymous)

General comments

The paper presents results from a regression approach aiming at extracting the 11-yr solar cycle signal from sea-level pressure and sea-surface temperature data. This is not a new topic, but one where existing studies, at least partly, disagree. A strong focus of the paper is on the differences of their results to those found in another paper (van Loon et al. 2007). This is interesting and reveals how tricky it is to extract a small, unknown signal (with an unknown measure) that interferes with many other signals out of noisy data. This is a common problem of many Sun-climate studies and likely a major obstacle to further progress. However, the focus of the paper is not clear.

The way it stands, the paper should be entitled "Comment on van Loon et al. (2007): : :" because that is basically the content (at least of the discussion section). It would be nice if the paper went further in discussing methodological aspects, not just of van Loon

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et al. (2007). However, in its present form the manuscript lacks the rigor necessary for this. Many other factors than that of composite versus regression should also be considered, e.g., how to account for the seasonality in the signal, for time lags involved, for other independent or semi-dependent effects, and for uncertainties in the data. The measure of solar activity itself should be addressed, and the statistical methods (robustness of estimators and particularly the significance testing) should be looked into. There have been previous debates (e.g., Coughlin and Tung, 2006).

If the focus of the paper is on the analysis of the signal, possible mechanisms should be discussed in some more detail. I suggest that the authors focus their paper either on a critique of the methods used in the literature or on the science.

Reply: The focus of our paper was intended to (a) provide a clarification of, and different angle on, the solar signal in SSTs found in previous papers, (b) to point out that the signal in SSTs at peak sunspot number is associated with ENSO cold events and (c) demonstrate an inconsistency between stated solar influence on tropical SSTs and on N. Pacific SLP, if these are to be regarded as linked through an ENSO-like process. We specifically did not address the issue of mechanisms in this short paper but clearly our results have important implications for the (mechanistic) interpretation of the statistical results. In rewriting the paper we would seek to spell out our aims more clearly and could introduce more discussion of mechanisms.

Specific comments Introduction:

The topic of Sun-Earth connection is one of intensive current debate, not only because of new mechanisms that have been put forth (Meehl et al. 2009), but also in the light of the current anomalously low solar activity and to some extent in view of the stagnation of the global temperature increase and the need for decadal predictions (Lean and Rind 2009). In my view a thorough critique of the methods is very important in this discussion, so the paper could potentially make a nice contribution if it had a broader, more methodological focus.

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Reply: See aims outlined above.

Comment: Similarly, the uncertainty of reconstructions of solar activity is a much discussed issue and I am surprised that sunspot numbers are used without referring to this discussion. The question what property of solar activity might be responsible for climate effects (e.g. total solar irradiance vs. spectral solar irradiance) is not addressed.

Reply: For statistical analysis of 155 years of data we are restricted for an indicator of solar activity to sunspot numbers, TSI reconstructions or some geomagnetic indices. The focus of this paper is on 11-year cycle variability so that the accurate representation of longer term secular variability is less important but we have carried out the regression using a number of TSI reconstructions (Lean et al, Krivova & Solanki, Hoyt & Schatten, Foster). We find that the 11-year signal is essentially unaffected by this choice. However, long term variations are not statistically separable from any climate change (linear trend or greenhouse gas) signal. We could certainly clarify this in a revised paper. We have done plenty of work on the UV vs TSI issue (indeed, starting a new dialogue with Haigh, Nature, 1994) but we consider that discussion is outwith the remit of this paper.

P. 25842, l. 15 and following:

Comment: The methods section needs more details. Why is a trend used and not a variable that captures more closely the greenhouse warming (e.g., radiative forcing)?

Reply: The choice of long-term trend has essentially no effect on the derived 11-year cycle signal. As discussed above the response to different climate indices becomes statistically mixed with that due to any solar secular trend.

Comment: What is the time resolution of the analysis (monthly, seasonal, or annual)?

Reply: All the results presented are for DJF.

Comment: What about other independent variables (QBO, other climate modes)?

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Reply: As far as we are aware the longest QBO series available only goes back to 1900 (Brönnimann et al 2007) so could not be used for analysis back to 1850. We have carried out some regressions using QBO back to 1900 and these reveal some very interesting results wrt the combined effect of the Sun and the QBO on the polar modes but conclusions wrt tropical SSTs and N. Pacific SLP are unaffected. We did not think it appropriate to expand the discussion in the current paper to include this.

Comment: How is the Nino3.4 index defined (simultaneous or with lead)? All these points need to be better exposed. Especially, the question of time lags is important when addressing SSTs. Energy balance models suggest lags of 6-24 months, with lags being larger over the oceans.

Reply: All our regessions indices are used simultaneously. As we are using DJF data any lag of less than a year is undefinable. We have carried out regressions with one year and two year lags and found very little signal which is unsurprising using a broad indicator of DSO variability.

Comment: The measure of solar activity should be justified here. The authors use the sunspot number, which has the advantage of being an observed property of solar activity. Although it is clear that the authors focus on the 11-yr cycle and not on low-frequency variability (which is where most reconstructions differ), I still think there should be some justification. How different would the result be if reconstructions by Krivova et al. (2007) were used?

Reply: As mentioned above we have used a range of different reconstructions but these do not materially affect the deduced 11-year cycle signal. We can certainly state this in a revised version of the paper.

Comment: Since ENSO itself might be affected by solar activity changes, the removal of ENSO is critical. The authors did the analysis both with and without including ENSO as an explanatory variable. However (to strengthen the methodological focus), other methods to remove ENSO could be considered or at least mentioned (e.g., Compo and

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Sardeshmukh, 2009).

Reply: We can certainly refer to other methods but as our results appear to be robust to the inclusion or not of an ENSO index we expect these to make little difference to our conclusions.

P. 25845:

Comment: Gleisner and Thejll (2003) show that the response in the Hadley cell cannot be identified directly in zonal mean form but needs some rectification.

Reply: OK but they use instantaneous multiple regression analysis and our point is that picking out different phases of the ENSO cycle will result in different conclusions.

Comment: In the comparison with van Loon et al. (2007) I am missing the argument whether or not the two results are statistically incompatible (given the uncertainties).

Reply: Our conclusions are not that the two results are statistically incompatible but that they are measuring different phases of the solar cycle and that van Loon's compositing method is vulnerable to mixing with an ENSO signal.

References: Compo, G. P., and P. D. Sardeshmukh (2009), Removing ENSO-related variations from the climate record. J. Climate (early online release).

Coughlin, K. T., and K. K. Tung (2006), Misleading patterns in correlation maps, J.Geophys. Res., 111, D24102, doi:10.1029/2006JD007452.

Gleisner, H., and P. Thejll (2003), Patterns of tropospheric response to solar variability, Geophys. Res. Lett., 30(13), 1711, doi:10.1029/2003GL017129.

Krivova, N., L. Balmaceda, and S.K. Solanki (2007), Reconstruction of solar total irradiance since 1700 from the surface magnetic flux, Astron. Astrophys., 467, 335–346.

Lean, J.L., and D. H. Rind (2009) How will Earth's surface temperature change in future decades? Geophys. Res. Lett., 36, L15708, doi:10.1029/2009GL038932.

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Interactive Comment