Atmos. Chem. Phys. Discuss., 9, C9907–C9910, 2010
www.atmos-chem-phys-discuss.net/9/C9907/2010/

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

Anonymous Referee #2

Received and published: 20 January 2010

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 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.

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. 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.

P. 25842, l. 15 and following: 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)? What is the time resolution of the analysis (monthly, seasonal, or

ACPD

9, C9907–C9910, 2010

annual)? What about other independent variables (QBO, other climate modes)? 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.

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?

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 Sardeshmukh, 2009).

P. 25845: Gleisner and Thejll (2003) show that the response in the Hadley cell cannot be identified directly in zonal mean form but needs some rectification.

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).

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,

ACPD

9, C9907–C9910, 2010

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.

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, doi:10.1126/science.1172872.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 25839, 2009.

ACPD

9, C9907–C9910, 2010

Interactive Comment