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Climate of the Past Discussions

Interactive comment on "Influence of solar variability on the occurrence of Central European weather types from 1763 to 2009" *by* Mikhaël Schwander et al.

Anonymous Referee #1

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The manuscript presents a detailed investigation of 11-yr solar cycle influence on weather types. The topic is of interest for the paleoclimate and solar-terrestrial communities, although a detailed knowledge of the weather types and their variability may be too farfetched for most of the CP readers. Some description of weather type characteristics is needed. Besides a new, unique reconstruction of weather types, the authors analyze a set of 4 simulations with a climate model forced by Total Solar Irradiance, only. This is a somewhat a strange model configuration and weakens the merit of the simulations for detecting mechanisms, as the "top-down", which believed to be the key mechanism influencing the weather types (see Introduction) is neglected. Most parts of the manuscript are well written and reading is straightforward and easy, except the

discussion part where it is difficult to pair results of this study to the text. There are some points other that need further clarification and improvement before publication of the paper.

Methodology

Weather types are analyzed by the means of composites. The 11-yr solar cycle is sliced in three groups/day of low, moderate and high activity. Are these groups of near equal size? I am concerned about the role of internal variability in the composites and how affects results. How confident are the authors that compositing results in true solar signals? Splitting the record to 50 yr chunks offers too little to this regard because it provides little evidence of consistency over time. This is recognized by the authors: "P8 L13 Although it can be difficult . . .". I would suggest to split to two sub-periods at best. For the same reason, Figure 7 can be supplied as a supplementary.

Significance in solar minimum

I always consider the solar minimum as the least perturbed state of climate not necessarily the reverse of the solar maximum. It is puzzling to me that the most noticeable changes in WSW and W types are detected in solar minimum, when the forcing is weakest. Could the authors elaborate on the reasons/mechanisms that can explain strongest signals in solar minimum and not maximum?

Model simulations

Perhaps I am missing something here, but my understanding is that the SOCOL simulations are forced only by TSI and in particular by the strong Sapiro et al. TSI reconstruction. There is nothing wrong by choosing a strong TSI reduction to facilitate the signal-to-noise detection. My objection here is on the specification of TSI and not SSI variability. Is there any particular reason to assume that solar signals in weather types are attributed to the "bottom-up" mechanisms? Most of the discussion in introduction emphasizes the importance of "top-down" mechanisms in transferring signals on the surface, a mechanism which apparently is missing in model runs without SSI forcing. In such a case, the low resemblance between reanalysis and modelled signals is not surprising to my understanding. Moreover, some similarities discussed in P.10 L30 is a matter of coincidence to me. So, it is difficult to understand the overall point of Section 3.4 given that the SOCOL runs are missing key mechanisms. The weakness of the simulations should be discussed in the text.

Mean difference (Section 3.1)

Do results of figure 5 compare with Fig1 of Ineson et al., 2011? Difficult to say for SLP. For temperature, I see some similarities but some differences as well. I could also consider presenting lagged anomalies (see my following comment)

Weather type classification (Section 2.2)

This section assumes a reader familiar with the different weather types and their within-type differences. I am afraid this won't be the case for most of the CP readers. For example, what does the "well discriminated types (P4 L 31)" mean? Or, "days with probability higher than 75%". I think a concise description of the main characteristics of the weather types is needed.

Lagged responses

The authors in P12 3rd paragraph, briefly discuss the lagged response of westerly types and try to compare with Gray et al. and Thieblemont et al., results. Same in P8 last paragraph. Inferring time lags is very interesting subject and I would recommend a proper presentation, dedicating, perhaps, even a new Section. This could be a valuable contribution to the number of recent papers discussing time lags as they can highlight the importance of atmosphere-ocean coupling.

Some additional considerations,

P1. L27: stratospheric ozone + "and heating"

P2. L10: "phase lag is expected": Perhaps this is not true by the sole action of "top-down" mechanisms. An atmosphere-ocean coupling is required for lags longer than one year at least.

P2 L20: found a response

P3. L3: do you mean Gray et al., 2010?

P3 L4: This is hardly true. Gray et al, show surface signals.

P3. I think the second paragraph should also be extended by discussing results of more recent model intercomparison such as CCMVal or SolarMIP. See (Austin et al., 2008; Hood et al., 2015; Misios et al., 2015; Mitchell et al., 2015) and references therein.

P3 L25: "It allows us . . . weather statistics". Is this true? What is the main difference to Huth et al., 2008b?

P4 L4: Description here is rather confusing. You should clarify that you analyze a merged dataset and not ERA-40 and ERA-int separately. Please elaborate how stitching was performed.

P4 L17 : Is it one of the revised products of sunspot numbers?

P4 L29: "from 1958 to 1998". Why not till 2009?

P6 L25: CO2, CH4, N2O (subscripts)

P7 L6: A quantitative difference of the forcing, long term and 11-yr cycle, should be given here.

P7 L10: . . .66th thresholds of sunspot numbers?

P7 L11: Still not clear how percentiles are calculated. Have you subtracted the 11-yr solar cycle before?

P11-13: It is very difficult to follow the discussion of the results. Please point to the associated figures.

P13, L11: “only partially”. This is a wishful thinking!

Figure 5: Difficult to separate SLP from geopotential signals. Please consider splitting this panel in two.

Austin, J. et al., 2008. Coupled chemistry climate model simulations of the solar cycle in ozone and temperature. J Geophys Res-Atmos, 113(D11): D11306. Hood, L. et al., 2015. Solar Signals in CMIP-5 Simulations: The Ozone Response. Q. J. Roy. Meteorol. Soc., 141: 2670–2689. Misios, S. et al., 2015. Solar Signals in CMIP-5 Simulations: Effects of Atmospherre Ocean Coupling. Q. J. Roy. Meteorol. Soc. Mitchell, D.M. et al., 2015. Solar Signals in CMIP-5 Simulations: The Stratospheric Pathway. Q. J. Roy. Meteorol. Soc., 141: 2390-2403.

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