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

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Response to:

The manuscript presents a detailed investigation of 11-yr solar cycle influence on weather types. Thetopicisofinterestforthepaleoclimateandsolar-terrestrialcommunities, 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

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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 clariïňĄcation and improvement before publication of the paper.

Answer: We thank the reviewer for the constructive comments. We agree on most comments and will include the suggestions in the revised manuscript. The description of the model in our first manuscript is probably too vague and needs to be improved since the model is forced by SSI (see description below). We will probably leave out the low frequency solar variability and focus only on the 11-year solar cycle. The introduction and discussion on bottom-up and top-down mechanisms will also be improved as well as the description of the weather types.

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 coniïňĄdent 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.

Answer: The groups are not exactly of equal sizes since more volcanic eruptions occurred under low solar activity. There are 195 months under low solar activity, 211 under moderate activity, and 212 under high activity. We will add this information on the figures or in the text. The confidence is quite high since the signal in some types in significant over 250 years, and also because it is consistent with previous studies. The signal found in the occurrence of weather types is consistent with the within-type differences. The significance will be added on the within-type composites difference plots.

We would keep the 1958-2009 sub-periods as a comparison with Huth et al. (2008). It is true that two sub-periods could make the results clearer and understandable. We will come to the same conclusions if we split in only two sub-periods. However, it would be more straightforward for the reader.

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?

Answer: We cannot provide any reasons why the signal could be stronger under low solar activity compared to high activity. We think that it comes from the fact that the long-term mean is also perturbed. If the low activity phase is not perturbed (one third of the months), then two thirds are perturbed (moderate and high). The long-term mean is therefore also perturbed and the differences under low solar activity seem larger. We could take the low solar activity phase as a (unperturbed) reference and then we would observe a strong increase in the occurrence of W and WSW types under moderate and high solar activity.

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 speciïňĄcation 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.

Answer: We realize that the model description in the Manuscript is lacking some information. The model was not forced by SSI and can include top-down and bottom-up mechanisms. The description of the model will be completed as follow in the revised manuscript:

"In the present investigation we have employed the Coupled Atmosphere-Ocean-Chemistry Climate Model (AOCCM) simulations carried out with SOCOL-MPIOM (see, Muthers et al. 2014). The SOCOL (Solar Climate Ozone Links) chemistry-climate model is coupled to the ocean-sea-ice model MPIOM. The SOCOL is based on the middle atmosphere model MA-ECHAM5 version 5.4.01 (Roeckner et al., 2003) and a modified version of the chemistry model MEZON (Model for Evaluation of oZONe trends, Egorova et al., 2003). The model has a horizontal resolution of T31 ($3.75^\circ \times 3.75^\circ$) with 39 irregular vertical pressure levels (L39) from 1000 hPa to 0.01 hPa. The horizontal resolution of the ocean component (MPIOM) is 3o varying between Greenland (22 km) and tropical Pacific (350 km). The SOCOL-MPIOM cannot reproduce the Quasi-Biennial-Oscillation (QBO), thus nudged to QBO reconstruction from Brönnimann et al. (2007). The MA-ECHAM5 (MPIOM) component calculates the dynamical processes in every 15 (144) minutes and atmosphere-ocean coupling takes place in every 24 hours (Anet et al. 2013a, b; Muthers et al. 2014). Muthers et al. 2014 employed SOCOL-MPIOM to carry out four transient simulations (namely L1, L2, M1, and M2) over the period AD 1600-1999 with all major forcings (i.e. greenhouse gases, volcanic eruptions, aerosols, and solar spectral irradiance), and interactive ozone chemistry. The SOCOL-MPIOM was forced with six bands of Solar Spectral Irradiance (SSI) reconstruction of Shapiro et al. (2011) over the Ultraviolet (UV), visible, and near infrared

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ranges. The L1 (M1) and L2 (M2) simulations were forced with large (small) mean solar amplitude of 6 (3) W/m2 with different ocean initial conditions for both runs. For more details of the model the reader is referred to Muthers et al. 2014. The model is well capable of simulating the top-down (stratospheric-tropospheric coupling) and bottom-up (coupled ocean-atmosphere response) mechanisms as proposed by Meehl et al. (2009)."

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).

Answer: The SLP and temperatures differences in Figure 5 are similar to Figure 1 of Ineson et al. (2011) with some variations in the location of the maximum differences. Differences are similar over Europe but quite different over the North Atlantic and Greenland. For example the positive SLP difference over Scandinavia in Figure 5 does not extend as far east (over Greenland) as in Ineson et al. (2011)

Weather type classiïňĄcation (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.

Answer: Since the submission of this manuscript the paper describing the weather types and reconstruction method has been published online (Schwander et al., 2017). We do not want to describe all the method again in this paper but we will improve the description of the weather types to make it more understandable. The probability refers to the method of reconstruction, it's just an indication on the quality of the reconstruction since there is no comparison possible with another weather types time series over such a long period. The reader should look at Schwander et al. (2017) for more information.

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.

Answer: Since the strongest signal in weather types occurrences is found without any lag we decided not to focus on lags. However, we will add histograms with a 1, 2 and 3-year lags as a supplementary. Also we will extend the discussion and comparison on the lags.

Some additional considerations, P1. L27: stratospheric ozone + "and heating".

Answer: Will be added.

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.

Answer: We agree, we will reformulate the sentence, we speak here more about a lag of a few months.

P2 L20: found a response

Answer: Thank you, will be corrected.

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

Answer: Yes, it makes more sense to cite a more recent paper.

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

Answer: Will be corrected.

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.

Answer: Thank you for the suggestion, we will complete the discussion with more recent references.

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

Answer: The difference to Huth et al. (2008b) is that we have almost 250 years of daily weather types (∼50 years for Huth). We will modify the sentence to mention that we have a longer time series of data.

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.

Answer: Yes we realize that the description is not clear enough. We will rewrite and add more information on the reanalysis data.

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

Answer: Yes, we will add this information.

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

Answer: Because some of the instrumental data used for the reconstruction stop in 1998. The reference was taken over a period where all data were available (see Schwander et al., 2017).

P6 L25: CO2, CH4, N2O (subscripts)

Answer: Will be added.

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

Answer: Thank you for the suggestion, we will add this information.

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

Answer: Yes, the same months were selected based on the sunspot number.

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

Answer: When we speak about the 11-yr solar cycle we always speak about the monthly sunspot number on which the percentiles where computed. The was used also for the Shapiro reconstruction and is visible in the reconstruction although it is sometime masked by the low frequency variability. We will probably focus only on the period 1958-1999 in the model simulations as a comparison with the reanalysis data. Also the low frequency variability of the solar variability during the period 1958-1999 is stable and we can focus only on the 11-yr solar cycle.

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

Answer: The discussion will be rewritten to be more understandable and completed with the suggestions from both reviewers.

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

Answer: We can remove this, it is true that we do not see the same signal in model simulations.

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

Answer: We will redo this Figure by splitting it or by changing the colors.

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