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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:

Review of the paper "Influence of solar variability on the occurrence of Central European weather types from 1763 to 2009" by Mikhaël Schwander et al., MS No.: cp2017-8

General comments

The paper uses a novel weather type classification that was constructed by the authors in a previous recent study, in order to identify and assess the potentially important regional aspects of solar variability effects on the weather types in central Europe for

the period from 1763 to 2009. The present paper expands the use of the weather type classification and contains new material.

However, the paper needs major improvements before it is considered for publication. The authors try to assess and compare the shorter term (11years) solar variability effect to the long-term (secular and super secular) changes, occurring at periods of 90-years or more. This attempt is not very successful, as it is not clear throughout the paper where they discuss which time scale. All sections of the paper, mainly the introduction, the data sections and the discussion on the model study, and, of course the conclusions, should be rewritten so that the paper's message is conveyed clearly to the reader. Suggestions on major issues are given below.

Answer: We thank the reviewer for the constructive comments. Most parts of the papers will be improved and rewritten. We agree that some important information is missing to have a good understanding of the methods. The model simulations need to be better explained to explain since it also includes SSI. We will probably just focus in the 11-year solar cycle to be more consistent and leave out the low frequency solar variability since it does not add any relevant conclusions to the paper. The introduction and discussion on bottom-up and top-down mechanisms will also be improved.

Specific Comments

The introduction section is rather poor on bibliography, and could be enriched more;. e.g.on page 3, line 5 they refer to Gray et al., 2005, an older paper compared to Gray et al., 2010. Moreover, they should at least mention the work of Meehl et al., 2009, or van Loon and Meehl, Seppala et al., 2009, Rozanov et al., 2012, Scaife et al., 2013, and at least refer to the work by e.g. Mitchell et al., 2015, Misios et al., 2016 on the solar signal in the CMIP5 simulations. (A relatively recent review on the mechanisms and effects is given also by Seppälä et al, 2014).

Answer: We agree that some important references are missing and that some more recent papers should be cited. The introduction will be improved and completed with

more recent papers.

The data section is incomplete. In the very ïňĄrst paragraph they mention that they used ERA-40 and ERA-Interim. My impression is that these two reanalysis data sets have been used as one. However, it is not clear if this is the case and, if yes, if there has been any check done on the homogeneity of the data, or if the possible discrepancies have been identiïňĄed and corrected.

Answer: We agree that some description of the data is missing. ERA-40 (from January 1958 to August 2002) and ERA-Interim (from September 2002 to March 2009) have been used together. The reason why ERA-40 was used until August 2002 and ERA-Interim from September 2002 is because the reference classification used to produce CAP7 (Schwander et al., 2017) was originally computed with ERA-40 (1958-2002) and ERA-Interim (2002-2009). For more information on the reference classification (CAP9) please see Weusthoff (2011). We tried to stay consistent with CAP7 (and therefore with CAP9) and use these two reanalysis dataset over the same two periods. The data were remapped to 1° x 1° in order to be combined together. We have not found any discrepancies but we will compare both reanalysis over a similar period as an assessment.

Section 2.1 should be clearly written, and the indices they used for the 11-year and longer term variability presented in a very clear way. For example, there is no call to Figure 3 in this section. They refer to Figure 4 but with no explanation as to what it contains, and the reader is left puzzled, since the Shapiro reconstruction is shown there without it being mentioned in the text. Moreover, I could not understand why they mention in the text that the fact that the sunspot cycle does not become negative is a limitation (this is also mentioned again later in the paper).

Answer: We agree on the comment; this section will be improved and the corresponding figures will be mentioned and better presented.

Section 2.3 It is not clear what are the time scales they discuss. Do they refer to the

11-year of the secular cycles? This should be very clearly mentioned here as well. The mechanism they refer to is the top-down mechanism, in which the stratospheric response and the signal transfer from there to the troposphere is the main pathway. This leads us to

Answer: In Section 2.3 we speak about the 11-year sunspot cycle. It will be mentioned more clearly.

Section 2.6, where they describe the model simulations. Again in line 21 they refer to low and high solar activity, with no clear indication as to what they mean. Moreover, and for the model simulations: Was TSI the only forcing? Or did they use also the appropriate SSI forcing? Was the model run in its full version with the interactive ozone response in the stratosphere? How is it achieved if one uses TSI variations only? Was the solar effect on ozone included in any way? If SSI variability with the solar cycle and the stratospheric response is not included, then one can have only the bottom up mechanism, and the comparison to e.g. Ineson et al. is not straight forward. In addition, what is the meaning of “It has the advantage to be a predominant forcing in the model..”? It is also not clear how the 11-year solar cycle is handled here. The Shapiro index and its use to deïňĄne “large solar activity”, “moderate amplitude” should be more clearly written.

Answer: We realize that the description of the model simulations in the manuscript is not clear enough. SSI is included in the model. Here is a clearer description that we will include 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° × 3.75°) 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 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)."

"It has the advantage to be a predominant forcing in the model.." means that since the Shapiro reconstruction has a higher amplitude (∼6 w/m2) than any other reconstruction, it consists of a strong forcing in the model. The upper boundary of the uncertainty of the Shapiro reconstruction was used as moderate amplitude (∼3 w/m2) in the model. Also the Shapiro reconstruction includes the 11-yr solar cycle (based on the sunspot number) although it is often masked by the low frequency amplitude.

We agree that the use of model simulations should be better explained and justified in the paper. 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.

Page7 line9-10, on the volcanic activity and the years that were removed. Why do you state there to “note that many of the important eruptions occur during a solar minimum”. Is there any possible connection? How does the removal affect your statistics if it was mainly done for solar minimum years? And more importantly, what type of solar minimum? Sunspot, or secular?

Answer: We are not aware about any connection between volcanic eruptions and the solar cycle. The fact that more volcanic eruptions occurred under low solar activity phases has mostly an impact on the size of (number of months) of the low solar activity class. The three solar activity classes (low, moderate, high) are not of the same size. There are 195 months under low solar activity, 211 under moderate activity, and 212 under high activity. We will add this information (number of months in each class) on the figures or in the text.

Page 7, lines 15 -18. How exactly was the anthropogenic forcing removed? What were the predictors? Was there only one predictor? Which one?

Answer: The predictor consists in the radiative forcing applied in the model calculated from major greenhouse gases (CO2, CH4, N2O and CFCs). They were taken from the PMIP3 database (Etheridge et al., 1996, 1998; Ferretti et al., 2005; MacFarling-Meure et al., 2006).

Section 3.3 SigniïňĄcance in the differences should be given. The same holds for every place where differences are discussed.

Answer: We agree, significance will be added on all figures and discussed in the text. Also it will be corrected in Figure 5 since we have found a small error in the significance plotted.

4 Discussion Page 11, lines 18-19. It is accepted that the 11-year cycle effects project

onto tropospheric circulation patterns like the Arctic Oscillation (AO) and the North Atlantic Oscillation (NAO) rather than are directly correlated to NAO or AO

Answer: We will reformulate the sentence to make it clear that we are not speaking about a direct correlation.

5. Conclusions page 14, lines 4-6. The present simulation and the forcings used (if indeed SSI variability and ozone related variability have not been used) do not allow the investigation of the top-down mechanism, which is in the heart of the weather type response..

Answer: SSI and the related ozone variability are included in the model (see above).

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