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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.

Anonymous Referee #2

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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.: cp-2017-8

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.

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)

The data section is incomplete. In the very first 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 identified and corrected.

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).

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

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 define "large solar activity", "moderate amplitude" should be more clearly written.

Page 7 line 9-10, 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?

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

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

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

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..

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Discussion paper