

## ***Interactive comment on “Solar and volcanic forcing of North Atlantic climate inferred from a process-based reconstruction” by Jesper Sjolte et al.***

### **Anonymous Referee #1**

Received and published: 10 May 2018

The authors in this interesting study fuse measurements of isotopic composition of oxygen with simulated results from an isotope enabled global climate model to reconstruct sea level pressure and temperature changes over the 1241-1970 period. The model gives spatial maps of pressure and temperature for each year, in an ensemble way, which is raked according to minimization of the distance between the simulated and measured PCs of the oxygen isotope. Then the authors infer volcanic and solar influences in the reconstruction. The proposed method is novel and may return some useful information about external forcings over the last millennium. The manuscript is generally well written and figures are presented in a crystal way. In the following, I am listing some thoughts/comments which hopefully will increase clarity of the manuscript.

C1

**Major comments** In my view, this study is more about evaluating the proposed methodology rather than detecting volcanic and solar signals. For example, the length of the text dedicated to volcanic effects in the reconstruction is a single paragraph only (1st paragraph page 7). Given the extended length of the detected response compared to other studies I would strongly encourage authors to provide more details. For example, to what extent results are influenced by the performance of ECHAM to eruptions. How the response looks if picked up actual model years not reconstructed? An additional validation method would be to examine the sensitivity of the results to the model choice. The PMIP3 simulations are ideal to this purpose but I admit is a lot of additional effort.

**Added value of reconstruction** The added value of the reconstruction is not properly assessed. The reader is left wondering whether similar skills (e.g. correlations) could be obtained by the model simulation alone. The authors should at least mention the how much correlations between the modelled NAO and 20CR or other reconstructions are improved in the reconstruction. Likewise for the modelled PC2.

**Use of solar forcing** There is an extensive description of the TSI forcing used (is there SSI variability? ) in the model which differs from those recommended in the PMIP3. I guess the continuity of the Muscheler reconstruction throughout the last 500 or so years does not immediately make it superior over the other reconstructions. I am wondering however if the choice of TSI really matters, given that the reconstruction does not really benefit by the timing of TSI forcing (authors comment on that in p.5 l6). TSI and other forcings are just increasing the phase space of the surface patterns which may or may not be chosen eventually in the reconstruction. How different the reconstruction would be by feeding in model data from a constant forcing (control) simulation?

**Top-down mechanism is missing** (related to the previous comment) I am pretty sure the ECHAM5 version does not properly resolve the so called “top-down” mechanism of SSI forcing on the stratosphere and subsequent changes in the troposphere and surface. To my understanding this is a key component of the proposed solar-NAO link.

C2

Given that weakens of the model it is not surprising that little evidence is found between NAO and solar forcing. So, the argument that solar forcing has little effect on NAO is not well justified. Likewise, in the discussion (p.8 last paragraph), the argument about increasing blocking comes hand in hand with the stratospheric response and the “top-down” mechanism, with is missing here. So it is likely that you may get right (to some extent) responses from wrong reasons.

Solar responses and time lags The 11-yr solar cycle SLP anomalies in Figure 4 do not seem to support the results of Gray et al., 2013. It is the different period that change signals? Could you please calculate signals from 1850 as to be consistent with the HadSLP2 years? The argument that signals maximize with 5 years lag is strange. 20CR shows almost opposite signals in lag 5 compared to lag 0, an antisymmetry not obvious in the reconstruction (Figure 4). My guess is that the correlation of PC2 with the 11-yr happened to maximize at lag 5. Thinking linearly, I would expect a negative correlation at lead time +1 years or lag 0, but in table 2 I see just zero. Even at lag 5 the explained variance by the 11-yr solar forcing is tiny. Authors should comment on the amazing correlation between reconstructed PC2 and 20CR PC2+PC3 (figure 6c). Why is it so?

Other It is a bit strange to present anomalies in solar minima minus maxima. Most of the studies I am aware of show max-min. Please describe the method inferring significance. It is mentioned in figures but should be clearly described in the manuscript. Same for the type of filtering applied on PC2s.

P.3 L.16 time lag of 4-6 years: This is solar maximum, no? (see my comment above)  
P.3 l.23 “fully prescribed CO2”: what do you mean? How it differs in E1-COSMOS?

p.3 parag.2.1: What is the horizontal resolution of the model. How many layers in the stratosphere? p.5. l.1: “fitting the PCs of. . .”: please make it clear what is the criterion. The sum of PCs? Individual PCs? p.5 l.16: to me it seems that only DYE-3 shows high correlation. In other high accumulation sites correlation is much lower.

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C3

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-32>, 2018.

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