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Climate of the Past
Discussions

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

Anonymous Referee #2

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——————— General comments ———————

This paper is proposing a new methodology for reconstruction atmospheric variability modes over the last millennium. This technique is using a climate model simulation including the online computation of the variation in concentration of oxygen isotopes, which are then compared with ice cores records. This comparison is then allowing, through a statistical approach, to find the winter atmospheric circulation that fits the best with the observations. Using this approach, the authors propose a new reconstruction of the two first modes of the variability of the atmospheric circulation over the North Atlantic sector. Then, the authors compare their reconstruction with time series

of volcanic eruptions and solar forcing and find a clear signature of the former on the first mode, and a smaller signature of the latter on the second mode.

This is a very interesting study, proposing an innovative approach to reconstruct past climatic modes of variability. The analysis led is thorough and the results found are very impressive. As compared to a former reconstruction of the NAO (first mode of atmospheric circulation variability in winter), the new one exhibits slightly better scores (not sure the difference is really significant) of validation and is also showing no loss of variance when coming back in time. The study is also providing a reconstruction of the second mode of variability, usually denominated as the East Atlantic Pattern (EAP). These are very useful reconstructions for the climate community, and the manuscript clearly deserves publication.

I have noticed quite a number of issues mainly related with the clarity of the explanations, and also, from time to time, I have noticed some too enthusiastic evaluation of their work by the author, while a scientific work requires objectivity and discussion of strengths but also of potential weaknesses and caveats of the methods, which are not sufficiently discussed at the moment.

In particular, I think it would be worth discussing:

1) The fact that the model used is quite coarse resolution, so that it may have large biases, potentially even in its representation of the atmospheric circulation and of its variability modes, which need to be better depicted.

2) The reconstruction is finally relying on very few data, only 8 cores from Greenland, while the Atlantic sector is huge. Validation score are relatively high (even though a correlation of 0.5 stands for less than 30% of the variance explained…), but we can easily envisage that more data will be helpful to improve the reconstruction. A discussion of this will be useful I think.

3) The link with solar forcing is far from being straightforward, and the authors push their

conclusions a bit too far I should say. Also, the variability is not necessarily related with external forcing, and large amount of variance can be purely stochastic (i.e. internal to the climate system). While lots of research is led on this topic, notably to explain the little ice age, this is simply not discussed at all here. What is the percentage of variance in our reconstruction that is not related to any external forcing? This is not easy to isolate, but a rough estimate would be interesting (e.g. Wang et al. 2017).

With a proper discussion of the following points, I think the manuscript will clearly deserve to be published in climate of the past.

————— Specific points: —————

- p.1, l.1: the two first sentences can be interpreted as almost contradictory. Can you please reformulate them to clarify what you have in mind here?

- P.1, l. 3: add "of" before "the effects"

- P. 1, l. 3: "A positive phase…": This is not true. Some of the historical eruptions have been followed by negative phase of the NAO (e.g. Agung) and the link between NAO and volcanic eruptions over the historical era is very small and sensitive to the selected eruptions (cf. Swingedouw et al. 2017, cited in the ms.). Please clarify.

- P. 1, l. 10: you should specify here that this is for winter season.

- P. 1, l. 16: "we observe a similar response" is not clear at all. Please reformulate what you mean here. The next sentence concerning "blocking frequency" is also quite unclear. Please avoid the word "likely" as it has a very specific meaning in IPCC report, which is not the one used here, since I have not seen any proper analysis of changes in blocking frequency in the manuscript.

- P. 1, l. 19: You'll need to define what you mean here by little ice age in terms of time period.

- P. 1, l. 19: "a clear link" is quite subjective sentence. Can you be more specific (i.e.

quantitative: the correlation is quite small so that the link is not that clear I would say).

- P. 1, l. 20: no "s" to "pattern"

- P. 2, l. 1-2: It could be worth to specify here winter season, since the whole paper is focused on this season.

- P. 2, l. 6-7: "Past changes….": Variability in atmospheric circulation is usually believed to be mainly stochastic, with a weak imprint of external forcing. This is why seasonal or decadal prediction are so difficult: the atmosphere is dominated by chaos, with very small part of the variance that is predictable… This is in contradiction with the word "attributed" from your sentence. Please clarify. Also, please add some references to substantiate the claim from this sentence.

- P. 2, l. 10: add coma after "millennium"

- P. 2, l. 14-15: "The NAO has also…": This sentence is very confusing. Please rephrase by just saying that solar variations are slightly correlated with SLP over the historical era in a few areas.

- P. 2, l. 23-24: what is also very important is the amplitude of the influence i.e. the variance explained by solar variability. It is possible this forcing has a slight influence, but how does it compare to the noise, i.e. the purely stochastic variation of the NAO. The signal-to-noise ratio is indeed key to evaluate here.

- P. 3, l. 1: replace "particularly" by "particular".

- P. 3, l. 21-25: can you please provide a few more information on this model like its resolution? A brief description of its biases in the region analyzed will be also enlightening to evaluate the limit of this model for the exercise performed. How does the isotopic variations in the ice core sites compare with observations? How many grid points are covering Greenland?

- P. 4, l. 11: this section 2.3 is key to the paper. Nevertheless, I find it a bit difficult to follow. They are very few references that support the method presented, so that it seems that this approach is entirely new. Is that correct? If so, I would like to have a longer description of it and a few examples to fully understand how the method works. Furthermore, I'm wondering how sensitive the results to the X2 measure are, which sounds a bit arbitrary and not very well justified.

- p. 5, l. 2: The authors depict "39 time series". This is not crystal clear what the time is here and what the members are. Can you please be more specific to improve this description (a scheme could be useful as well for instance).

- P. 5, l. 2: "reshuffled model": a model is a tool with which you can perform simulations, providing climate variables. Thus, I'm not sure that "reshuffled model" is a proper terminology. I think, you are speaking here of the "reshuffled output from the simulation". This remark is true throughout the manuscript where model is used inconsistently.

- P. 5, l. 29: "Note...": This is not a very expression. Normally all what is written in the manuscript is worth to be noted.

- P. 5, l. 30: "is associated": it is not clear what is substantiating this claim. Please clarify.

-P. 5, l. 30: "Figure S3": I find the pattern of EOF2 very different between model simulation and reanalysis. This should be said somewhere.

- P. 5, l. 33: add a coma after "reconstructions"

- P. 6, l. 10: what about the biases of the model? I believe this can also explain your differences between model simulations and reanalysis! Climate models are not providing perfect representation of reality, the opposite is true.

- P. 6, l. 14 add "in" after "consists"

- P. 7, l. 16: "Scandinavian blocking-type pattern". You should support this by a reference. Also, I would have argued that a Scandinavian blocking is referring to a change

in frequency and is usually an anticyclone that remains blocked over the Scandinavia, following the weather regime approach (e.g. Vautard et al. 1990, Ortega et al. 2014 in line with ice core analysis and weather signature). Can you please clarify this sentence?

- P. 7, l. 22: "it should be noted". Avoid this type of subjective comment.

- P. 7, l. 30: "looks slightly different": this is a very subjective judgments, and I would rather say that they have hardly anything in common. Can you please provide a more objective metric of their similarity (spatial correlation for instance)?

- P. 7, l. 31: "wave structure". OK, but the signs are almost opposite in Fig. 4 a and c... So, this is not very convincing as a similar pattern!

- P. 8, l. 3: maybe state that this secondary pattern is usually denominated as the EAP. Also, it is better to avoid "likely", except when the meaning is in line with IPCC precise definition.

- P. 8, l. 14: this positive feedback is very weak, and hardly significant in the observations... (which is why the NAO is so difficult to predict at the seasonal scale, and the recent improvements seem to be more related with tropical teleconnection and frontal dynamics around the Gulf Stream region...).

- P. 8, l. 30: this line is very speculative and not supported by any references. The influence of the North Atlantic SST on atmospheric circulation is mainly not significant in winter season in the observation, or at least largely debated, so this hand-made explanation sounds a bit speculative I should say. I would at least replace "likely" l. 28 by "possible" given its high level of speculation.

- P. 9, l. 1: "Although the authors..." Indeed, they do not... They rather propose that the LIA would have been intrinsic and related to (unforced) rapid changes of the SPG. Furthermore, detection-attribution analysis (e.g. Schurer et al. 2014) found almost no signature of solar forcing on NH temperature, implying that LIA was hardy forced by solar variations. You should discuss this as well I would say. How do you reconcile this with your interpretations?

- Figure 1: what about adding a sliding window correlation here? It could also be useful for Figure 2 and Figure 6. It will help to evaluate when the time series are well-correlated by another mean than just by eye.

References:

Ortega P., D. Swingedouw, V. Masson-Delmotte, C. Risi, B. Vinther, P. Yiou, R. Vautard, K. Yoshimura (2014) Characterizing atmospheric circulation signals in Greenland ice cores: insights from the weather regime approach. Climate Dynamics 43 (9-10), pp. 2585-2605, DOI 10.1007/s00382-014-2074-z. Schurer A.P., Tett S.F.B., Hegerl G.C., 2014. Small influence of solar variability on climate over the past millennium. Nat. Geosci., 7, 104-108. Vautard R (1990) Multiple weather regimes over the North Atlantic analysis of precursors and successors. Mon Weather Rev 118:2056–2081 Wang et al. (2017) Internal and external forcing of multidecadal Atlantic climate variability over the past 1,200 yearsâĂć Nature Geoscience volume 10, pages 512–517.

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