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Interactive comment on "Volcanic imprint in the North Atlantic climate variability as recorded by stable water isotopes of Greenland ice cores" *by* Hera Guðlaugsdóttir et al.

Anonymous Referee #2

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General comments

This paper proposes to evaluate the impact of volcanic eruptions on the North Atlantic circulation using both Greenland ice core data and climate simulations of the last millennium including isotopes. The focus is put both on the response to high latitude and tropical eruptions. The analysis searches for responses up to 20 years after the onset of the eruption using climate weather regimes. The main results suggest that tropical eruptions may enhance the occurrence of NAO+ and Atlantic Ridge weather regimes

while NAO- and Scandinavian Blocking are favored by high latitude eruptions.

The topic of investigation is interesting and the questions tackled are very relevant to improve our knowledge on the response of atmospheric circulation to volcanic eruptions, which is still uncertain. The methodology sounds promising, notably the use of weather regimes. Nevertheless, I have number of serious issues that may prevent the publication of this paper in its current form.

1) The statistical analysis is poorly depicted and the use of weather regimes is not appropriately done in my opinion. The authors do find a number of significant responses, even 20 years after the eruptions, but the details concerning the way the significance is evaluated is not sufficient to be understandable. As shown by Menegoz et al. (2018), the variability in the North Atlantic sector is very large, and the need for a lot of members is absolutely necessary to have robust responses. The use of only 5 volcanic eruptions in the composite analysis may prevent any robust conclusions. More should be done concerning this statistical significance of the results using for instance bootstrap techniques. Also, the methodology related with the use of weather regimes is not clear at all. Menegoz et al. (2018) is also providing useful diagnostics to use this tool. For instance, Wiskher plots seem absolutely necessary to evaluate the impact of volcanic eruptions on the occurrence of the different regimes following the eruptions, in order to gain insights on the significance of the results, as compared to internal variability of the climate model. Once again, more than 5 members will be necessary for this. Furthermore, I am a bit surprised to find strong signal in the atmosphere so late after the onset of the eruption. As mentioned in the paper, this could be due to an an interaction with long-term changes in the ocean and sea ice, but usually such interactions are very weak in climate models.

2) There is a total lack of dynamical analysis of the results. In a sense, the analysis is restricted to repeating the same composite diagnostic to several volcanic eruptions or dataset. This is a bit short in my view to provide clear insights on what is going on following the volcanic eruptions analyzed. The use of a climate model should allow to

dig a little bit into the processes at play, which is crucial to provide an improvement in our understanding.

3) The novelty as compared to previous studies using similar models are poorly discussed and the experimental design of the simulation is not precise enough. A similar analysis by Zanchettin et al. (2012) seems to find similar results concerning a long-term response to volcanic eruptions, while Toohey et al. (2014) found that the response might be very sensitive to the way the forcing related with volcanic eruptions is implemented. Also, the two former studies used different statistical analysis of the significance of their signal, which is indeed crucial. What is this new study providing in regard of these two former studies using the same family of climate models?

You will find in the following a few more detailed suggestions that may allow to improve the manuscript and bring it towards publication standard of climate of the past.

Specific comments

- Introduction: please provide a few references to substantiate it (e.g. review by Robock 2000, Timmreck et al. 2012; Swingedouw et al. 2017)

- l. 49: You jump from 1st EOF to third. What is the second? Usually it is the East Atlantic Pattern, a mode whose negative phase resembles the Atlantic Ridge. Please read more carefully Cassou et al. (2004) who is describing this (they mentioned this as well).

- l. 83: replace “compliment” by “complement”

- l. 106: since the model simulation is only assimilating data from ice cores, it is not a big surprise that the results from ice core data and model agree.

- l. 112-114: This sentence is not clear at all, nor the following one. I do not get

what is done. Also, you should specify which type of data you used for the clustering. Usually weather regimes are made using daily data. Is it what have been done? Also, it is not mentioned which season is used. All this part concerning the weather regime computation using clustering technique should be largely improved. Also, I do not get what is done with this decomposition since it is not much discussed afterwards in the manuscript which is mainly showing composite of O18, temperature or SLP. Please clarify your approach here and do a proper analysis of weather regime changes in occurrence as done in Menegoz et al. (2018) for instance.

- L. 121: what is “r”? Correlation I assume. Here I do not get what is done.

- L. 132: Your Monte Carlo approach should be further described

- L. 135: If I understand correctly you are doing a Student t-test to evaluate the significance of the composite, with a degree of freedom of 5, equal to the number “n” of volcanic eruptions? Usually it is n-1 that is used for the degrees of freedom in a Student t-test. Thus, you should end up with 4 (this may change the significance of your results), which is very small, once again highlighting the need for more members in your analysis of the very noisy atmospheric circulation. Bootstrap approaches may be more appropriate here to evaluate the significance of your composite, since it will better account for the fact that the atmospheric variables are usually not independent variable. Analysis of significance is very key for your results, so a very particular care should be taken here to avoid to analyze noise-induced signals (which I suspect given the long-term response found, which is quite unusual in the atmosphere that no memory). In that sense, the analysis of processes at play should also help to evaluate if the signal you find is coherent in terms of dynamics.

- L. 141-142: the radiative forcing estimate are coming from Sigl et al. (2015) according to Table 1. Nevertheless, the simulation you are analyzing is using Jungclaus et al. (2010) forcing, so I assume a different forcing than Sigl et al. (2015). Even though the latter is certainly better than the former, since it is more up to date, I think you need to be coherent with the forcing used in the simulation. At least an evaluation of the differences concerning the volcanic eruption selected should be done.

- L. 146: a reference would be useful to support this claim.

- L. 185-186: "when compared with O18 pattern…" I do not get why the clustering is not use here to estimate the changes in occurrence in the different regimes. This would help to better evaluate the significance in the results and would be more coherent than a spatial correlation. Indeed, as shown in Toohey et al. (2014) the response to volcanic eruptions might be different than the preferential modes of variability. Also, this diagnostic of spatial correlation is dubious, since several patterns can be correlated, while the counting of day within each regime is far more instructive quantitatively.

- L. 415: "already reported by Sjolte et al. (2018)" then you should clearly highlight what is new in your study as compared to this former one that also look at the impact in volcanic eruptions in the same simulation…

- L. 424:" identification of this prolonged NAO+ in O18". You only found a NAO-like response in your O18 data, not a prolonged NAO+ (your data is not NAO…). Furthermore, such a signal not present in Ortega et al. NAO reconstruction… Is it in other NAO reconstructions?

- L. 430: a mechanism is mentioned, but nothing to substantiate it. Will it be possible to dig a bit more towards mechanistic understanding of your signal (and a better synthesis of your results to gain space, or just show the most robust and new ones).

- L. 440: Can you remind the reader how Zanchettin et al. (2012) explained their results. Since you use a similar model, can you check that the same mechanism is at play?

- L. 443: Can you explain how you relate the changes in the AMOC with your NAO response. See Gastineau et al. (2011) for some hints.

- Conclusion: since your study is mainly based on ice core data (either directly from

Greenland data or assimilation run that only assimilate this data) this should be highlighted as a major caveat. Indeed, even though the variability mode has different impacts over Greenland, Greenland is covering only a small part of the weather regime fingerprints, and the use of over regions might be very useful (cf. Ortega et al. 2015).

- L. 477: I do think you need to try answer this. Indeed, when you will have sorted out the really robust signal and novelty as compared to former studies using same data, you may need to provide mechanistic interpretation of the results, this what model simulations are made for in a sense: understanding processes to corroborate hypotheses.

Additional references:

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Swingedouw D., Mignot J., Ortega P., Khodri M., Menegoz M., Cassou C. and Hanquiez V. (2017) Impact of explosive volcanic eruptions on the main climate variability modes. Global and Planetary Changes 150, pp. 24-45.

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