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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.

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Dear reviewer, We are thankful for your instructive comments that will indeed serve as an improvement to our study. It is clear that several factors needs improvements. Other factors seem to cause confusion that need further clarifications. We have provided some answers/clarifications to the reviewer‘s comments here below:

1) First issue: We do see the reviewer‘s point and will address this in a revised manuscript where the bootstrapping method will be used as well as assess the use of other methods (e.g. Whisker plots). Furthermore, as can be seen in the answer‘s to reviewer #1, more eruptions have been added to assess the long-term climate signal

in the Greenland ice cores (dates back to 551 AD). This will however be difficult for the time period of 1241-1978 due to the frequency of NH eruptions and the disturbance of EQ eruptions on the climate signal of NH eruptions. This will be discussed in detail and a clear comparison between five and eight eruption results made.

2) Second issue: This is true. However, this study is not a modelling study and therefore we cannot use the model, that forms the basis for the atmospheric circulation reconstructions used (by Sjolte et al. (2018)), to do further experiments. We are merely using the selected reconstruction parameters to assess the signal identified in the stable water isotopes of Greenland ice cores and assign the climate signal to a specific weather regime if possible. This important point needs clarification that will be added to a revised manuscript.

3) Third issue: We will add more precise discussions on previous model studies that are relevant to ours and clarify the novelty of ours.

In addition here below are the authors response (AR) to the additional comments:

Introduction: please provide a few references to substantiate it (e.g. review by Robock2000, Timmreck et al. 2012; Swingedouw et al. 2017). AR: Ok.

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). AR: Yes, we are aware and will clarify this.

l. 83: replace "compliment" by "complement". AR: Ok.

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. AR: That is true. However, since we are studying if specific weather regimes are present after volcanic eruptions and if they can be identified in Greenland ice cores by using the reconstructions as a reference (and different method), this needs testing.

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. AR: We thank the reviewer for his detailed eye and indeed an improvement is needed. Monthly mean standardized data is used - and to compare with Greenland ice cores we only use winter data/output. This section will be improved and explained in more detail in a revised version.

L. 121: what is “r”? Correlation I assume. Here I do not get what is done. AR: Yes, correlation. In order to assign a post volcanic atmospheric circulation field (retrieved from the reconstructions) to one of the four main weather regimes in the NA with some certainty, say in year 2, we compare the post-volcanic field to the 1200-year average weather regimes retrieved by clustering ECHAM5 SLP by calculating the correlation coefficient r. This will be further clarified in a revised manuscript.

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

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 maybe 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. AR: As mentioned in the reply to the main comments above, this will be improved in a revised version.

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. AR: This we will do and add a note on this in the manuscript.

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

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. AR: Indeed that is true and it would be ideal if possible. However, the four weather regimes do not emerge in the clustering of the reconstructed d18O, due to the variability of the reconstruction being biased towards NAO-type variability as well as weather patterns and isotope patterns not being a one-to-one match.

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.. AR: Indeed that is true, but we would also like to emphasize that we are not using a simulation in our study but an atmospheric circulation reconstructions (along with Greenland ice cores). Since this seems to cause some confusion we will clarify this in a revised version.

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? AR: That is true, we will rephase and look at the reconstruction statement more closely.

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). AR: Here we might have stepped further than reasonable concerning the fact that we are not doing model experiments. This will be altered for to avoid over-interpretations.

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? AR: Indeed we can, an excellent point that will be added in a revised version.

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. AR: Again, a good suggestion and will be added in a revised version. 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 high-lighted 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). AR: Yes, we agree and will add such a note in a revised manuscript.

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. AR: We do agree and aim to do exactly that in our future research. However, since this is particular study is not a modelling study this will be difficult except suggesting possible mechanism.

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