RC 4 with responses

General

The paper investigates the climatic and historical context of a tropical volcanic eruption at the beginning of the 17th century using a combination of proxy, historical and modeling evidence. The authors initially hypothesize a prominent influence of the state of the North Atlantic Subpolar Gyre (SPG), initiating a sustained cooling over Europe. Authors conclude that the SPG could have an important role. However, their results remain inconclusive taking into account their combined evidence using different reconstructed metrics (e.g. Baltic Sea Ice, North Sea Winds).

The SPG-shift hypothesis came from the previous studies by Moreno-Chamarro et al. (2017a, 2017b). It is not an original hypothesis of this manuscript; nor does this manuscript set out to prove the existence of this mechanism. Instead, building on past studies supporting the SPG-shift mechanism and using the new data drawn from high-resolution proxies and historical observations, we aim to (1) specify climatic and environmental conditions ca.1600 in order to (2) determine whether such an SPG shift could have been triggered by the 1600 Huaynaputina eruption and thereby to (3) evaluate how to compare data from historical climatology (e.g., written descriptions and proxies from human activities) with simulations. It appears that most of the referee's subsequent concerns and recommendations derive from this misimpression of the manuscript's scope and intentions.
We propose to clarify the language of the introduction to avoid this misimpression among future readers.

The basic structure of the manuscript is not in an optimal shape. The reader is confronted with different parts of a classical paper, resulting in a mix of background information, methods, data sets and conclusions presented in the different sub-chapters. This might be sourced in the fact that it is an interdisciplinary research paper. However, some crucial statements and physical mechanisms contradict in different sections of the paper.
Therefore I suggest that i) the manuscript should be completely re-written ii) the proxy- and historically derived hypotheses should be clearly formulated in the beginning and (statistically) tested by a more comprehensive suite of available CMIP6 simulations [e.g. those CMIP6 model simulations with appropriate ocean models and according horizontal and vertical resolution – in the present manuscript only one Earth System Model is used] and iii) the different parts and disciplines should be conceptually better coordinated.

The SPG-shift hypothesis was not derived from the high-resolution proxies and historical observations examined in this study. The SPG mechanism was derived from modeling in previous studies and tested against other longer-term, lower-resolution reconstructions in previous studies (Moreno-Chamarro 2017a, 2017b). Given their greater spatial coverage, homogeneity, and continuity, the longer-term, lower-resolution reconstructions considered in those previous studies were more suited to testing the hypothesis that an SPG shift such as that found in the simulations was the cause of persistent winter cooling in the real world. However, those reconstructions were unable to test whether the 1600 eruption was the trigger for such an SPG shift. Although such an SPG shift occurred more often in simulations with eruptions, it also occurred in 2 simulations without eruptions.

Thus, previous studies left open an interesting question: If the SPG-shift mechanism did occur, was it triggered by the 1600 Huaynaputina eruption?

Moreno-Chamarro et al. 2017a, 2017b tried to find an answer to this question through sensitivity experiments in the model world, but they did not reach a definite conclusion. Our present study has therefore drawn on new high-resolution proxies and historical observations to address this question. We first attempted to identify within the simulations some necessary set of initial conditions for the SPG shift that might be tested in those observations. However, no such initial seed could be identified. Because we were unable to determine some testable initial seed for the SPG shift (such as a state of the NAO), we focused our study on identifying the precise timing of climatic and environmental changes associated with the SPG shift. Although timing alone could not definitely determine whether or not the eruption triggered the SPG shift, it could add strong weight to either inference. If climatic and environmental changes associated with the SPG shift had begun several years after the eruption, as found in the simulations, then we could have concluded that the eruption trigger was substantially more probable *a posteriori*. In fact, we found that those changes -- including increased sea ice and winter cooling -- commenced before the eruption. This finding does not eliminate the possibility of an eruption trigger, since the pre-eruption changes could have arisen due to internal variability or other unidentified forcings. However, it makes an eruption trigger less probable *a posteriori*. This investigation has also enabled us to better explain the societal impacts of climatic variability and change ca.1600 and to make recommendations for future comparisons of simulations and data from historical climatology.

Within this scope, the manuscript adopts a standard structure (Introduction, Methods, Results, Discussion, Conclusion). The only exception is an additional section explaining the modeling and SPG-shift mechanism in the previous Moreno-Chamarro 2017 studies (section 2). This additional section prevented the introductory section from becoming overly long and difficult to follow. It also enabled us to explain the previous modeling for a wider audience, including paleoclimatologists and historical climatologists, in keeping with the interdisciplinary scope of the article. It was not intended as a substitute for a "methods" section about modeling. No new modeling was performed for the present manuscript, and any such modeling would be beyond the means and scope of this study.

In the following I provide some suggestions how to re-structure the manuscript.

Specific

Introduction/Basic Concept

The introduction should contain the basic background information to understand the concept and eventually the conclusions of the paper. Therefore it is of ultimate importance to be conceptually sound and also introduce the main concepts elaborated in the manuscript. For instance, the SPG is not introduced at all. Also, the model used is never explained related to the fact how the model is capable to realistically simulate the SPG. For this a dedicated methods and data section is necessary where the different components of the paper are comprehensively explained. In the current version the first part is a mere repetition of results already published elsewhere (Morene-Camaro et al., 2017) even including the same set of figures (cf. Figure 1) that is already published.

There was no new modeling performed for this study; and additional modeling would be beyond its scope. For the modeling methods, please see the 2017 Moreno-Chamarro et al. studies. The methods in this study concern the reconstruction of climate and environmental conditions from high-resolution proxies and historical observations and their comparison to previously developed simulations.

This also relates to the 2nd important concern: The authors only use one model from the CMIP6 suite. For their period under consideration a larger number of simulations, also for ocean models, is available in the Earth System Grid Federation (ESGF) platform. Since results for MPI already have been published and this very mechanism might be evident only in the MPI model, the question is whether all of the CMIP6 models, or at least those with similar horizontal resolution show a similar response. This is even more important when comparing model derived results to real-world derived hypotheses to test the robustness of the model derived results and to finally discriminate between I) internal vs. forced changes and ii) model-vs-model intrinsic variability related to the individual structure of the Earth System Model.

This study has drawn on new high-resolution proxies and historical observations to specify conditions around the time of the 1600 Huaynaputina eruption and thereby determine whether that eruption could have triggered an SPG shift similar to that found in previous simulations. There was no new modeling performed for this study, and additional modeling to run tests of the SPG mechanisms would be beyond its scope.
The previous studies (Moreno-Chamarro 2017a, 2017b) used data only from the MPI-ESM-P model (in its CMIP5 configuration) because it is the only one for which we had a dedicated set of sensitivity simulations to explore the impact of the Huaynaputina eruption on the climate. Including other last millennium simulations would have blurred the discussion because it would have meant dealing with different models and model sensitivity to external forcings, different volcanic forcings, different background climate states at the time of the eruption, and different internal variability. The only model with a close enough setup is the CESM last millennium ensemble, which includes sensitivity simulations with different external forcing. However, it is not clear whether this model, the CESM-CAM5_CN, shows any sensitivity in the subpolar region to the volcanic forcing (Otto-Bliesner et al., 2016), although a newer model version shows cooling in the North Atlantic during the Little Ice Age associated with a SPG weakening (Zhong et al., 2018). There is currently limited data for other CMIP6/PMIP4 last millennium simulations available at the ESGF nodes (and not for the MPI-ESM model).

References:

Otto-Bliesner, B.L., Brady, E.C., Fasullo, J., Jahn, A., Landrum, L., Stevenson, S., Rosenbloom, N., Mai, A. and Strand, G., 2016. Climate variability and change since 850 CE: An ensemble approach with the Community Earth System Model. Bulletin of the American Meteorological Society, 97(5), pp.735-754

Zhong, Y., Jahn, A., Miller, G.H. and Geirsdottir, A., 2018. Asymmetric Cooling of the Atlantic and Pacific Arctic During the Past Two Millennia: A Dual Observation-Modeling Study. Geophysical Research Letters, 45(22), pp.12-497.)

Methods/Hypotheses

The basic setup of the authors using a combination of different disciplines to address a certain questions is a good asset. However, the potential and per-requisites should be formulated conceptually more sound. For instance, in the present setup the hypotheses should be derived based on proxy and/or historical evidence. In a second step a potential physical mechanism should be motivated explaining the initially formulated hypothesis (e.g. changes in SPG and its impact on European temperatures). In a third step this should then be tested in the model world, most preferentially using a suite of comprehensive Earth System Models simulating this period and using state-of-the art statistical tests (for instance Boot Strap methods using control simulations to derive reference climatic states). In the present version this concept is reversed and the initial hypotheses are derived from the climate model. In general, this is also possible but it is of ultimate importance to state this clearly and also present a way of how this (set of) hypotheses is falsified.

The formulation and testing of a new climate mechanism is beyond the scope of this study. The 2017 Moreno-Chamarro et al. studies already developed and tested the mechanism examined in this study. Our primary question is: If the SPG-shift mechanism examined in those previous studies did occur, could it have been triggered by the 1600 Huaynaputina eruption? To answer that question, we have used high-resolution proxies and historical observations best suited for specifying local and regional conditions ca.1600.

The lack of a sound statistical testing scheme and a careful inspection of the different conclusions derived in specific parts of the manuscript results e.g. in the following contradicting statement:

Moreno Chamarro et al. (2017b) found consistencies at multidecadal scales between simulations with a weakened SPG and reconstructed changes in several geophysical variables of the North Atlantic after ca. 1600 CE. The study did not conclude that the late 16 th -century volcanic cluster was necessary for the SPG shift, which was instead mainly attributed to intrinsic variability of the simulated climate system. Sensitivity simulations of the period 1593-1650 with no volcanic forcing yielded SPG shifts similar to those in the volcanically forced simulations. [cf l. 141 ff]

vs.

1. *This study has examined high-resolution proxies and historical observations to investigate whether the 1600 Huaynaputina eruption triggered persistent cooling in the North Atlantic region by initiating a regime-shift of the North Atlantic subpolar gyre toward a persistent weak phase in the early 17 th century,**as shown by paleoclimate model simulations**. [cf. l. 371 ff.]*

The conclusion derived from the model analysis showed that the shift might be simply due to intrinsic or internal climate variability. However, in the conclusions authors sate the volcanic eruption triggered the regime shift initiated by the volcanic eruption. Moreover, the paleoclimatic model simulations only relate to the MPI-ESM model the authors used for their investigations.

The “as shown by paleoclimate model simulations” refers to the second part of that clause (“regime shift of the North Atlantic subpolar gyre toward a persistent weak phase”) rather than the first part of the clause (“eruption triggered”). Therefore, there is no contradiction. We will correct the sentence to remove any ambiguity.
As our discussion makes clear, we do not believe our results have resolved the issue of whether the 1600 Huaynaputina eruption was the trigger for persistent winter cooling. In fact, we have found that winter cooling and expanded sea ice preceded the eruption, thus reducing the probability that an eruption triggered an SPG shift in the real world.

An important information that was also never mentioned in the manuscript is that a number of volcanic reconstructions is available that have already been used for simulating the impact of volcanic eruptions on climate (e.g. Crowley and Unterman (2013); Gao et al. (2008), Toohey et al. (2016)). Especially the strength of larger tropical eruptions can vary up to a factor of two within the change in aerosol optical depth (AOD), the most important radiative physical moment in the stratosphere in the context of explosive volcanic eruptions. This should and could be taken into account by including a 2nd ESM simulation (e.g. the CCSM4 CMIP6 model used the Gao et al. 2008 data set in contrast to the Toohey et al. 2016 volcanic data set used in the present simulation). Integrating a 2nd set of simulations would help to better assess the impact/change of the SPG on the climate in Europe in the different Earth System Models.

Details on the volcanic forcing were already provided in Moreno-Chamarro et al. (2017a, 2017b) and in more detail in Jungclaus et al. (2014). These can be included again in the revised manuscript, if necessary. Testing the SPG mechanism and its sensitivity to volcanic forcing in additional climate models would be beyond the scope of the paper.

Statistical Tests

The general setup of the manuscript would greatly benefit by implementing a statistical test scheme with a clear formulation of a Null hypotheses that is falsified by an appropriate statistical test. Especially in the virtual world of the Earth System model this could be (quite easily) achieved. An option is for instance to design a test in the context of a bootstrap method: The null hypothesis is that the SPG has no influence on European temperatures. The nominal level can be set even to a two-sided test with 5 % . The test can now formally be carried out using sub-samplings of the different trajectories of the SPG in terms of block bootstrap by using control simulations. The test should be applied to the canonical pattern between the state of the SPG and European temperatures. If the sub-sampling leads not to statistically significant negative deviations of European temperatures in the presence of a shift in SPG, then the null hypothesis cannot be rejected. Eventually, these tests should be carried out for simulations with and without volcanic forcing.

It is not our hypothesis in this study that the SPG does/doesn't influence European temperatures. Our question is: If the SPG-shift mechanism examined in previous studies did occur, could it have been triggered by the 1600 Huaynaputina eruption?
We attempted to identify precise initial conditions for the onset of the SPG-shift mechanism but were unable to find such an initial seed. It is precisely because we were unable to determine a precise set of initial conditions required for the SPG shift (such as a state of the NAO) that we focused our study on identifying the precise timing of climatic and environmental changes associated with the SPG shift. Although timing alone could not definitely determine whether or not the eruption triggered the SPG shift, it could add strong weight to either inference. If climatic and environmental changes associated with the SPG shift had begun several years after the eruption, as found in the simulations, then we could have concluded that the eruption trigger was more probable *a posteriori*. In fact, we found that those changes -- including increased sea ice and winter cooling -- commenced before the eruption. This finding does not eliminate the possibility of an eruption trigger, since the pre-eruption changes could have arisen due to internal variability or other unidentified forcings. However, it makes an eruption trigger less probable *a posteriori*.

Physical mechanisms
The authors mention a couple of (important) physical mechanisms that might support their hypotheses.
A first example relates to the NAO: at several places in the manuscript (cf. l. 69; l. 297) the authors mention the North Atlantic Oscillation as physical mechanisms explaining part of the temperature variability and being important also in the context of volcanic eruptions citing different authors. I wonder why the authors do not briefly explain the main mechanism suggested for the NAO in the first winter after volcanic eruptions (so called mid-Winter warming in Europe because of a positive state of the NAO, Kirchner, 1999; Zambri et al., 2017;). What mechanism is giving rise to such a response ? How robust is such kind of response and what effect does it exert on the winter temperatures in Europe ?

We thank the reviewer for drawing this issue to our attention. We propose to expand discussion of the NAO/AO between lines 149 and 150. As discussed in Hernández et al. 2020, the precise NAO values are uncertain but both the Ortega et al. 2015 and Trouet et al. 2009 studies indicate roughly average NAO index values in the 1590s, declining in the decade following the 1600 eruption. Thus, the state of the NAO would not appear to be a strong explanation for the cooling before the eruption; nor is there evidence for an NAO+ response following the Huaynaputina eruption, unlike some other tropical eruptions. As recent studies indicate (Bittner et al., 2016; Coupe and Robock, 2021) a post-eruption NAO+ response with Eurasian winter warming appears to be contingent on tropospheric conditions at the time of the eruption rather than an automatic response to stratospheric aerosols.

We can also include a plot of the NAO index during 1590–1610 in the different ensembles (that is, with and without volcanic forcing and with and without an SPG shift) to illustrate that neither phase dominates either before or after the Huaynaputina eruption.

Sources:

Bittner M, Schmidt H, Timmreck C, Sienz F. Using a large ensemble of simulations to assess the Northern Hemisphere stratospheric dynamical response to tropical volcanic eruptions and its uncertainty. Geophys Res Lett. 2016;43(17):9324–32
Coupe, J, and Robock, A.: The influence of stratospheric soot and sulfate aerosols on the Northern Hemisphere wintertime atmospheric circulation. J. Geophys. Res. Atmos., 126, e2020JD034513, doi:10.1029/2020JD034513, 2021
Hernández, Armand, Celia Martin-Puertas, Paola Moffa-Sánchez, Eduardo Moreno-Chamarro, Pablo Ortega, Simon Blockley, Kim M. Cobb, et al. "Modes of Climate Variability: Synthesis and Review of Proxy-Based Reconstructions through the Holocene." Earth-Science Reviews 209 (2020): 103286. https://doi.org/10.1016/j.earscirev.2020.103286.

A second mechanism mentioned in this context relates to changes in blocking frequencies that are believed to be larger in the aftermath of volcanic eruptions and/or are an important mechanism explaining cold and very cold winters over (western) Europe (l. 66 ff). The authors argue that the blocking is independent to changes in the NAO. This is a bit surprising, because the NAO is the leading mode in Europe's winter variability and changes in the blocking should also effect the state of the NAO.

The increase in blocking frequency during the Little Ice Age was not associated with a persistent change in NAO phase in the simulations (Moreno-Chamarro et al., 2017b). Although a link between the NAO and blocking frequency has been established from observations, it usually refers to interannual variability. The time scales considered in the model were much longer (multidecadal to centennial) for which the NAO-blocking link might work differently. Furthermore, the model might misrepresent their link, but this has not been explored and such assessments would be beyond the scope of the paper.

A third mechanisms relates to the role of sea ice (l 220 ff.). First, also sea ice concentrations can show a spatially heterogeneous pattern, especially when the entire North Atlantic region including Greenland is taken into account. In this context changes in the NAO can lead to dipole patterns with anomalous high sea ice around Greenland, and low sea ice over western Europe and vice versa. If this is not the case at least it should be motivated which canonical Circulation-sea ice patterns could lead to a spatially homogeneous response and/or whether direct radiative changes caused by volcanic eruptions could compensate or offset dynamically induced dipole patterns.

The dipole response of sea ice to NAO found in observations operates on annual to multi-annual time scales (Bader et al., 2011). The sea ice response follows a multidecadal to centennial reduction in the SPG and the related oceanic heat transport in the model, as explored in Moreno Chamarro et al. (2017a,b). The two mechanisms can be complementary since they operate on different time scales.

Reference:

Bader, J., Mesquita, M. D., Hodges, K. I., Keenlyside, N., Østerhus, S., and Miles, M.: A review on Northern Hemisphere sea-ice, storminess and the North Atlantic Oscillation: Observations and projected changes, Atmos. Res., 101, 809–834, https://doi.org/10.1016/j.atmosres.2011.04.007, 2011

A last mechanism I would like to mention here relates to the direct vs. indirect effects of volcanic eruptions on climate:
The summer cooling is, by contrast, absent in the no-shift ensemble, which comprises mainly simulations without volcanic forcing (8 out of 12), the two ensembles show minor differences in oceanic variables such as the barotropic stream function and winter sea-surface temperature in the North Atlantic, which are weakly impacted by the volcanic forcing in the short term. Larger differences in these variables between the ensembles emerge over the following decades, particularly after the 1610s, in association with the SPG slowdown, as shown in Figure 3.[l 162 ff.]
Here the authors even state that the summer cooling is absent in those simulations without volcanic forcing. Therefore the question remains as to whether a change in SPG is really necessary to initiate the sustained cooling. Also, a more objective formulation how the shift is quantified would be necessary to test if the deviation from the mean state is large enough to speak of a regime shift.

We say the summer cooling associated with the direct radiative effect from the volcanic forcing is absent in those simulations without volcanic forcing. Therefore, there is no contradiction here. We will clarify the wording to avoid possible confusion on this point. A SPG weakening is directly related to a multi-decadal to centennial cooling of the subpolar North Atlantic (Moreno-Chamarro et al., 2017a,b). However, we address a different question in this paragraph: whether the shift and no-shift ensembles show different short-term responses after the Huaynaputina eruption that can later result in an SPG shift.

Synthesis with proxy and historical reconstructions
In its present form the different chapters are not integrated into a consistent manner. Although this step is usually the most demanding, authors should at least indicate that their approach in comparing reconstructions with the world of the climate model is not a state-of-the art approach. For instance, forward models would be at hand to directly simulate respective proxies. In its present form the authors just use the information directly from the climate model without any further (advanced) processing. This represents an additional source of uncertainty in their conceptual framework.

As previously explained, our study does not aim to develop a new climate mechanism from paleo data and test it in the model world (or vice versa). Instead, we draw on new high-resolution proxies and historical observations to examine whether a previously derived and tested mechanism could have been triggered by an eruption.

Ours is one of the first studies to compare simulations with data from historical climatology (e.g., historical written descriptions and proxies from human activities) in this manner. Thus, the question of whether it is “state of the art” is a complicated one. On the one hand, these historical climatology data lack the spatial coverage, continuity, and homogeneity found in many paleoclimate reconstructions, and thus they are often less suited to deriving and testing climate mechanisms. On the other hand, they provide new kinds of precise, localized information unavailable in paleoclimate reconstructions and may therefore have unique and valuable applications in model-data comparison studies. In this case, our application of historical climatology concerned the timing of a possible mechanism to determine whether it could have been triggered by an eruption. Although our results turned out to be inconclusive, we anticipate they will guide future studies, as explained in lines 376-392.

The use of forward modeling for historical climatology is new and rare compared to that for paleoclimatology, since it presents the additional challenge of modeling what would be observed and recorded by a human observer. Nevertheless, it is feasible and may be a good plan for a future study study; therefore, we will include this suggestion in our discussion section.