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Comment on cp-2021-82

Anonymous Referee #4

Referee comment on "The 1600□CE Huaynaputina eruption as a possible trigger for persistent cooling in the North Atlantic region" by Sam White et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-82-RC4, 2021

Review for the manuscript

The 1600 Huaynaputina Eruption as Possible Trigger for Persistent Cooling in the North Atlantic Region

by Sam White et al.

Submitted for publication in *Climate of the Past*

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 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.

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.

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.

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 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.

- *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.

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 2^{nd} 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 2^{nd} 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.

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.

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 ?

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.

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.

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.

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.