Responses to Referee #1

I recommend major revisions for this paper. It ignores some relevant research work on this topic, which needs to be addressed. And fundamentally, I don't see what is really new here. We already know, as can be seen in the many previous papers referenced here, that volcanic eruptions can affect ENSO and the polar vortex. What does using one imperfect climate model, and analyzing the results in detail teach us? Shouldn't the study compare multiple climate models? In any case, the analysis needs to be redone, considering the points below.

Thanks very much for your constructive comments on our manuscript. In terms of the novelty, our study presents new findings in view of previous studies. We will better highlight new findings in the revised manuscript with providing more discussions in comparison with previous studies. Firstly, we explore much details of temporal evolutions of ENSO according to different eruption latitudes and the corresponding physical mechanisms responsible for the diversity. Secondly, we provide a first quantification of ENSO contribution to global monsoon drying responses depending on eruption latitudes, which has important implications for decadal prediction and geoengineering. Thirdly and more importantly, we find varying responses in polar vortex and Arctic Oscillation-induced surface climate following tropical, northern and southern eruptions with distinct physical processes identified.

Regarding the limitation of single model simulations, we argue that utilizing single models with multiple ensembles has an advantage over multi-model studies, particularly in separating robust signals from internal variability influences and exploring detailed physical mechanisms that drive the signals. Using multi-ensemble single model simulations has been accepted as useful for studying the volcanic influences given its rare occurrence and large influences of internal variabilities. In this respect, CESM LME provides the largest ensembles simulated with all and individual external forcings for the last millennium (Otto-Bliesner et al., 2016) and have been widely used in recent studies (e.g., Stevenson et al., 2016; Mishra and Aadhar 2021; Benton et al., 2022). We will provide a discussion about the model dependency issue in the revised manuscript.

The climate model has not been evaluated for its ability to simulate observed El Niños, and its Arctic Oscillation climate. How well does CESM simulate El Niño? This has to be tested and documented before we can trust its El Niño response to volcanic eruptions. How well does it do this for the recent observational period?

Thanks for the good point. There are some previous studies that evaluated CESM for ENSO and AO simulations. For example, Stevenson et al. (2016) showed that CESM-LME can simulate ENSO intensities better than previous versions but with an overestimation compared to observations. Although our CESM-based climate responses to volcanic eruptions seems robust when compared with proxy observations and other models (e.g., Adams et al., 2003; Maher et al., 2015), we agree that model skills need to be checked, which would increase the robustness of our findings. In the revised manuscript, we will add information about model performances regarding ENSO and AO simulations and teleconnection responses based on available literatures as well as further analyses and will also discuss its implications.

The southern and tropical set of volcanic eruptions seem to produce the same forcing of the climate system. Are they really distinct, and why are they not considered together? The aerosol distribution (Fig. 1g) looks very much like the tropical one (Fig. 1a) with the largest loading in the Tropics. How can you justify considering these eruptions separate from tropical ones? Also the Fig. 6 results for southern and tropical eruptions are the same. They really should be combined in the analysis.

The southern and tropical eruptions look similar in the meridional aerosol distributions, which is due to weaker asymmetric aerosol loadings in southern eruptions. However, they have clear differences in inter-hemispheric aerosol loading ratio as well as surface responses. Also, although they are relatively similar compared to northern eruptions (Fig. 6), they show distinct meridional distributions of lower stratospheric temperature and zonal wind responses. In the revised manuscript, we will explain these differences more clearly with improving figures accordingly.

The figures need to be revised, removing the bogus horizontal lines and putting black dots on the insignificant results, not the ones that are significant, so we can actually see them.

The pdf you provided has a very small font and the text and figures only cover part of each page. I find this annoying. I advise you in the future to make it easy for reviewers – not hard.

We will improve figures as suggested. Sorry for the small font, we used the template but will increase font size throughout.

You have ignored the work of Coupe and Robock (2021), who found that in general the CESM Large Ensemble did not correctly simulate the El Niño or the winter warming after the three most recent large tropical eruptions, but did correctly simulate the winter warming if SSTs were specified. You have to reconcile your results with these.

Coupe, Joshua, and Alan Robock, 2021: 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.

Thank you for notifying us of the interesting study. We will check influences of three recent eruptions on the winter warming in comparison with Coupe and Robock (2021) and thereby discuss possible role of SSTs in shaping the relation between polar vortex, AO and winter warming responses.

The paper continually refers to northern eruptions and southern eruptions, but does not define them until lines 97-99. The definition needs to be given the first time these phrases are used. If they are eruptions that occur in high latitudes in the respective hemispheres, above what latitude? And would the 1982 El Chichón eruption be a northern one, since the aerosols stayed in the Northern Hemisphere?

We will make this definition clear in the revised version. Actually, the volcanic forcing information comes from polar ice core reconstructions and exact locations where they occurred are unknown for many eruptions. We will add more information on the eruption latitude as much as possible. Although El Chichón eruption occurred at tropical region, the aerosols stayed much in the Northern Hemisphere. That's why it was classified into tropical or northern eruptions depending on studies. We will clarify this point as well in the revised manuscript.

Figs. 3a and 3b have blue contour lines that are not explained in the caption. What are they?

They represent climatology of seasonal precipitation. We will revise the caption. Thanks.

Section 3.2.1 goes into detail about ocean circulation changes, but never explains why the El Niño after the volcanic eruptions is delayed for a year, in contrast to observations after 1963 Agung, 1982 El Chichón, and 1991 Pinatubo. If the model is wrong about this, how are we supposed to accept the results of the paper?

Thank you for the good point. We will further discuss this issue of observation-model discrepancy in El Niño occurrence timing in the revised manuscript. One factor could be different pre-eruption oceanic conditions, as discussed in Paik et al. (2020), where warmer water volume in the western equatorial Pacific might induce faster occurrence of El Niño in the observations. Many model studies (e.g., Maher et al., 2015) showed that El Niño responses is delayed for a year after volcanic forcing is its peak, which will be mentioned as well in the discussion.

The figures are drawn with GrADS, but many have horizontal lines that should not be there. You need to remove all of them from the figures, and this can be done in several ways. Check the GrADS forum for the solutions.

We will revise figures following your suggestion.

I don't understand why 10 mb is chosen for analysis of stratospheric wind anomalies. This is typically 26 km, much too high to be of significance for tropospheric influence. I know this model has a peak response there, but is it correct?

We chose 10 mb to analyze polar vortex responses following previous studies (e.g., Bittner et al. 2016). We will add related information in the revised manuscript.

Polvani et al. (2019) claim that even an eruption the size of 1991 Pinatubo was not large enough to produce a significant change in the AO. How do you reconcile your results with theirs, considering you used the same climate model?

Polvani, L. M., Banerjee, A., & Schmidt, A. (2019). Northern Hemisphere continental winter warming following the 1991 Mt. Pinatubo eruption: Reconciling models and observations. *Atmospheric Chemistry and Physics*, **19**, 6351–6366. https://doi.org/10.5194/ acp-19-6351-2019

Actually, the volcanic eruptions mainly used in our study are much stronger than 1991 Pinatubo eruption. We will add a discussion by checking polar vortex, AO and winter warming responses following Pinatubo eruptions from CESM-LME and comparing results with those from Polvani et al. (2019).

In addition, the 23 comments in the attached annotated manuscript need to be addressed.

We will address all those comments in revised manuscript. Thanks very much again for your thoughtful comments.

References:

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