Response by authors

We thank Reviewer #1 for their appreciation of our study and for their helpful comments on the discussion paper.

We are convinced that a revision of our original manuscript that accounts for the Reviewer's criticism is feasible. Specifically, to address the four major points raised by the Reviewer, in the revised manuscript we plan to:

- Better put CESM1-WACCM in the context of state-of-the-art chemistry climate models (see also our response below), also referring to recent studies using the same model version used here (e.g., Jiang et al., 2024; Li et al., 2023; Clyne et al., 2021).
- Consider rephrasing critical aspects of the manuscript (such as the title and the abstract) to de-emphasize our experiments as volcanic eruption ones. A possibility we are considering is to refer to stratospheric aerosol injection experiments, instead. This should also solve other questions about the comparability of our results with observations from historical events raised by the Reviewer in their specific comments;
- Provide additional information about the spatio-temporal characteristics of volcanic aerosol and associated radiative flux anomalies;
- Better illustrate and discuss the spatial structure of anomalies involved in the bottom-up mechanism, in particular better clarify the role of the land-sea temperature contrast in the North Pacific/Western North American region, which appears to be at the core of the wave activity anomaly.

More specifically on the realism of the model version used here with respect to current versions, focusing on the atmospheric component WACCM, we will refer to Gettleman et al. (2019), who provide an extensive comparative analysis of versions 4 and 6, with a detailed description of biases in both versions. There are obvious improvements in WACCM6 compared to WACCM4, such as a higher resolution, an interactive QBO (nudged in our simulations) and more evolved chemistry. One of the important aspects of atmospheric variability for our study is the simulation of NH sudden stratospheric warming events, for which WACCM6 has a slightly higher frequency of occurrence than WACCM4 (mostly due to more late winter simulated events). We will also refer to papers comparing general climatic features of CESM1 and CESM2 (e.g., Mills et al., 2020; Holland et al., 2024): as typically found also for other models, the papers point to the critical role of simulated cloud responses and feedbacks for explaining differences across results from different model generations.

The Reviewer also expresses some concerns on the stratospheric cooling over the polar cap detected in the first winter in both the cpl and atm-only experiments. We do acknowledge that this needs to be more clearly presented and discussed. In brief, during boreal winter there is no aerosol-induced stratospheric warming at high latitudes, which is why the warming is only detected at mid-latitudes where the aerosol still has an impact on shortwave radiation. As we mentioned in the original submission, the polar vortex strengthening then encloses cold polar air at high latitudes that explains the stratospheric cooling identified over the polar cap. In addition to better describing this mechanism in the revised manuscript we will add more analysis to support our interpretation.

The last major comment of Reviewer #1 concerns the aerosol induced stratospheric warming where according to him it should be weakened in the second and third winter due to decreased aerosol concentrations that in turn could cause the detected polar vortex weakening in the 2nd and 3rd winter. This is a valid point. However, in order to address this concern we would at least need an additional cpl experiment where the aerosol concentration is held at constant maximum values over, say, 2-3 years before declining. Indeed that would be of interest but out of the current scope. Thus we will leave it for future studies and in stead add discussions on this possibility in a revised manuscript.

The concerns that Reviewer #1 has on the definition and number of SSWs are closely related to the concerns of Reviewer #2. We acknowledge that we need to expand the definition of SSWs that we use in the identification algorithm based on Charlton and Perez, 2007 and detailed therein. Regarding the number of SSWs we refer to Figure 1R and the associated answer in our response to Reviewer #2.

Furthermore, all the specific comments by the Reviewer are pertinent, and we will account for all of the suggestions and requests of clarification in the revised manuscript. In particular, also following the suggestion by Reviewer #2 to restructure the results section of the paper, in the revised manuscript we plan to articulate it as follows: first, present the whole dynamics in the cpl experiment for the three post-eruption winters, in sequence; then, present the relevant dynamics in the atm-only experiment to disentangle the role of the bottom-up mechanism in the second post-eruption winter only. We will also largely rewrite the discussion. We plan to move most parts about the dynamical interpretation to the results section (also following a comment in this direction by Reviewer #2), to refocus the discussion on limitations of our study and on broader implications for our general understanding of aerosol-climate interactions. All statements will also be checked for correctness and clarity, tackling all the technical comments by the Reviewer.

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