Response to editor's comments

"Mechanisms of hydrological responses to volcanic eruptions in the Asian monsoon and westerlies-dominated subregions" by Zhihong Zhuo et al.

We are very grateful to Editor Allegra N. LeGrande for your efforts and time. We have considered the comments thoroughly and carefully read and compared this study and studies you mentioned, i.e. Colose et al. (2016) and Zanchettin et al. (2022). Considering differences in main focuses of different studies, we did minor revisions in the manuscript, and give our detailed reasons here. The list of the editor's comments (in red) as well as our responses (in black) are listed below. The revised texts are shown in blue.

Upon re-reading this manuscript and puzzling over what seems like odd figures, I realize that Figure 2 is really rather inconsistent with Colose et al 2016 Figure 1. This becomes important for this whole paper because in the present Zhou analysis, there is really only 1 example of a 'large' southern hemisphere eruption. I don't really understand why this is, so I replotted the volcanic forcings for GISS (these are in AOD space, not Tg loading space, but the former is scaled to apply forcing to climate models). If we zoom in on the 1450's, we see NH (20-90N), TR (20S-20N), and SH(90S-20S) look like this… so why is it classified as GNH?

The criteria for classifying volcanic events is different between Colose et al. (2016) and this study. Colose et al. (2016) classified volcanic events into three categories: $ASYMM_{NH}$, $ASYMM_{SH}$ and SYMM, based on a "25 % difference in aerosol loading between hemispheres" criteria. As can be seen from the category name, it emphasized the symmetric and asymmetric forcing. However, in this study, the classifications, i.e. GNH and GSH, emphasize the aerosol injection in specific hemispheres, as was defined in the manuscript "Following Zhuo et al. (2020), we pick out volcanic events in 1300-1850 CE that have larger northern hemisphere volcanic aerosol injection (NHVAI) than that of the 1991 Mount Pinatubo eruption (17 Tg SO_2 based on the GRA volcanic forcing index) as GNH classification. To explore inversed hydrological impacts of interhemispherically asymmetric VAI, another classification, with volcanic events in 1300-1850 CE that only have southern hemisphere volcanic aerosol injection (SHVAI), is constructed as the GSH classification.". To meet the criteria, based on the Gao et al. (2008) volcanic forcing reconstruction, there is only one event with large SH aerosol injection than Pinatubo magnitude. Thus the criteria for the aerosol magnitude of the GNH and GSH classification is also different. As noted in the manuscript, considering the limited aerosol magnitude, but the same number of 12 volcanic events in the GSH classification, GSH classification mainly serve as a reference for the GNH classification, to show the difference between with and without NH aerosol injection.

For the 1452 event, we knew that, based on Gao et al. (2008) volcanic forcing reconstruction, it has 44.6 Tg and 92.9 Tg aerosol loading in the NH and SH, respectively. But climate in the NH is largely affected by the large aerosol injection in the NH, especially for regional climate responses. Thus, we still included it, as it meets the classification criteria with NHVAI much larger than that of the 1991 Mount Pinatubo eruption. We know that uncertainties exist from studies based on volcanic classifications, but also other sources. Zhuo et al. (2020) had a whole section discussing sources of uncertainty. We know that different volcanic events have different impacts, especially regional impacts, which largely depends on the source parameters of the volcanic eruptions and pre-eruption initial conditions of the climate system. These are important questions to be further explored and answered by the research community, and is also a focus of researches we are doing now.

For my previous comments…

Above, the authors do not mention the VolMIP results that specifically explore the impact of the initial ENSO state on the simulated response. https://gmd.copernicus.org/articles/15/2265/2022/

It is too important and relevant to the discussion. I disagree that the relatively small ensemble used here (and in the previous paper) is an adequate stand-in for ENSO discussion. Its needs its own section plus context woven throughout the text. The models used here DO INDEED appear in Davide's analysis… except that there was intentional sampling across a variety of initial conditions. It should be pretty straight-forward to 'look up' the range of responses, the implications for precip, convergence, temperature, etc. and carry on. This paper really does haveinsights into how initial condition projects onto temperature, heat fluxes, etc. And it is a MUCH larger sampling of volcanic forcing which is rather ephemeral, and sometimes hard to tease out beyond the background 'noise'/variability.

Sorry, the "Zanchettin et al., 2020" was a typo in the previous "response to editor" letter. In the previous revised manuscript, it was written as "...its potential future phases with improved protocol addressing the pre-eruption ENSO state (Zanchettin et al., 2022) can be valuable resource to investigate these questions.", which refers to the same VolMIP paper as suggested by the editor.

In Zhuo et al. (2020), it was said that "A limited number of ensemble members might also introduce uncertainty in the model results", but it didn't mean it is a stand-in for ENSO discussion. There is a whole paragraph in Zhuo et al. (2020) discussing ENSO and hydrological impacts. As investigated, the hydrological pattern remain largely unchanged after regressing out ENSO. Thus ENSO can not expain different hydrological responses between the westerlies and monsoon-dominated subregions of Asian. To understand this is the main focus of this study, instead of the connection between ENSO and hydrological impacts itself. Thus, we didn't discuss ENSO in order to avoid redundance.

Yes, part of the models used in this study appear in Davide's response, but this study used the CMIP5 model data, while models in Davide's study are in their CMIP6 version. As stated in Zelinka et al. (2020), "climate sensitivity is larger on average in CMIP6 (1.8–5.6 K) than in CMIP5 (2.1-4.7 K) due mostly to a stronger positive low cloud feedback, but recent consensus places it likely within 1.5–4.5 K". We evaluated hydrological responses in the AMR in CMIP5 before investigating the mechanisms. Definitely, it's valuable to further investigate the mechanisms with both CMIP6 and VolMIP data after evaluating the applicability of CMIP6 and VolMIP for regional studies. This was stated in the manuscript, "Future studies with PMIP4/CMIP6 data (Jungclaus et al., 2017) with updated volcanic forcing reconstruction (Toohey and Sigl, 2017) can contribute to better understanding on this topic." and "The Model Intercomparison Project on the climatic response to Volcanic forcing (VolMIP, (Zanchettin et al., 2016) and its potential future phases with improved protocol addressing the pre-eruption ENSO state (Zanchettin et al., 2022) can be valuable resource to investigate these questions."

We completely agree that Davide's study is very important, and VolMIP simulations are very useful for understanding the impact of volcanic eruptions that beyond the background noise/variability. When this study was conducted, the VolMIP data was not available yet. Actually, before conducting this study, I considered to use data from VolMIP and visited Claudia Timmreck at the Max Planck institute, but was told that it still takes time to conduct VolMIP simulation at that time. We didn't mention the detailed results of Davide's study on the impact of the initial ENSO state on the simulated response, because we didn't focus on understanding the initial state of ENSO on the hydrological impacts of volcanic eruptions. This is a very important and comprehensive research question that needs in depth investigation, which, actually, is the main focus of the study we are doing now. We don't want to mess it up with a brief discussion in this study. But we would like to emphasize the important of this topic referencing Davide's study, thus we revised the discussion as shown in line 338-349 in the revised manuscript:

The disagreement may result from different initial state of ENSO used in different studies. Through analyzing the model output from the Model Intercomparison Project on the climatic response to Volcanic forcing (VolMIP, Zanchettin et al., 2016), Zanchettin et al. (2022) found that the pre-condition of ENSO impacts temperature and precipitation responses after volcanic eruptions. This points out the importance of investigating the dependency of post-eruption ENSO and hydrological responses to pre-eruption initial conditions. More studies are also needed to understand the ENSO and hydrological responses to SH volcanic eruptions. Besides, the interaction between post-eruption ENSO and monsoon precipitation varies in different monsoon regions. A weakened African monsoon due to post-eruption cooling in Africa leads to the El Nino response after tropical eruptions (Khodri et al., 2017), but a more frequent occurrence of El Niño in the first boreal winter after eruptions lead to an enhanced EASM (Liu et al., 2022). The interaction among ENSO, monsoon and volcanic eruptions remains unclear. The VolMIP (Zanchettin et al., 2016) and its potential future phases with improved protocol addressing the pre-eruption ENSO state (Zanchettin et al., 2022) can be valuable resource to investigate these questions.

Here is a place where you could add an extra sentence wrt ENSO.

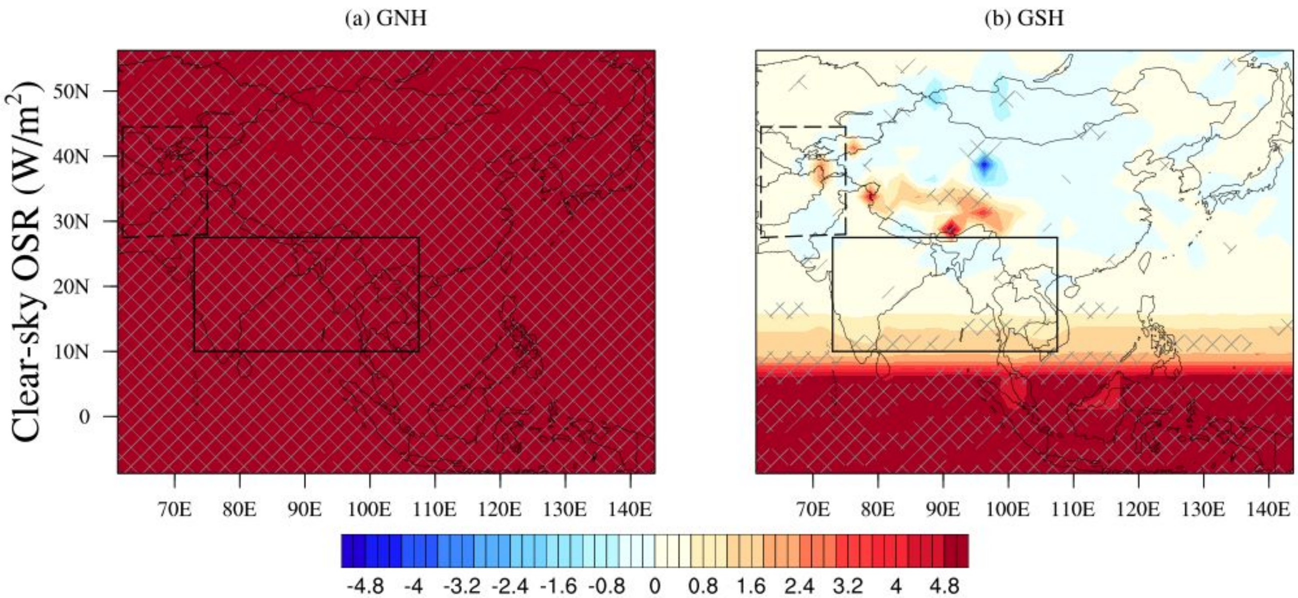
This paragraph focused on the mechanisms explored by previous studies and the needed studies focusing on regional scales of the AMR. We acknowledge that ENSO is related to the hydrological response, but it can not explain regional differences of the hydrological patterns in the AMR. Zhuo et al. (2020) investigated this and showed that consistent with Iles et al. (2013) and Iles and Hegerl (2014), regressing out the effect of ENSO only results in a lower amplitude response, but the temporal and spatial patterns remain largely unchanged.". Thus, it is not the ENSO that lead to different hydrological impacts between the RWA and RDA, whereas, the aim of this study is to explore the mechanism of the different regional hydrological responses. Thus, we do not think it fits to add a sentence wrt ENSO here, but as suggested previously by the editor, and combined with the comment above, we think it's important to discuss ENSO-related uncertainties and unresolved questions. Thus, we added a sentence wrt ENSO and Davide's study in the concluding remarks, as shown in the answer to the above comment.

The figures with the little dots and hashes that are one way for insignificant in some and the other way in others is not good either.

Like every coin has two sides, different figures also have different advantages and disadvantages. As answered previously, we tried different ways and finally used this. Because the others with slashes and plus signs show the significant results, then with dots for the insignificant results. These can show the main results. And with different signs, it can exactly show the change from highlighting significant data to insignificant data, and the dots won't affect the view of wind arrows. We think this works after thinking twice.

Something is really odd with this figure. Why is the whole of GNH hashed and GSH hashed hardly at all? Why is there a line right across 5N in GSH? Is OSR is really in excess of 20W/m2 throughout the whole tropics in GNH but near 0 for GSH? There could be a problem with the diagnostics. This may be a sampling issue as at the top, or it may be a fundamental issue with the diagnostics or ??? But, the present plotting doesn't really make sense. If there authors DO decide to keep their NH SH classification, there needs to be some attempt to scale the vastly different forcing applied to each such that they are comparable when lined up on figures like this.

This figure exactly reflects the different aerosol injection magnitude in the GNH and GSH classification. Based on the criteria, all the events included in the GNH classification have aerosol injection magnitude than that of the 1991 Pinatubo eruption, the included events have aerosol injections that are 1~5.5 (average 2.2) times larger than Pinatubo eruption, thus it largely reduces the clear sky shortwave radiation by reflecting the incoming solar radiation. Zhuo et al. (2021) simulated Pinatubo strength eruption with MPI-ESM, which showed up to 8~9 W/m^2 reduction of global mean clear sky shortwave radiation. 20 W/m^2 of reduction is reasonable, considering the 1~5.5 (average 2.2) times of injected aerosols. For GSH, there is no NH aerosol injection, and the aerosol injections in the SH are small, it's reasonable to have limited response, especially in the NH. The line across 5N is not a diagnostic problem, but relatively uniform responses to zonally averaged forcing which are in the same colorbar scale range. If we use a smaller scale with a lot of levels, as shown in the figure below, slight differences emerge across 5N.



However, the scale is used to emphasize the GNH classification and the RWA and RDA, instead of the GSH and tropical regions, and aimed to show the difference between the GNH and GSH classification.

The main focus of this study is on the mechanism exploration, instead of the classification and related responses, as stated in the manuscript and explained above, GSH has limited magnitude, we include this classification, because it has the same 12 events included in the classification, which serves as a reference to the GNH classification, and show the contrast between with and without NH aerosol magnitude. The aerosol magnitude in the GSH classification does not affect the main focus of the study.

The y-axis labels are clearly an improvement.

Thanks.