Authors' Response to Reviewer 2

Review of "Mount Pinatubo's effect on the moisture-based drivers of plant productivity „by Ram Singh et al. 2023

The aim of the current study is to understand how volcanic eruptions affect ecohydrological conditions and plant productivity. The authors used the NASA Earth System Model to simulate the 1991 eruption of Mt. Pinatubo and to detect its response on soil moisture and evapotranspiration as short-term ecohydrological controls on plant productivity. Using the Soil Moisture Deficit Index (SMDI) and the Evapotranspiration Deficit Index (ETDI), they find that about 10-15% of the land area shows statistically significant dry or wet patterns in the volcanically perturbed climate conditions for 1992 and 1993, and between 5-10% in the following years (1994-1995). The authors focus on three regions that show different responses in EDTI and SMDI. In Equatorial Africa, decreases in both indicate a likely negative impact on crop productivity, while in the Middle East region increases indicate a positive impact on crop productivity. North Asia on the other hand, shows an increase in SMDI and a decrease in ETDI, indicating that crop productivity has probably decreased, but not due to water-related factors.

The paper needs some major improvements. It needs to be more streamlined and the results should be discussed in a broader context, see general comments below. I therefore recommend publication only after major revisions.

We sincerely appreciate the reviewer's thoughtful comments on the manuscript. We have carefully addressed these comments, resulting in substantial revisions that have significantly improved the overall presentation of the results. Responses to specific comments are provided in blue text under each comment, with italicized text indicating specific additions to the manuscript. Pointers to specific changes in the main manuscript are highlighted in red brackets.

General comments

- Part of the paper reads like a model evaluation paper of the climate response to volcanic forcing in the MATRIX version of the GISS ModelE2.1 (Bauer et al., 2020). I am therefore confused as to the purpose of this paper. If validation of the Pinatubo simulation is one of the aims of the paper, this should be clearly communicated. The evaluation of the primary dependent variables temperature and precipitation is quite lengthy and has been done before in other contexts, see points below.

  Indeed, a model evaluation is not the goal of this manuscript, but we have to present some evaluation to demonstrate that the model has skill in simulating the climate impacts of volcanic aerosols. This also highlights the modeling capabilities of GISS ModelE (MATRIX) for interactively simulating the volcanic aerosol properties and climate responses which governs the ecohydrological conditions that further affect the primary productivity.
  To reduce the the length of this section, we decided to move a large portion of the microphysical and radiative properties discussion (including figure 1 in the submitted version is now figure S3 in supplementary info) of volcanic aerosols to the supplementary information (section S2; line 39 in supplementary info).

- Regarding the evaluation, I wonder why you do not put your aerosol microphysical model results into a broader context and relate them to recent work. Quaglia et al (2023) published last year an extensive multi-model data comparison of the Pinatubo episode with different aerosol microphysical models.
  Thanks for pointing this out. Quaglia et al., (2023) has demonstrated the model inter-comparison results for the experiment protocols covering a range of strategies for volcanic aerosol injection strength and altitudes and demonstrated the control of various factors on simulating the microphysical properties of volcanic aerosols in different aerosol microphysics. The estimate of the injected SO2 and plume height for the Pinatubo eruption followed in our study (SO2=15.2 Tg, Height: 25km) are not exactly equal with the cases studied by Quaglia et al., 2023. Their model setup nearest to our simulation uses 7 TgS (14 Tg of SO2) and a plume height of 22 km, and another with the same amount of SO_2 and a plume height in the range of 18-25 km. We have added the following lines in the description, which is currently supplementary section S2 (line 52-59 in supp. info), since this part is now in the supplement to present our MATRIX results against the broader context of the 6 models utilized by Quaglia et al., (2023).

  > *"Quaglia et al. (2023) have presented a detailed evaluation of the control of aerosol injection strength and altitude on microphysical properties of volcanic aerosols using models with interactive chemistry and microphysics under the Interactive Stratospheric Aerosol Model Intercomparison Project (ISA-MIP). Broadly, ModelE (with the MATRIX aerosol microphysics code) simulated well the evolution of the volcanic plume (AOD, effective radius, and aerosol dispersion) compared to the closest match to our configuration of a sulfur injection strength (~ 7 TgS = 14 Tg of SO2) at injection heights both at 22km and the range 22-25 km presented by Quaglia et al., 2023."*

- The GISS E2.1 model has been used before to study the impact of volcanoes on climate, so this aspect is not really new. I wonder why you do not discuss the climate response after the Pinatubo eruption in your model version with that simulated with the CMIP6 version of the GISS ModelE2.1 (Kelly et al. 2020) in the historical runs and in the Pinatubo VolMIP ensemble (Weierbach et al., 2020). From my point of view, the only open/interesting question here is I how does the surface climate response change when you calculate the aerosol microphysics online.

  - Kelley et al. (2020) and Weierbach et al. (2023) (we think by Weierbach et al. (2020) the reviewer meant Weierbach et al. (2023)) have used a prescribed volcanic forcing as monthly mean AOD in the stratosphere, in line with the CMIP protocol. The volcano response when using monthly mean climatology of AOD can be very different from when SO_2 is injected in the atmosphere and chemistry converts it to sulfate, and then dynamics transport it around. MATRIX has been used elsewhere (LeGrande et al., 2016; McGraw et al. 2024, Osipov et al. 2021, Singh et al.2023) for different eruptions, including Pinatubo, but the aim of this manuscript is not to compare against them. Recently, McGraw and Polvani (2024) pointed out the importance of including the interactive treatment of volcanic aerosols in context to better explain the volcanic impact on rainfall, by attributing the direct effect of aerosol-radiation interactions through the stratosphere-tropospheric exchange of energy. This can have further impacts on clouds and regional rainfall, together with a thermodynamical shift of ITCZ at the surface.

We modified the relevant section (line 112-119) and added the following sentences in the manuscript to highlight the importance of using the interactive aerosol chemistry version of GISS model for this study:

"We assess the impact of the Mt. Pinatubo eruption on the model-simulated climate via multiple pathways, ranging from primary dependent variables to higher-order responses that influence plant productivity. The use of prognostic aerosols enhances the simulations by capturing dynamically consistent feedbacks between the climate response and volcanic aerosols, including aerosol-radiation interactions and stratosphere-troposphere energy flux exchanges (McGraw and Polvani, 2024)"

- Related to this. I do not understand why you need an aerosol model for your study of the impact of volcanic forcing on moisture-based drivers of plant productivity. This study would also work with prescribed volcanic forcing: you could use the historical CMIP6 ensemble (Miller et al., 2021) or the 81-member VolMIP Pinatubo ensemble (Weierbach et al., 2023). For these simulations, you would have more than 11 ensembles, which would make your results even more statistically robust.

  Miller et al., 2021 prescribe the volcanic aerosol using the extinction and aerosol size based upon the model estimate and observations. Weierbach et al., (2023) has also used that version of the GISS model, but importantly the VolMIP experiments are conducted with a pre-industrial (PI) climate.

  Since this study focuses on the volcanically driven climate pathways of impact assessment, we utilized the interactive chemistry version of the GISS model to include all climate feedbacks to the volcanic eruption-generated radiative perturbation (McGraw and Polvani, 2024). Additionally, the climate metrics that need weekly scale data, cannot be calculated accurately when using prescribed, monthly and zonally mean forcings.

The authors are right that there are not many studies on this recent topic. However, soil moisture changes due to volcanic eruptions have been discussed in the context of volcanic impacts on the carbon cycle (e.g. Fröhlicher et al., 2011). There are also some interesting discussions in Zuo et al. (2019a ,b) on the hydroclimate response after a volcanic eruption, where not soil moisture but other relevant hydroclimate parameters are related to NPP. Furthermore, there is a broad discussion in the geoengineering community about the impact of stratospheric aerosol on soil moisture and food production for solar geoengineering, see for example Cheng et al. (201). These issues should be addressed in the paper.

We appreciate the reviewer for pointing out these studies. We have included the following sentences addressing the relevance of these studies in context to the objectives of this manuscript.

Line 102-107 (introduction)

"Studying a large (10xPinatubo) volcanic eruption, Fröhlicher et al. (2011) have shown that the terrestrial carbon pool is sensitive to the regional (in the tropics and sub-tropics) soil-moisture content through the net-ecosystem productivity. Using the geoengineering large ensemble simulations with CESM model, Cheng et al. (2019) have analysed the changes in terrestrial hydrological cycle and discussed the future soil-moisture response and its drivers under a geoengineering scenario"

Line 76-78 (introduction)

"*It is also shown that volcanic eruptions can alter regional rainfall and hydroclimate in general, which could prominently affect regional plant productivity (Zuo et al., 2019a;b)*."

- The authors speculate a lot in the paper about potential impacts on crop productivity, but they have not shown any GPP or NPP anomaly plots. I wonder why, as this would strengthen the paper considerably.

  Thank you for pointing this out. The aim of this study is to focus on hydroclimate metrics. Transpiration is the most dominant process contributing to AET on land and is strongly correlated with photosynthesis. Thus, an increase in AET serves as a reliable indicator of an increase in GPP. Consequently, we have chosen not to emphasize GPP in this analysis. However, we have revised portions of the manuscript to address this concern. Additionally, we included a plot illustrating the seasonal anomaly of GPP in the supplementary information (Figure S9) and provided a discussion on plant productivity in the conclusions section, along with examples of similar findings from other studies (line 712-728; Conclusion section).

"*Kandlbauer et al., (2013) examined crop responses (using C3 and C4 grasses as proxies) to the 1815 Tambora eruption using the HadGEM-ES model in three regions very similar to those in our study. Their findings suggest that plant productivity decreases with positive changes in soil moisture in the higher-latitude Asian region. In the mid-latitudes over the Southern Europe/Middle East region (adjacent to our MDE region), volcanic eruptions may enhance plant productivity by providing additional soil moisture through increased rainfall. However, in the MDE region in our study, we found that the applied irrigation also benefits soil moisture supply along with the increased rainfall. Furthermore, both studies report a decrease in productivity in the tropical region. In general, these results complement the findings of this study, which suggest that if sufficient water is available in the Southern Europe/Middle East region, volcanic eruptions may enhance plant productivity. In contrast, in the far northern latitudes, water is not the primary driver of plant responses, and productivity is likely to decline. Seasonal-scale changes in gross primary productivity (GPP) confirm the regional trends in plant productivity following the eruption. The simulations show a more pronounced decrease in GPP in the northern high-latitude* *region and a significant increase in GPP over the European and Mediterranean regions. Additionally, distinct patterns of decrease and increase in GPP are simulated in the tropical northern and southern regions, respectively (Figure S9)).*"

Specific comments

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Line 23-24:	Could be deleted. if model evaluation is not a specific subject of your paper. Modified the sentence by removing the radiative response. (line 23-24) *"The model simulates a mean surface cooling of ~0.5 °C following the Mt. Pinatubo eruption."*
Line 27:	You do not show any agricultural response in the paper so please be careful with your wording. Here, Agricultural response points to the agricultural drought indices (SMDI & ETDI), we have re-written the sentence to clarify this: (line 26-28) *"We find that up to 10-15% of land regions show a statistically significant hydroclimate response (wet and dry) as calculated by the Soil Moisture Deficit Index (SMDI) and Evapotranspiration Deficit Index (ETDI)."*
Line 28:	Not clear what you mean with "these higher-order impacts". Re-written the sentence (line 28-29) as *"Results confirm that these impact metrics successfully present a more robust understanding of plant productivity"*
Lines 41-44	Too many references for something well known and obvious. I suggest that you refer here instead to some overview papers e.g. Marshall et al (2022), Kremser et al (2016), Timmreck et al (2012). Modified and added new references (line 41-43)
Line 48:	Again, you can reduce the amount of citations and refer to some overview papers or the recent model intercomparison paper. Modified and citation added (line 47)
Line 53 ff:	See point above. Modified (line 53)
Lines 73 ff:	Please cite here in addition or instead of Toohey et al (2016) the most recent paper for the mid-6th century volcanic impact on post-volcanic climatic and societal response over Scandinavia: van Dijk et al (2023). Thanks, citation included at line 70-71
Lines 193-195:	It is not clear here why you use the climatology of the years 1950-2014, maybe you refer here already to the supplementary material. 1950-2014 climatology is used to calculate the anomalies generated due to volcanic perturbation of Pinatubo eruption. Also, this period serves as the

reference calibration period for agricultural drought indices (SMDI & ETDI) calculation. Please also check our response to the other reviewer about PCH, and the new figure S2. Relevant explanation in main text modified in line 207-218.

"…….However, directly comparing the difference between the two ensembles (PCH and NP) is an alternative approach to presenting the Pinatubo effect (see Supplement Figure S2). Using either approach leads to the same general conclusions, with only small quantitative differences. Nevertheless, we chose to remain consistent with the baseline requirements for other metrics as well and used the historical climatology for period 1950-2014 as the baseline for the core of our analysis. The coloring emphasizes the significant regions of anomalies. But we also emphasize the difference in calculations: the grey areas show no significant change between the PCH and NP ensembles, while the anomalies are PCH ensemble mean minus climatology"

Line, 227: Index instead of Indices

Thanks, Corrected

Lines 239-240: Please use SI units, even its not typical for SDI, you can put the "feet metric" in brackets

Since the soil-moisture indices nomenclature has distinguished using the depth in feet, we prefer to leave it in feet. We also added the equivalent numbers in SI unit in brackets throughout, as additional information.

Line 247ff: Here and in the following lines the reference style seems not correct.

Corrected

Lines 284-286: Concerning the justification, it is quite difficult to see in the supplementary Fig S1 the difference between the three reference periods. Maybe a difference plots between them would more useful.

Figure S1 shows the anomaly for the year 1992 with respect to 3 different reference climatology periods. The difference of the differences can be more confusing than explanatory.

Lines 309-311: This sentence sounds strange, please reformulate.

Modified as follows: Line 317-320 "*…which then decreases with time due to the deposition of volcanic aerosols (English et al., 2013; Sato et al., 1993). In this study, the model-simulated aerosol optical depth (AOD) due to volcanic aerosol and radiative forcing is larger than the previously reported AOD of 0.15 and forcing of -4.0 to -5.0 Wm^{-2} due to the Mt. Pinatubo eruption (Hansen et al., 1992; Lacis et al., 1992)*"

Lines 357-370: It is not clear to me why the lower stratospheric temperature response is relevant for your specific topic

We modified the Figure **2 (submitted version)** as Figure 1 (revised version) and removed the panel showing the lower stratospheric temperature response. We also modified the result discussion and moved it to the supplementary text (section S3.0), along with figure S4.

Lines 416 -421: So why choose for your analysis few realizations with prognostic aerosol instead of more realizations with many members?

We used all 11 realizations (which is not a few, in our opinion) for the analysis. Here, we are pointing out the possible causes of higher and spatially varying characteristics of rainfall. Polvani et al. (2019) pointed out that a large ensemble is good for the robust response, but it is computationally expensive. Singh and AchutaRao (2019) showed that our set of 11 ensemble is sufficient to represent significance in climate response at the regional scale.

We added the following in the text and modified the explanations (Line 407-415):

"We acknowledge the signals due to the model's internal variability when averaging the impacts across multiple ensembles, but 11 ensembles are a good compromise between few vs. many ensemble members which was shown to be sufficient to represent significance in climate response at the regional scale (Polvani et al., 2019; Singh and AchutaRao 2019)."

Line 436: Section 3.4.

Thanks, corrected (Line 430)

Lines 444-446: This sentence is not clear, please reformulate.

Modified (line 438-439)

Line 535: What are "elements of a time lag between precipitation"?

We modified the sentence (line 528-530):

"Additionally, SMDI_2 (top 2 feet or 0.6 m) and ETDI have demonstrated a slow development of drought conditions, beginning by the end of the year 1991 (SON season), reflecting a time lag between seasonal precipitation patterns (Narasimhan and Srinivasan 2005)."

Lines 644 ff: Does this happen in all realizations or is it just a coincidence in the ensemble mean? How meaningful are changes of individual weeks?

We highlighted these 2 weeks as simulated in the ensemble means response (shown in Figure 9). These sentences (line 635-636) shows the importance of considering such high temporal resolution impact metrics in context to agricultural productivity regardless of volcanic forcings.

Lines, 691 ff: I think you can shorten the part about the model performance/evaluation substantially. You need not to list the references here when you already have included them in the text

Thanks for pointing this out, we modified the text (entire paragraph between lines 680-702 and kept the only lines 683-685) and deleted the earlier cited references.

Line 719: "volcanic forcings due to the Mt. Pinatubo eruption" sounds strange, please Revise

Modified the full sentence as follows (line 689-692):

"These drought indices confirm the moisture-driven dry and wet patterns

observed in early 1992 and the following years over the tropical regions and mid-latitudes of the Northern Hemisphere, respectively, as a response to the radiative perturbation caused by the Mt. Pinatubo eruption."

- Tables:
  - Table 1 can be merged with the figure caption of Figure 8
    We think the reviewer refers to table 2 here. We have deleted this and the details of the regions are now included in the caption of figure 7, as suggested (line 553).

- Figures:
  - In general: The multi-panel figures (5, 6,7) are too small and hard to read and therefore not very convincing. I strongly recommend to reduce the number of panels by either showing only specific years or specific seasons. This would make the figures much more readable and therefore much better emphasize the point. The missing panels (seasons, years) could be put into the supplementary material.
    -Thanks for the suggestion. We have modified these figures (Figure 4,5,6 in main text, and Figure S6 in supplementary) by zooming over each panel by removing the high latitude regions over the Arctic and Antarctica where no vegetation exists.

  - Figures 3, 4 : 1st two panels are useless and could be deleted.
    - Thank you for pointing this out, We preferred to keep the DJF and MAM as a confirmation of our methodology which shows that the mean climate under both experiments (Pinatubo & counterfactual case) are same till the eruption in June 1991.
    - References:
      - Please update the reference to Brown, H. Y, Geosci. Model Dev., 17, 5087–5121, https://doi.org/10.5194/gmd-17-5087-2024, 2024.

    Modified

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