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

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.

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.
    - 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.
    - 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.
    - o 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.
- 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.

- 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.

Specific comments

Line 23-24:	Could be deleted. if model evaluation is not a specific subject of your paper.
Line 27:	You do not show any agricultural response in the paper so please be careful with your wording.
Line 28:	Not clear what you mean with "these higher-order impacts".
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).
Line 48:	Again, you can reduce the amount of citations and refer to some overview papers or the recent model intercomparison paper.
Line 53 ff:	See point above.
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).
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.
Line, 227:	Index instead of Indices
Lines 239-240:	Please use SI units, even its not typical for SDI, you can put the "feet metric" in brackets
Line 247ff:	Here and in the following lines the reference style seems not correct.
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.
Lines 309-311:	This sentence sounds strange, please reformulate.
Lines 357-370:	It is not clear to me why the lower stratospheric temperature response is relevant for your specific topic
Lines 416 -421:	So why choose for your analysis few realizations with prognostic aerosol instead of more realizations with many members?
Line 436:	Section 3.4.
Lines 444-446:	This sentence is not clear, please reformulate.
Line 535:	What are "elements of a time lag between precipitation"?
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?
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
Line 719:	"volcanic forcings due to the Mt. Pinatubo eruption" sounds strange, please revise

- Tables:
    - Table 1 can be merged with the figure caption of Figure 8

- 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.
- Figures 3, 4 : 1st two panels are useless and could be deleted.

- 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.

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