

This manuscript aims to study the effect of volcanic eruptions on moisture-based drivers of plant productivity. The results indicate a 0.5 °C surface cooling due to the Mt. Pinatubo eruption. However, this is not a major finding, as previous studies have already reported this level of surface cooling following the volcanic activity at Mt. Pinatubo.

The major flaw of the study is the lack of a direct statistical analysis of the impact of atmospheric air pollution from volcanic activity on climatic and hydroclimatic parameters. Individual anomalies in simulated explanatory variables were inferred as results of volcanic eruptions, but only over 10-15% of the area. No direct evidence of the decrease or increase in plant productivity is presented. Terminology in the manuscript is not clearly defined. Section 2.3 on methods is difficult to understand and poorly written. The interchangeable use of terminologies adds to the confusion. For example, "counterfactual (sometimes counter-factual) ensemble simulation" and "no-Pinatubo ensemble simulation" refer to the same simulation but are used interchangeably throughout the text.

Please find below some additional comments:

In Figure S3, abbreviations such as LW, WS, and NET are not defined either in the main text or in the figure caption.

Line (337). "The zonal AOD shows the dispersion and transport of aerosol poleward after the eruption." However, it is not clear from Figure 1 how this conclusion is achieved. It would be better if the grid cell level variation with temporal scale is represented after the eruption event.

Line343: QBO is not defined.

Line 343-355: The spatial connection of AOD's effect on temperature changes cannot be established using Figure 1. Grid-level correlations could provide more insight into their connectivity.

The seasonal anomalies of temperature and rainfall presented in Figure 2 and Figure 3 are with reference to long-term climate conditions that is from 1950-2014. No rationale is provided for this approach either in the result section or in method section. How to account for GHGs radiative effect during this long-term reference period? The anomaly comparison years for

temperature are 1991-1995 but for rainfall only two years are presented. No explanation for this inconsistency is available.

Lines 335-335: The results shown in Figure 4 are significant only over forest land (Congo tropical forest and Russian boreal forest), which has a much deeper root zone compared to croplands. In Figure S6, the land mass area has pixels of a similar color to the ocean. Is there any explanation for this?

Since the study is focused on drivers of plant productivity, remove the ocean region from temperature and rainfall figures to have consistent study area with SMDI and ETDI.

Figure 7 should just be in supplementary materials. However, the rationale for selecting areas in different regions is unclear. It appears that the selection is based on regions with clusters of grid cells showing significant anomalies.

A point-by-point response to the first review was not provided by the author, making it difficult to evaluate whether the major concerns were fully addressed. However, upon reviewing the track-changes-enabled draft, the following anomalies were observed.

Lines (172-177): Why delete all the lines when only change in the original text is the reference to Figure and correction in cited paper?

It's not clear what revision is made between lines 260-265. It seems like cut and paste.

Lines between 378-384, All the revision is cut-paste only change is reference to Suppl. Figure.

Revision between Lines 763-775 appears to be selecting alternative word. It does not add value to the text.