2 Response to Reviewer #2's comments

This paper aims to investigate the effect of ENSO on global dust emissions, concentration, and deposition. A multivariate predictive model and the Ganger causality test were used to analyze dust-relevant output from 12 CMIP6 models.

General Comments

A major and critical shortcoming of this paper, in current form, is the lack of detailed presentation of methodology, results, and discussion. The paper seems to be written hastily.

Response: We thank the reviewer for your comments. We agree with the reviewer that some parts of the manuscript need improvement. We modified the manuscript based on your suggestions as below.

2.1 Methodology: Authors only provide a few lines about their approach, and refer readers to their previous works and the supplementary document. However, a brief description of the method should be presented in this section.

Response: We thank the reviewer for raising this point. We moved part of the supplementary to Section 2.2 to further clarify the Methods used in this study as follows:

"We use the following multivariate predictive model (Mosedale et al., 2006; Stern and Kaufmann, 2013) to estimate the causal links between the ENSO and dust deposition:

$$\boldsymbol{X_t = \sum_{i=1}^{p} \alpha_i X_{t-i} + \sum_{i=1}^{p} \beta_i Y_{t-i} + \sum_{j=1}^{m} \sum_{i=1}^{p} \delta_{j,i} Z_{j,t-i} + \varepsilon_t} \quad (1)$$

where X_t is the annual mean (or seasonal mean) dust deposition for year t, Y_t is the ENSO index, and $Z_{j,t}$ is the confounding factor j for year t. In the predictive model presented in equation 1, while assessing the effect of Y on X (i.e., the contribution of the term $\sum_{i=1}^{p} \beta_i Y_{t-i}$ in predicting X), the possible influence of past X events are considered by adding the term $\sum_{i=1}^{p} \alpha_i X_{t-i}$. Thus, the causal influence of Y on X, if detected, is robust and the impact of past X events are accounted in the analyses. Here, m is the number of confounding factors and $p \geq 1$ is the order of the multivariate predictive model. The optimal order p is computed by minimizing the Schwarz criterion or the Bayesian information criterion (Schwarz, 1978). The optimal orders may be different for each model.

Here we take into account the impacts of confounding factors and therefore provide further information of the real-world teleconnections. In the analyses, we use three different confounding factors; hence, *m* is equal to 3. The noise residuals ε_t and the regression coefficients α_i, β_i and $\delta_{j,i}$ are computed by using the multiple linear regression analysis of the least squares method. We detrend and normalize all the climate indices."

2.2 Additionally, authors stated that they studied confounding effects of other climates modes, namely NAO, SAM, and IOD. First, what is the basis for choosing these modes(?), and second, no analysis or sensitivity test regarding this treatment was provided.

Response: We thank the reviewer for raising this point. We added the following sentences to Section 2.2 to explain the selection of the three factors Indian Ocean Dipole, Southern Annular Mode, and the North Atlantic Oscillation:

"The climate modes SAM, the IOD and the NAO are the important sources of global climate variability (Hurrell et al., 2003; Luo et al., 2012; Roxy et al., 2015). For instance, the NAO is the prominent mode of atmospheric circulation variability over the North Atlantic and surrounding regions (Delworth et al., 2016; Hurrell et al., 2003) and variations in NAO are crucial for the environment and society (Hurrell et al., 2003). The IOD affects climate extremes over the Indian Ocean and surrounding areas (Abram et al., 2008; Kripalani et al., 2009; Kripalani and Kulkarni, 1997) and might cause severe economic consequences (Ummenhofer et al., 2009). The SAM is the major mode of atmospheric circulation variability in the southern Hemisphere (Cai et al., 2011; Raphael and Holland, 2006). In addition, changes in these modes may affect the variations of ENSO (Abram et al., 2020; Cai et al., 2011, 2019; Le et al., 2020; Le and Bae, 2019). Nevertheless, it is likely that these factors may alter the influences of ENSO on dust activities."

2.3 Finally, what is the basis for choosing these 12 models as data crucial for these analyses are missing in three of them (table S2)?

Response: We thank the reviewer for raising this point. We selected all the models with accessible dust-related data. Considering the total models is somewhat low (i.e., 12 models), we kept all these models. To our knowledge, dry and wet deposition of dust are key variables which directly affect local environment, thus we selected all models having these data.

We add the following sentences to Section 2.1 to clarify this point:

"We limited our study to all the models having both dry dust and wet dust data (i.e., there is total of 12 models with accessible dry dust and wet dust data as described in Table S2). Dust deposition on land and ocean surface are important metrics to assess the impacts of dust activities on ecosystems and environment (Bao et al., 2017; Fan et al., 2006; Jickells et al., 2005; Jiménez et al., 2018; Kanakidou et al., 2018; Schulz et al., 2012). Additional data of od550dust and emidust supplied by these 12 models provide further understanding of ENSO impacts on dust activities."

2.4 Results and Discussion: Results were presented and discussed in a highly qualitative manner without any in-depth analysis as required in a manuscript with an archival value. Authors only reported the fraction of total "affected area" over ocean and land, but this number alone is not useful in understanding the true impact of ENSO on dust activities in different regions of the globe.

Response: We thank the reviewer for raising this point. The "affected area" provide important information on the scale of ENSO impacts. The purpose of this work is to provide simple and robust conclusions about the causal effects of ENSO on global dust activities. To our knowledge, these analyses are lacking, and the results described here have not been shown before. We should note that we also provided information on the impacts of ENSO on dust activities at regional scale, as well as the consistency between models in simulating the connection between ENSO and dust activities.

2.5 As expected, individual models show drastically different results (figures 4-6), but conclusions of the paper were based only on the ensemble mean results with minimal discussion about the difference between models. Note that the chosen models use different dust emissions and deposition, as well as dust size partitioning schemes, so ensemble mean results must be interpreted with caution.

Response: We thank the reviewer for raising this point. The use of ensemble mean is widely used and is expected to reduce the uncertainty related to the connection between ENSO and dust activities. In fact, despite using different aerosol model, there is high agreement (i.e., denoted by stippling in Figures 1 and 2) between models for the results. Our conclusions are mainly based on this high consistency.

We partly discussed the difference between models in Section 4 as below:

“Regarding the consistency across models, the response of dust emission to ENSO is much stronger in the models INM-CM5-0, MIROC-ES2L, and UKESM1_0_LL compared to other models (Figure 6). This difference might be due to the use of different dust schemes and soil properties in this model which lead to higher dust emissions (Mulcahy et al., 2020; Zhao et al., 2022). As models use different parameters to estimate dust emissions (Thornhill et al., 2020), this discrepancy leads to low consensus across models in modeling the response of dust emissions to ENSO (Figures 3 and 6).”

2.6 Finally, several conclusions of the work are not supported by the current results, for example, “ENSO may initiate dust activities in …. (line 93)”, “dust deposited in the South Pacific and the Southern Ocean might be originated from central Australia and southern South America (line 117)”, “weak causal impacts of ENSO on regional dust emissions of major dust sources (Figure 3) may indicate the important role of human influences in igniting local dust activities… (line 132)”.

Response: We thank the reviewer for raising this point. We removed the lines 93 and 117 to avoid confusing the readers.

We rewrote line 132 as this line serves as a discussion rather than a conclusion as below:

“Substantial influences of ENSO on dust emission over central Australia (Figure 3) suggest an agreement with earlier work (Marx et al., 2009), while we observe weak causal impacts of ENSO on regional dust emissions of major dust sources (Figure 3).”

“Previous studies indicate the important role of human influences in igniting local dust activities (Duniway et al., 2019; Webb and Pierre, 2018).”

Specific Comments

2.7 Line 15 and all other places through the manuscript: Caution must be practiced with the term “concentration” as the relationship between dust concentration and dust AOD depends on the pre-defined and assumed dust particle size distribution, which is different in different models.

Response: We thank the reviewer for raising this point. We removed the term “concentration” to avoid confusing the readers.

2.8 Line 15 and all other places through the manuscript: Change “transportation” to “transport”

Response: We thank the reviewer for raising this point. We corrected as your suggestion.

2.9 Line 57: "Dry and wet deposition is related to different types of dust and aerosol." Not clear what authors mean here.

Response: We thank the reviewer for raising this point. We removed this sentence to avoid confusing the readers.

2.10 Line 82: How are "areas affected by ENSO" defined? What are the criteria considerd?

Response: We thank the reviewer for raising this point. We added the following sentence to clarify the criteria for computing the areas affected by ENSO:

"In Figure 2b, the areas influenced by ENSO are computed as the areas limited by the cyan contour line as shown in Figures 1 and 2a (i.e., *p*-value is lower than 0.33 or ENSO is unlikely to exhibit no causal effects on dust activities over these regions)."

2.11 Line 116: Change "original" to "originated"

Response: We corrected as your suggestion.

2.12 Figure 1: What is the significance of studying dry and wet deposition separately in figure 1, as paper provides no insightful comparison between the two?

Response: We thank the reviewer for raising this point. As we introduced in Section 2.1, dry and wet deposition are related to different processes of dust deposition. Hence, direct comparison is not necessary. We combined the results in 1 Figure as these dust deposition processes are complementing for each other.

2.13 Figure 2(b) and S2: There two figures don't provide any addition information beyond just one number mentioned in the text, so they should be removed.

Response: We thank the reviewer for raising this point. We think these Figures provide quick summary and illustration for the Text. Hence, we would like to keep these Figures.

2.14 Table S1 and S2: Should be merged into one table

Response: We thank the reviewer for this suggestion. We would like to keep these Tables separately to avoid a too big Table. It would make the presentation easier.

2.15 The following recent publication might be of interest to the authors: https://acp.copernicus.org/articles/22/2095/2022/

Response: We thank the reviewer for this suggestion. We include this publication as a reference.

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