Responses to reviewers' comments

Dear Editor:

We would like to thank the two reviewers for their additional comments on our manuscript. We include a point-by-point response to each comment and *"revisions in the revised manuscript"* to address the comments below. The annotated line numbers refer to the revised version of the manuscript with markups along with this response.

We believe our response has addressed the reviewers' comments as described in more detail below.

Yours sincerely,

Jun Meng

On behalf of the authors

Anonymous Referee #4 (Report 1):

The Authors have addressed my main concern that the observational comparison relied on a global AOD comparison by including additional analysis, including dust concentration data. The evaluation has thus been improved and the new Figures are good.
Response: Thanks for the assessment.

The secondary comment regarding the benefits of a regional constraint over a global one have has been partially addressed with the inclusion of the North American regional study and an assessment of how varying the dust emission magnitude by +/- 500Tg around the proposed 2000Tg flux impacts regional comparisons. I agree there is more work here to be undertaken in the future here. A small additional discussion of what regional refinements could be undertaken in the future and what observations are missing/needed is thus beneficial in place of such an analysis.
Response: We have add texts in section 3.4 to discuss more about the future work on dust emissions.
Lines 354-356: *"More dust-specific observations are needed to constrain dust emissions for the Asian deserts region and other deserts."*

Also, in Section 3.4 it is stated:
"Although the central Asian deserts and regions with AERONET observations (Fig. S10) are better represented by the simulation with global total annual dust emission scaled to 2,500 Tg." Is there a reason a harmonized emission dataset was not produced which, for example, would include emissions from East Asia from this increased emissions dataset, while retaining the baseline from the other at 2000Tg?

Response: A lack of observations over Asian deserts region, for example a dust concentration measurements network, is the reason why we did not provide a dust strength scale factor for this region at this stage.
We added in the text in lines 353-354 : *"We refrain from applying a regional scale factor to the central Asian deserts given the paucity of in situ measurements."*

Minor Comments:
Throughout: Suggest changing "Sahara" for "North Africa"
Response: Great suggestion. We have replaced "Sahara" with "North Africa" for texts when Middle East and Asian deserts are involved.

L47: For completeness consider adding ocean phytoplankton fertilization.
Response: We have added references for considering the ocean phytoplankton fertilization.
Line 44-45: *"on the biosphere by fertilizing the tropical forest (Bristow et al,. 2010; Tang et al., 2017) and ocean (Jickells et al., 2005; Guieu et al., 2019; Tagliabue et al., 2017)"*

L64: Factor of 16? or over an order of magnitude?
Response: The factor for varying over one dimension is around 16 (from 4° to 0.25°). The overall model horizontal resolution in two dimension will vary by over a factor of 100 (from 4° x 4° to 0.25° x 0.25°).

L187: The range of global dust emissions given at 426 – 2,726 Tg/yr for AeroCom models is not the range given in Table 3 of Huneeus et al. (2011) which is 514 – 4313 Tg/yr. Is there a reason for this discrepancy?
Response: Thanks for catching this. We were counting the sum of North Africa and Middle East dust emissions reported in the abstract of Huneeus et al. (2011) in our previous version of manuscript. We realized that the global total dust emission range in Table 3 of that reference is more appropriate to cite. We have corrected the numbers in the revised manuscript.
Line 183: *"which are in the range of the current dust emission estimates of over 514 – 4,313 Tg yr^{-1} (Huneeus et al., 2011)."*

Anonymous Referee #1 (Report 2):

I appreciate authors' effort into revising the manuscript. I think objectives of the work are more clarified now and enhanced evaluations help with building confidence in the new model. I still have one concern that I hope the authors can address. New updates to the model comprised of two factors: 1) offline vs. online dust calculations and 2) scaling the total dust emissions to 2000 TG/yr. The manuscript's title, abstract, and conclusions seem to emphasize on the first factor (e.g., line 25: "These updated offline dust emissions based on high resolution meteorological fields strengthen dust emissions over relatively weak dust source regions, such as in southern South America, southern Africa and the southwestern United States. Identification of an appropriate dust emission strength is facilitated by the resolution independence of offline emissions."), but arguably the second factor seemed to have even more pronounced impact in a number of cases (e.g., see figure 6). I refer back the authors to my previous comment (comment # 5) that scaling can indeed be easily implemented together with an online model, so ALL the benefits observed here should not be attributed to the offline modeling approach. I hope this makes sense and convincing to authors to conduct a fair comparison, which eventually helps with the quality of the manuscript.

Response: Thanks for the comment.

We added texts in lines 358-362: *"Although the main purpose of this manuscript is to develop and evaluate an offline grid-independent inventory, it is worth noting that online models have the capability to scale to a target source strength. In that context the global source strength identified here may be of use for global online models to scale to the global source strength, with the caveat that differences in dust parameterization, dust optics, and deposition may affect performance."*

Also as a very minor point: I suggest providing the link to the IMPROVE's main webpage when citing the network (http://vista.cira.colostate.edu/Improve).

Response: Done.

Line 104: *"We use ground-based surface fine dust concentration measurements over the US from the Interagency Monitoring of Protected Visual Environments (IMPROVE, http://vista.cira.colostate.edu/Improve/) network."*

References

Jickells, T. D., An, Z. S., Andersen, K. K., Baker, A. R., Bergametti, G., Brooks, N., Cao, J. J., Boyd, P. W., Duce, R. A., Hunter, K. A., Kawahata, H., Kubilay, N., laRoche, J., Liss, P. S., Mahowald, N., Prospero, J. M., Ridgwell, A. J., Tegen, I., and Torres, R.: Global Iron Connections Between Desert Dust, Ocean Biogeochemistry, and Climate, 308, 67–71, https://doi.org/10.1126/science.1105959, 2005.

Guieu, C., Azhar, M. A., Aumont, O., Mahowald, N., Levy, M., Ethé, C., and Lachkar, Z.: Major Impact of Dust Deposition on the Productivity of the Arabian Sea, 46, 6736–6744, https://doi.org/10.1029/2019GL082770, 2019.

Tagliabue, A., Bowie, A. R., Boyd, P. W., Buck, K. N., Johnson, K. S., and Saito, M. A.: The integral role of iron in ocean biogeochemistry, 543, 51–59, https://doi.org/10.1038/nature21058, 2017.