Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2021-4-RC2, 2021

Comment on acp-2021-4

Anonymous Referee #2

Referee comment on "Contribution of the world's main dust source regions to the global cycle of desert dust" by Jasper F. Kok et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-4-RC2, 2021

The authors present a very interesting and solid study on the contribution of major dust sources to the global dust cycle, with a unique framework that integrates an ensemble of global model simulations with observational constraints. This work is well designed and of broad interest to the dust community. The writing is clear and thorough. The topic of the paper is well suited for ACP. I recommend the manuscript for publication after only minor revisions.

General comments:

(1) The authors should give more “background information” related to the inverse model dataset in section 2. For example, the inverse model includes both natural and anthropogenic dust aerosols. It also excludes the high latitude dust emissions. Moreover, is the sum of the emission from the nine sources equal to the global dust emission in the inverse model? I think it would be important for the readers to make these things clear before moving to the results.

(2) In section 2.2, the contribution of each source region to global dust loading and DAOD from the AeroCom simulations is scaled by the lifetime and MEE of this work. This method assumes that the ratios of T_r to T_{glob} and $\square_r$ to $\square_{glob}$ are roughly similar in the AeroCom model and the analysis in this work. But the inverse model and model ensemble have larger size range than the AeroCom models, which is supposed to cause different lifetime and MEE (Table 1). Would this introduce any uncertainty to the AeroCom estimates? If so, how large is the uncertainty?

Specific comments:

Page 2, line 62: I would suggest replacing the words “ice nuclei” with “ice nucleating particles (INP)”. The old term “ice nuclei” is thought to be misleading by the ice nucleation community (See section 4.1 in Vali et al., 2015).

Page 5, EQ (1)-(8): I find these equation sets quite complicated and not friendly to the reader. I would suggest the authors to add a few descriptions about the equations in text before presenting them.

Page 6, line 161 and line 169: By “our analysis”, do you mean your model ensemble (as stated in line 154) or your inverse model? If it is model ensemble, why not use the inverse model here?

Page 7, line 176-179: It seems Figure 2 does not have subplots d to i?

Page 7, last paragraph: Please double check the numbers in this paragraph. Some of them are slightly different with Table 1. For example, the dust loading from Sahel ranges from 1.5-5.6 Tg in Table 1 but is written as “1.5-5.7 Tg” in the text (line 197).

Page 11, line 255-258: The East Asian dust is also known to be lifted by convection and basin-scale mountain-valley circulation. Why does it have smaller lifetime than dust from North African and the Middle East & Central Asia?

Page 11, line 260-261: Why does Figure S5 present results related to model ensembles instead of the inverse model? It seems this paragraph and Figure 3 mainly discuss the dust lifetime of the inverse model.

Page 13, line 290-291: What kind of correlation analysis is conducted here? How do you quantitively get the contribution of dust lifetime to the seasonal variation? Or in other words, how do you get the number “one third” here?

Page 16, line 345-346: I think Figure 6 only shows that less dust from Southern Hemisphere (SH) is transported to the Northern Hemisphere (NH). It does not have direct implication on the transport efficiency. The weaker emission in the SH (Table 1) may also result in the smaller contribution of SH dust to NH. If the authors want to discuss the transport efficiency here, it would be better to normalize the dust concentrations by the dust emission flux from each source.

Page 17, Figure 7: The title for subplot g is not fully visible.

Page 22, line 497: I guess it should be Table 3 instead of Table 2.

Page 27, line 631: Shi and Liu (2019) only discusses the dust glaciation effect in the mixed-phase clouds. Please refer to other literatures for the cirrus cloud effect (e.g., Liu et al., 2012).

Reference

Liu, X., Shi, X., Zhang, K., Jensen, E. J., Gettelman, A., Barahona, D., Nenes, A., and Lawson, P.: Sensitivity studies of dust ice nuclei effect on cirrus clouds with the Community Atmosphere Model CAM5, Atmos. Chem. Phys., 12, 12061–12079, https://doi.org/10.5194/acp-12-12061-2012, 2012.

Vali, G., DeMott, P. J., Möhler, O., and Whale, T. F.: Technical Note: A proposal for ice nucleation terminology, Atmos. Chem. Phys., 15, 10263–10270, https://doi.org/10.5194/acp-15-10263-2015, 2015.