Review of Kok et al., Improved representation of the global dust cycle using observational constraints on dust properties and abundance, 2020, ACPD

Overview

This article is well presented and well written, and fits well within the scope of ACP. The authors present a new methodology to constrain and improve dust models using an inverse modeling approach. This is evaluated compared to independent observations of surface dust concentration and dust deposition from around the globe. The authors show that their results deliver significant improvements in modeled parameters which have previously been extremely challenging for dust models. This work is a significant undertaking, and delivers important results to the field of global aerosol modeling. My comments are only minor clarifications and I recommend publication after they have been answered.

General Comments

The article is complex and somewhat lengthy – however the authors strive to carefully explain their methodology, which is a crucial part of the paper, and the description of the method is clear and appropriate. Therefore I advise retaining the current amount of detail and length (and not shortening the article, as suggested by reviewer 1).

Specific Comments

L57-58 – It is not just wind speed which is important to emission, but also the infrequent but strong wind occurrence which can contribute disproportionately strongly towards dust emission due to the non-linear scaling mentioned. Cowie et al. (2015) and Roberts et al. (2017) are relevant papers towards understanding and representing dust-generating winds in observations in models, with a focus on high-wind speed occurrence, which should be mentioned.

L86-87 – sentence is unclear.

Introduction – readers may find the scope of this article fairly similar to several previous articles by the lead author and research group (e.g. Adebiyi et al. 2020, Adebiyi & Kok 2020. Therefore, it would be helpful to guide the reader briefly through the steps in which this piece of work differs from others, either in the introduction or wherever the authors consider logical. Improvements here would contribute a large amount to increasing the significance of this paper.

L208-212 – it is not clear how the authors use the GEOS model to extend the size range for CESM and ModelE2 – please give more details (and also relate to comments for the supplement).

Section 2.3, p8 – take care when writing 'this study' and referring to Ridley et al. (2016) – it is not always clear that the authors mean Ridley et al. by 'this', as opposed to referring to the current article under review. 'That study' or a repeat of 'Ridley et al' would be clearer.

Section 2.3, 2nd & 3rd paragraphs – given that the reanalysis datasets also include aerosol data assimilation, why not just use them for DAOD rather than the Ridley dataset? What is the advantage of the Ridley dataset over the reanalysis datasets in general?

L412-413 – it would be useful to include the description of this dataset, the 'Saharan Dust Transect.'

L424-425 – this reasoning is unclear. "because most of the dust at this site originated locally from within and near the national park where the station was located" – does 'this site' refer to the Zimbabwe or Australian site?

Section 4.1 – Figs 3e and f are barely mentioned in the text. What does fig 3e tell us (seasonal DAOD) that fig 3f (annual) does not? What do their differences indicate?

L572 – O'Sullivan et al., 2020, is another recent publication finding that also found that coarse dust is deposited too quickly in models, and could be referenced here. Also L952.

L572-575 – what does a similar plot of either MEE or DAOD for dust longwave properties look like? (in terms of the panels shown in fig 5). Given that the authors point out that the longwave radiative effects of dust are also important, it would be useful to provide some information from this study regarding the LW properties.

Table 3 – include DAOD and MEE wavelength in caption or table header.

Section 4.3.1 – regarding figure 6 – it might be expected that if one took a multi-model mean the result might agree fairly well with the observations as well. Since the inverse model incorporates input from several models, can the authors explain and confirm why the inverse model can be taken as agreeing better with the observations that a multi-model mean might? I suppose this can be inferred from the size of the error bars compared to the range individual models, though it would be useful to include a discussion of this.

L738-9 – But isn't MERRA used for DAOD in the SH, rather than the Ridley dataset?

L953-954 – "This could be done either by similarly applying the constraints on the globally averaged size distribution (Adebiyi and Kok, 2020)" – can the authors be more specific about how this could be carried out in an online (climate) model?

Section 5.3 – various other datasets of global Dust AOD are now available, which may have similarities and/or differences to Ridley et al. (2016). For example, Pu and Ginoux (2018) based on MODIS data, datasets based on CALIOP satellite AOD where dust AOD is derived based on shape, and combinations of MODIS/MERRA data (e.g. MIDAS, Gkikas et al., 2021). A discussion of the possible implications of applying a different DAOD dataset might result in would be beneficial.

Figures

Fig 3 – caption "same as panel b" – should be panel e?

Figure 5 – what is meant by 'number load' – should this be number concentration?

Figure 6 – it is impossible to distinguish between the black and brown lines, and also very difficult to make out the letters identifying different models.

Fig 7 – what is meant by 'seasonal surface concentration' when given as one number? Same applies to fig 8, 10 and 11.

Fig 7 and fig 8 – could the mean of Aerocom models also be shown on the figures, since means of improved models and mean of ensemble models are shown, for clarity?

Supplement

CESM/CAM4 – A description of how the additional largest size bin was generated is missing.

L90-91 “Simulations for each of the nine source regions were obtained by tagging each particle size bin from the different source regions.” – it is not clear what is meant by this statement – please expand.

L96-97 – “We did not use the largest bin because it exceeds the 20 µm maximum diameter used in the inverse model.” – this statement appears to contradict the caption for table 1, main article, which indicates that the maximum size bin has been extended for this model.

L215 – “mm” – should be microns?

References

Cowie et al., The importance of rare, high-wind events for dust uplift in northern Africa, GRL, 2015, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015GL065819

Gkikas, A., Proestakis, E., Amiridis, V., Kazadzis, S., Di Tomaso, E., Tsekeri, A., Marinou, E., Hatzianastassiou, N., and Pérez García-Pando, C.: ModIs Dust AeroSol (MIDAS): a global fine-resolution dust optical depth data set, Atmos. Meas. Tech., 14, 309–334, https://doi.org/10.5194/amt-14-309-2021, 2021.

O’Sullivan et al., Models transport Saharan dust too low in the atmosphere: a comparison of the MetUM and CAMS forecasts with observations, Atmos. Chem. Phys., 20, 12955–12982, https://doi.org/10.5194/acp-20-12955-2020, 2020.

Roberts et al., New Saharan wind observations reveal substantial biases in analysed dust-generating winds, Atmospheric Science Letters, 2017, https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/asl.765