

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2022-406

Anonymous Referee #2

Referee comment on "Global distribution of Asian, Middle Eastern, and North African dust simulated by CESM1/CARMA" by Siying Lian et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-406-RC2>, 2022

Review of "Title: Global Distribution of Asian, Middle Eastern, and Saharan Dust Simulated by CESM1/CARMA" by Siying Lian et al.

The authors present a global dust transport modelling study to provide a quantification of the contribution of key dust source regions in North Africa, Asia, and the Middle East to the global dust distribution. The CESM1/CARMA model results are evaluated with ground measurements of dust size distribution, vertical profiles from ATom aircraft observations, and AERONET sun photometer measurements.

The research question is compelling as well as relevant. However, the present work does not make use of its potential and shows considerable weak points. Primarily, it does not adequately motivate what distinguishes this work from previous model studies. While there are some improvements in the representation of large dust particles, the comparisons with aircraft profiles point to known model uncertainties in vertical transport and wet removal, which are not adequately addressed and raise questions about the robustness of the estimates of vertical transport contributions. The modelled size distribution nevertheless shows weaknesses for dust particles beyond 10 μm diameter, and the opportunity is missed to investigate the role of giant dust particles, which according to measurements make a significant contribution up to the mid-troposphere but are not considered or underestimated here and in many other models. I also miss a detailed description of the relevant transport processes in the respective regions and their seasonal cycle. Unfortunately, the implications for example on heterogeneous freezing are only speculated.

Language wise, the manuscript is already in a good state, although there is a tendency that abbreviations are not explained. However, the quality of the figures needs to be improved in terms of label font size and a meaningful axis and colour bar labelling.

Therefore, the manuscript can only be recommended for publication after major revisions.

Detailed Comments:

- Page 7/8: Why are the comparisons with ATom2-4 not shown in the main text? In addition, the model results should be evaluated with CALIOP observations and ground-based lidar measurements.
- Figure 1: Please provide a meaningful label for the y-axis or explain in the figure caption. Despite the improvements in the representation of coarse dust particles compared to the AeroCom models, particles larger than 10 μm are still underestimated. This fact should be discussed in more detail, especially with respect to the generally underestimated role of giant dust particles in current dust models.
- Pages 14–16, Sect. 3.3 and 3.4: Possible reasons for the discrepancies between model results and observations have to be more sufficiently discussed. What is the reason for choosing the wavelength 1020 nm, would not 500 nm or 550 nm be more common?
- Pages 19/20, Sect. 4.2: How can these contributions to the vertical layering be interpreted with respect to the model uncertainties in vertical mixing shown in Fig. 5? What are the relevant meteorological processes behind these transport patterns? Describe in detail and refer to literature.
- Page 21, Sect. 5: The effects on cloud freezing are only speculation. Could you make any conclusions on the basis of your model simulations?
- Page 24, lines 484/485: Is this actually a result of this study? I could not find a corresponding description in the m