

Interactive comment on “Sensitivity of the Variability of Mineral Aerosol Simulations to Meteorological Forcing Datasets” by Molly B. Smith et al.

Anonymous Referee #2

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General comments:

The manuscript addresses the highly relevant issue of the representation of the variability of mineral dust in state-of-the-art global model simulations. The study is pursued using a set of simulations including runs with the global model CAM4/5 driven by different boundary conditions and two further other global models. The article is generally well written, and the number and quality of tables and figures is good.

What disappoints is the simplistic and superficial analysis of different correlation parameters without relating them in detail to the varying surface characteristics and meteorological drivers in specific geographic regions, and to global atmospheric circulation patterns (NAO, ENSO, etc.). In the presentation of results, there is no discrimination

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in the contribution of different desert regions to local dust conditions, and explanations of processes behind the correlations are missing. As an example, the link between precipitation and dust emission in the Sahel is left to the reader's interpretation. The contribution of Asian dust sources to the northern hemispheric dust cycle seems underrepresented in the discussion. A more detailed description and explanation of the figures would be desirable.

Specific comments:

1. Page 2: The Introduction lacks a brief overview of what is already known about source strength, seasonal and inter-annual variability of dust emissions, as well as their main drivers for important dust source regions. What is expected to be seen in the model results? An outline of the paper at the end of the Introduction would be helpful.

2. Page 3, line 18: Could you provide the scale factors here?

3. Page 6, lines: 9-11: I might have overseen this but I could not find the results in the manuscript. So, please omit the paragraph or present the other species in the text.

4. Page 6, lines: 23-24: You say that only sites where more than 50% of the modelled AOD was from dust are included in this comparison. To me it seems that due to this filtering the comparison is not independent. Would not it be better to compare observed coarse-mode AOD with modelled dust AOD or to use the angstrom coefficient to filter dust events? There is no further description of the filtering below. Which wavelength has the AOD used? For an overview it would be good to have the location of observation sites on a map.

5. Page 7, lines 26-28: I do not understand why these two records have been chosen, which have a much shorter and inconsistent time coverage, while considerably less but still several AERONET observations are available in the southern hemisphere.

6. Page 9, lines 5-23: The analysis is too general. At least it should be clearly de-

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scribed which desert contributes to the dust load at the individual sites, and then the variability and relevant changing atmospheric and/or surface conditions have to be evaluated respectively. I do not see why for the AODs you are limited to regions close to source regions (line 20).

7. Page 10, lines 12-13: “[. . .] insight into how much of the variability in dust is driven by SSTs.” Can this be quantified any further?

8. Page 10, line 17: Here and elsewhere in the text, even though it is obvious, please mention the dust source affecting the geographic region under consideration.

9. Page 10, lines 29-32: Could also vegetation-related changes in surface roughness explain the trend in the Sahelian dust emissions as discussed by Cowie et al. (GRL, 2013), and would these changes be considered by any of the models used? What exactly are the mechanisms behind these correlations?

10. Page 11, line 13: Correct: “[. . .] supporting the idea that the source strength decreased within the time period 1990 – 2005.”

11. Page 11, lines 29-31: Here, you may include a study by, e.g., Marsham et al. (GRL, 2011) who showed that coarse-resolution models usually fail to reproduce dust emission events related to moist convection.

12. Page 12, lines 1-27: The database for the analysis in the southern hemisphere is too small to be statistically significant. Problematic is also the shorter and, in particular, inconsistent time coverage of the measurements. Moreover, the two sites are affected by mineral dust from different sources, which is not discussed at all. You could easily broaden your database by using AERONET sun photometer observations.

13. Page 15, lines 15-17: The ‘global average’ is not a larger source. Could you please specify the smaller sources meant?

Technical corrections:

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1. Page 2, line 8: Do you mean “4-fold fluctuations”? 2. Page 5, line 26: Wording: “[. . .] it [the model] can only be operated using reanalysis datasets, [. . .]”. 3. Page 7, Eq. 1: A proper equation would look more scientific. 4. Page 11: IAV needs to be resolved. 5. Page 18: Correct: “whether other drivers”.

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