Atmospheric
Chemistry
and Physics

Open Access

Discussions

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

Anonymous Referee #1

Received and published: 30 September 2016

The authors compare several model frameworks, including different forcing data, different forcing methods and different models, to estimate the sensitivity in the seasonal and interannual variability of dust emission and transport. They find that the correlation in variability is roughly similar between simulations using different meteorological forcings and different models, while simulations forced by sea surface temperature only do not perform as well.

Considering the high uncertainty in dust simulations, investigating the sensitivity to different forcing data sets and models is an important contribution to the field of dust modelling. However, the manuscript presents a lot of information that is not necessarily relevant and that dilutes the actual results of the study. In addition, several parts of the

methodology and results are unclear, which make the interpretation further confusing.

The manuscript thus likely need to be rewritten before it can be considered for publication in Atmospheric Chemistry and Physics.

General comments

A. The model description is often confusing and strongly needs to be reorganized and clarified.

B. The discussion of the results jumps between a large number of figures and tables split between the manuscript and the supplement, which need to be reduced.

C. A significant part of the results further appears not to be significant, thus the discussion needs to be more focused to emphasize the actual contribution of the manuscript.

Specific comments

1. Introduction

p.2 l.7-15: this part is disconnected from the previous one and needs to be separated into a new paragraph; the different space and time scales and their relevance are confusing and need some hierarchy; what are "4x fluctuations"? p.2 l.15: this is partially redundant with the following lines and should be reorganized. p.2 l.21: this is obvious and could be more specific (interannual variability of dust emissions?). p.2 l.28-29: the purpose of the sentence is unclear. p.2 l.31: a short paragraph to describe the contents of the paper would be helpful at the end of the introduction.

2. Methodology

p.3 l.5-10: the meaning of "model" is confusing here between CESM, CAM, GCM... A description of the nature of these "models" would be helpful. p.3 l.12: "when the winds are strong" needs to be clarified. p.3 l.15: "as described in more detail elsewhere" needs to be rephrased. p.3 l.25-27: the purpose of the sentence is unclear. p.3 l.31-p.4 l.7: the methodology is unclear. Which variables are nudged? What about the

wind? What is the MATCH/CAM approach? Is MATCH used here? p.4 l.8-11: some details about the data are needed e.g. which reanalysis is used (NCEP and ECMWF are forecast centres). p.4 l.11-13: the purpose of the sentence is unclear; discuss conclusions of these studies? p.4 l.18-20: more details are needed, e.g. how the model is initialized and what is computed in the atmospheric model (CAM?) exactly (winds cannot be computed alone). p.4 l.20-22: this is unclear; what is the "correct input format"? p.4 l.31 onwards: the paragraph should be moved and merged with the description of AMIP above. p.4 l.34: why is there "no inconsistency between the atmospheric model and observations"? p.5 l.9-10: the purpose of the sentence is unclear. p.5 l.11-21: the contrast with CAM needs to be better emphasized. p.5 l.30 onwards: this should probably appear earlier, as MATCH is referred to in the previous section. p.6 l.9-11: this appears disconnected from the other paragraphs. p.7 l.5-7: the definitions are confusing. p.8 l.1-4: this is very confusing; does it mean that the method is wrong?

3. Results and Discussion

p.8 l.8: does it mean that the simulations are taken from previous studies? p.8 l.11: should it be S3? p.8 l.24-25: the sentence is vague. p.9 l.1: which data? The purpose of the sentence is unclear. p.9 l.5 onwards: there is a lot of information presented in many figures and tables split between the main manuscript and the supporting information, which makes the discussion very difficult to follow; the information needs to be presented in a concise way and the discussion better structured. p.10 l.17-32: this should be moved to the introduction. p.11: the whole discussion is confusing (is there a trend or not?) and its purpose is unclear, as it is not related neither to the monthly nor to the interannual variability. p.12 l.7-19: CAM4 exhibits much higher monthly variability than the other models. p.12 l.19-21: models driven by MERRA and NCEP are clearly better than ERAI and AMIP. p.12 l.25-27: the statement is weak. p.13 l.1-25: the results do not appear robust. p.13 l.28-31: this should be moved to the methods. p.13 l.33-p.14 l.2: are the AMIP simulations meaningful then? p.15 l.8-10: is Figure 13

relevant then? p.16 l.28: should this be Figure 16? Are Figure 14 and 15 needed at all? p.16 l.28-30: isn't it obvious that a whole year of data is required to estimate the annual mean?

4. Conclusions

p.17 l.11-12: this again requires the set-up of AMIP simulations to be better explained and asks if they are meaningful. p.17 l.25 onwards: the results do not appear that clear in section 3.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-638, 2016.