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Atmospheric Chemistry and Physics Discussions

Interactive comment on "How reliable are CMIP5 models in simulating dust optical depth?" *by* Bing Pu and Paul Ginoux

Anonymous Referee #1

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This work examines the performance of seven CMIP5 climate models with interactive dust emissions schemes against dust optical depth (DOD) from MODIS Deep Blue aerosol products. The performance assessment to reproduce magnitude, spatial pattern and variations of observed DOD is conducted in nine regions, namely North Africa, Middle East, Northern China, North America, India, Southeastern Asia, South Africa, South America and Australia. Furthermore, interannual variations of DOD are also examined together with the impact on it of controlling factors such as 10 m surface wind, precipitation and surface bareness derived from leaf are index (LAI) data. In order to examine the relative contribution of these controlling factors to DOD multiple linear regression is applied on both, observations and models. Calculated regression coefficients in addition to observed and simulated controlling factors are then used to project

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DOD to the future (both observations and models).

The authors show that although the models can reproduce the global distribution of DOD over land under present conditions, with a better representation over northern than southern hemisphere, the interannual variability of DOD is all in all not well captured by CMIP5 models. Furthermore, models also do not reproduce the observed relations between the DOD and the examined controlling factors. Projected changes of CMIP5 model mean under the RCP8.5 scenario are presented and compared to projections of a regression model.

The research presented is interesting and the paper is well written. As the authors mention in their introduction, performance of CMIP5 models to simulate dust has received little attention and this work is a good first step to change this. I recommend this paper to be published in ACP after some comments have been addressed.

General Comments:

1. The authors highlight the importance of examining the performance of current climate models in simulating dust and they choose to assess this performance by evaluating simulated DOD. In fact, in lines 73-75 the authors claim that evaluating DOD in "CMIP5 models will provide a clear picture of model capability of dust simulation". Although optical depth is a very common variable when it comes to validate models with respect to aerosols (be it dust or any other species), it is an integrated variable and therefore it does not provide any insight into the performance to reproduce the vertical distribution of aerosols. It has been shown that regional and global dust models can present similar performance in simulating AOD but present large diversity in emissions, deposition, surface concentration and vertical distribution (Huneeus et al., 2016). Although that study refers to forecast application, it is consistent with the findings in Huneeus et al. (2011). The authors should acknowledge this limitation in the discussion or conclusions, that although this evaluation is informative and necessary, it does not provide a full picture of current climate models to simulate dust. This similar

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performance in optical depth compared to large diversity in other parameters such as emissions, deposition and surface concentration might be linked to the practice to use AOD to tune dust simulations. Is this a practice that is also used in climate models?

2. In addition to examining the DOD projections from CMIP5 models, the authors also project DOD using calculated regression coefficients and compare these results to the simulated ones. I have to admit that I have difficulties in seeing the usefulness of this exercise. What is the point of it? The authors state that similarities are found between both projections “which may be informative” without specifying for what they might me informative. What do differences and similarities of both projections tell us?

3. The authors could improve the description of the methodology applied in the study. Regression coefficients are computed by regressing DOD from MODIS onto the observed controlling factors, the same procedure is repeated with model outputs to obtain “model” regression factors. Now when the interannual variability is examined, in line 320 it is unclear whether the reconstructed DOD using model regression factors or the one based on observations. I would have thought the former but then lines 332-335 refer to the observations making me doubt what reconstruction is then used in the analysis. Furthermore, regression analysis on observations is done at 1°x1° resolution (lines 207-208) while for model outputs the regression analysis is done at 2°x2.5° resolution. But at what resolution are the reconstructed projections done? at the observation or the model resolution? Potential impacts on the regression coefficients due to different resolution should also briefly be discussed.

4. I find it confusing that the paper is build around the seven CMIP5 models with interactive dust emissions to examine their performance to simulate DOD. But when presenting and describing the projections, the reconstructed ones based on the 16 models are considered. I understand and agree with the authors in the reasons to include more models, but then I would have expected that when examining the model performance (both climatology and interannual variability) these reconstruction (from the 16 models) also would be considered in order to be able to draw any conclusion

from their projections. How good do these reconstructed projections (16 models) perform when compared with observations in present conditions? Sure, outputs of figure 9 and S8 are similar, but are they for the same reasons? Unfortunately analysis in figure 6 cannot be reproduced for the 16 models. Maybe it would make more sense to base results with respect to reconstructed projections in section 3.3 on figure S8 and move current figure 9 to the supplement (basically swaooing as it is now) and then build on how these results are also seen (or not) in the 16 models.

Specific Comments:

Page 5, lines 73-75: See general comment above, I would suggest reformulating the statement.

Page 6, lines 98-100: Given the importance of DOD in this study I suggest you briefly describe the method how DOD was derived from AOD and specify the modifications you applied to adapt the method to collection 6.

Page 7, line 134: Table 2 is referenced without any reference to Table 1. At present Table 1 corresponds to information on the models used in this study which is addressed in section 2.3. Tables should be arranged according to the order they are referenced in the text.

Page 8, lines 138-143: Surface wind speed, bareness and precipitation are defined as controlling factors without providing any evidence or explanation why these parameters. However in lines 321-331 the authors explain why these parameters have been selected. I suggest moving these lines forward to section 2.2.

Page 8, line 156: Remove PRECL.

Page 9, lines 164-182: A reference to (current) Table 1 should be made in this section. In addition, information on the 16 models used in the future projections needs to be provided.

Page 10, line 188: Provide a reference for the mass extinction efficiency used.

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Page 10, lines 191-201: The authors illustrate the difference between the derived DOD and simulated one from one of the seven CMIP5 models with interactive dust emissions. It seems arbitrary why this model is used and not any other of the seven models? Is the intention of these lines to validate the derived DOD and therefore the chosen method? If that's the case then a more thorough validation should be done such as comparing the derived model mean DOD from all 16 models to the model mean from the seven CMIP5 models. Otherwise I don't see the point of having these analysis.

Page 11, lines 216-220: What period is considered in this analysis, same as observations, ie 2004-2016?

Page 11, line 226: Please provide some information on these 16 models, which models are they? Are the seven model with interactive dust emission part of these 16 models? Do they have prescribed emissions? A similar table as Table 1 should be included with relevant information of these 16 models.

Page 13, lines 258-260: How do the authors explain the shift to the north in the DOD by HadGEM2?

Page 13, line 269: remove "than other seasons".

Page 13, line 270: add "by the model mean" after "captured".

Page 13, lines 269-271: Since individual models are illustrated, authors should not only focus on the multi model mean but also on the individual models and their differences with respect to the multi model mean and the observations. For instance, MIROC and GFDL do not present the observed variability, in particular over N. America and India and they also present a different variability than the other models over northern China, with the peak closer to the observed one.

Page 14, lines 276-277: The MODIS DOD peak in Australia is hardly seen.

Page 15-16, lines 319-321: Which reconstructed DOD is used here? is it the one considering observed regression coefficients and simulated controlling factors? Or is it the one using simulated regression coefficients derived from model DOD and model controlling factors? Also, are only the seven CMIP5 models with interactive dust emission considered? The authors should be more specific which reconstruction they refer. Also, couldn't the correlation based on reconstructed DOD be integrated in the figure as an additional column?

Page 16, lines 321-332: move these lines to section 2. See general comment.

Page 18, lines 378-380: I have difficulties seeing the similarities in North Africa and the Middle east between the MODIS and CMIP5 regression coefficients pointed out by the authors. I actually see more the differences between both regressions in both regions. I would suggest the authors review the analysis in these lines.

Page 22, lines 470-473: On which results are the authors basing this statement. I suggest specifying.

Pages 22-23, lines 465-490: These lines would fit better in the discussion section.

Page 24, line 522-524: The statement seems something that would fit better in the conclusion section. Consider moving it.

Page 25, line 546: Suggest replacing "quite well" with something more academic.

Page 27, line 583: In which way are similarities between both projections "informative"? What information do they provide.

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