Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-242-RC2, 2018

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 #2

Received and published: 12 May 2018

The article presents an in-depth analysis of the CMIP5 models′ ability to reproduce the dust optical depth (DOD), considering both seasonal and inter-annual variability, as well as the driving factors behind those DOD levels. The observational data used are DOD over land derived from MODIS Terra-aqua data; bareness derived from AVHRR; 10m wind speed from ERA-Interim reanalysis; and precipitation from PRECL.

The analysis of the driving factors is performed by regressing the observed DOD from MODIS over land to the observed/reanalyzed driving factors. The analysis is then extended to future climate scenarios (RCP8.5) using both the CMIP5 models' dust outputs and the regression based on present day observed relationships between DOD and the driving factors.

The main results/conclusions are: 1) Models behave better over the NH large dust

sources. 2) Models do not reproduce interannual variability 3) The constraints from bareness in models are underestimated and the influences of wind speed and precipitation are overestimated. 4) A corrected projection of DOD based on the regression model is proposed. There are some similarities between the projections and the corrected projections.

The paper is very interesting, includes novelties and deserves publication. However, I have several doubts and comments that need clarification and further discussion.

General comments

1) DOD from MODIS: It is not clear what the DOD derived from MODIS refers to. Is it total dust optical depth or coarse dust optical depth? I understand that it refers to the total dust optical depth (fine and coarse) but I was confused when the product was compared to the coarse (O'Neill) product from AERONET. Can you please explain better the derivation of DOD from AOD in the paper? Given the importance of the dataset for the paper I feel it is not enough to refer the reader to other publications. Also, can you provide an estimation of the uncertainty of this product? The confidence of satellite data over the different regions is assessed by comparison with AERONET (few stations, low spatial coverage), CALIOP, and considering the number of days with available DOD per season. Results show that while in Africa, South America, Middle East and some Asian regions confidence seems to be high, for some regions in Asia/North America it largely depends on the season.ÂăIn my view, the strength or confidence on the DOD data by region should be considered when discussing: the modelled DOD evaluation at the regional level, the regression method projections and discrepancies with CMIP5 models.Âă

2) DOD from CMIP5 models: The authors compare the DOD derived from the selected CMIP5 models using Eq. (2). Using a value of 0.6 everywhere and for every model is an important simplification as it depends on model-dependent assumptions on size distribution and other issues such as the size range considered. While 0.6 may be

a reasonable value for GFDL-CM3, how can we be sure it is ok for other models? Is there any other model for which you could compare this assumption in addition to GFDL-CM3.

3) Clear sky vs all sky values: While the authors have made an effort to gather the largest possible amount of DOD data by using both Aqua and Terra, the results of the comparison between MODIS DOD and model DOD may be quite affected by the use of all sky values from the models instead of clear sky values. Can you at least quantify this effect by for example using clear sky DOD from GFDL-CM3? How large is this effect? This may be potentially important in areas with seasonal clouds and precipitation. Could this be one of the reasons for the strong disagreement in some regions?

4) Interannual variability: One of the findings of this study is that the interannual DOD variation is not very well captured by the CMIP5 models. It is stated that "models probably misrepresented these [controlling factor] relationships, in addition to their incapacity of capturing the interannual variations of individual controlling factors". Because of their nature, CMIP5 models cannot (and are not meant to) represent year-to-year variations of the driving factors in such a short time period. Therefore, the first part of the statement is just speculative, i.e., one cannot know whether the relationships are misrepresented from that analysis alone. I strongly believe that this part should be better discussed both in the results section and the conclusions. I also believe that the comparison between CMIP5 model output and observations in Figures S4 to S6 is not needed. Isn't it obvious that CMIP5 models cannot represent year-to-year variations of each season in a 12-year period?

5) The role of surface bareness: one of the important conclusions of the study is that "constraints from surface bareness are largely underestimated while the influences of surface wind and precipitations are overestimated". I have a few doubts/comments on this:

a. How can you know that the constraint from surface bareness is largely underestimated? Given your methodology, couldn't it be that the constraint of surface bareness is correct in absolute terms but the effect of precipitation (through soil humidity) is overestimated? This should be clarified. b. While I think that the methodology is sound, it is not clear to me how year-to-year variations of around 2-3 % in LAI (Figure S7) can have such an impact in the interannual variability of dust in Northern Africa. Because this conclusion has important implications, could you further discuss this point? What would be the physical mechanisms that could explain this? Can you provide the same figure (S7) but for the model derived bareness (both present day and future projections)? How well do the models compare with the observed range of variability of the LAI in arid regions (the Sahara for example)?

6) Regression method projections vs. CMIP5 projections: The regression method used to derive DOD in future scenarios is based upon 16 CMIP5 model variables (surface wind speed, bareness and precipitation) and compared to dynamical projections of only 7 CMIP5 models (those with online dust schemes). Partly, differences in future trends might come by differences in driving variables. You state [line 439] that projected DOD changes using the full sample or only 7 models are very similar. If so, why not using the same 7 model outputs as drivers? This would enhance consistency. Finally, why the similarities between the two approaches in some regions may be informative?

Minor comments:

- I suggest to list multiple references to the same topic chronologically, unless there are reason to order them differently, e.g. in the introduction.

- I think the column heading "Dust emission scheme" is somewhat misleading as the given references describe the implementation of a dust emission scheme, not the scheme itself. Perhaps rewording to "Dust emission implementation" or similar would help.

- I suggest changing Eq. (1) to Bareness = exp(-LAI). Also, is there a reference for this

equation?

- L. 146-147: I would normally not consider a resolution of 80km “very suitable to study the influence of wind speed on dust emission and transport on small scales”. I understand the intent of this statement, but I suggest rephrasing this to avoid misunderstanding.

- L. 160: I suggest to delete “relatively high” as well as “quite”

- L. 192: GFLD-CM3 should be GFDL-CM3Âă

- Line 205: clarify which DOD is regressed onto observed values, i.e. satellite derived DODÂă

- Fig. 3i: It is very hard to see the MODIS DOD pattern for Australia. Can this be improved?

- Fig. 4 is ok, but quite dense

- L. 311: variability instead of variations

- L. 328: wind erosion instead of “soil erosion from wind”

- Line 431: centaury should be centuryÂă

- L. 457 ff: Sometimes it is not clear if “models” refers to the CMIP5 models or projection ‘models’.

- Figure 6. It is difficult to sort out the different elements, e.g. the strength of the regression depending on the shading intensity is not visible. I would suggest: to make a zoom per region, or to display dependencies from the 3 variables in independent maps, and to use the same resolution for MODIS and CMIP5 maps to make easier a direct comparison.Âă

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-242, 2018.