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EGU

Interactive comment on "An observationally-constrained estimate of global dust aerosol optical depth" *by* David A. Ridley et al.

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This manuscript describes a method of combining satellite and model data in order to estimate the global dust AOD (DAOD). The principal idea here is that models do a good job of simulating non-dust AOD, and satellites do a good job of retrieving the total AOD, so the difference between the two should be a good estimate of DAOD. While I applaud the authors on their creative effort, and the obviously massive amount of time undertaken to complete this work, I find there to be a couple of major issues with the methods that I suspect are contributing to a bias in their global DAOD estimate, and increase the uncertainty. Thus, I am suggesting a major revision.

Signed, Amato Evan

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Discussion paper

Major Comments 1. A major assumption of this method is that model DAOD is biased, but that model AOD is not. However, this assumption, at least the part about model AOD not having any systematic bias, isn't justified. The authors suggest that they are accounting for errors related to underestimation of the non-dust AOD by reporting their global DAOD with a 2-sigma uncertainty range (P13, L15). However, if the models systematically underestimate the non-dust AOD, this will induce a high bias in their reported global DAOD, and thus simply increasing the uncertainty range isn't really appropriate. We need to know if there is a bias, particularly because a low bias in modeled non-dust AOD would serve to push the hybrid global DAOD estimate closer to the aerocom mean, and possible closer to the MERRAaero estimate.

One could determine if such a bias exists by comparing histograms of AOD for the models and AERONET, over land regions and over-water regions where there is no dust (but there is smoke, anthro. aerosols, and marine aerosols). The difference in those histograms can be used to calculate a bias (which could be corrected) and uncertainty in the models' non-dust AOD. These errors can then be carried through to the final global DAOD calculation.

2. I am also very concerned about use of the models' spatial structure of DAOD (the horizontal pattern of long-term mean DAOD). In Eqn 2 the authors rely on the spatial structure of modeled DAOD in order to estimate their hybrid global DAOD. The implicit assumption is that while the models' may exhibit biases in the absolute value of DAOD, they do a good job of reproducing the long-term mean spatial structure. However, later on in the paper (P11, Section 4.3) the authors examine the signs of the difference between modeled DAOD and that from their hybrid method in Fig 9 (Africa, N Atl, Gulf of Guinea), suggesting that the models emit too much dust at the source to compensate for the fact that wet and dry deposition is far too strong. So on the one hand you are saying that the spatial structure of model DAOD is good (Eqn 2) and on the other hand it's not (Fig 9).

If your hypothesis is correct, that the models emit too much dust because deposition is too strong, then Eqn 2 will introduce a bias into your global DAOD estimate depending on the relative fraction of regions (Fig 1) that are over dust emitting areas and those that are downwind. I think this means that because your regions in Figure 1 are overwhelmingly near or over dust sources, your final global DAOD estimate could be biased low? I'm not entirely sure... But the bottom line is that, given this bias in the spatial structure of dust from the models, there is an additional source of uncertainty in the global DAOD estimate, and potentially a bias, related to the distribution of the regions you choose (Fig 1). I'm not exactly sure how you can address this. Maybe add more over-water regions and redo the estimate only using over-water regions, the only using over-land regions, then using both (via Eqn 2)? Or maybe the way to address this potential bias/uncertainty is to recalculate global DAOD using an equal distribution of regions over dust sources and regions downwind of dust sources (also in Eqn 2).

3. Lastly, I think models report AOD even in the presence of 100% cloud cover. So, in the model, there could be an aerosol layer overlaying stratus clouds, and the model would save an AOD value. However, in the satellite world, there would be no AOD retrieval. Does this discrepancy induce a bias? Can you examine the model data (I guess you'd need daily or hourly output) to see?

Minor Comments

1. P7, L4: Spelling, "main" not "man"

2. Should alpha have a region superscript in Eqn 1?

3. P11, L25: Why would a lack of convectively driven dust emissions cause an overestimation of DAOD? Seems like it would be the opposite.

4. P7, L26: You write, "In the regions analyzed here the AOD is predominantly driven by dust aerosol, limiting the influence of the model non-dust AOD" but this simply isn't true. Region 1 (N. Atl) also has a big biomass burning contribution in the boreal winter. Regions 8 also has a contribution from anthro. aerosols from N. India during the dry

monsoon season. Same for region 10 (from Pakistan and Iran).

5. P10: Cloud filtering: Interesting that you are getting such a strong correlation between the two. Misclassification of optically thick dust as cloud may be pretty common, FYI.

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