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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Comparing multiple model-derived aerosol optical properties to collocated ground-based and satellite measurements" *by* Ilissa B. Ocko and Paul A. Ginoux

**Anonymous Referee #2**

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In this manuscript, "Comparing multiple model-derived aerosol optical properties to collocated ground-based and satellite measurements" the authors compare two different versions of the NOAA GFDL model with measurements of aerosol optical properties. They demonstrate the importance of looking at more than just the AOD when assessing the model performance and highlight deficiencies in the model representation of aerosol, such as biomass burning aerosol not lofted high enough in either model. The research clearly highlights the difficulties in modeling basic aerosol seasonality and loading in polluted regions. However, most AEROCOM studies do look at more than just the AOD when assessing the aerosol in models (e.g. Kinne et al., 2006, Huneeus



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et al., 2011). Therefore, I'm not sure how novel the multiple-metric approach truly is, a point that is highlighted in the abstract and throughout the work. The research presented is valuable but some aspects of the research need revisiting and the conclusions need improving.

Major Comments

1) I'm left feeling that the model representation of aerosols is generally poor in the regions compared, and that this might be a combination of emissions (definitely for biomass burning), spatial resolution, potentially optical properties, aerosol size distribution etc. While the authors show that comparing multiple metrics with observations can provide more insight, there is little in the way of concrete evidence that the those insights have helped improve the understanding of the discrepancies between model and observations. I don't mean to be overly critical, and realize the simulations are time consuming, but I think the authors must justify their choice to stop at the point of speculation and not perform further simulations to try understand which of the many plausible causes actually contribute to the observed discrepancy. Key findings should be presented more concisely if possible, and more from the viewpoint of the underlying causes rather than the models being X% higher or Y% lower than the observations which is of limited use to the reader.

2) If I understand correctly, the AERONET observations used are for 440nm whereas the model is at 550nm. This will cause a general high bias in the AERONET AOD relative to the models. The difference may be small where coarse aerosol dominates but this will increase up to maybe $\sim$25% in regions with fresh, fine aerosol, such as biomass burning regions. I don't think the current comparison is rigorous and recommend converting AERONET AOD to 550nm. AERONET provides AOD at multiple wavelengths (and the Angstrom Exponent) so it is trivial to calculate the AERONET AOD at 550nm.

3) Also regarding the comparison with AERONET, is the comparison of the closest





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grid box to the AERONET site, or has the model grid been interpolated to the exact site location? Lack of interpolation may make a substantial difference where there are strong gradients in aerosol.

4) With the CM3 model, it is difficult to understand how much of the discrepancy with observations might arise from the climate model meteorology (rather than using re-analysis fields). The authors do average over a 5-year period using the model, but it would be useful to see the interannual variability of the models on Figure 4 & 5 and some understanding of the interannual variability in the CALIOP observations.

5) It would be interesting to use the difference between the model and the observations to understand how the error in the models translates into uncertainties in the radiative effects and the interhemispheric forcing asymmetry. These are discussed qualitatively, but is it possible to expand this into some quantitative assessment using other model output fields ( surface and TOA radiative effect, etc.)?

6) I do not think the bullet-point conclusion format works well when the results are not concise. Splitting some of the conclusions into bullet points while others remain in paragraph form sees arbitrary. Please consider revising the fragmented conclusions into a more holistic discussion of the findings and how future research should proceed based on these findings.

Minor Comments

pg1 ln 29 Aerosol can travel 1000s of km in a week, so I wouldn't say it is localized around sources. Perhaps more localized than GHGs.

pg4 ln 9 Include a reference for the optical properties of BC and dust discussed.

pg5 ln 14 Add "(see Section 3.1)" regarding "computed offline" to let the reader know this will be explained.

pg5 ln 29 Remove extra period.

pg 8 please add to the description how SOA formation is treated. This is simplified and often underestimated in many models so is a potential source of discrepancy between the observations and the models.

pg 9 ln 8 Make it clear to the reader why using different years is not expected to be an issue.

pg11 ln 31 "have better magnitudes" - please rephrase.

pg12 ln 9 Remove extra punctuation

pg18 ln7 "Very nice job", please reword.

pg19 ln29 "poor emissions databases" this is very vague. Are any of the examples given included or not?

Figures 4 & 5

-in the caption, please state what the error bars represent.

-I may have missed it in the text, but the reason for missing data at Alta Floresta and other sites should be stated. I assume it is the lack of high enough AOD during that season for SSA retrieval?

-is it possible to add CALIOP AOD to these? This would be helpful when AERONET and CALIOP are often compared qualitatively in the text.

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