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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 #1

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The manuscript by Ocko and Ginoux presents a comparative study of two versions of the GFDL model, an older (CM2.1) and a newer one (CM3), against optical properties data from AERONET and CALIOP. The manuscript is clearly written and of interest to the science community, especially those using any version of the GFDL model. The analysis focuses on 4 urban locations and 3 sites influenced by significant biomass burning. Those sites, although spread around the globe, are not representative of the global atmosphere, since they represent a very small fraction of the surface of the Earth with exceptionally high pollution levels, at least seasonally. In addition, the coarse model resolution is not capable of resolving the very localized heavy pollution of the

urban centers studied, which can lead to spurious conclusions. Although I understand that there is value in comparing a global model with urban data and the authors made a considerable effort to justify that, I firmly believe that the absence of comparisons against places where the model has a chance to give good results is critical in assessing model performance. The apparent incapability of the model to resolve urban pollution also greatly degrades model skill, ending up with a not so flattering model performance, even the newer version of it, despite the great amount of work invested over the years, which resulted in large improvements in the parameterizations since the older version. I do not recommend publication in the present form, at least not until some analysis is included from locations where there is either regional pollution or cleaner conditions.

page 1, line 14 is mentioned below as 1.14, etc.

General comments

Section 3.1 (about the older model description) has some very strong assumptions about aerosol modeling. These include the absence of nitrate (6.1), the concentrations (not fluxes) of sea salt that scales with wind speed over the ocean (6.22) (what happens over land?), the zero sea salt over 850hPa (6.23), the offline aerosols coming from different (thus inconsistent) sources (6.31-7.1), the fixed 80% RH for optical calculations which is not even used for BC and OC (7.4-5). I understand that this is an older generation model that is probably not used any more, but in any case with such assumptions the correlations with measurements is expected to be poor. The fact that the new model performance is not greatly better is very surprising. I believe that the authors made the choice of using and presenting that old model to contrast the improvements in the newer model, something very useful for both the users of the GFDL model and its output (so they will look at both model versions) but also for the people that only care about the current model skill (that will look only the newer version comparisons). However, especially for the audience that belongs to the first group, the model performance probably degrades, as presented here (e.g. Figures 4-5, 15.8-9,

and 19.16). This comparison though is biased towards the urban stations where the models are not expected to perform well, which is something that even the authors acknowledge (11.29-30). A fair comparison really needs background (not necessarily clean) stations. A great example for this is Oklahoma (10.22-11.6 and figure 4), which is the only urban station captured. This is not a surprise, since the station is not in a city, but downwind of one, and represents regional pollution.

Another argument against comparing with background and even remote stations can be found when comparing the results of Naik et al. (2013), presented in 8.30-32: The global AOD biases are within 5% or 2%, while the differences presented here are significantly larger, and frequently exceed a factor of 2 (section 4.2.1). I understand the motive to accurately capture the very high pollution regions where aerosol-climate interactions maximize, but these are not representative of the global atmosphere and should not be used as a metric of model skill, as is done here.

The discussion is overly qualitative at times, in too many places to be able to enumerate. There are several examples, most of which include wording like “slight”, “reasonably”, “somewhat”, “a better/worse/nice job”, “better magnitudes”, “fairly well”, “correlates well”, etc. More quantitative statements need to be used throughout.

Specific comments

1.14: please put the names of the models in the abstract.

1.24-27: Longwave aerosol absorption is also an important climate driver.

3.5: . . .treatments IN THE TWO MODELS are. . .

3.11: Delete first instance of word “instruments”.

3.11: Describe a bit more the cities, e.g. population, including any other information that might be useful for the reader. Throughout the manuscript there are scattered information, e.g. types of fuels burned in the area, meteorological conditions, etc. This is a good place to have them all together.

4.6-8: BC has an Ångström exponent of 1 across the visible spectrum when externally mixed (see paragraph 112 in Bond et al., 2013), while a spectral dependence is measured for coated BC aerosols. Since BC is homogeneously mixed and not coated in this study, this statement is probably misleading.

4.13: To my knowledge, hardly any model uses interpolations when doing comparisons, primarily because the model uncertainties are probably larger than the concentration gradient in a grid box. Unless the authors believe the opposite, which would then require to justify why this approach was not followed, I recommend dropping the sentence.

4.25-26: How do you use temporal colocation with CALIOP, which only has day/night profiles at specific times a day? Simply take the level 3 product and compare with the modeled monthly mean? If yes, this is not what colocation means.

5.29: delete extra dot.

7.23: . . . Second, SOME (please say which) aerosol. . .

7.25: Aerosol indirect effects are not considered in this study (5.16-17), so either drop this sentence or remind the reader.

7.27: . . .to be HOMOGENEOUSLY internally mixed. . .

7.23-27: Is there nitrate aerosol in this version of CM3? I know there is from recent publications of the same group, but is it present in this current study?

8.12: “Transportation” –> “Transport”.

8.15: “property” –> “properties”.

Figure 3: How do you break down the per-component AOD when internal mixing is assumed? This is important information to be in the text, e.g. in 9.21.

9.28: Why the Jaegle et al. (2011) paper is cited? Is this parameterization used in

CM3? Please say so, if yes.

Section 4.2.1 is too long. I propose splitting it in two (or three, given my request for background stations), with the second part starting 13.16.

11.7-8: Delete “Upon further investigation”.

12.9: Fix typo in punctuation.

12.18: model shows –> models show.

12.30: delete both commas.

13.27: scale –> magnitude.

13.28: capture –> include.

14.3-17: Alta Floresta experienced severe deforestation at the beginning of the dataset used in the manuscript, which later declined significantly. This is probably why the error bars are too large during the dry season: not because of the strong interannual variability, but due to the steep decline of biomass burning in the area over the years. You might want to consider using a shorter period of time from the available long time series, one that is more representative of the simulated period.

14.24: shown –> present.

17.3: I might have missed it, but what is the assumption for the vertical distribution of biomass burning emissions in CM3?

18.26: properly –> accurately.

References

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