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## Comment on acp-2022-545

Anonymous Referee #2

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Referee comment on "An evaluation of biomass burning aerosol mass, extinction, and size distribution in GEOS using observations from CAMP<sup>2</sup>Ex" by Allison B. Marquardt Collow et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-545-RC2>, 2022

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The authors evaluate the effects of recent modifications in the GEOS model (version 5.22, 5.25 and GOCART2G) on aerosol-related estimations. To that effect, they use remote sensing and in situ measurements from the recent CAMP<sup>2</sup>EX airborne field campaign (Philippines, Aug-Oct 2019). Their study focuses on the evaluation of modeled Biomass Burning (BB) aerosol speciated mass and total backscatter, scatter, extinction, single scattering albedo and size distribution as well as modeled relative humidity, temperature, and planetary boundary layer height. This paper is well structured, its results important but needs clarification in many places. It will be worthy of publication once the issues below are properly addressed.

### Major comments:

- . We recommend the authors combine a few or move some figures to the appendix, especially the ones that are barely analyzed in the text (e.g., Fig. 9).
- . The recent presence of nitrate and Brown Carbon (BrC) aerosols in GOCART and GOCART2G needs more references and descriptions. We recommend that the authors include a table of microphysical and optical properties for all the species present in the model.
- . The changes applied to the different models in Table 1 should be further described and the authors should focus on explaining the potential effects of these changes on the modeled aerosol microphysics, spatial distribution, optical properties etc.

. A high-level diagram illustrating the different modules in the model as well as the many changes in Table 1 would be helpful. The diagram could also emphasize what this paper has investigated in more detail (e.g., RH).

. Throughout the paper, we recommend a clear discussion of all the error sources in the model.

### **Detailed Comments:**

. Line 14: "serving as cloud condensation nuclei". As written, it seems that this is the only way BB impacts radiative forcing. We recommend either re-wording or adding direct and semi direct radiative effects as well.

. Line 19: The authors should be clearer on which satellite/ ground-based sensor(s) is(are) used and in which model version.

. Line 24: Why not say "Aerosol extinction within GEOS is a function of the mass of different aerosol species, the ambient relative humidity, the assumed spectral optical properties and particle size distribution per species".

. Line 25: "aggressive" is not usually used in that case. Maybe "high" or "overestimated".

. Line 27: "a mode radius" – does GEOS assume only one size mode for its particle size distribution of OC? Aerosols are usually (at least) bi-modal so this should be discussed.

. Line 31: See comment for line 14.

. Line 37: "smoke and biomass burning aerosol" reads as if these two things were different.

. Line 49: SSA and its link to aerosol light absorption needs to be briefly described; and we recommend writing "... due to different assumptions for aerosol...".

. Line 51: We recommend simplifying and writing "An additional source of uncertainty would be the biomass burning aerosol emissions".

. Line 59: This should be "anthropogenic". The distinction between "white (anthropogenic) and brown (biomass burning, BB) OC" is not clear. Some BB aerosols can be labelled anthropogenic (e.g., prescribed fires) and it's not clear what the authors mean by "white" OC. This needs more description.

. Line 66: "two moment cloud microphysics" is mentioned abruptly here, with no obvious link to what was written previously or afterwards. If kept in the text, "two moment cloud microphysics" should also be described.

. Line 73: "future" is written twice.

. Line 80: "over the Philippine Sea".

. Line 82: A table listing the instruments, measurements, size ranges, resolutions and references is recommended here, like Table 1 in Edwards et al., [2021].

. Line 85: Is the AMS instrument operated by the LARGE team during CAMP<sup>2</sup>EX? It usually isn't the case so that would be new. It does not seem to be the case in Stahl et al. [2021]. Please check and clarify.

. Line 92: "50% uncertainty" needs a reference or "[personal communication from ...]" and if true, this should be better explained. Again, it was not mentioned in Stahl et al. [2021].

. Line 93: "inconsistencies between measured mass concentrations and optical properties". This is not clear. It should be explained/ illustrated/referenced.

. Line 117: GOCART (legacy) usually uses [e.g., Chin et al., 2002, 2009, 2014] as references. Also, GOCART was already introduced in line 62 (with the right reference i.e., Chin et al., 2002).

. Line 117: should read "hydrophilic and ... hydrophobic" or "hygroscopic and..."

hydrophobic”.

. line 118: “size bins per model species” is not clear here. It should be added that nitrate was not originally included in GOCART [e.g., Chin et al., 2002, 2009, 2014] but was developed more recently in the GMI model [Bian et al., 2017], which is an off-line chemistry model. It was then later implemented in the GEOS/GOCART model, followed by GOCART2G. As for BrC, the authors should specify that is it present in both GOCART and GOCART2G? If there is a specific reference describing GOCART2G, the authors should mention it. If not (Colarco et al., 2017 might be the only “indirect” reference), they should describe what is meant by BrC chemically and optically.

. Line 121: “The optics look up tables for each aerosol species are the same as described by Colarco et al. (2017)”. We can’t seem to find these look up tables in Colarco et al. (2017). We recommend that this paper adds a table describing these optical properties.

. Line 125: it would be better to write “bias corrected AOD observations from the Moderate Resolution Imaging Spectroradiometer (MODIS) aboard Terra and Aqua are assimilated in all the models of Table 1 except “No GAAS”. Is MODIS the only sensor that is assimilated in all models and is AERONET only assimilated in GEOS 5.22 and not the rest?

. Line 131: “... as well as deposition and wind-driven emissions of dust and sea salt”. Why not wind-driven emissions of BB or urban pollution?

. Line 137: This sentence is not clear and should be re-written.

. Line 155: What is meant by “The diurnal evolution of ... the lower troposphere”?

. Line 157: “Relative humidity was selected for this evaluation since it is used in the optics lookup tables for aerosols”. We recommend specifying “in the model” here. This is where more information on the model and its different modules would be helpful in the introduction. Why not plot modeled PBLH and measured MLH on Figure 1.

. Line 165: The angstrom exponent and its link to aerosol size should be explained

. Line 169: “or localized urban emissions not in the CEDS emissions dataset” This is not clear. The first part of the sentence is about AERONET measurements (and sources of errors in the Level 1.5 data) and the second part seems to be about the model.

. Line 176: How are the MLH vs PBLH computed? Wouldn't we not expect MLH and PBLH to be the same? This paper is focused on evaluating modeled BB aerosol composition, microphysics, and optical properties. The authors should explain why they are also evaluating the simulated PBLH (e.g., impact on modeled aerosol vertical distribution).

. Line 177: Some figures show the three model versions of Table 1 and some do not. We recommend consistency. We recommend adding "(not shown here)" after "indistinguishable"

. Line 179: "This trend ...".

. Line 182: We recommend describing the link between the spectral dependence of the aerosol backscatter and the size of particles.

. Line 185: quantify "slight improvement"

. Line 189: "there is a larger impact of the change in relative humidity between GEOS 5.22 and GEOS 5.25 than the aerosol updates implemented in GOCART2G" This sentence is not clear and illustrated.

. Line 191: "compares well"

. Line 192: "is located too high due to the height of the boundary layer in the model." The PBL is defined as a strong gradient in the aerosol scattering profile. This feels like circular reasoning.

. Line 193: Why not show lidar ratios as well?

. Line 197: "based on the region of interest" makes it sound as if HSRL aerosol type is based on the location, but it is not.

. Line 199: "the GEOS aerosol speciation for each HSRL2 derived aerosol type" – as the authors compare GEOS aerosol speciation for different aerosol types, why not analyze the results and evaluate whether the composition agrees well with the types in a quantitative

way? Are the GEOS species “correctly translating” the HSRL aerosol types? This would be similar to the work of Kacenelenbogen et al. [2022]

. Line 200: “drastic decrease in the sample size above 2 km” should be quantified. And “There is also a focus placed on the GOCART2G” can be replaced by “We focus on...”.

. Line 202: Figure 5e – The authors should explain the differences between “fresh smoke” and “smoke” from HSRL

. Line 203: “smallest sample size of the aerosol types” should be quantified.

. Line 204: “This could indicate deficiencies in the model’s optical properties for smoke, the transport, meaning the smoke plume is not in the correct location without the data assimilation, or uncertainties in the emissions” could be changed to “This could indicate deficiencies in the model’s smoke optical properties and transport (i.e., the smoke plume is not in the correct location without the data assimilation), or uncertainties in the BB emissions.”

. Line 207: “HSRL2 can have difficulty distinguishing between the two” This sentence needs more information and a reference.

. Line 211: “LARGE optical array is in situ and can provide a direct comparison between extinction and aerosol composition” this might be misleading if the AMS instrument is not operated by the LARGE group. Also, the extinction is for the total aerosol and composition is per species. The authors should clarify. The authors should also describe how these in situ measurements are selected i.e., airborne vertical profiles, constant altitude legs etc.

. Line 213: “representative of fine particles that are efficiently sampled by the inlet”; “were subsampled such that only particles with an aerodynamic diameter less than 5  $\mu\text{m}$  were included...”

. Line 234: “total aerosol mass concentration overestimated in GEOS”. We recommend the authors show and evaluate the modeled total aerosol mass concentration profile.

. Line 237: “an additional buildup of black carbon”

. Line 241: "This results in positive values for the analysis increment for black carbon mass" This sentence is not clear and should be re-written.

. Line 245: "Since brown carbon originates as a portion of what was organic carbon prior to GOCART2G, it is being included as organic carbon in the figure." This sentence is also not clear and should be re-written.

. Line 247: "In general, there is not enough aerosol for these two species in the model." Could be replaced by "In general, these two aerosol species are underestimated in the model."

. Line 255: Figure 7d

. Line 265: Figure 9 seems to have minimal value in this paper and could be replaced by 1-2 sentences in the text. The relative humidity plot is not discussed, and the lowest altitude is not quantified on Fig. 9.

. Line 268: "... optics look up tables are unchanged" -- This should be emphasized in Table 1 and in the description of Table 1.

. Line 269: "the aerosol mass concentration and relative humidity have the potential to differ in each of the model simulations" -- This should also clearly be stated in Table 1 and its description.

. Line 270: "the relationship between the two and the optical properties remain the same." This sentence is not clear.

. Line 274: "the ratio of sea salt" – the authors should consider replacing "ratio" by "fraction"

. Line 277: "given the preference for coarse mode sea salt in GEOS (Bian et al., 2019)". This sentence is not clear.

. Line 279: We recommend adding "in the model" after "The deficiency in sulphate and nitrate"

. Table 2: Instead of "LARGE Observations", the authors should write the name of the instrument e.g., "Aerodyne HR-ToF-AMS"; and the four digit in "0.0677:1" do not seem necessary.

. Line 286: "... are displayed in Figure 10"

. Line 287: "... which is always positive and representative of dry conditions"

. Line 289: "It is evident that GEOS needs a large bias in the mass concentration of organic carbon to accurately represent dry extinction." This sentence is not clear. The authors should rephrase. Also, they should consider quantifying the bias by providing an envelope around the 1:1 line and a percentage of points within this envelope.

. Line 310: The authors should provide the ranges of SSA values in Pistone et al. [2019]

. Line 312: "Nearly all observations have..."

. Line 324: "section 3.2"

. Line 326: The authors should describe the "chemical influence flag" and which gases it uses.

. Line 330: The authors should explain why the size distribution is bi-modal for FIMS and unimodal per species in GEOS

. Line 357: "... parameterizations as well as..."

. Line 384: "there is evidence of this in the FIMS observations from CAMP2Ex" – this sentence needs more information.

## References:

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