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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Constraining global aerosol emissions using POLDER/PARASOL satellite remote sensing observations" *by* Cheng Chen et al.

Anonymous Referee #2

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This manuscript presents an interesting approach to improving estimates of global aerosol emissions for use in global modelling, using the model adjoint to facilitate the minimization of a cost function against satellite-retrieved aerosol optical depth (AOD) and absorption aerosol optical depth (AAOD). Starting from a standard emission inventory as prior, this allows an *a posteriori* emission data set to be derived which improves the match of the resulting model to the chosen observations. The manuscript is sound and well presented, and merits publication in ACP, provided the comments below can be addressed.

Main comments

1. The whole procedure here is based on a single global model which is used for optimizing the emissions, and then the same model is used to evaluate the resulting dataset. Implicit in this is that the *a posteriori* emissions are tailored to this specific model: some of the changes from the *a priori* emissions may represent corrections of genuine errors in the emissions, while other changes may instead be compensating for model errors (for example, increased/decreased emissions to balance over/under-estimation of removal rates). This is acknowledged in passing (e.g. p.28, lines 27–29), but its implications for the applicability of the *a posteriori* data set should be discussed further. In particular, it should be made explicit in the manuscript whether this is presented as being an improved emission data set for use in this particular global model, or for more general use (in which case some justification for its wider applicability is required).
2. In relation to both the PARASOL/GRASP data used for the optimization, and the MODIS and OMI data used for evaluation, the possible impact of spatial sampling biases is briefly mentioned, but no attempt is made to quantify this. There is also no mention of the temporal resolution of the data, nor the additional impact of temporal sampling biases due to fixed satellite overpass times. (Are temporal means from the model used, or are model values temporally collocated to the satellite overpass against which they are compared? Might unaggregated Level 2 products allow for better collocation with the model?)

Additional minor comments

p.2, lines 3–4. This suggests that "harmonizing" emissions between different models is a good thing; however this is only true if there is confidence that they are converging on some kind of "truth". Merely adopting similar emissions without reducing their possible errors is likely to result in the multi-model ensemble of AOD and AAOD becoming under-dispersive.

p.5, lines 5–7. This seems to be assuming that it is the values for *uncoated* BC which are applicable, despite the fact that much of the BC in the environment is coated with sulfate, organics or other species. Some justification for the reliance on uncoated properties should be given.

p.5, line 25. Spurious "are" in "We used anthropogenic emissions are from the. . . "

p.5, line 25. A citation should be given for the "HTAP2 emissions" if possible.

p.6, line 5. Please clarify whether 0.05 here is an absolute or relative error. (Because AOD is dimensionless, it can't be obviously inferred from the units.) It's worth making it explicitly clear if the error is uniform, or dependent on the AOD (as is the case for some common retrievals).

p.7, Table 1. If SO_2 and SS are included in the table, it should be recapitulated in the caption that these are not subject to optimization/refinement in the work presented here, and that this is why the values are necessarily unchanged in the *a posteriori* data set.

p.10, line 23. The notation "BC-0.03; OC-0.11" is confusing, as the hyphen is easily misinterpreted as a minus sign attached to the number.

p.16, Figure 6. The scatter plots are very unclear, since there are a very large number of overlapping data points in the bulk of the data. Perhaps a density plot would be more appropriate. Also, AOD has a strongly skewed distribution (much closer to lognormal than normal) and the distribution might be clearer if presented on logarithmic axes.

p.17, lines 5–6. It is briefly mentioned here that bias increases in some cases (this is true of NMB for AOD, SSA and AAExp in the left column of Figure 6; and MB for AOD and SSA in the right column). However, the authors do not really discuss the reasons *why* the process of optimizing the emissions is leading to a worsening of the bias. These reasons should be explored further in the discussion.

p.19, lines 8–9 While 202/282 sites improved for AOD is a strong result, the other figures are lower and by AAExp (84/167) it is only half the sites which are improved which is pretty much a null result (unless the improvements on these sites are more substantial than the degradation at others). These figures should be reframed to make clear which of these are significant results, preferably with reference to a clear statement of statistical significance.

p.25, Figure 12. There are significant deteriorations (leading to strong positive bias) in the *a posteriori*, particularely over Asia in MAM and SON, and in Eastern/Southern Africa and South America in SON. These need to be more clearly referred to in the text, with some discussion of the likely causes.

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