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**Interactive
comment**

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

Anonymous Referee #1

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The authors derive simultaneously global and annual emissions of black carbon (BC), organic carbon (OC) and desert dust (DD) by constraining the GEOS-Chem model with POLDER/PARASOL spectral aerosol optical depth (AOD) and aerosol absorption optical depth (AAOD). Emission fluxes of sulphate (SU) and sea salt (SS) are not estimated and remain constant with the inversion. The inversion system applied in this work is an extension of one previously applied on a regional scale and is based on an adjoint of the GEOS-Chem model. The inversion method is applied to the year 2010 and an extensive indirect validation is conducted against independent AOD and AAOD measurements. The simulated AOD and AAOD with the a priori and a posteriori emissions are compared against equivalent observations from AERONET, MODIS and

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OMI. In addition, the authors compare the estimated fluxes with estimates from the literature. The estimated emission for BC, OC and DD are 18. Tg/yr, 109.9 Tg/yr and 731.6 Tg/yr, respectively, representing a change of 166.7%, 184.0% and -42.4% with respect to the a priori emissions. The research presented is innovative, very interesting and the paper is well written. I recommend this paper to be published in ACP after some minor comments have been addressed.

General Comments:

1. Although the authors describe the inversion system in general terms and present references providing further description of the system, they could give more information making it easier for the reader to understand the system. For instance, the authors did not specify whether the 7 DD bins in the model are perturbed homogeneously or not. It seems that they are but this should be made clear to the reader. Also, although the authors define the diagonal terms in both covariance error matrices it is not stated clearly whether both are actually diagonal matrices, one assumes they are since this is often the case, but again this should be stated explicitly. Finally, although the authors conduct an extensive validation of the inversion system and also compare the simulated AOD and AAOD between the a priori and a posteriori simulation, at no point do they actually show the POLDER products that are used to constrain the model and the improvements of the retrieved fields with respect to these products. Although one can only assume that the estimated emissions improve the simulated fields with respect to the inverted observations, presenting maps of the POLDER AOD and AAOD could contribute to understand the differences with the independent observations used in the validation. These maps could be included as supplement material if the authors prefer not to increase the number of figures in the manuscript and the global average could also be added to Table 2. 2. The inversion system applied has the advantage of being able of identifying new sources. This is a "nice feature" of the system since it could provide missing sources not included in the a priori emissions. However, the authors do not indicate whether the system actually identifies any missing source, only that

emissions are reduced or increased. It would be interesting to know whether the initial emissions miss any source present in the final estimate., in particular for the HTAP inventory used as a priori. 3. In section 4 the authors compare the retrieved total BC and OC emissions with estimates found in the literature. However given that multiple biomass burning and anthropogenic emission inventories exist, I would suggest that in addition of comparing the total fluxes as they do, they also compare them separately against biomass burning and anthropogenic inventories.

Specific Comments:

Page 5 line 25: remove "are" from the beginning of the sentence.

Page 8 line 6: I do not consider that seasonal BC and OC variations between a priori and a posteriori emissions can be called similar. Although the second maxima (Aug & Sept) is also observed in the a posteriori, this is not the case for the first maxima where there is a shift between both emission fluxes; while the a priori peaks in March, the a posteriori does so in Apr-May. The authors should better describe the differences and similarities between a priori and a posteriori in this figure.

Page 23 line 9: change "has also reported" to "has also been reported"

Page 23 line 10: review formulation of the sentence after the coma; "where are".

Page 23 line 17: section 3.3 indicates the statistics that will be used in the evaluation with independent measurement and the RMSE used in this section is not included. Why the change? I suggest either the RMSE is included when presenting the statistics in section 3.3 or here the analysis is limited to the statistics presented in that section.

Page 26 line 9: remove "the" between "OMI-observed" and "aerosol".

Page 28 line 28: Replace "probability" with "probably" and move "the" before "retrieved".

Page 29 line 3: A parenthesis is missing, most likely before Feng et al.