

Reply to anonymous referee #1

This paper provides a valuable study of the emissions of black carbon in Southeast Asia using an inverse model. The work is extensive and the results relevant, I therefore recommend publication in ACP.

The main issue with the paper is its length. According to the title, it is mainly about the emissions of BC in Southeast Asia. However, reading it, it also turns out to be about the use of different types of cost functions, and about the sensitivity of the penalty terms, and about Bousserez's new technique, and about the sensitivity to grid resolution, and about comparing different prior emissions, and about the difference in surface BC concentrations... There is value in describing in detail the modeling work, but it seems to me that a lot of this could have gone into supplementary material (or separate papers) to make a more readable paper.

Reply: We appreciate the reviewer's comments and support of this work. We agree that the steps necessary for addressing the title issue were numerous, given that this is the first attempt to invert this type of data. We took into consideration the suggestions to move some of this content to a supplement, such as Fig S2. However, given the criticism from reviewer 2 regarding interest in seeing more details of the inversion, we feel that aspects related to the cost function, penalty term, and error estimation etc. apparently warrant inclusion in the main manuscript. However, we have attempted to clarify / streamline these sections following suggestions from both reviewers.

Pg 28397: Sec 2.5: The explanation of the methods was hard to follow. I think the text could be reworked to be clearer about what is going on and why. Actually, I would probably recommend putting a, b, d into supplementary in order to streamline the paper although that's just a suggestion.

Reply: We have extensively revised and renamed Section 2.5 to make it much clearer. We retain the original organization of the manuscript given the second reviewer's comments as explained above.

Fig. 5: Given that the spatial patterns are similar, I would have found a single 2D map preferable, and then maybe a bar chart by region to show the differences. I would then just focus on the "best" inventory and relegate plots using the others to supplementary.

Reply: There are significant differences between April and October that are worth seeing. The suggestion to show only results using one emission inventory was considered. Definition of the regions would require an additional figure, or if overlaid on Fig 5 would occlude some results shown on these figures. So there wasn't a clear savings evident in terms of space. To more specifically view differences between the simulations at the measurement site locations, we refer the reader to Fig 18 in revised manuscript.

Sec. 4.1 did not use the penalty term, even though the penalty term is central to the inversion technique.

Reply: Inclusion of the penalty term will mute the impact of exploring different formulations of the observation term in the cost function. We now state in the revised section 2.5:

“Here we do not consider the penalty term in the cost function in order most clearly assess how formulation of the observation term impacts the inversion.”

Maybe the discussion of the different cost function methods can be placed into supplementary (it could probably have been a short paper on its own?)

Reply: We appreciate the reviewer’s suggestion, but again given the second reviewer’s comments we have kept this content within the main manuscript. But this section has been entirely rewritten for clarity and brevity.

Fig. 15 and text on Pg 28408: The sensitivity tests on the penalty function could have been described in more detailed or left out (preferably the latter).

Reply: We thank the reviewer’s suggestions. This part has been rewritten for clarity, and Fig 15 has been moved to the supplemental.

One question I had concerned the use of urban BC measurements in a model with a 0.5 degree grid. Maybe the authors could add a brief mention of this.

Reply: We were similarly concerned; hence we addressed resolution error when comparing the model results with the ground-based BC measurements in Sections 5.3.

Minor comments:

Please do some spell-checking, especially of the figures, eg:“CALIPOS0”, “Thus of cost function”

Reply: Revised.

Pg 28393-13: “those of OMI-based” is a sentence fragment.

Reply: Revised.

Fig. 18: “downcaling”

Reply: Revised.

Fig. 18: should label blue/red as before/after.

Reply: Revised.

Fig. 19: “Indan”

Reply: Revised.

A note on terminology: you should either have “a priori” or “prior”, and likewise “a posteriori” or “posterior”. eg. Fig 13. Should be “a priori”. Fig. 11 should be “prior” (or “a priori”)

Reply: Revised throughout.

Don’t equations 2 and 6 need an equal sign?

Reply: We have revised them to make it be clearer. Eq.2 (now Eq. 3 in revised manuscript) represents the observed BC AAOD at each vertical layer and Eq.6 (now Eq. 7 in revised manuscript) represents the observed BC AAOD column, respectively.