

Interactive comment on “The composition and variability of atmospheric aerosol over Southeast Asia during 2008” by W. Trivitayanurak et al.

Anonymous Referee #1

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This work presents an analysis of model simulated aerosol concentrations over Southeast Asia, focusing on Malaysian Borneo during the time period of the ACES/OP3 measurement campaign. Model estimates of aerosol distributions and budgets are presented along with comparison to observations from plane flights. Further, there is extensive comparison of model estimated AOD to MODIS AOD for the region and time period of interest. A significant model bias relative to MODIS is noted, and sensitivity calculations (enhanced emissions / particulate formation) are found not to substantially reduce this bias. The article reads clearly and is well organized. While the authors encounter some interesting conundrums regarding model fidelity, these perhaps are not investigated in as much detail as could be, and the paper focuses more on cataloging the model results. Overall, I think it is nearly suitable for publication, though the scientific significance could be greatly enhanced from further analysis and interpreta-

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tion regarding comparisons between the sensitivity runs, all sets of observations, and implications for sources of model error.

1 Comments

- The abstract could use at least one or two sentences of general motivation / background information for those readers lacking a predisposed interest in aerosol in Borneo during July of 2008.
- Also, the abstract contains only one sentence regarding model skill. I think the authors may be overlooking what is potentially the most interesting aspect of their work – that there are large unexplained biases in the model calculations over this region. Focusing on this aspect could actually be of greater interest to the broader aerosol community than details of the model budget.
- Abstract, last line: “that the model” – what aspect of the model?
- p22038: “rather than test the model skill at partitioning between SO_2 and SO_4^{2-} .” Again it seems the authors are shying away from the more challenging, and interesting, aspects of this study.
- p22045: A table of the statistical analysis (bias, error) for the model / aircraft data would be informative.
- Would it make a difference if comparisons were made to other MODIS products, such as the Deep Blue AOD retrievals, or those wherein the GEOS-Chem model is used for the retrieval (e.g., works of Drury et al., Wang et al.)? Or are the authors otherwise accounting for assumptions made regarding aerosol properties and distributions in the MODIS retrieval vs their model?

- Fig 5: Are the OC and SO₄ extinctions anti-correlated in the FT? Also, if one were to normalize these plots by the value of the aerosol concentrations, which species would make the largest contribution to extinction relative to its mass concentration? Would that help guide investigation of which sources of error could likely be leading to the model bias?
- In comparing Figs 5 and 6, why does SO₄ seem to play such a greater role in AOD than in extinction?
- p22047: “only on days when AOD is relatively low.” Does this description hold true for the SO₄ extinction peaking near day 50, where it seems like the total extinction is also high?
- p22051: “that chamber yields do not represent atmospheric formation”. This statement seems overly broad and I urge the authors to consider a more carefully worded conclusion. There are many studies of chamber yields under conditions of varying NO_x levels, particle acidity, aqueous content, organic aging, photochemical state, etc. The present modeling study has ruled out which ones, precisely? In general, the conclusions here regarding SOA could be better placed in context with recent advances in this field regarding the importance of these process, most of which are not included in the present modeling study (and thus the persistent bias in estimated SOA concentrations is perhaps anticipated).
- I found it odd that none of the results of the sensitivity runs were compared to the in situ observations, only to the MODIS AOD, and encourage the authors to revisit comparisons to the former during the sensitivity calculations. Is there even a mechanism by which the model could match both the AOD and still retain reasonable agreement with the profiles measured from the BA-e146 flights, or are the implications of these two data sets in opposition?

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2 Technical corrections

- p22039: The first paragraph describing the model is nearly devoid of references documenting the basic model configuration, which seems odd.
- p22041: Define “PAR”
- p22045: Define r .
- p22045: This is a really long paragraph that seems to cover many different things. I would suggest breaking it up a bit.
- p22046: Does this, and all subsequent analysis, refer to only the MODIS fine-mode fraction? If so, could the authors make this clear by some notational distinction, like fAOD, for the benefit of those reading nonlinearly?
- Fig. 3: Is it possible to use a better color scheme for the negative / positive plot? It’s not readily obvious where the zero color lies.
- Section 4.3: It wasn’t clear to me – were all of these different emissions perturbations applied globally or only to the Borneo region?
- p22050: Define “SOG”

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