

Interactive comment on “Evaluation of biomass burning aerosols in the HadGEM3 climate model with observations from the SAMBBA field campaign” by B. T. Johnson et al.

Anonymous Referee #2

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The authors present an evaluation of the performance of two aerosol simulation modules used within HadGEM, against a range of observations. The manuscript is thorough and well written, and falls within the scope of the journal. Some of the arguments made require some further clarification or quantification. After this and some other minor issues are dealt with, I recommend publication in ACP.

Main comments

My main question is about the scaling factors of GFED emissions used. I understand that they were chosen so as to "match the magnitude of AOD observations" (p16,17) / "give good agreement between modelled and observed mid-visible AOD" (p9,127). However, no further description of how the exact scalings were chosen is given, and

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yet the scaling factors make it all the way into the abstract, where they read as a conclusion about emission underestimates by GFED. Even slightly different scaling factors might change the discussion of AOD comparisons in section 3.1, in particular regional features such as discussed e.g. on page 16, lines 18-20. From the global/regional AOD values given in Figures 2-4, which reflect the entire aerosol distribution in the model, it seems that there's some range of scaling factors that might fit (as the authors also discuss on page 31. I'd suggest adding some sensitivity studies here, quantifying (through model bias calculations or similar) that any AOD comparison improvement in GLOMAP over CLASSIC is robust, and maybe toning down the focus on the scaling factors to be more in line with the authors own discussions in the Conclusion section.

Further, it's difficult to properly see from Figures 2-4 what regions are most affected by the improved aerosol treatment. I suggest adding some difference or ratio plots (maybe just versus one of the MODIS collections) to highlight the changes.

Figure 7 is another example where I don't quite follow the authors argument that GLOMAP is a clear improvement (page 19-20). It seems that some observed species ratios are closer to CLASSIC, some to GLOMAP, and unifying MAC to 10 m²/g also seems to pull both ways. Please add some quantification of the improvement here.

As the authors themselves point out (p29,117), sensitivity tests of the injection height assumptions of BB aerosols would be very useful - both for the present analysis, and for the aerosol community. Adding 1-2 simulations here would further increase the relevance of the paper.

Minor/technical issues

- Many references are made to two as-yet unavailable manuscripts ("Darbyshire et al., in preparation"). As comparisons to SAMBBA is a main point of the paper, and topics such as data averaging are important for reproducibility, this was somewhat annoying when trying to understand the analysis presented here.

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- P5I21: Please give a brief explanation of the MetUM, even if it's probably known to most readers.
- P6I20: So SO4 and BC emissions are climatologies, but BB emissions have annual variations? Or do they change through interpolations? Please clarify.
- P7I15: Will the assumption of no sea salt transport over land in CLASSIC affect AOD comparisons in BB-emission coastal regions? Does this contribute to differences to GLOMAP?
- P12I1: Add "of" between "prevalence" and "moist".

[Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-442, 2016.](#)

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