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Discussions

Interactive comment on "Uncertainties in global aerosols and climate effects due to biofuel emissions" *by* J. K. Kodros et al.

Anonymous Referee #1

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This paper presents global model simulations results on the effect of biofuels on global aerosol concentrations and aerosol climate effects. Using the global CTM GEOS-Chem combined with the TOMAS aerosol microphysical model, the authors explore the sensitivity of the results to a large number of parameters, including biofuel emission amount, composition, size, hygroscopicity and mixing state, as well as different schemes for nucleation and secondary organic aerosol.

The paper is too long and I still get lost in the details even after reading it several times. The authors focus on uncertainties in the biofuel emissions, on the one hand, and on uncertainties in the model parameterizations (aerosol direct effect), on the other hand. This leads to a huge amount of results (Table 4 and Fig. 2), which are discussed one after the other without a clear common thread. The authors should make an effort

to provide a proper synthesis of all these results and help the reader to identify the take-home messages in a clearer way.

The topic of biofuels is relevant also in the context of policy making, therefore a connection to real cases should be shown: how realistic are the tests with different values for the total emissions, or with different size distribution parameters? Is it possible to link the chosen parameters to actual policy measures/scenarios or to observational estimates?

My suggestion is to rewrite the paper focusing only on the uncertainties in the biofuel emissions and also to put these results into a broader context. In my opinion, the analysis of the parametric uncertainties in the direct effect calculation (Sec. 2.2) does not fit to the scope of the paper. It is rather a technical issue related to aerosol modelling techniques and it could be presented in a separate paper, for example in GMD.

Another major concern I have is that all the discussed simulations cover only 1 year (2005, P10207 L5). This does not allow any statistical analysis and it is not possible, for example, to tell whether the differences in DRE between the various experiments are statistically significant.

Other remarks:

- The introduction in too long: there is a clear unbalance among the different topics. Most of the exposed concepts are well known and can be found in textbooks (Seinfeld and Pandis, Bohren and Huffmann): there is no need for such a detail here. The introduction should also stress more clearly what the novelty aspect of the paper is.

- The calculation of the cloud-albedo effect makes several simplifying assumptions and the related uncertainties are not discussed. For example, what are the limitations of using a monthly-mean cloud climatology instead of an online calculated cloud field? Which quantities are used from the climatology: only cloud cover or also, e.g., liquid water path?

- The overview of all simulations given in Table 2 is confusing. This table should be restructured, showing the columns for all relevant parameters (BC and OA mass, GMD, SD, etc.) and their respective values in all simulations.
- Do the results for Bejing and Addis Ababa (Fig. 2c-d) refer to a single model gridbox? If so, I do not think that these can be taken as representative of the corresponding regions. I would rather show a spatial average over a broader region (e.g., China) and the corresponding spatial variability.
- The recommendations given at the end of the paper are too generic and do not add much to what is already known in terms of uncertainties in the emission data.

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