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Atmospheric Chemistry and Physics Discussions

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

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We thank reviewer 1 for their constructive response. We have extracted the critical comments by the reviewer and presented them in italics in-line below.

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.

To address this concern we have made several changes, including: a shorter introduction, more discussion on Table 5 and general results in Section 3.1, and an outline of sections 3.2-3.8. We will upload a version of the document with track changes that shows our modifications.

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?

While biofuel emissions are important to current and future policy decisions, this paper is meant to focus more on exploring the parameter space on what is currently being assumed in global models. We sought to understand how confidently we could estimate the sign of the aerosol-climate effects from biofuel emissions. If we are not confident in current climate impacts, we cannot possibly estimate the potential climate benefits from suggested policy decisions. We also hoped to determine which measurable parameters should be focused on in future studies in order to reduce uncertainty in the DRE and AIE from biofuel emissions. We plan to explore possible policy scenarios in future work.

We have added the following text to the introduction to clarify the goals of this study: "We do not explore specific future or policy-relevant biofuel-change scenarios (with the exception of one, simple 90%-biofuel-reduction scenario) as we focus on the uncertainties in biofuel aerosol effects in our present-day simulations. We plan to perform biofuel-change scenarios in future work."

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.

The main point of this paper is to quantify the uncertainties in the aerosol-climate effects due to biofuel aerosol emissions. Uncertainties regarding mixing state (i.e. Sec. 2.2) lead to a range of direct effects (positive to negative) in even a single pair of simulations. As such, we strongly feel this uncertainty adds to the overall discussion and conclusions of this paper. The mixing state of black carbon (and brown carbon) is an on-going area of research. This study provides motivation to better understand the morphology of black carbon. Additionally, choosing one mixing state for the paper would bias our results either in the positive or negative direction across all sensitivity simulations.

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.

From Jeff Pierce's earlier response: "Using multiple years to determine the statistical significance is essential when perturbing GCMs (e.g. changing an aerosol parameter), and the perturbation affects the meteorology. Thus, the control run and the perturbed run will have entirely different meteorology, which creates noise in climate forcings (is the difference between the simulations due to differences in the parameter or the meteorology?). Generally, 10-20 years of simulation is required to determine the strength of the perturbation (though this time length depends on how strong the actual effect of the perturbation is). We actually started our analysis using the GISS-E-TOMAS GCM, but ultimately we had to switch to a CTM because we could afford to run all of the simulations for 10-20 years.

In our work here, we use a CTM with fixed meteorology, so both the control and perturbed runs have the exact same meteorology. For radiative effects between control and perturbed simulations, single-year estimates are the norm for CTMs (e.g. Carslaw

et al., 2013; Spracklen et al., 2011; Ma et al., 2012). Yes, the forcings won't be exactly the same between different years (because the meteorology changes between years), but the variability is small. We will run some extra years of BASE and NOBIOFUEL and put these numbers into the paper to show that the signal is well above the variability."

We have run our 'BASE' and 'NOBIOF' simulation for 3 more years and found less than 3% change in mass and number concentrations and +/- 0.001 W m^{-2} differences in DRE and AIE between each year. We have added text to Section 2.3 to clarify.

Carslaw, K. S., Lee, L. a, Reddington, C. L., Pringle, K. J., Rap, a, Forster, P. M., Mann, G. W., Spracklen, D. V, Woodhouse, M. T., Regayre, L. a and Pierce, J. R.: Large contribution of natural aerosols to uncertainty in indirect forcing., Nature, 503, 67–71, doi:10.1038/nature12674, 2013.

Spracklen, D. V., Carslaw, K. S., Pöschl, U., Rap, A. and Forster, P. M.: Global cloud condensation nuclei influenced by carbonaceous combustion aerosol, Atmos. Chem. Phys., 11(17), 9067–9087, doi:10.5194/acp-11-9067-2011, 2011.

Ma, X., Yu, F., and Luo, G.: Aerosol direct radiative forcing based on GEOS-ChemAPM and uncertainties, Atmos. Chem. Phys., 12, 5563-5581, doi:10.5194/acp-12- 5563-2012, 2012.

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.

We have added the following lines discussing the novelty aspect of the paper: "To our knowledge, this is the first paper to study the sensitivity of aerosol climate effects to uncertainties in biofuel emissions and processes using an online aerosol microphysical model."

We have re-written parts of the introduction with these suggestions in mind. Specifi-

cally, we have reduced the discussion on mixing state to have a better balance among the different topics (distilling essential points of paragraphs 4 and 5 into paragraph 3). This significantly shortens the introduction. See our track changes version.

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?

We take the cloud amount (i.e., cloud fraction) and the liquid water path (derived from the cloud optical depth) from the ISCCP cloud climatology; we have added further details to Section 2.2 to clarify this. The offline determination of the cloud-albedo effect, using monthly averaged aerosol distributions and monthly averaged cloud data cannot capture the full complexity of aerosol-cloud interactions, or the effect of variations in either CDNC or cloud properties occurring on shorter timescales. However, we feel that this method allows us to understand the relative importance of the uncertainties in aerosol emissions and properties explored here. We have added further discussion on our method and its limitations to Section 2.2.

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.

Thank you for this suggestion. We agree and have changed the format of the Table 2.

Do the results for Beijing 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 results in Fig 2c-d were for a single model gridbox. We have updated this figure to

be a spatial average over a larger region. Figure 2c now corresponds to 'East China' (34N-42 N and 100E-120 E) and Figure 2d corresponds to 'East Africa' (2S-10 N and 35E-45E). This leads to a slight change in the absolute values of the calculated DRE and AIE, but does not greatly change the discussion or conclusions.

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.

We have added the following text to express specifically what is learned in this study: "In particular these measurements should include information on aerosol properties a few hours after emissions to better reflect to coarse spatial and temporal scale of global models. Lab data without aged emission information is incomplete and may introduce uncertainty into global modeling studies."

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