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

### **Anonymous Referee #2**

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The manuscript by Kodros et al. provides an extensive assessment of the uncertainties related to aerosols from biofuel emissions. The manuscript is well written and considers several interlinked uncertainties. The manuscript is suitable for publication after minor revisions.

Although the study includes even 18 sensitivity simulations, the manuscript is written in such form that the main results can usually be extracted with ease. Nevertheless, I would suggest minor revisions to help the reader. Currently, supplementary information is not provided with the manuscript. I feel that all the material provided is relevant to the goal of the manuscript. However, perhaps some of the more technical results could be moved to supplementary part, as they could still be used in the main texts as now. E.g. Figure 6, 8, 10 and 11 could be possibly moved to supplementary.

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Section 2.2: If I understand correctly, monthly average cloud fraction is taken as from GEOS5 for DRE and from ISCCP-D2 for AIE? What is the reason for this choice, since it seems to lead to inconsistent assessment of the two radiative effects?

Page 10209, line 14 onwards: where are the aerosol concentrations sampled from, cloud bottom?

Page 10209, line 14 onwards: The indirect effect of aerosols is extremely poorly constrained, and in many cases even difficult to analyze from simulations. I think the method for calculating the aerosol indirect effect (Spracklen et al., 2011) contains too many oversimplifications of the complex aerosol-cloud interaction. Additionally, using monthly averages for both aerosol concentrations and cloud properties can lead to potentially large uncertainties, since both fields vary in small timescales and are extremely coupled through e.g. aerosol formation and deposition processes. Apparently, mixed-phase and ice-clouds are not considered. Although I understand the authors' need to explicitly express the indirect effect in terms of  $W/m^2$ , I would suggest to report e.g. changes in CCN concentrations instead. If forcing is to be reported in current form, some additional discussion of AIE method in section 2.2 is needed.

Page 10210, line 10: What emission source does "assumed size distribution" relate to? Is this applied to all emissions, including e.g. fossil fuel use and agricultural burning?

Page 10210, line 15: Why a fixed timescale, and not coupled with simulated chemistry?

Page 10210, line 19: What method is used for SOA formation, and are there separate assumptions on the precursors and SOA volatilities from biogenic vs. anthropogenic sources?

Table 5: Although the idea of an overview of main parameters is good in order to summarize the extensive simulation results, I don't feel that the table provides something that couldn't be (or isn't already) said in the text. Why "Emissions size" in DRE and "Emission size distribution" in AIE? Why is composition not at all in AIE?

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