Atmos. Chem. Phys. Discuss., 15, C2126–C2130, 2015
www.atmos-chem-phys-discuss.net/15/C2126/2015/

Atmospheric Chemistry and Physics Discussions
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ACPD
15, C2126–C2130, 2015

Interactive Comment

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

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Received and published: 30 April 2015

Thank you, reviewer 1, for the thoughtful review. I want to respond to your main points quickly in case you want to iterate on these prior to the paper closing (expected June 2) and Jack writing the final author comments. I generally agree with your "other remarks", so I won't address those here.

Regarding the breadth and scope of the paper, I want to group the 2nd and 4th paragraphs of the review here (and I'll respond to the 3rd paragraph later). Those paragraphs are:

"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."

"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."

We do test a large number of different uncertain parameters, and we spent a lot of time thinking about what to include and how best to frame it. . The uncertainties in mixing state (the aerosol direct effect uncertainties mentioned above) lead to a range of effects (positive to negative) even in a single pair of simulations. The main point of the paper was to quantify the uncertainties in the climate effects of biofuel aerosols, and since the uncertainty in the mixing state is a significant contributor to this, we strongly feel that it belongs in this paper. Moreover, if mixing state is indeed unconstrained (as shown by measurement studies), removing this uncertainty leaves us in an awkward position of *choosing* a mixing state that could potentially bias our results (across the emission uncertainties) in one direction (positive or negative forcings).

The uncertainties in emissions, processing and mixing state (generally spanning the range of what different models assume) all lead to a widely unconstrained climate forcing from biofuel emissions (even the sign of the forcing is uncertain). This was our intended main point of the paper, and we will revise so that this is more clear. There is currently a lot of interest and research into the climate co-benefits (in addition to the health benefits) of reducing combustion air pollution. Our results show that unless we reduce the uncertainty space in many different dimensions, we have little ability to quantify the climate impacts of reducing black carbon and other species from biofuel.

We're not the first group to suggest this; however, to my knowledge no one has explored the parameter space systematically as we have.

We did spend quite a bit of time trying to organize the paper to make it easier to read (though based on the review, you may find this hard to believe). We will go back to thinking of ways to re-organize to make it a clearer read. If you have ideas (beyond what you've already stated) on how to do this while keeping the mixing-state uncertainties in the paper, we'd greatly appreciate it.

Paragraph 3 from the review:

"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?"

These are absolutely important questions, and we're currently funded by the US EPA to address them as part of a team with lab and field measurements. We did the work on this first paper as the lab and field measurements were starting, so the point of this first paper at it's inception was to understand the parameter space based on what is currently being assumed about biofuel aerosol (e.g. Are there parameters that *aren't* important? We found hygroscipicity fit this. Do we even know the sign of the climate forcing with confidences? No, we don't). As part of this EPA grant, our plan is to have 1-2 future papers that include (1) refined emissions and properties from the lab and field measurements, and (2) what are the climate and regional health responses to plausible cookstove/biofuel future changes? Thus, we have intentionally stayed away from policy-relevant scenarios here and left them for this/these future paper(s) (other than the simulation where we multiplied all emissions by 0.1, but this was mostly to test the linearity of the response). In addition, we did not want to include policy relevant scenarios here because (as you point out) the paper is already long and contains

ACPD
15, C2126–C2130, 2015

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multiple dimensions, which we feel all belong in this paper (see above).

The range of uncertainties used in this study are taken from observational studies. We describe the choice of uncertainty ranges and corresponding simulations in Section 2.3; we will consider revising this section to make it more clear. Many of the emission parameters and model processes are commonly used in global models, and we felt it was important to demonstrate the range of climate impacts that result from using different values.

Paragraph 4 from the review:

"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."

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 actually 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.

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