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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Radiative effects of inter-annually varying versus inter-annually invariant aerosol emissions from fires" *by* Benjamin S. Grandey et al.

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Aerosol emissions from open-burning fires show a distinct inter-annual variability. This manuscript by Grandey et al. quantifies the impact of the inter-annual fire emission variability on global and regional radiative effects. The authors present results from simulations with the Community Atmosphere Model version 5 (CAM5) which compare top-of-atmosphere radiative flux pertubations for inter-annually varying and inter-annually invariant emission inventories. The application of inter-annually varying fire emissions reduces the mean net radiative aerosol effect by about 23% globally and up to 58% regionally. The simulated global changes in precipitation, however, are not significant.

The scientific topics addressed by this manuscript are within the scope of ACP and

the study design is adequate for investigation of the given research questions. Overall, the paper is well structured and well written. The results are presented in a clear and understandable way, supported by neat figures and tables. The detailed discussion of the individual regional impacts appears slightly long-winded in some parts, but it provides substantial information for the overall conclusions of the study and might as well be beneficial for subsequent future studies. The reference list is comprehensive and largely reasonable. Due to the large differences in the overall fire aerosol radiative forcing between CAM5 and other global aerosol models, slightly more detailed descriptions and discussions of the used emission inventory uncertainties, the applied microphysical aerosol properties and the cloud shortwave radiative flux pertubations in the model, are desirable. In addition, I have some minor comments and suggestions.

I recommend publication in ACP if the authors sufficiently address the following comments in their revised manuscript.

General Comments:

1. The fire emission data set GFED applied in this study is a well-established and frequently used fire emission data set and there are no objections to use it for the purpose of this study. However, fire emission estimates based on other retrieval methods (e.g. the Global Fire Assimilation System GFAS, Kaiser et al., 2012) provide emission estimates which show substantial regional emission flux differences compared to GFED (e.g. Kaiser et al., 2012 and Zhang et al., 2014). Therefore I suggest at least to mention the emission estimate uncertainties in more detail in Section 2.2 and in the discussion of the results.

2. Globally, the radiative flux perturbation (RFP) of more than 1 Wm-2 of both, the 'conventional' and the 'revised' fire emission approaches presented in Section 3.1.1 are surprisingly large compared to most other studies which provide top-of-atmosphere radiative forcings of fire emissions which range between -0.3 Wm-2 and +0.1 Wm-2 (for a list of references see Veira et al., 2015, Section 5, page 7188). Can you provide a

more detailed explanation, why this is the case? The comparatively large values of the anthropogenic / total RFP in CAM5.1 provided by Shindell et al., 2013 (cited on P3, L16 and P5, L18 in this manuscript) do not necessarily explain the very large radiative effect of the fire aerosol emissions in particular. Might the black carbon (BC) and organic carbon (OC) atmospheric lifetime largely differ in the other models? Which refractive indices for BC and OC are used in CAM5 (for a comparison to ECHAM-HAM2, see Zhang et al., 2012)? What specific implementation in the cloud microphysics is included in CAM5, but not in the other models?

3. Cloud shortwave RFP is identified as major driver of the total net RFP differences between the simulations with inter-annually invariant and inter-annually varying fire emissions for many regions in the results section. Can you provide any numbers on high and low cloud cover changes in these regions? Do you see distinct differences in vertical cloud profiles? Some numbers would be helpful to further explain the non-linear saturation effects which are nicely presented in the discussion section.

Specific Comments:

Abstract:

P1, L14-15: '[...], we need to more accurately quantify the effects of aerosols [...]'. This sub-clause is a very general statement and does not provide specific outcomes of this study. Therefore the last sentence of the abstract can be shortened by pointing directly to the specific importance of the inter-annual variability of fire aerosols quantified in this study.

Introduction:

P1, L21-22: The references referring to peat fires in Indonesia are slightly misleading in this context. Although the sulfur content of the biomass varies substantially, all fire categories named in line 16-17 include emissions of sulfur dioxide as the emission factor inventories by Andreae and Merlet, 2001 and Akagi et al., 2011 demonstrate.

P1, L22-23: Does the statement 'These aerosols have a negative impact on [...]' refer to sulfur dioxide emissions from fire? It should be clarified that Lelieveld et al., 2015 quantified the overall impact of biomass burning aerosols (including black carbon and organic carbon) on human health, not the human health impact of sulfur emissions from fires in particular.

P2, L1: 'Aerosols scatter or absorb incoming sunlight'. The conjunction 'or' is misleading as both processes, scattering and absorption, apply to most atmospheric aerosol species.

P2, L7-8: '[...] many other aerosol effects on clouds have also been proposed.' Please provide examples of these 'other' effects.

P2, L11 and L14: In the reference list, there are two references given for Wang et al., 2009. Therefore it is unclear, which of the two papers is referred to in line 11 and line 14, respectively.

Methods:

P4, L14-18: In this paragraph, it should be explicitly stated that the black carbon and organic carbon fire emission estimates from GFED applied in this study represent an important source of uncertainty (see general comment 1).

Results:

P5, L26: Can you provide numbers for the increase in snow cover? It would be interesting to see the quantitative relationship between snow cover increase and surface albedo change.

P9, L19: Comparing Figure S1 to Table 1, it is interesting to see that a statistically significant overestimation of the net RFP strength is found for Central America and Southeast Asia. In contrast, the Eastern Maritime Continent and Temperate North America show temporal OC emission patterns of similiar magnitude like those of Central America and Southeast Asia, respectively (Figure S1), but nevertheless no statistically significant overestimation of the net RFP is simulated for the conventional approach in these regions. Might these findings indicate that the influence of the meteorology varies significantly in these regions or is this due to the saturation effects?

P10, L1-2: Why does black carbon suppress large-scale precipitation in such a way? If available, an analysis of vertical temperature profiles / atmospheric stability changes might be helpful for a better understanding although the discussion of these relations does of course not represent the focus of this study.

Discussion:

P11, L1: ‘Rather, across much of the globe, the “conventional” approach leads to a systematic overestimation of net RFP [. . .]’. Table 1 and Fig. 6c show that the statistically significant overestimation is largely limited to some specific regions. Therefore the use of the statement ‘much of the globe’ is inappropriate in this context.

P12, L12: What ‘other factors’ do you think of? Might interaction with other, non-fire related aerosol species play a role?

Conclusions:

P12, L28-30: This paragraph represents a nice description of those regions, where ignoring the inter-annual variability leads to an overestimation of the net radiative effect of fire aerosols. However, as shown in Table 1, there are many other regions, where no significant differences between the two scenarios are found and this should also be stated here.

P13, L3-5: At this point I suggest to provide some implications and ideas for subsequent future studies, which could be initiated by the results and conclusions presented in this paper, e.g. the quantification of daily vs. monthly fire emissions or a deeper analysis of the cloud micro-physical processes which are responsible for the found saturation effects.

All figures presented in the manuscript represent elaborated and easily readable vi-

sualizations of high quality with detailed figure captions. Figure 6 and Figure 8 are neat and self-explanatory figures, but in the final manuscript the figure size should be increased in order to improve the readability of the stippling.

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