Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-599-RC1, 2016

Atmospheric Chemistry and Physics
Open Access
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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Received and published: 10 August 2016

Grandey et al. compute the global radiative effects of fire aerosol emissions in a series of simulations with CAM5. In this experiment they compare the radiative effects that result when a monthly climatology is used to drive the emissions, to the radiative effects that result when emissions include interannual variability. The magnitude of the forcing is greater when the monthly climatology is used, suggesting a saturation of the first indirect aerosol effect, and shedding light on previous estimates of fire aerosol radiative forcings which typically use the climatology approach. There is no similarly large difference in precipitation between the two emissions schemes. Results are broken down by aerosol species and by region which highlights the greater interannual variability of

fires in boreal regions compared to the tropics and subtropics.

The design of the experiment is thoughtful and well suited to answer the questions posed in the introduction. The main result, a 20% decrease in RF magnitude when using the interannually varying fire emissions in this model setup, has implications beyond fire modeling and for Earth System modeling since many major ESM projects use decadal mean fire emissions. I have some comments, mostly minor, and suggestions for discussing the saturation effect differently.

General comments:

1. The argument in the Discussion section (Pg 11 Lines 3-6) for saturation of the aerosol radiative effect on clouds for the high fire emission years and locations (as opposed to the steady mean emissions) seems right, but the test of this hypothesis, illustrated with Figure 9, is not as convincing. Figure 9 shows how the sublinear response of the indirect effect to increases in aerosols is evident for a few grid points in a region, but does not give reason to think that the emissions vs. RFP relationship at these three grid points should apply generally. Especially give that two of the five points selected do not show the same relationship. And since the five points in the figure were already chosen specially because they had the largest FMEAN-F{yyyy} difference (among the other criteria), the figure does not show that the saturation effect matters here necessarily.

What if you were to plot timeseries (maybe monthly?) of Boreal Asia region average OC emissions, CCN (I think there is a standard CCN 0.2%SS variable output in CAM5?) and RFP, or some metric of forcing efficiency, with each timeseries standardized so they could be plotted together. Just one F{yyyy} could be plotted with FMEAN I think. This could show whether peaks and valleys in the interannual varying OC/CCN timeseries are matched by similar increases and decreases in RFP or if this saturates at the high extremes as would be expected. I am not sure if this effect would be clearly visible on a regional average but this might be something to try.

I also suggest including a standard reference for the saturation effect (maybe at Pg 11, Line 4) such as Boucher and Pham (2002). And since the first indirect effect is not isolated from all aerosol/cloud effects in this study, i.e. lifetime effect, semi-direct effect, it would be helpful to include discussion of how the additional aerosol effects factor in to estimates of the saturation effect. Or how they cloud the conclusions that can be drawn.

2. Related to this, I think it would be helpful to include more details about how aerosols interact with the radiative budget in this model setup, probably in the Methods section 2.1. For example, mentioning the different effects that are included in the RFP such as the direct effect, semi-direct effect, etc., explaining how aerosols can perturb the radiative flux in this model setup. This is also important to explain for the hydrological response which is discussed in Section 3.2 and shown in Figure 8. Aerosol impacts on both stratiform and convective clouds are included even though aerosols in the model do not directly interact with the convective scheme (Section 2.1). So here is another instance where explaining how the aerosols indirectly impact convection in the methods would be helpful.

Minor comments:

Pg 5, Lines 15-17: There are not many estimates of fire RF but you could include Chuang et al. (2002), -1.16 Wm-2 (first indirect effect only), and Ward et al. (2012), -1.38 Wm-2 (also CAM5), as additional points of comparison.

Pg 6, Line 9: The foundation for the rest of the results section is developed very nicely in this section. For this line, please include how you define statistical significance here.

Pg 6, Lines 16-17: It is mentioned here and shown in Figure 5 that fire aerosol impacts on forcing extend over ocean but it seems like only land area is considered in the regional estimates? In Ward et al. (2012) we also found large forcings (< -5 Wm-2) over ocean for fire aerosols, in fact, globally the largest indirect aerosol forcings were for the marine stratocumulus decks off the coasts of western Africa and South America.

If forcings over ocean are not considered here, can you discuss how the answers might be different if these areas were included?

Pg 7, Lines 26-27: I wonder if you can speculate why the BC emissions have a higher cloud sw forcing efficiency in these regions, and why the BC sw cloud forcing does not have a consistent sign in general when looking at all global regions. Perhaps this is a result of semi-direct effects (e.g. Sakaeda et al., 2011; Koch and Del Genio, 2010).

Pg 8, Lines 4-6: This summary point for the South American region is clear and nicely explained. I think it is important to note again here that aerosols are only microphysically active for stratiform clouds. This could be especially important in the tropical regions.

Pg 9, Line 1: Why is EQAS divided into two sub-regions for this study?

Pg 10, Lines 9-12: This sentence left me wondering why this is the case – that is that the fire aerosols lead to a precipitation increase. I am not sure it is feasible to tease out the reasons for this given the scope of this study, but it might be worth looking at what other changes are occurring such as changes in cloud fraction, cloud depths, CDNC, etc. that might provide some insight. Or maybe this has to do with differences in snow cover due to fire aerosols.

Pg 10, Lines 15-16: This is a good point, and might be a good place to note that Tosca et al. (2013) and Clark et al. (2015) do show a dynamical response in precipitation to fire aerosols using a slab ocean, although with non-interannually varying emissions. Another place these could be cited would be on/near Pg 9, Line 24.

Pg 13, Line 8: It is great that the data from this study are made available and so easily accessible.

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Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-599, 2016.