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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Variability, timescales, and non-linearity in climate responses to black carbon emissions" *by* Yang Yang et al.

Anonymous Referee #1

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This study concerns the climate response of black carbon (BC) emissions in a fully-coupled climate model. The study is motivated by the potential for BC emissions reductions, and the authors evaluate the non-linearity of emission perturbations and the transient response. As BC has received attention from a policy-perspective to reduce global warming (e.g. CCAC and the Arctic Council), investigating possible non-linearities in emission perturbations and the transient response of BC is important and highly relevant for ACP.

Unfortunately, I think the study fails to answer these questions. Since the authors do not find any significant climate change from present-day BC emissions, the authors conclude that BC emission cuts may not be detectable and that the climate impact of BC should be expressed directly in terms of emissions.

First; Do the authors mean that we cannot say anything about the climate impact of BC? I would argue that emissions are not climate impacts. As this paper is clearly motivated by policy-relevant questions, I am confused about what the authors are trying to conclude.

Second; I agree that cutting BC emissions would not be detectable on a thermostat in the real world. As a matter of fact; few things would, except radical changes like cutting CO2-emissions to zero. Is this relevant? As researchers, we must use models (this is why we use them) to provide our best estimate on the climate impact of e.g. cutting BC emissions, and then it is up to the policy-makers to decide if it is worth it in terms of costs, feasibility, co-emitted species etc.

Third; the simulating period for these runs are too short to make these conclusions. If the simulation period was long enough, I argue that the authors would 1) be able to detect a signal from present-day emissions and 2) quantify the non-linearity of different emissions perturbations. Are the temperature sensitivities in Figure 8 significantly different from each other? The authors refer to natural variability as error bars, which I find a bit odd.

The most important finding in this study, I think, is the short transient response of 2-3 years and the lack of a long-term response that the authors find for BC . This contradicts the much-used study by Boucher and Reddy (2007) where it is shown that BC both has a short-term response and a long-term response (ocean). If this is true it will be important for policy-makers, as a rapid BC mitigation will not be crucial for reaching e.g. the 2-degree target and can be delayed for some time. Physically, this means that BC emissions mostly influences the boundary layer over land surface, and do not warm the ocean due to a stabilization of the marine stratocumulus clouds. Would this be specific or sensitive to the model and the cloud scheme? In Boucher and Reddy (2007), they use an impulse temperature response function with both a short-term and a long term. How certain are the authors that there is no long-term response? In L404 you state that 'by our observation that there is no detectable long-term trend after the

initial transient period'. This is a bit vague. Can you perform a hypothesis test to see if there is no long-term trend? But, again, the simulation period is too short to estimate any long-term responses.

I suggest that the authors either extend their simulation period or significantly tone down their conclusions. But if the latter; I am not sure how much added value this study will provide. However, if the authors do extend their simulation period (yes, this will require some extra work), I think this study can be an important contribution to the field.

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