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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivity of atmospheric aerosol scavenging to precipitation intensity and frequency in the context of global climate change" *by* Pei Hou et al.

Anonymous Referee #3

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Hou et al. systematically investigate the effect of precipitation frequency and intensity on aerosol scavenging using a coarse resolution global model with a rather simplistic description of aerosol scavenging. The topic is especially interesting since the changes in precipitation characteristics (e.g. more extreme precipitation and possibly less drizzle) constitute an important contributor to the climate change signal. While the finding that the change of the black carbon lifetime in a changing climate might be dominated by changes in precipitation frequency and not in precipitation amount does not seem overly surprising in the light of the cited literature, this study nevertheless seems very interesting and useful to me, especially since to my knowledge this study represents the first attempt to investigate the topic in such a focused and systematic fashion. The

study nicely explains why an increase in total precipitation amount does not necessarily lead to a decrease in aerosol lifetime (independent of changes in spatial patterns that may for example impact some regions with high emissions more than others). In my opinion the manuscript serves to highlight a rather interesting and important topic and in spite of some limitations it can serve as a very good base for further studies. I recommend to publish the manuscript subject to minor revisions.

Specific comments/suggestions

1. l. 86ff: I think that (a) scaling the precipitation by a uniform factor for each grid box and (b) using a stochastic function where precipitation is turned off regardless of whether it is heavy or light precipitation may in principle lead to different outcomes compared to what might be expected from climate change (in which e.g. strong precipitation intensity may be enhanced while weak precipitation may decrease or remain unchanged and models also suggest very distinct spatial patterns) and I am not sure that the results from these very idealized sensitivity tests can be used to deduce a quantitatively correct answer for the climate change signal. I suggest to discuss this point. Also, as far as possible, I would appreciate if the authors could put their estimate of the change in aerosol lifetime into the context of other estimates from the literature, e.g. in the conclusion section in l. 241, although most of the existing literature estimates will not be directly comparable since they look at different regions and times. For example, Fang et al. (2011) estimate a change in lifetime for their SAt tracer. I also wonder if it would make sense to construct Fig. 2 for each region separately?

2. Especially the time period covered by the TRMM dataset is rather short, so that influences of internal variability are likely to play some role at least on a regional bases. On the other hand, the increase in precipitation intensity is consistent with expectations in a warming climate. It would also be interesting to see what part of the changes in precipitation frequency in Fig. 4 may be associated with internal variability, although I realize that this is outside the scope of this study. I think it would nevertheless be good to more explicitly mention that some of the regional trends may at least in part

be due to internal variability e.g. in line 221. For example CMIP5 model simulations suggest that the effect of internal variability even on multi-decadal regional precipitation trends can be rather large, especially for small regions. The global average changes, on the other hand, are much more directly related to the forcing strength. The large spread in the values of precipitation frequency in Fig. 4 may also be an indication of internal variability, although I am not sure if one can obtain an estimate based on the existing literature. Further research which is outside the scope of this work may be required to quantify this. One way to "filter out" the effect of internal variability might be to compute the average change in the BC lifetime over all regions, although one could argue that this also means loosing other information that is contained in the regional averages (e.g. differences due to different characteristics of the regions) and that the regional lifetimes are generally of larger interest than the global average. My recommendation would nevertheless be to compute the 30-year changes of the global average BC lifetime for all the land areas (with the contributions from the individual regions weighted by the size of the individual regions) and also for the entire globe and to state the values in the conclusion section. This may then also facilitate a more meaningful discussion of the results from this study in relation to existing literature.

3. The parameter range that is explored in Fig. 2 seems very large in the context of global climate change and there seem to be relatively few sensitivity simulations that are in the range of expected climate change. On the other hand, any potential bias that results from this will most likely not be overly large in the light of other uncertainties that stem from incomplete knowledge of actual and expected precipitation changes, uncertainties in the scavenging formulation, and possibly also uncertainties related to the design of the study (see my point one #1 above).

Other specific comments/suggestions/questions:

1. In the introduction, there are a few cases (Salzmann et al., line 28; Trenberth et al., 2007, line 29; Trenberth et al., 2011, line 31; Dawson et al., 2007, line 36; Fang et al., 2011, line 40) in which it might be nice to know what the cited findings are based

on (e.g. observations, regional/global model, theoretical arguments, combination of modeling and observations, models constrained by observations, etc).

2. l. 59: in addition to the URL, please also cite at least one paper that describes GEOS-Chem, even if it not exactly the version that is used here.

3. l. 72: unit of P?

4. l. 78: did the authors check whether the results are sensitive to this definition?

5. l. 115f: did the authors check whether the result is sensitive to this?

6. l. 157 and lines 165ff: good points that are nicely explained.

7. l. 178: are those the standard deviations of the yearly means?

8. l. 240 ff: "precipitation changes" is used here and also further below. It would be better to be more specific regarding whether this is mostly frequency or intensity.

9. l. 251: "feedbacks" are usually understood to be mediated by sea surface temperature (SST) change. In a model run in which SSTs are prescribed based on observations, the effect of aerosol on SST during this period is actually taken into account. But the authors are right in the sense that assessing the magnitude of the feedbacks is not possible in such a setup.

Suggestions for technical corrections

l. 15: omit "simulation" l. 19: aerosols -> aerosol l. 26: other atmospheric elements -> soluble trace gases l. 67: details -> detail l. 86: control -> the control l. 93: simulation tests -> sensitivity tests l. 98: rate -> rates l. 104: precipitations -> precipitation l. 108: We -> . We l. 126 control -> the control l. 149: that -> that this l. 200: same -> the same l. 232: have -> has l. 315: year? l. 346: control -> the control

Fig 1: please increase the size of the labels (and/or magnify the figure) and increase the resolution so that the figure can be magnified on the screen. Please also consider increasing the resolution of Fig. 5.

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