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Interactive Comment

Interactive comment on "Aerosol–radiation–cloud interactions in a regional coupled model: the effects of convective parameterisation and resolution" *by* S. Archer-Nicholls et al.

Anonymous Referee #1

Received and published: 31 October 2015

This paper describes the impact of biomass burning aerosols in the Amazon on radiation and clouds using a regional model. Analysis of the model results focus on two domains: one with a grid spacing of 5 km, with and without a convective parameterisation, and a the other with a grid spacing of 1 km that does not need a convective parameterisation. While the paper does bring up some valid points regarding how to interpret aerosol-radiation-cloud interactions predicted by models such as WRF-Chem, I am not sure what new information is obtained from this model sensitivity exercise. Not enough context is presented regarding the present results and those published previously. Therefore, I do not think the up-front purpose and conclusions derived from this study have not been articulated well enough. There are also a number comments I have that need to be addressed before the paper would be suitable for publication.

General comments:

Discussion of uncertainties: The authors are correct to point out missing aerosol-radiation-cloud processes in models, such as WRF-Chem, and the dilemma of handling aerosol-radiation-cloud interactions using a nesting approach when convective parameterisations are needed on coarser-scale domains while they can be neglected on finer-scale domains. In other words, scale-dependency issues. However, there are other limitations in their approach that warrant more discussion and the uncertainties associated with those could have an impact on the findings from the sensitivity simulations. Some of the processes are briefly acknowledged, like SOA and aerosols in ice-phase clouds, while other processes are not mentioned, such as secondary activation. In general, I think a discussion section (or more discussion in the existing text structure) is needed to place the present results in the context when specific processes are missing or uncertain. Additional experiments could explore the impact of those uncertainties on aerosol-radiation-cloud interactions. For example since SOA is not simulated, biomass burning emissions could be increased or decreased to examine how changing aerosol mass impacts the metrics presented in the figures. I note that once aerosol concentrations get large enough, there are not likely to be further impacts on clouds, but I would expect the largest changes happening in transition regions with low pristine aerosol concentrations and high aerosol concentrations associated with smoke.

Observational evaluation perspective: The authors need to stress that this is a model sensitivity exercise. No observations are presented to support the likelihood that the simulated aerosol-radiation-cloud interactions are realistic or not. The authors use the SAMBBA field campaign period; however, the present modeling study could have been done for any period in the Amazon or elsewhere where biomass burning is important. I understand they are leveraging a previous modeling study, but I have to review the paper as it stands by itself. I have several specific comments below along these lines.

Specific comments:

Page 27450, lines 18-19: The phrase “The 1 km domain simulated clouds less horizontally spread” is awkward and needs to be revised.

Page 27450, line 26: Change to “the publically available version of WRF-Chem” or “the version of WRF-Chem distributed to the community”. As indicated by the authors later, there are efforts underway that do include these effects, but are not yet readily available.

Page 27451, lines 16-17: Technically it is only the absorption that is included in the semi-direct effect (https://www.ipcc.ch/publications_and_data/ar4/wg1/en/ch7s7-5-2.html), and not scattering

Page 27453, line 8: same comment about wording of WRF-Chem as comment on page 27450, line 26.

Page 27454, line 1: The authors state that the purpose of the paper is to “evaluate” how aerosol-radiation-cloud interactions are captured in WRF-Chem. To me “evaluate” means comparison with observed quantities, which are not presented in this study. I think a better word is “illustrate”, since this is a model sensitivity study only. While the study may be illustrative for WRF-Chem users, it does not provide any quantitative information on performance. This needs to be made clear.

Page 27454, line 5: I think “cumulus parameterisations” needs to be changed to something about “with and without the use of a cumulus parameterisation”. I got the impression that multiple cumulus parmeterisations were to be tested, but instead found out later that was not the case and the investigators simply turned on and off a single cumulus parameterisation.

Page 27454, line 7: I agree this is a true statement, but the authors can make this statement much stronger. Knowing the details on how feedbacks are handled is im-

ACPD

15, C8784–C8789, 2015

portant for ALL models, including climate models. The number and type of feedbacks various from model to model, making comparisons between models problematic. Also, some aspects of aerosol-cloud interactions are highly uncertain and poorly constrained by data (i.e. heterogenous ice nucleation). Therefore, I think a little more discussion is needed here to justify this aspect of the paper.

Page 27454, line 19: I think “significant improvement” is an overstatement of the results from that paper. The authors of that paper do note “some improvement”, but it is really difficult to see in their figure that modest improvement.

Page 27457, lines 7-8: The authors note that no SOA treatment is used in this study and then provide a few sentences noting the uncertainties in parameterising SOA. It is true that SOA is still uncertain in models; however, I do not agree that the present model is capable to represent OA mass. If I understand correctly, all OA in their simulation originates from POM emissions, anthropogenic and biomass burning. I'm assuming biomass burning dominates in this region. But I would expect that OA mass is dominated by biogenic SOA, in the absence of biomass burning. Are the authors assuming not much SOA is produced by biomass burning? There is debate in the literature on this subject, with some models including a SOA from biomass burning (e.g. Shrivastava et al., JGR 2015). If there were comparisons of observed and simulated OA in the Archer-Nicholls (2015) paper, some discussion of that is warranted in the paper. Is the model too high or too low in simulated OA? OA will be the largest fraction of aerosol mass, and thus influence CCN. So SOA is a critical point in these simulations when assessing cloud-aerosol interactions.

Page 27459, line 27: As far as warm-cloud only processes, Yang et al. (JGR, 2015) describe a version that now includes ice-borne aerosol.

Page 27460, Section 2. Wet removal is not described in any way. This is an important process that seems to warrant some discussion on how it is handled for the various simulations (25, 5, 1 km).

ACPD
15, C8784–C8789, 2015

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Interactive Discussion

Discussion Paper

Page 27460, line 3: The Berg et al. paper is now published so the reference should be updated.

Page 27461, line 7: It would be useful to include, somewhere in the manuscript, a short summery of the performance of the model in the paper cited here.

Page 27461, line 24: aer_rad_feedback=0 may be familiar to WRF-Chem users, but is not very useful for a wider audience. This could easily deleted.

Page 27462, line12: Would it be possible to include TRMM precipitation estimates over the domain for these periods? Or was precipitation evaluated in the previous paper?

Page 27464, line 11: For the absorbing BBA, I assume the authors mean the BC emitted by the primary emissions rather than the OC. Does the model include a treatment of absorbing brown carbon? It would be useful to clarify this point in the model description section.

Page 27464, lines 25-27: I assume the authors are talking about the model results here, but sometimes it is not clear whether they are talking about observed or simulated values. Here and elsewhere, it would be useful to include "simulated" (or some other words) to let the reader know what they are talking about would be useful.

Page 27466, line 25: I don't understand the logic of connecting the Grell 3-D scheme and its ability to predict the semi-direct effect. The semi-direct effect would result from the radiation parameterisation. I think this must be a poorly worded sentence.

Page 27467, line 1: Change the title of this section to "Sensitivity to a convective parameterisation". The authors are only looking at one parameterisation here, and their results will likely vary if other cumulus parameterisations are used.

Page 27468, line 16: Secondary activation is likely to be important for deep convection (see Yang et al, JGR, 2015). The authors should discuss the implications of neglecting this process in the present simulations.

ACPD

15, C8784–C8789, 2015

Page 27469, line 19: This section is largely a "summary" section. There are very few conclusions. Either change the section name or re-write the text in this section.

Page 27471, line 17: As far as convective invigoration, I suggest the authors read Fan et al., PNAS, 2013. I believe that paper had a similar conclusion; however, they found that the most important part was that aerosols lead to a larger and longer lasting anvil. So, I am wondering if the authors could look at their results to determine whether simulations with and without fires changed cirrus amount detrained from convection. As the authors speculate, the current model formulation may not be complete. The PNAS paper also used spectral-bin microphysics that may behave differently than two-moment schemes, in terms of cloud-aerosol interactions.

Page 27472, lines 9-14: While I don't disagree with these statements, what is really missing here are means to evaluate whether parameterisations for cloud-aerosol interactions in deep convection are producing the right results for the right reasons. In other words, some observational and theoretical work is needed as well. Parameterisation development needs to be constrained by observations. Shallow cloud systems are far simpler and it has been easier to have confidence in how aerosol-cloud interactions are treated in those systems and in situ measurements of aerosols, cloud droplet number, etc. can be made within clouds. Such sampling is more problematic for deep convection.

Figure 2: Add the date and time at the top of each panel.

Figure 3: The first phrase is awkward, change the first phrase to "Temporally averaged column AOD at 550 nm over the 5 km domain". Add date and time at the top of each panel.

Figure 4: Add the date and time at the top of a) – c).

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 27449, 2015.

ACPD

15, C8784–C8789, 2015

Interactive Comment