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Atmospheric Chemistry and Physics Discussions

# *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 #2**

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The paper provides a detailed description of sensitivity simulations with the WRF-Chem model over a biomass burning region in Brazil. Sensitivities to horizontal model resolution (5 km vs. 1 km) and to the use of a convective parameterization (in the 5 km resolution setup) have been tested, and the model analysis have focused on direct, semi-direct and indirect effects of aerosol from fire emissions. In my view, the paper is well written and well documented. However, I question the scientific value of the results since the paper is purely a model study without any comparison to observations, and I have several comments that need to be addressed.

General/major comments



The authors state that the purpose of the study is to "critically evaluate how regional aerosol-radiation-cloud interactions are captured in WRF-Chem, . . ." (p. 27454, l. 1-9, see also p. 27463, l. 5). Although I realize that comparison with observations has been presented in a companion paper, the present study does not include any comparison with observations, and it is therefore difficult to know which of the experiments are more realistic. In particular, I think the value of what we learn from running with and without convective parameterization at the "grey zone" scales (i.e., <10 km) is limited when there is no idea of which is better. Evaluation against observations of clouds or precipitation, if available, would make this sensitivity experiment of with/without convective parameterization much more useful. At present, I do not agree that the paper is an evaluation paper, it is more a description of what happens when running with different setups.

Although I can understand the authors' statement that "The shorter case-studies at high-resolution were prioritized over a longer, low-resolution setup for the purpose and scope of the current investigation" (p. 27472, l. 1-3), the fact that the model region is tiny and the simulated time periods are few and very short, makes it difficult to generalize the results and make broader conclusions. Adding comparison to observations could possibly make up for this, as it may give some idea of which model setup is better.

The authors caution that the calculations of radiative balance should not be seen as robust calculations of radiative forcing (p. 27474, l. 10-11). However, I am not convinced that the method is good enough for drawing conclusions such as on p. 27466, l. 10-11 and p. 27467, l. 18-22 for simulations over such short time and for such a small region. A forcing imposed, e.g., by a reflecting compound such as sulfate, would rapidly lead to a decrease in surface temperature, which again would lead to reduced LW radiation from the Earth's surface, and hence contribute to maintain radiative balance. Supplement Table 4 shows that the near-surface temperature is affected by inclusion of an aerosol layer. In a long global climate model run this is solved by running with fixed sea-surface temperatures. A better, but more complex method would be to include double radiation calls, such as the method of Ghan et al. (2012), to quantify the direct, semi-direct, and indirect aerosol effects. Please justify the method used to calculate the radiative balance.

While the introduction and model description sections are well referenced, the results section contains very little comparison and reference to other work (with the exception of Zhang et al., 2008). Several papers deal with the impact of biomass burning aerosols on meteorology and radiative forcing so this could easily be added. What about other regions, either of Amazonia, or in other biomass burning regions such as central and southern Africa, or Indonesia? Have similar or different results been found there? E.g., the result that fire aerosols stabilize the atmosphere and inhibits convection and cloud formation (p. 27465, l. 20-22) has also been found before, e.g., recently in tropical Africa (Tosca et al., 2015), and could be mentioned.

The paper does not include any estimation of uncertainties in the results (except in the Supplement), but a statement that many of the results are not statistically significant (p. 27471, l. 23-25). In my view, it would still be useful to include some estimation of uncertainties. Including statistical significance based on a Student's t-test or similar could be useful when interpreting the results, and give the reader an idea of which results are robust and which are not.

P. 27459, l. 28 – p. 27460, l. 2: Do the authors have an idea of how big of an impact this has on the results presented for the 5 km domain?

P. 27466, l. 22-26: I am not sure this assumption and statement can be made without any observations showing that the results are more realistic in the 1 km domain.

P. 27470, l. 26-28: Again, how can this conclusion be made without comparison to observations?

Minor comments/corrections:

P. 27454, l. 1: This -> The



P. 27459, l. 11: caries -> carries

P. 27462, l. 17-28: The aerosol loadings are mentioned several times, but this is not shown in Fig. 2. Perhaps add reference to Fig. 3 in this paragraph?

P. 27469, l. 27: subsistence -> subsidence

P. 27471, l. 20-21: This is an interesting finding and could be made more clear in the abstract?

P. 27472, l. 19: Remove "to the" after "includes".

P. 27487, l. 3: Scenrios -> Scenarios

P. 27489, l. 2: averaged -> accumulated

References:

Ghan, S. J., Liu, X., Easter, R. C., Zaveri, R., Rasch, P. J., Yoon, J. H., and Eaton, B.: Toward a Minimal Representation of Aerosols in Climate Models: Comparative Decomposition of Aerosol Direct, Semidirect, and Indirect Radiative Forcing, J. Clim., 25, 19, 6461-6476, 10.1175/jcli-d-11-00650.1, 2012.

Tosca, M. G., Diner, D. J., Garay, M. J., and Kalashnikova, O. V.: Human-caused fires limit convection in tropical Africa: First temporal observations and attribution, Geophys. Res. Lett., 42, 15, 6492-6501, 10.1002/2015gl065063, 2015.

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