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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Large simulated radiative effects of smoke in the south-east Atlantic" *by* Hamish Gordon et al.

Anonymous Referee #1

Received and published: 29 June 2018

In this study, Gordon et al. studied the radiative effects of smoke induced over a 1200 km2 area of south-east Atlantic using HadGEM climate model at convection-permitting and global resolution in conjunction with several satellite observations and ground based observations (from LASIC campaign over the Ascension Island). The study finds that smoke aerosols can induce significant cooling effect over this region via alter cloud properties. Whether the semi-direct effect or the indirect effect dominates depends on the selection of autoconversion scheme. The paper has significant scientific merit and is clearly written with great details. The reviewer recommends publication after some minor revisions.

General comments:

1. As for regional-to-global scale modeling study, 10-day period seems to be too short

for estimating radiative effects of smoke. As shown in Figure 1a of Zuidema et al. (2018, GRL), during the fire season over this region, the fire activity fluctuates widely from day to day. By the end of the simulation period (i.e. Aug. 8), the smoke concentration as measured over Ascension Island was much higher than any other days (except Aug. 13) in 2016. Therefore, -22.0Wm-2 cooling sounds very dramatic but the authors failed to emphasize that this value is calculated for the most polluted period during the fire season in many places. The authors can either run model (global model only if regional model is too expensive) for longer period, or emphasize the period and region, over which the model simulation is conducted in text, especially in the abstract.

2. Although the results in the paper are thoroughly discussed with many details, the reviewer suggests the authors to include some additional results for the interests of the scientific community. Firstly, in addition to show the time series of modeled and observed AODs in Figure 8, please include the spatial patterns of modeled AOD in comparison with MODIS observations (averaged over study period or during Aug. 2 and Aug. 7) in main text or supplementary. By doing so, the authors can further justify the performance of HadGEM/UK Met Office Unified models. Secondly, many previous modeling and observational studies disagree on the effect of smoke on cloud-top height. The reviewer strongly suggests the authors to examine smoke-induced changes in cloud-top heights by comparing three cases after the discussion of Figure 13.

Specific comments:

The abstract seems to be way too long – almost close to 500 words. Please condense the abstract, if possible around 300 words.

Page 2, line 12: You mean" Once entrained, smoke. . .can have very different effects to cloud aloft"?

Page 2, line 15: This hypothesis may be true, but please explain why this is the case. (For example, maybe cite Figure 3 in Chand et al., 2009, Nature geoscience?)

Page 3, Figure 1: It may be better to label Ascension Island in the figure instead of saying it in the caption.

Page 4, line 5 to line 8: I am not familiar with the models used in this study. To drive a model with 4km resolution with a model with 65 km resolution, is this the downscaling or nesting technique? For regional models like MM5 and WRF, the ratio between the resolutions of the outer domain and inner domain is usually 3:1 or 5:1. As in this study, the ratio is 16:1, which is quite large. With such large ratio, can signature of smoke plumes in meso-scale be properly simulated?

Page 5, line 5 to line 7: please rewrite this sentence, using the phrase like "the vertical resolution of model grid boxes. . ."

Page 6, line 4 and line 14: 120nm is typical size of freshly emitted size of smoke particles; however, it should be in accumulation mode. Why put smoke particles into insoluble Aitken mode?

Page 6, line 4: ". . .highest at the surface. . .reach zero at 3km above ground." Is this based on any observation or modeling studies? If so, please cite the reference.

Page 6, line 10:"In any case,. . .rather than emission" Is this the case? Is this based on any studies? If so, please cite the reference.

Page 6, line17: The authors only mentioned BC, how about POM emissions? What is the mass ratio between BC/POM in this study?

Page 8, line 26: Since the second aerosol indirect effect is considered in the case with Kogan (2013) scheme, does that mean this case is more realistic compared to the case with default scheme?

Page 10, line 10: Change "8th August" to "8 August " to be consistent with the text.

Page 13, line 18: Change "height of the aerosol layer" to "the height of the aerosol layer bottom".

Page 17, line 2: Any reason such large box is used?

Page 19, line 17: If I remember correctly, high humidity associated with smoke plume as mentioned in (Adebiyi et al., 2015) is due to water vapor emissions from fires. Are water vapor emissions considered in the study?

Page 28, line 2: By "table" you mean table 2?

Page 28, line2-line5: It is very confusing when the authors say "compare line x to line y". In addition, I believe that the line numbers are not correct according to Table 2. Please rewrite the text in parentheses.

Page 33, section 9.3: It would be better to compare modeled semi-direct effect to Sakaeda et al. (2011) in this section.

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