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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The significant role of biomass burning aerosols in clouds and radiation in the South-eastern Atlantic Ocean" *by* Haochi Che et al.

Anonymous Referee #2

Received and published: 24 August 2020

This paper uses UKESM1 simulations to study the contribution of different processes to the radiative effects of biomass burning aerosols. The topic is important, the presentation quality is good and the paper shows interesting results. I don't have major concerns about the analysis, but I find there is the need for a more detailed description of the methodology and some additional analysis. I also believe that most of the supplementary figures belong to the main body of the paper (see specific comment below). The paper is worth publishing, but given that it needs additional work in a number of areas, I am recommending a major revision.

GENERAL COMMENTS

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Discussion paper

Only two years of model simulations are used. They are chosen because they coincide with observational campaigns. However, the observational data are only used to perform an initial assessment of the model's simulations and to justify the use of UKESM1 for the subsequent analysis, which is entirely model-based. Then, why not use a longer simulation period? This will allow to get more robust estimates of the BBA effects in the region, and to quantify the role of interannual variability.

The methodological description needs additional work. Especially, I think a brief description of how the different experiments are combined to decompose the BBA effect into individual contributions is lacking. How accurate is this decomposition? What are the caveats?

Given that this is a model-based study, the accuracy of the results will not only depend on the representation of the BBA plume and the cloud climatology, but also on how good the model is at representing the cloud response to the drivers of changes. For example, the realism of the strong radiative cooling of the semi-direct effect will depend on how well UKESM1 represents the cloud response to a strengthening of the inversion. Figure S4 touches on this, but only in passing. You cite a reference (Adebiyi et al., 2015) that uses radiosondes to look into this. How does the change in the inversion strength in UKESM1 compare to the one observed with radiosondes? I acknowledge that a comprehensive assessment of how well UKESM1 performs in this respect is out of the scope of this paper, but putting the UKESM1 changes into context would be very helpful.

The title should be more especific, and should capture the main message of the paper.

SPECIFIC COMMENTS

P3L22-P4L5: Most of this paragraph probably belongs to the methods section. Only the last sentence describes the objectives of the paper. I'd suggest transferring the description of the campaigns to the methods, and expand on what the paper is about.

Figure 1. The spatial resolution of the model results seem to be much higher than N96. Am I misinterpreting what that figure shows?

Figure 3 caption. "The domain in Fig. 3a, ranging from 30° S to 10° N and from 40° W to 30° E, is the areas this paper interested in." I believe the description of the area of interest belongs to the main text.

Figure 3 caption. " The grey box in the map (cloud box) representing the cloud areas where the averaged low cloud fraction is above 0.58." That would be the 0.58 isoline, unlikely to have a rectangular shape. Please provide a clearer explanation of what this box is.

P7L18. supersaturation (SS). This is an unfortunate acronym. In general, I don't see the need for using an acronym to compress a single word. For instance, we don't normally use MP for microphysics. Or, what would you use for subsaturation?

P9L2. "LWP from BBA absorption shows a steady negative gradient from west to east". I might be looking at the wrong region, but the gradient in the cloud region looks positive to me. Please clarify,

Figure 7. This figure shows changes in cloud albedo using the ISCCP simulator. I might have missed it, but I believe that the use of the ISCCP simulator is not documented in the methodology. Which simulator variables are used? Why is this approach better than looking at changes in cloud fraction and cloud radiative effect?

Discussion of Figure 8c. It would be nice to show a plot with the actual change in the strength of the inversion that drives this strong semi-direct effect.

Figures S5 to S8. I feel that these figures belong to the main paper, not to the supplementary material. They can be arranged all together in a multi-panel figure with showing the baseline climatologies, complementing the figures showing the changes due to different processes.

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