Interactive comment on “Direct and semi-direct radiative forcing of biomass burning aerosols over the Southeast Atlantic (SEA) and its sensitivity to absorbing properties: a regional climate modeling study” by Marc Mallet et al.

First of all, we would like to thank the reviewer for the various remarks. We have taken them into account in the new document.

Anonymous Referee #1

Received and published: 5 August 2020

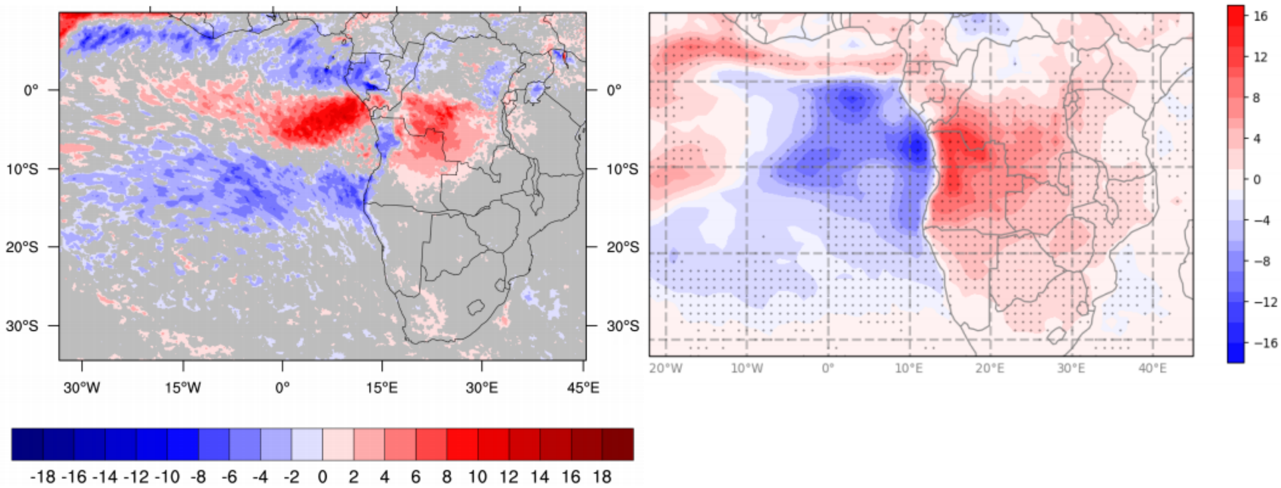
This is a very interesting and important study topic. The manuscript described regional climate models simulation of biomass burning aerosol over southeast Atlantic, which draws very few attention in the literature but may have important influence due to the persistent intensive emission from South America. The modeling approach is reasonably, with solid validations and in-depth discussion of the result. The sensitivity simulations with different absorbing properties provided upper boundary estimates of the direct and semi-direct effect of aerosol. This is a well-organized study with fluent professional writing. Therefore I would recommend this manuscript to be accepted with very minor revisions, following are some detailed comments.

Comment#1: The spatial distribution figures have very low DPI (although the information could be read), please make them more clear. Also, some figures have national boundaries but some don’t, please keep it consistent. The curve figures have lines too slim, please consider make them bolder.
All the figures have been now improved following the different points indicated by the reviewer. National boundaries have been added for both figures and the size of the curves has been increased.

Comment#2: Line#23: Unnecessary to sate “the approach of using two . . . of the results”
This sentence is now removed.

Comment#3: Line#35: the subsidence of air mass, water vapor, etc? please rephrase to be more clear
This sentence has been improved in the new version.

Comment#4: Line#36: so what is the overall semi-direct effect?
This is a very interesting remark and we have now estimated the SDRE of BBA for the JAS period (2000-2015) for the two regional climate model. The following figure clearly indicates a negative (positive) SDRE where the low cloud fraction is increased (decreased) as shown in the Figure 11e and f. The important positive SDE over the Angola/Congo in ALADIN is due to changes in the high could fraction. For RegCM, a more uniform negative SDRE is obtained over SEA. In terms of magnitude, the SDRE is between ~-2 and -10 W m^{-2} in RegCM over most of the SEA, higher that the mean value (-3.0 W.m^{-2}) reported by Sakaeda et al. (2011) at a climatic scale. All the points are now discussed in the new version (part 5.3) and the following figure has been added in the supplement material (Figure S7).



SDRE estimated by ALADIN (left) and RegCM (right) regional model (JAS season).

Comment#5: Line#39: "the results indicate . . . to the absorbing properties of BBA" this is certainly true, please make more specific statement of the innovative finding from this study

This is right and we have now included more statement on the DRE of BBA (at TOA) in the abstract using the following sentence : « Over the Sc region, DRE varies from +0.94 W m^{-2} (scattering BBA) to +3.93 W m^{-2} (most absorbing BBA)."

Comment#6: Table 1. Horizontal resolution: 12km, 80km

This is now changed in the Table 1.

Comment#7: Line#134: "In ALADIN-Climat . . ." I don't understand this sentence, do you mean the boundary conditions were derived from simulations for a larger domain with biomass burning emission?

This was effectively not clear, sorry. This sentence indicates that the ALADIN model is not forced at the lateral boundary by the long-range transport of aerosols. This means that, compared to RegCM, some bias in AOD could be due to the advection of particles that are not emitted directly in the ALADIN domain (see comment #14). We think the impact is minor as most of biomass-burning emission are included in the domain for the period studied here (JAS), but not necessary negligible.

Comment#8: Line#189: Does CTL include direct and semi-direct effect of other aerosols?

Yes, this important point is now indicated in the text.

Comment#9: Line#199: GFED gives fire emission as "dry matter" or "total carbon", what's the emission factors used to calculate aerosol emission?

In the RegCM and ALADIN simulations, we have directly used the emissions of BBA aerosol species already prepared in the CMIP6 dataset to force the two models at the surface. We have adjusted these emissions by using a scaling factor (1.5; similar for the two models) directly on the BC/OC emissions. The methodology used to derive and calculate the emissions is described in van Marle et al. 2017, which is referenced in the article.

Comment#10: Line#202 and section2.1.3: I am confused here, section2.1.3 mentioned BBA is treated as one type of aerosol in the model, so why the emission is upscaled for BC and OC separately?

In the models, the BC and OC GFED emission are used and merged to force the emission for the specific « smoke » tracer, which is then declined in fresh and aged BBA. This allows better comparisons with observations as mentioned in the article. In parallel and as used in the HadGEM model, a similar scaling factor is applied to BC and OC particles to reduce the bias with observed AOD (Thordnill et al. 2018).

Comment#11: Line#203: need reference for the scaling factor
The recent reference of Pan et al. (2020) has been added in the new version to highlight the fact that a large number of important scaling factors have been proposed for different emission datasets (GFED, QFED, FINN, GFAS and FEER).

Comment#12: Line#215: Raw GFED has 3-hour intervals.
This point has been precised. As the study is focused on climate simulations, we have effectively used monthly-mean emission data set and the diurnal cycle of smoke emission has not taken into account. This could impact the temporal variations of the aerosol loadings. This point is now mentioned in the text (2.1.4).

Comment#13: Line#303: this section mainly described model evaluation of LCF, no detailed discussion was made regarding microphysical properties
The cloud microphysical properties were not analysed in this study. In all simulations, we have fixed the cloud effective radius to 10 µm and the first indirect effect of BBA is absent in the two regional models. In that sense, we have focused our analyses on the LCF and LWP evaluation. However, it should be noted that the cloud optical depth (over the Sc region) has been validated in Mallet et al. (2019). These important points are now indicated in the text (2.1.4 and 3.2).

Comment#14: section3.3.1: why AOD simulation bias is bigger in certain months, such as Jan-Apr and Sep-Dec; what's the correlation coefficient between simulation and satellite, with raw monthly data intervals?
This is an interesting point and the differences detected in AOD during Jan-Apr and Sep-Dec periods could be due to different resasons as the long-range transport (especially for ALADIN that does not include chemical forcing at the boundaries) or some bias in the dynamic and the precipitation. In parallel, we can also note the high variability in the different products (reanalyses or remote-sensing) for these two seasons. For example, the two RCM are in a good agreement with MACv2 and MERRA data compared to MODIS and CMAS-RA. These points are now mentioned in the text and the temporal correlations with MODIS and MISR are now included in the Figure 4 for the two models. This shows a better agreement for RegCM (~0.95) compared to ALADIN (~0.80).

Comment#15: Line#381-387: please provide more details to demonstrate the plume rise of biomass burning in the two models because it decides if BBA will get above or below cloud.
This important point is indeed not discussed enough in the text and may explain some of the differences. The figure S3 indicates the BBA extinction (at 550 nm) and clearly shows an efficient transport of BBA between 1 and 4 km over the ocean in accordance with results of Das et al. (2017). This figure indicates also that the base of the smoke plume is lower in RegCM and may explain differences in ACAOD between the two regional models. This specific point is now clearly indicated in the new version in the part 3.3.2.

Das, S., Harshvardhan, H., Bian, H., Chin, M., Curci, G., Protonotariou, A. P., et al. (2017). Biomass burning aerosol transport and vertical distribution over the South African-Atlantic region. Journal of Geophysical Research.

Comment#16: Fig5. The two model simulated different change of ACAOD from 2008 to 2009, please explain why

The differences in ACAOD between the two RCMs are mainly due to the simulated AOD and the cloud top, which are respectively higher and lower in RegCM for these years, compared to ALADIN. This point is now added in the text.

Comment#17: Line#446: prescribed SST can also be altered by the aerosol effect?
This is effectively right as prescribed SST are also constructed using in-situ observations. This point is now mentioned in the new version (part 2.1.1).

Comment#18: Fig.8: RegCM legend is vertical
This is now changed in the new version.

Comment#19: Fig.11: why there are missing values?
The missing values are non-significant in the ALADIN model. This point is now clarified in the caption.