Dear Editor,

We first would like to thank the reviewer for all the remarks and suggestions very useful to improve the manuscript. We have tried to take into account most of the mentionned following points.

This is a well written and straightforward paper on model simulations of heating rates compared to and at times constrained by the ORACLES and LASIC field campaigns. I found it easy to ready and well laid out. They take a best available run with the model, and clearly spell out modeled radiative, temperature and PBL effects. They note biases and deficiencies, particularly in reference to wo and smoke vertical profile.

I have a few minor comments (listed below) but one major comment. I would like to direct the authors to Tom Ecks paper https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/jgrd.50500 on the seasonal trends of wo over Africa. One thing the paper failed to account for is that wo over Africa has a very strong and very predictable trend due to a systematic shift in grass burning in the early season to more wooded fire late in the season, roughly 0.83 in early July, to 0.92 in mid-October at 440 nm. Yet, the real part of the index of refraction and the size distribution remain relatively static. This makes for an outstanding natural partial derivative on the sensitivity of the system to black carbon. However in the paper the black carbon mass fraction is static for the burning season. While I do not think that they necessarily need to do another run (as the model simulation is for a the middle 2 months and results are largely aggregated), I think that at least a paragraph or two needs to be present adding context to their run and providing rough error estimates, sensitivity and implication (if any) of this strong seasonal trend. In particular, please compare this finding to what you found in the model (Line 618).

I have lots minor comments that I think might clarify the paper. Some of this is because it is just the way the model was constructed and the investigators are sort of stuck with it. I don't mind so much of their assumed parameters are out of expected range by a little bit, but it should probably be noted. Also, things that seem minor information is actually very helpful later on when people try to reconcile model runs and observations. So please do your best to address these

We agree on this important limitation concerning the representation of smoke optical properties (notably absorption) in the ALADIN-Climate model. As fires are not explicitely resolved in the model, it is difficult to take into account changes in optical properties of smoke during the biomass burning season. In that sense, we have only investigated here the August-September period when smoke SSA remains low, around 0.84-0.86 at 500nm (AERONET retrievals ; Eck et al., 2013). In addition, this ALADIN-Climate simulation has been also constrained by recent in-situ observations (Zuidema et al., 2018) obtained within the marine boundary layer at Ascension island (SSA of ~0.80 at 550 nm in September 2016). Anyway, it is clear that this hypothesis is important and could have some implications on biomass burning shortwave (SW) heating rate and direct radiative effect especially for climate simulations including all the biomass buring season (from July to late october).

To address this specific point, we have now performed a new simulation, named SMK_SSA, which includes less aborbing smoke, more representative of the late season (September-October) as noted by Eck et al. (2013). In this sensitivity simulation, SSA has been fixed to 0.92 (550 nm) for smoke; the rest of BBA parameters being exactly similar. The description of this new simulation is now included in the paragraph 3.1 and this limitation of the ALADIN-Climate model is clearly reminded in the part 2.2. To illustrate these new results, the figure 13 has been modified (see the new Figure 13 below) and a new Table (Table 3) has been included.

Along the text, additional explanations are now provided in terms of sensitivity on (i) SW heating rate (paragraph 4.2.4.3), (ii) direct radiative effect exerted at the top of the atmosphere and (iii) the different impacts on surface SW radiations, temperature, sensible heat fluxes and PBL height.

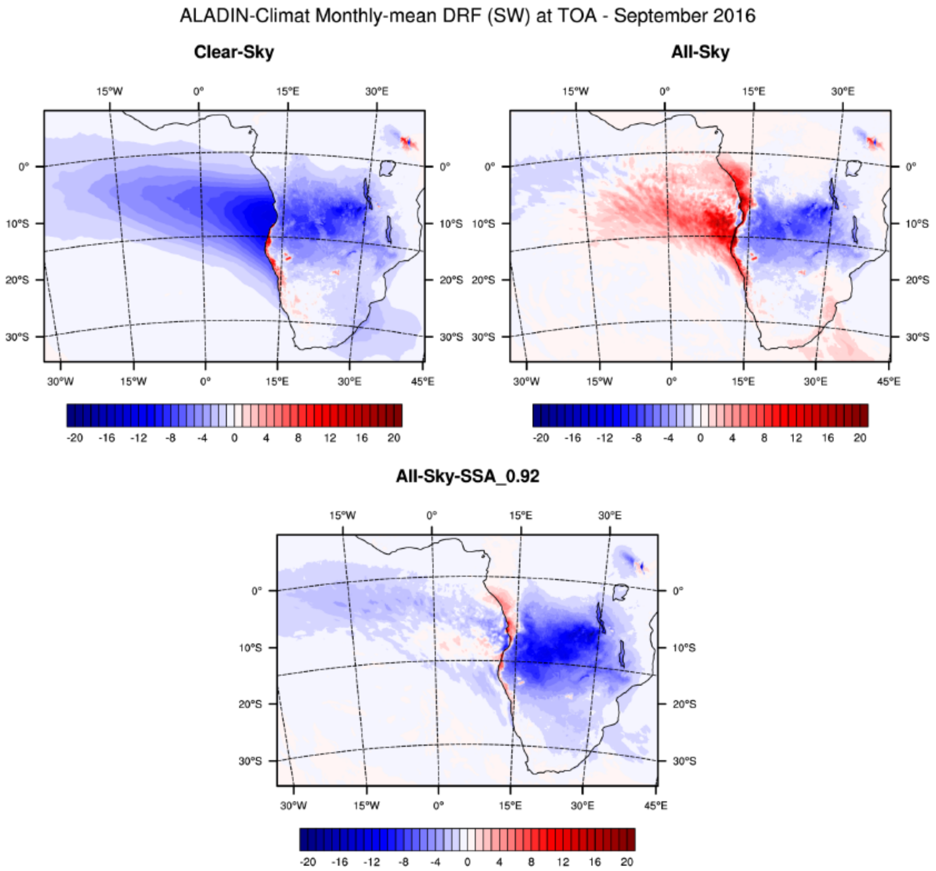
In terms of SW radiative heating and direct radiative forcing, this new ALADIN simulation indicates :

- a significant decrease of the SW radiative heating induced by smoke. For example and over the Box_S (defined over the sources of biomass burning), SW heating at 3km is passing from +1.15°K by day to + 0.58°K by day. This point could moderate the heating induced by smoke at the end of the biomass burning season. This point is now included in the article (paragraph 4.2.4.3),

- a significant change in the monthly-mean (September 2016) DRF at TOA, passing from a positive (+4.2 $W.m^{-2}$) to negative (-0.54 $W.m^{-2}$) direct forcing (see new Figure 13 and Table 3) over the Box_O (ocean). This means that the positive direct forcing at TOA could be lesser in intensity at the end of the BBA season (late october). This important point is now mentioned in the part 5.1 and the results of the new ALADIN-Climate (SMK_SSA) simulation with more scattering smoke (SSA of 0.92) are included in the Table 3,

- a more intense negative DRF at TOA over smoke sources due to more scattering BBA. Over the box_S, the monthly-mean value DRF is increasing from -3.9 $W.m^{-2}$ (SMK) to -7.3 $W.m^{-2}$ (SMK_SSA). This specific point is added in the discussion. Values are reported in the Table 3,

- a less important positive DRF is observed at TOA along the Southern African coast and Gabon due to more scattering BBA. This important point is now indicated in the paragraph 5.1,

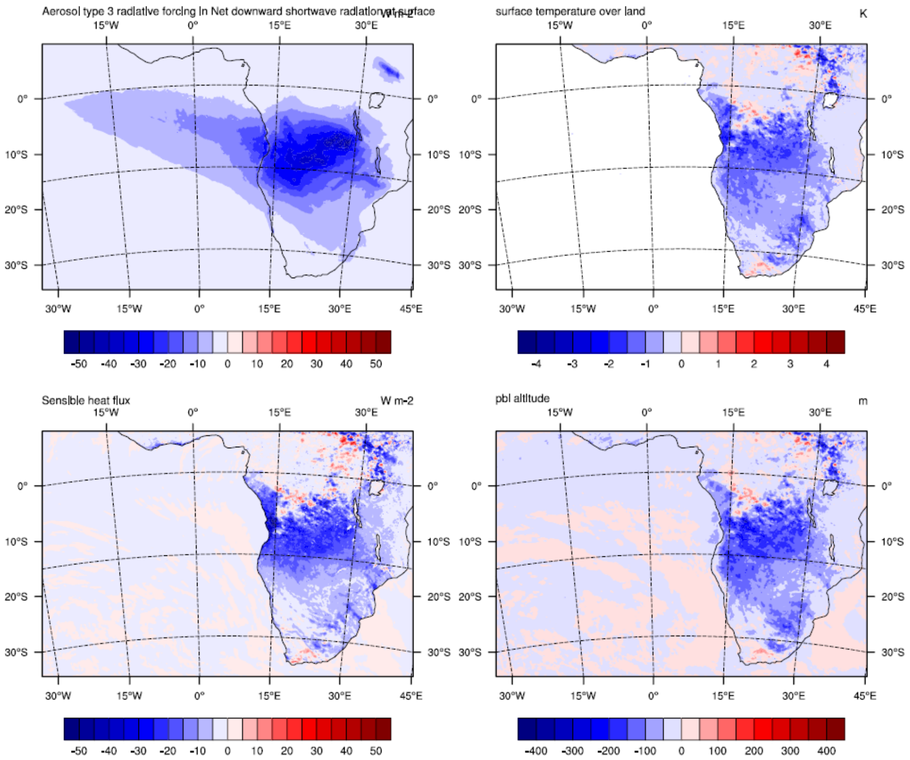


New figure 13 including (bottom) the monthly-mean DRF exerted at TOA for more scattering smoke (SMK_SSA simulation).

Concerning the impact of BBA on other variables (SW radiations at the surface, surface temperature, sensible heat fluxes and PBL height), we did exactly the same figure as Figure 14 but for the new simulation (SSA_SMK). This new figure (see below) has been added in Supplement material (S8) and the main results are discussed in the Part 5.2. We have notably added these clarifications :

«Finally, the comparisons with the SMK_SSA simulations (not shown, Figure S8) indicate a decrease of the surface radiative forcing both over continent and ocean. As reported in Table 3, the monthly-mean DRF at BOA is about -39 $W.m^{-2}$ and -25 $W.m^{-2}$ over Box_S (biomasse burning sources) for the SMK and SMK_SSA simulations, respectively. The same result is obtained over SAO. This is due to the decrease of SW radiations absorbed by smoke in the SMK_SSA simulation, increasing the SW radiations reaching the surface. This could be also due, to a lesser extent, to some changes in aerosol loading due to modifications in the dynamics and precipitation between the two simulations. This induces a less pronounced impact of BBA on the surface temperature and sensible heat fluxes in the SMK_SSA run. The increase of SW surface radiations, associated to low absorption by BBA in SMK_SSA, decrease the

impact of smoke on the PBL development (Figure S8). As mentioned previsouly, these results suggest that the impact of BBA on the surface fluxes and dynamics are certainly slightly lower at the end of the biomass burning season.»



The new Figure S7, which is now included in the supplement material.

Line 79: the first few times please state worming/cooling in association with positive and negative DRF for clarity for readers
This point is now included in the new version.

Line 163. The use of OC/BC as biomass burning tracer with fixed microphysical and optical properties and basically being in the same emission category with anthropogenic is somewhat problematic and their hypothesis 'implications" are almost certainly violated in the study regime, especially on the northern end of the core biomass burning feature. This is a recurring problem in the modeling community, and has led to significant discussion within the ICAP community. The bottom line is that carbonations species are fundamentally different from biomass burning and anthropogenic/biogenic sources, and should be treated separately in models. But, model architecture is not so easy to change. I think the authors need to be clear about this up front and add a few lines discussing specifically what this does to the simulation. Fortunately for them, biomass burning particle evolution tends to be rather fast, slowing down by the time it reaches the coastline (https://www.atmos-chem-phys.net/5/799/2005/). This said, however, African smoke has shown evidence of evaporation/sublimation as noted in https://link.springer.com/article/10.1007/BF00708178.
We agree with this remark, which represents one of the main motivations for including two new specific tracers in the ALADIN-Climate model to represent BBA, as presented in the part 2.2. This allows now to take into account specific properties for smoke particles, as the hygroscopic, e-folding time and optical properties. It allows notably to distinguish those particles from carbonaceous aerosols emitted from anthropogenic emissions with different properties. This point is now more detailed in the text (part 2.2).

Line 171. Again, based on lit review https://www.atmos-chem-phys.net/5/799/2005/ is more in line with Vakkari. It is complicated because one has to decide what the initial state is to start the clock ticking. There is substantial evidence of difference in smoke properties from the base and top of a smoke column too. I think this is fairly moot though given the large scale nature of the simulation.

This is right, and the reason why we had performed an additional simulation using a different efolding time (provided in the Appendix; Figure S1). We show that using a value of 3 hours (Vakkari et al., 2018) leads to change in AOD of about ~0.05 over the biomass burning region. This suggests, for this specific case, a modest impact on AOD compared to the hypothesis made on the POM to OC ratio. This point is indicated in the part 3.1 and deeper discussed in the new version.

Line 188. Just an FYI, you should note that these values of MEE are just on the upper half of what has been gravimetrically observed https://www.atmos-chem-phys.net/5/827/2005/acp-5-827-2005.pdf But 5 is a nice round number.
Thank you for this interesting review paper on smoke optical properties. We have incorporated it in the new version to discuss the values used in the ALADIN-Climat model.

Line 242: See comment on line 171
Please, see above concerning the use of a new simulation with different efolding time.

Line 247: this ratio is also a bit high. Consider, OC makes up about 40-50% of mass, so a ratio of 2.3 makes over 100% of mass, and we know that Africa smoke is dominated by grass fires which have a high inorganic fraction.
We agree on the fact that there are strong uncertainties on the POM to OC ratio. In this work, we have finally retained the value of Formenti et al. (2003). However and due to this large uncertainy, we have also conducted additional sensitiviy tests using two different values (2 and 3) of the POM to OC ratio. The results are presented in the Figure S1 (Appendix) showing an important sensitivity of ~0.2 on AOD over the box 5-15S/15-25E. This point is now more discussed in the new version.

Line 284: I think the site is now Mongu Inn instead of Mongu
This is now changed.

Line 293: Which AERONET version was used? V3 came on line recently so it is not obvious.
We have used version2/level 2 AERONET retrievals. This point is now detailled in the new version.

Line 326: What was the altitude above clouds the reflectance was taken at?
The reflectance measurement was taken at 1430 m, just prior to the profile. In-situ cloud data shows cloud top heights at 600m. In that sense, the reflectance has been measured about ~800 m above cloud tops. This specific point is now mentioned (Part 3.2.3.2).

Line 554; CALIOP is all CAPS
This is changed.

Line 627: How much do you think assumptions of hygroscopicity versus speciation plays into this? Granted, this is mostly an absence of BC in smoke, but you still may have a factor of 2 floating around given the high RH in the MBL.
We think the bias is mostly due to the aerosol speciation in this case. Hygroscopic properties that we used in the model is not able to explain such important differences in SSA. In addition, an external mixing hypothesis is used in the model, excluding the possible « lensing » effect (associated to increase of absorption).

Line 635: can you elaborate here?
This point is now more detailled in the new version.

Paragraph around Line 683: This paragraph is bordering on a non-sequitur. Wv is a great tracer, but is fundamentally different from RH. So careful how you talk about humidity and optical properties.
This is effectively right and we have now modified this paragraph.

Paragraph around 695: Can you compare model versus measured f(RH) directly here?
This point is a very interesting but, unfortunately, we could not conserve in the model both the dry and wet optical properties of smoke aerosols and we only calculate the wet properties for direct comparisons with in-situ/satellite observations.

Paragraph starting 760:

On PBL impact: Please be specific what you mean by PBL height, are you referring to the actual top of the PBL (that can be somewhat amorphous given the depth of the entertainment zone in cloud atmospheres, but comes out often as a hazy model metric) or are you referring specifically to the top of the mixed layer? If you are referring to a systematic change in the base of the inversion, please state that clearly throughout. Also, Just curious, any wind impacts ? Mark Jacobson years ago was reporting big wind impacts in global GATOR simulations. See any evidence ? Regardless positive or negative it is worth mentioning any notable wind impacts.

In this study, the PBL height corresponds to the top of the PBL. This is now detailed in the text. In parallel, we have also investigated the impact of BBA on the near-surface (10m) wind speed. The results are presented in the following figure. In our case, we obtain a general decrease (of about -0.5 m.s^{-1}) of the surface wind over most of the continent. Over the ocean, the impact of BBA is more complex with the presence of a regional contrast, characterized by an increase (decrease) of the surface wind around 0-10°W/15-30°S (latitudes higher than 15°S). This point is now mentioned in the part 5.2 of the new version.

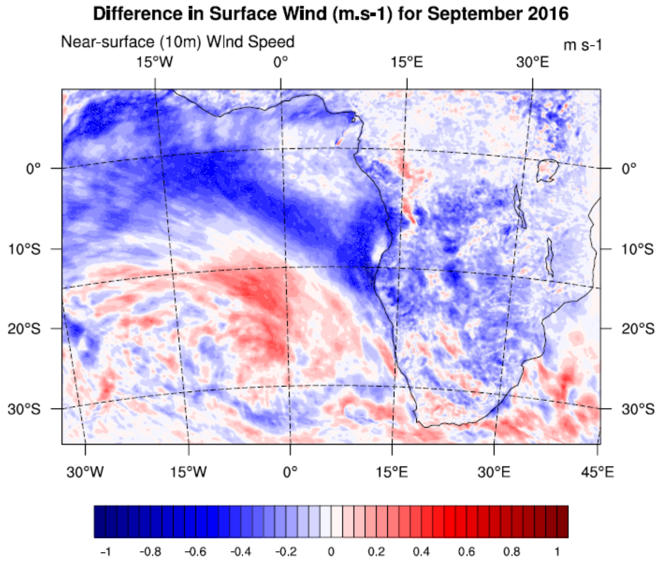


Figure indicating the changes in the surface wind speed due to BBA (averaged for September 2016).

Also, can you calculate a specific surface temperature change per unit optical depth? See this for comparison https://www.atmos-chem-phys.net/16/6475/2016/

This is an interesting remark and we have now calculated the changes of surface temperature per unit of AOD due to the presence of BBA. The results we obtained (averaged for all the period of simulation) is about -2.5° per unit AOD (at 550 nm). We observe that this value is consistent and higher to the one (-1.5°) published by Zhang et al. (2016) for a massive biomass burning event occuring over Central Canada during June 2015. The difference could be due to the absorbing properties of BBA, which are more pronounced in the present study compared to Zhang et al. (2016) (SSA of 0.94). This could favor higher dimming effect and impact on the surface temperature over the Angola region. This interesting point is now mentioned in the part 5.2 and the reference of Zhang et al. (2016) is added.