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Interactive comment on "Simulation of the transport, vertical distribution, optical properties and radiative impact of smoke aerosols with the ALADIN regional climate model during the ORACLES-2016 and LASIC experiments" *by* Marc Mallet et al.

Anonymous Referee #2

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Review of "Simulation of the transport, vertical distribution, optical properties and radiative impact of smoke aerosols with the ALADIN regional climate model during the ORACLES-2016 and LASIC experiments" by Mallet et al., submitted to *Atmos. Chem. Phys.*

In this study, the authors compare a simulation of stratocumulus clouds and biomass-burning aerosols over the southeastern Atlantic to aircraft and satellite retrievals of

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Discussion paper

clouds and aerosol properties. They find that the simulation is satisfactory in the first order, although aerosol extinction and absorption and cloud fraction are underestimated, and cloud optical thickness is overestimated. A simulation nudged to reanalysis outperforms the free-running model because nudging improves simulated relative humidity, which in turns improves aerosol extinction through hygroscopicity.

The paper is interesting and well-written. Many aspects of modelled aerosols and clouds relevant to the direct radiative effects of biomass-burning aerosols are evaluated against multiple observational datasets. The discussion is convincing and supported by a large number of figures.

I have only one main comment: the authors should clearly set expectations in section 3, by which I mean to state clearly what the model should be capable of in terms of reproducing spatial and temporal variability, and what comparison the satellite products are able to usefully provide. The reason it matters is that the model seems to be using monthly-averaged emissions, which may not even be for the year 2016. So the model cannot be expected to reproduce daily distributions, even when nudged. In addition, temporal sampling of satellite retrievals is limited, as stated by the authors, which means that model and observations should really be co-located temporally before being compared (Schutgens et al. doi:10.5194/acp-16-1065-2016 2016). This is not done here consistently, so there the comparisons can only be qualitative. In addition, I note that it is becoming more challenging to avoid circular reasoning, i.e. not comparing models using satellite-derived emissions to the same satellite products, and to reanalyses that assimilate some of those same products. This is not a criticism of the paper, but it suggests that having multiple, independent observational datasets for the same variables is becoming increasingly important.

Addressing my main comment, and the other comments below, should represent minor revisions.

Other comments:

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- Line 63: It would be good to define DRF here: with respect to no-aerosols. Note that the IPCC calls that direct radiative *effect* (DRE). *Forcing* is when defined with respect to pre-industrial aerosols.
- Line 65: the "well-known" cooling effect is only true on a global average, so there is no contradiction really. I suggest rephrasing to contrast the top-of-atmosphere radiative effect of scattering and absorbing aerosols.
- Line 78: Which domain?
- Lines 111–112: What are the roles of AEROCLO and CLARIFY in this paper?
- Line 142: It would make more sense to start the chain of processes with surface emissions.
- Line 160: "also represented" – I suppose that semi-direct effects do not have a dedicated representation in ALADIN. They implicitly derive from direct effects.
- Lines 164–165: Strictly speaking, smoke is often anthropogenic – it is just that emission people cannot tell the two components apart so call the dataset "biomass-burning".
- Line 169: Is the fresh mode hygrophobic?
- Line 172: "more aged" is unclear. Once in the aged mode, aerosols cannot aged further in the model. Or are you saying that an e-folding time of 3 or 6 hours won't make much difference for SAO properties?
- Line 183: Is that the mean over the year or over the biomass-burning season? The latter would make more sense.
- Line 227: "forced-mode configuration" is ambiguous – does that mean fixed SSTs?

- Line 229: Need more information about those CMIP6 emissions, including a reference. This is a crucial aspect of the model, which will influence its capabilities and the interpretation of the comparison to observations. Is the dataset GFED- or GFAS- (i.e. satellite-)based? Are emissions really for the given year or just interpolations between key years, liked they did in CMIP5?
- Line 234: The boundary layer is probably deeper than just the first model level.
- Line 235: The Dentener recommendations are more complex that just injecting into the first model level. See their Table 2.
- Line 244: Is the ratio applied to the emissions or when mass is transferred from the fresh to the aged mode?
- Line 253: Worth noting that 0.15 represents about 20% of BBA AOD, so not a small change.
- Lines 298–302: Why is that a good thing for aerosol retrievals? Better correction of the Rayleigh contribution?
- Line 364: “ice clouds are not processed” is unclear. Does that mean that scenes containing ice clouds are discarded completely?
- Line 386: Does that retrieval suffers from the issues raised by Haywood et al. doi:10.1256/qj.03.100 (2004)? If so, that is a problem for the present study.
- Lines 395–396: Suggests shortening the title to “Reanalyses of atmospheric composition”.
- Line 424: But at this stage of the analysis, it is not yet known that clouds are too bright – it will be shown in the following section.
- Line 431: What kind of parameterizations are they?

- Lines 441–442: It would be worth noting that indirect effects are relevant to DRF, because DRF depends on the albedo of the underlying stratocumulus.
- Line 448: Note that the CAMS Reanalysis, successor to MACC, covers 2016, so could be added to the comparison. See https://apps.ecmwf.int/data-catalogues/cams-reanalysis/?class=mc&expver=eac4
- Lines 455, 477, and 482: Those large differences are surprising because MERRA is supposed to be assimilating MODIS! Perhaps a different collection? The fact that MERRA assimilates MODIS should explain the good temporal correlation, though.
- Lines 458–472: I agree that land-ocean contrast in satellite products are worth investigating further. At first, I though that marine aerosols could possibly explain why there is more AOD over ocean than over land. But if we assume that the contrast observed on Figure 3 south of the BBA plume, say 20S, is only due to seasalt, we only get about +0.1 contrast. Reporting that to within the plume leaves about 0.1-0.2 of contrast unexplained.
- Line 462: MODIS products include uncertainties so it is a good place to use them. Perhaps show an uncertainty range on Figure 4?
- Line 472: “more robust” – in terms of sampling yes, but the AOD retrievals are also more uncertain over land than over ocean because the surface albedo is larger.
- line 509: How is ACAOD calculated in the model? It is not always easy to determine where the cloud top is.
- Lines 523–524: What did Shinozuka et al. find?
- Lines 531–539: That analysis supports the idea that injection heights are not that important. Aerosols are lofted by convection anyway.

- Line 552: Is the decrease in extinction driven by a decrease in mass?
- Line 562: The statement on advection contradicts line 141. I fail to see why the model could not represent those BBA incursions into the BL – it might be that the model of Gordon et al. is wrong!
- Line 587–589: So the RH biases go in the right direction to (partly) explain the extinction biases.
- Line 615: The agreement is good but observational uncertainties are large.
- Lines 624–632: That paragraph is confusing. Is the comparison fair? Is the model simulating BBA on the days of the comparison? Can we be sure that LASIC is observing transported BBA and not local sources?
- Line 633: "reflect" – the observations are insufficient to link that absorption to ageing during transport. I am not convinced the model is wrong here.
- Line 650: Are all those studies based on modelling?
- Line 674: Section 4.3 is interesting. Essentially aerosol DRF errors in the SAO are driven by non-aerosol aspects. It is however unclear if the increased water vapour is due to the fires or because of transport in convective air masses. I suppose it is the latter, since the model does not emit water vapour with fires, nor does it account for additional buoyancy from the fires. Although lines 722–723 are ambiguous about what the model really does.
- Line 554: "we suspect". We were promised a bit more. Can we have an integrated assessment of what the different model biases in CF, COD, ACAOD, and SSA mean for DRF?
- Table 2: It would be useful to add a column listing the periods covered by each product.

Technical comments:

- Line 105: Consist to -> is to
- Line 187: g has not yet been defined, unless I missed it.
- Line 221: The definition of the domain encompasses the main biomass-burning sources *of that region*, and also the transport to the Atlantic ocean.
- Line 224: Suggest moving the Mlawer reference to line 155 for consistency with FMR.
- Line 234: Not sure "accordingly" is the right word here.
- Line 241: produce -> produced
- Line 357: CER has not yet been defined.
- Figure 7: Could the orography be put in a colour that is not in the colour scale used for aerosol extinction? Grey perhaps?

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