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Atmospheric Chemistry and Physics
Discussions

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.

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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 refer-

ence 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

Line 79: the first few times please state worming/cooling in association with positive and negative DRF for clarity for readers

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 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. 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.

Line 242: See comment on line 171

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.

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

Line 293: Which AERONET version was used? V3 came on line recently so it is not obvious.

Line 326: What was the altitude above clouds the reflectance was taken at?

Line 554; CALIOP is all CAPS

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.

Line 635: can you elaborate here? 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. Paragraph around 695: Can you compare model versus measured f(RH) directly here? 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. 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/

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