Review of: Combining POLDER-3 satellite observations and WRF-Chem numerical simulations to derive biomass-burins aerosol properties over the Southeast Atlantic region.
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Recommendation: Accept with Minor Revisions

This is an interesting study, in which the authors delve into the representation of the SSA and AE in the WRF-Chem model, and use POLDER/GRASP retrievals to improve the model representations, and then examine the impact of changing OC/BC ratios, refractive indices, etc. in the model. It's a good idea and brings to the forefront the model representations, which can otherwise seem like a black box in publications. My comments are relatively small.

A main one is that I do not see any support for the idea that dust is present in the marine atmosphere in July. It's included in WRF-Chem, and the satellite retrievals produce a coarse-mode aerosol that could be dust or sea salt, but the authors do not authenticate its presence. Does CALIOP identify dust over the ocean? How about the Haslett or DenJean papers examining the southerly July flow? If the evidence for dust is slim, why not experiment with a model dust representation that excludes dust? I would suggest the authors do so if they cannot better support that the presence of dust is real.

An additional comment is that the authors underemphasize the present of BBA in the boundary layer. In July, a large percentage of the total BBA is in the boundary layer, as opposed to above the clouds. The Haslett and DenJean papers document this (as likely others coming out of DACCIWA although I am not as familiar with their literature) as do publications focused on Ascension Island (see, e.g., Zhang and Zuidema, 2021, ACPD and Zuidema et al 2018). ORACLES observations from the month of August also suggest this is likely the case, e.g., Kacarab 2020 ACP and Redemann ACP 2021.

Another comment is that the literature references did not make full use of the new results that have come out as a result of ORACLES/CLARIFY/LASIC/DACCIWA, and to the extent that they do, the references tend to be present later in the paper, as opposed to helping to establish the context within the introduction. I have listed some at the end of this, and either mention in the specific comments or as part of the references how I think they contribute.

A small comment is that the English in places sounds labored, using extra words that a native English speaker would leave out. I make a note of a few such spots below.

More minor/specific comments:

Title: the title is not entirely accurate I feel, as the study is more about using satellite retrievals to constrain the WRF-Chem aerosol representation. Perhaps the authors want to reconsider.

Introduction, lines33-35: worth noting is that southern Africa, which is the focus of this study, by itself produces one-third of the global annual carbon emissions from BB, according to the cited Werf paper.

Intro, line 38: Costantino and Breon 2013 is not really the right reference for documenting the aerosol transport. The African Easterly Jet-South is not yet active in July. Knippertz et al. 2017 might be a better fit for this, or references within.

Intro line 38-40: I am not sure the anticyclonic circulation responsible for long-range transport off of the continent is well established by July. Adebiyi and Zuidema 2016 suggests it isn't. And neither does Fig. 4. Fig. 4 does show an anticyclonic circulation in a couple of places but I don't see those affecting the regions selected for this study, shown in Fig. 2. Given that the authors have the model winds at their fingertips, perhaps they could say more about the circulations affecting their selected regions? It seems like the land domain might also be influenced by the west African monsoon? Do any of the cited papers discuss July? I am not sure they do.

introduction, 2nd paragraph, p.2: portions of this discussion feel dated, through the focus on the aerosol above clouds and neglect of the boundary layer BBA. Try to update.

p.3: This is a nice literature overview. I'm confused why the Denjean, Taylor, Pistone papers aren't included here. It's also a very long paragraph, could it be split into two?

p. 3 line 93: CLARIFY and ORACLES references should be included here.

p. 3 line 95: what does ANR stand for?

P.4 line 106: remove 'to perform'

P. 4 lines 115-118: would be nice to see more documentation of this, is this based on the authors' own analysis?

p. 7 fig 1: why include August and September? They are not used. I would suggest removing these panels.

p. 11 lines 291-292: on what basis do the authors believe that desert dust emissions from north Africa may significantly contribute to the total aerosol load?

p. 13, figure 4: is the ECMWF reanalysis the Interim analysis or ERA5?, also, the wind vectors are very difficult to read. Please replot with fewer and thicker vectors. It also seems to me, based on Fig. 9, that the fields at 500 hPa could be removed from Figures 4-6 without loss.

p. 14-15, lines 347-349: I cannot see winds capable of a westward aerosol transport in Fig. 4. I think the authors may be confusing the meteorology of September, which I suspect is what the cited papers focus on, with that of July.

p. 16 lines 384-386: given the finding that GOCART seems to raise too much dust, why not include an experiment in which its presence is reduced? (also 'dusts' -> 'dust')

p. 17 line 404: did the Koffi papers evaluate July explicitly? There is a strong seasonal cycle to the aerosol vertical structure, see, e.g. Redemann 2021, ACP

p. 18 line 435: "on the opposite" -> "In contrast"

p. 18 line 439: are the authors suggesting an aerosol invigoration effect on shallow clouds? This seems very unlikely to me and I see no reference. I would suggest just attributing the cloud paramterization, although it confuses me the parameterization cited (Lin) is a microphysical parameterization according to Table 1. Wouldn't the boundary layer scheme be the more likely cause?

p. 18 line 464: sea salt mixed with smoke I would think. What does the model say?

P. 18 lines 467-468: see also Shinozuka 2020 ACP, which shows many models share these aerosol layer altitude biases.

p. 19, fig. 10 left panel+discussion: is this for the free-tropospheric aerosol layer? An average over the full column? I'm confused by this, and how the coarse mode is increasing with distance. Incidentally the orange star is placed near Ascension Island, and some assessment could be done using the LASIC datasets if interested.

p. 22 lines 532-534: no mention of dust in the DACCIWA data description, further leading me to think an experiment should be done where it is removed from WRF-Chem and a further assessment done to see what additional changes have to be incorporated, for WRF-Chem to match the satellite retrievals.

p. 25 line 568: "an increased of the aerosol absorption' => 'increased aerosol absorption'

p. 25 line 573: "consistently' ->'consistent' here and elsewhere.

p. 28, line 643: is there any evidence for more desert dust becoming incorporated into the aerosol layer as it moves westward other than from the WRF-Chem model? Any observations of this?

p. 29 line 661: remove "that", 'Located" "the geographic coordinates area"

p. 30 line 692: is the Leahy 2007 representative of July? Eck 2013, Zuidema 2018 both show a strong seasonal evolution to the SSA so good to make sure it's about the same time frame.

p. 33 references: the formatting of the references is not consistent, check to make sure they fit the ACP format. It also seems like there are more references than are used?

Additional reference suggestions:

Adebiyi, A. and P. Zuidema, 2016: The role of the southern African easterly jet in modifying the southeast Atlantic aerosol and cloud environments. *Q. J. R. Meteorol. Soc.*, **142**, p. 1574-1589 doi: [10.1002/qj.2765

Haywood, J. M., S. Abel, P. Barrett, et al, 2020: Overview: The CLoud-Aerosol-Radiation Interaction and Forcing: Year-2017 (CLARIFY-2017) measurement campaign, *Atmos. Chem. Phys.*, **21**, p. 1049-1084, doi:10.5194/acp-21-1049-2021

Kacarab, M., et al, 2020: Biomass Burning Aerosol as a Modulator of Droplet Number in the Southeast Atlantic Region. *Atmos. Chem. Phys.*, **20**, p. 3029-3040, doi:10.5194/acp-20-3029-2020 - this focuses on the month of August, so slightly later than July, nevertheless supports the idea that in the early part of the BB season, much aerosol is also present in the boundary layer, and, indicates the microphysical implication.

Knippertz, P., Fink, A. H., Deroubaix, A., et al..: A meteorological and chemical overview of the DACCIWA field campaign in West Africa in June–July 2016, Atmos. Chem. Phys., 17, 10893–10918, https://doi.org/10.5194/acp-17-10893-2017, 2017

Redemann, J., R. Wood, P. Zuidema, et al, 2020: An overview of the ORACLES (ObseRvations of Aerosols above CLouds and their intEractionS) project: aerosol-cloud-radiation interactions in the Southeast Atlantic basin. *Atmos. Chem. Phys.*, **21**, p. 1507-1563, doi:10.5194/acp-21-1507-2021

Shinozuka, Y., P. E. Saide, G. A. Ferrada, et al. 2020: Modeling the smoky troposphere of the southeast Atlantic: a comparison to ORACLES airborne observations from September of 2016. *Atmos. Chem. Phys.*, **20**, p. 11,491-11,526, doi:10.5194/acp-20-11491-2020 - could be interesting (in a future study) to see how the new WRF-Chem representation does within this same comparison, the data are accessible in an easy meta format. Also useful for more context on where WRF-Chem falls in the pantheon of aerosol model representations. The downward 'slumping'of the aerosol layer shown in Fig. 9 is also shown to be common to many other aerosol modeling efforts.

J. Zhang and P. Zuidema, 2021: Sunlight-absorbing aerosol amplifies the seasonal cycle in low cloud fraction over the southeast Atlantic: *Atmos. Chem. Phys. Disc.*, doi:10.5194/acp-2021-275 - more information on July aerosol observations.