

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2021-333

Anonymous Referee #1

Referee comment on "Modeled and observed properties related to the direct aerosol radiative effect of biomass burning aerosol over the southeastern Atlantic" by Sarah J. Doherty et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-333-RC1>, 2021

Review for "Modeled and observed properties related to the direct aerosol radiative effect of biomass burning aerosol over the Southeast Atlantic" by Doherty et al.

This study presents a thorough comparison between a number of modelling frameworks and in situ observations made during the ORACLES field campaign that took place over the southeast Atlantic Ocean during three month-long periods in three consecutive years. The study focuses on parameters and quantities that are used in quantifying the direct radiative effect from biomass burning aerosol. A first order calculation of the direct effect using observations and comparisons to the models is presented. The study helps identify key failings in the ability of the models to reproduce the observations, which will be very useful for focusing future studies.

Although the manuscript is considerably long I believe the in-depth presentation of methods and the authors' treatment of uncertainties is on the whole necessary. However, I believe some figures and passages of text can be improved to improve the flow of the study and highlight the key messages being presented throughout. I therefore recommend this manuscript is published in ACP once the, largely minor, comments below are addressed.

Main comments:

My main concern is the length of the manuscript. On reading it I often found that the key messages in each section or paragraph were not as clear as they could be. For shorter

manuscripts this would be fine, but due to the length of this manuscript I strongly suggest the authors rethink some aspects. The figures are very large and I found myself endlessly scrolling through them. I suggest replotting the figures to make them smaller and more compact. For instance, try and combine Figures 7-9, same for 10-13, etc. The second aspect that can be improved is the construction of the paragraphs. The manuscript occasionally contains sections consisting of short paragraphs (for example see lines 1560 to 1580, 1600 to 1625) which break the flow of the manuscript and makes it difficult to identify the key messages. To make the manuscript more readable I suggest the authors go through the manuscript and make sure the key messages for each section are clearly delivered. Some paragraphs can be reduced in length with just the key result put forward – the addition of exact values and consideration of individual grid boxes sometimes made it difficult reading.

I don't feel that section 4.1 brings much to the manuscript. I believe the methodology is somewhat flawed and the outcomes are not used in the rest of the manuscript. Using the models to provide the test of representativeness is entirely dependent on the ability of the models to reproduce the observations – which as shown later in the manuscript isn't great. This is further illustrated by the lack of consistency between the two models used. At the end of the section there is no discussion on what the results actually tell us and how they influence the rest of the manuscript. Even in the summary it is difficult to understand what the outcomes of the analysis are. Does this tell us anything that Shinozuka et al. (2020) does not? Does the analysis mean that the proceeding comparison is not representative of the climatology? I suggest either removing the section or making the outcomes clearer.

The DARE section is the highlight of the paper. In its current form it feels like a second paper that was appended onto the manuscript. The authors may wish to consider moving the summary section to the end and integrate the DARE section into the manuscript.

Minor comments:

Line 61. This one sentence hides a lot of the importance and uncertainties and of ARI and ACI. Maybe expand to give a better account of why understanding ARI and ACI is important?

Line 243. How sensitive is the weighting function to the chosen value for the standard deviation?

Line 250. Please provide a characteristic size for the accumulation mode.

Line 283. Please clarify what the 'original PSAP instrument' is referring to.

Line 305. It may be useful to include a sentence that sums up what satellite product you end up assigning to each cloud property variable.

Line 388. Can you include a concluding sentence that answers the question at the beginning of this paragraph?

Line 485. The figure shows ratios and the text discusses percentages. Please change one of them to make it consistent.

Line 508. Doesn't Figure 3f show a bias up to +200% at one level? Or are you talking about the column mean?

Line 700. Is this consistent with Shinozuka's conclusions for representativeness?

Line 940. Do the models also show weak wet deposition?

Line 956-958. Are you discussing the characteristics of the observations or comparing against the models?

Line 958' "a broader vertical extent towards the core of the plume". This doesn't make sense to me.

Line 963. There doesn't seem to be much consistency in the 'over-estimation above and below the plume centre' for WRFCAM in Figure 7 or in table 1.

Line 965. I'm surprised you don't point out the substantial and largely consistent overestimation at lower altitudes for GEOS.

Line 1136. Spelling mistake: humidification

Line 1144. Do you have a sense of how sensitive your results would be to gamma?

Line 1152. Please can you include the calculated ranges?

Line 1173. Having subsections for each model would be beneficial for the reader.

Line 1173. The WRF-CAM5 section is difficult to follow, but the other models are better. I suggest the authors look at this model section and try to improve the clarity of it.

Figure 10. It would be useful to have a title above both columns to easily differentiate the two without having to refer to the caption.

Line 1224. 'closer to or greater than 1.0' this is not consistent though..

Figure 11. 'as in figure 8, but for the GEOS model' I don't think the cross ref is correct. There are also other instances of incorrectly cross-referenced figures so please check all captions.

Line 1520. 'SSA is consistently higher than that observed' but aren't observations for low RH? If so, isn't 'consistently higher than observed' actually good?

Line 1523. What trends are being referred to here?

Figure 15. The observations bar makes it seem like you are comparing like for like, but it may be more appropriate to make the observations bar unfilled as it for low RH, and therefore more comparable to the UM-UKCA unfilled bar?

Line 1598. But the UM-UKCA dry vs ambient SSA are very different – doesn't this go against this statement? Does it suggest the model is completely wrong?

Line 1614. Any ideas why this is occurring?

Line 1713. So the models are accidentally correct because they don't include absorption by brown carbon. Do the models provide information on brown carbon content?

Line 1965 (and 1988). Do you mean QFED2 rather than GFED?

Line 1972. Could differences in model dynamics lead to discrepancies?

Line 2017. Has this been reported in previous literature?

Line 2021. 'the plume top terminating at lower altitude in the observations than in some of the simulations' I thought the HSRL data showed that this wasn't actually the case and that the aerosol was indeed present at these higher altitudes? (see line 1236)

Line 2093. '8-11 for all four' is this the range or difference? Please can you clarify

Line 2008. This estimation of the direct effect must implicitly assume that there are no rapid adjustments that may have influenced the underlying cloud field. Do you assume that all retrievals used are consistent with a sufficiently separated smoke-cloud scene? I think adding a sentence to clarify the assumption would be useful.