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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The significant role of biomass burning aerosols in clouds and radiation in the South-eastern Atlantic Ocean" *by* Haochi Che et al.

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In this manuscript, Che et al. run the latest generation of UKESM to investigate aerosol direct, semi-direct, and indirect effects from biomass burning smoke plumes produced by agricultural burning over the southeast Atlantic Ocean. The headline finding (in my read) is that the semi-direct effect of cooling due to increased cloudiness from a stronger cloud-top inversion dominates the overall radiative forcing, offset substantially by the direct effect of smoke absorption and reinforced marginally by indirect effects.

The manuscript is well organized and the findings appear sound for the most part. (I do have some questions below, mainly pertaining to the Twomey effect). The text

could use some areas of clarification and potentially additional information. I would not anticipate any major new analyses would need to be undertaken to address my comments, so I therefore recommend publication following minor revisions.

General comment:

My biggest (really, only) concern with the results is that the indirect effect estimate seems unrealistically small given the change in cloud droplet number concentration you diagnose. From a simple back-of-the-envelope calculation of the Twomey effect, a doubling of cloud droplet concentration (you report 56% of CDNC are from biomass burning) should lead to a radiative forcing of O(10) W/m2, as in Lu et al. (2018). This is before taking into account the small liquid water increases you find. In my own work in the region (Diamond et al., 2020), I've estimated that a 5% increase in climatological CDNC from the influence of shipping produces a radiative forcing of $\sim$-2 W/m2 during austral spring. I thus find it hard to believe that doubling CDNC would produce less than that in austral winter.

Specific comments:

1. Title: The title is currently rather non-informative. I would recommend highlighting that the semi-direct effects dominate (really the highlight of the paper in my opinion) in the title, and also mentioning that this is a global climate modeling study (as opposed to in situ or satellite observations or high-resolution process modeling).

2. Page 2, Line 1: I don't follow why the net sign of the aerosol radiative effect, rather than its magnitude, signifies the importance of aerosol in this region.

3. Page 3, Line 3: I would argue that in situ results showing abundant biomass burning influence within the MBL at Ascension Island from the LASIC campaign (Zuidema et al., 2017) and throughout the SE Atlantic Klein-Hartmann box over the remote ocean from ORACLES (Diamond et al., 2018; Kacarab et al., 2020) are more directly relevant to your point about smoke-cloud interaction.

4. Page 3, Line 5: I don't believe the cited literature backs up the claim that "most" of the BBA is entrained into the MBL.

5. Page 3, Line 24: The large eddy simulation results of Yamaguchi et al. (2015) and Zhou et al. (2017) also seem relevant to cite/discuss here.

6. Page 4, Lines 1-2: The BBA plume subsiding and gradually meeting the rising MBL is true in the mean, but the picture is much more nuanced in reality, as instances of smoke-cloud contact were seen to be highly variable between and even with flights during the ORACLES and CLARIFY campaigns. It is also not necessarily the case that smoke-cloud contact corresponds instantaneously with the MBL being polluted, as discussed in Diamond et al. (2018).

7. Page 4, Line 13: Is dust included as one of the "five interactive log-normal aerosol modes"? I only count four other components (sulfate, sea salt, black carbon, organic carbon). The phrasing currently is confusing, as it sounds like the dust representation is entirely separate.

8. Page 5, Line 9: Somewhere in the manuscript you should discuss the implications of only looking at one part of the biomass burning season (July-August). It is well known that the BB plume properties change over the course of the biomass burning season, influenced in part by meteorological shifts like the strengthening of the southern African Easterly Jet (AEJ-S) in September and October that corresponds with a more elevated plume (Adebiyi & Zuidema, 2016).

9. Page 5, Lines 17-20: I'm surprised that you do not use any of the new products from MODIS or SEVIRI that account for above-cloud aerosol absorption. I would recommend trying the comparison using one of those products or at least discussing the issues with traditional AOD products that cannot retrieve AOD in the presence of clouds.

10. Figure 1: What altitude is being shown, or is this a column average? There is a

large amount of vertical variability in the plume (as seen in Figure 2) so the 2D picture is a bit difficult to interpret.

11. Page 6, Line 4: I do not understand why you only compare September AOD when the analysis focuses on July-August. As discussed earlier, there are known differences in plume location throughout the biomass burning season, in part driven by different meteorological factors between July-August and September-October. Thus, it's entirely possible that the model could represent one part of the season well but the other poorly if it is not representing those meteorological shifts properly. Figure S2 should be replaced with a new version including July and August.

12. Page 6, Lines 12-13: As discussed above, although this description makes sense in the climatology, the picture we found in the field is much more complicated than the mean suggests. For one, much of the smoke in the marine boundary layer at a given location may have been entrained upstream and not necessarily reflect the properties of the plume above-cloud at the time of sampling (Diamond et al., 2018). It may be worth noting that although the mean field shows a plume subsiding from east to west, actual plume distribution and occurrence of plume-cloud contact at any given time is more nuanced.

13. Page 7, Lines 6-7: Are these percentages for the column burden? It may be worth also reporting the values for the marine boundary layer separately (if they differ), as the MBL CCN concentration is what matters most for cloud droplet activation.

14. Figure S3: Figure S3 is an exact copy of Figure 3. I believe the figure the authors meant to include would show the change in CCN due to BBA?

15. Page 7, Line 27: This should be testable by looking at the average strength of the cloud-top inversion between the different model runs directly.

16. Page 8, Line 4: This statement needs qualification, as the SS increases where most of the cloud mass is. Are you only referring to the westernmost region? Or this

actually supposed to say that the increase in SS is noticeable in the net (decrease from microphysics is more than compensated by increase from absorption)?

17. Page 8, Line 20: BBA being 56% of the CDNC is less than the 68% figure quoted above for CCN, but is that for the column or MBL only? It would be more relevant to compare the fraction of MBL CCN that is from BBA to the CDNC change, as the BBA aloft does not activate.

18. Page 8, Line 30: The various LES studies cited in this review (Yamaguchi et al., 2015; Zhou et al., 2017) and by the authors (Herbert et al., 2020) seem relevant to reference here in addition to the classic study of Johnson et al. (2004).

19. Page 8, Line 33: This is due to the absorption effect lowering the relative humidity within the MBL, correct? It would be helpful to be explicit about this.

20. Page 9, Lines 2-3: The LWP effect of BBA absorption is to increase LWP as one moves from west to east. The text is written to make it sound as if LWP is decreasing from west to east due to BBA absorption. The text should be clarified here.

21. Page 9, Lines 16-17: You should clarify that the BBA in the MBL suppresses CDNC through the semi-direct effect here, not the indirect effect (which actually causes CDNC to increase substantially).

22. Page 9, Lines 23-24: As mentioned in the general comment, this result is very surprising given the large increase in CDNC, which should lead to an albedo increase of $\sim$0.05-0.10.

23. Page 10, Line 3: I don't understand how the indirect effects could have led to a warming given both CDNC and LWP increase. Did cloud fraction decrease anywhere? Or could this just be due to weather noise between different initializations?

24. Page 10, Lines 20-21: This sentence should be rewritten for clarity. The semi-direct effect is not cooling at cloud top and warming below; rather, above-cloud semi-direct effects lead to a TOA cooling whereas below-cloud semi-direct effects lead to a TOA

warming.

25. Page 10, Line 29: The results of Gordon et al. (2018) are also averaged over a different region that you are using, correct? It would be helpful to compare the values averaged over the same region, as the spatial mismatch could also lead to discrepancies.

26. Page 10, Lines 31-33: I would be more believing of this argument (the kappa values in Gordon et al., 2018, do seem unreasonably high) if you did not find a significant increase in CDNC even with your lower (and probably more realistic) kappa values in this study.

27. Page 11, Lines 4-6: If you're talking about TOA radiation, isn't the relevant effect that less OLR makes it out due to the radiation coming from the relatively cool cloud tops rather than the warmer surface? Zhou et al. (2017) discuss the potentially important role of LW radiative effects in BBA-cloud interactions.

28. Page 12, Lines 6-7. Increasing the inversion strength, rather than "lowering the temperature inversion"? Or are you talking about lowering the height of the inversion? I'd argue that has more to do with the clouds not being able to grow via entrainment

29. Page 12, Line 12: Cloud top/base is maybe not the most useful shorthand here, as the increase in SS occurs throughout the cloudy layer near the continent, where the cloud deck is most prevalent in general. The base/top difference only shows up further offshore.

30. Page 13, Lines 4-5: The global and regional indirect effects are "similar" in that they're both indistinguishable from zero... maybe you can argue the global effect is from long range transport and MBL advection, but I wouldn't necessarily highlight this idea in the very last sentence of your paper.

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