

# ***Interactive comment on “Modeling global radiative effect of brown carbon: A larger heating source in the tropical free troposphere than black carbon” by Aoxing Zhang et al.***

## **Anonymous Referee #1**

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In this study, the authors implement a brown carbon scheme into a global model, evaluate a series of simulations with varying assumptions about photobleaching and convective transport against observations from HIPPO, SEAC4RS and DC3, and then estimate the heating rates and DREs from their simulation which best fits observations. This is certainly an interesting topic which warrants further modeling studies to explore the impacts of large uncertainties in our understanding of the properties and evolution of BrC in the atmosphere. There are however two major issues in this manuscript that should be addressed prior to publication (as well as more minor issues described below):

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1. This modeling study shares significant methodological overlap with previous modeling efforts for BrC, particularly Wang et al, ACP, 2018 and Brown et al., ACP, 2018, the first of which compares to the same BrC observations used here and the second of which uses the same model. Both of these previous model studies explore photobleaching. The primary novelty of this study is therefore the focus on convective transport. The authors should therefore be careful not to overstate the novelty of their work, and acknowledge and contrast to the existing literature throughout (Introduction, Results, Conclusions), particularly how assumptions made in this study might differ from these previous studies, why different assumptions might have been implemented, and how this impacts the comparison with these previous studies.

2. While the heating rate conclusions are the most interesting aspect of the manuscript, the results are substantially overstated. Figure 13 shows that BrC heating rates barely exceed those of BC in the UT in convective regions. Given that uncertainties on the simulation (convective parameterization, removal, optical properties, etc, etc, etc) are large, the authors cannot state with high confidence that the heating rate from BrC exceeds BC. In particular, given that this study does not include any observational evaluation in the tropics, where the authors suggest this effect is most important, this conclusion is unsupported. The authors should temper the discussion of these results. Similarly, the manuscript title should be modified to eliminate overstatement of the results.

#### Additional Comments

1. Line 70: missing name on reference
2. Lines 91-95: specify meteorological years simulated
3. Section 2.2: Should discuss how many different emission factors (i.e. biomes) are used in the inventory and whether their resulting BrC inventory adequately represents the variability in fuels.

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4. Section 2.2: Given that the authors rely on comparisons between BrC and BC later in the text, they should include details on BC aging and optical properties in this section.
5. Lines 130 and 133: use the same wavelength for MAEs so that they can be compared
6. Line 203-204: specify that this statement applies to the default model
7. Lines 199-215: discussion of BC removal should also reference and compare to approach of global model study of Q. Wang et al. (JGR, 2014).
8. Line 243: requires citation at the end of the sentence.
9. Lines 245-252: it would be useful to discuss why BrC scavenging isn't treated similarly to BC scavenging in each simulation
10. Figure 7: Why don't the authors compare observed and simulated OA mass and BrC absorption directly?
11. Line 262: "During DC3 experiment, respectively." Is not a sentence
12. Line 263: "Both the observations and model simulations show the increase of BrC/BC ratio at in the upper troposphere"; statement is inaccurate, model simulations do not show increase.
13. Lines 261-284: This discussion is a little confusing. It should be clear from the text that the NCB and ICNB simulations are inconsistent with observations, but also that no model simulation captures the increase in BrC/BC as observed, particularly for SEAC4RS.
14. Line 314: I think the authors mean DRE not "radiative forcing" here to be consistent with their earlier discussion.

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-594>,

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