Comment on acp-2022-193

Anonymous Referee #1

Referee comment on "Biomass burning CO, PM and fuel consumption per unit burned area estimates derived across Africa using geostationary SEVIRI fire radiative power and Sentinel-5P CO data" by Hannah M. Nguyen et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-193-RC1, 2022

General Comments:

This work develops a biomass burning CO emissions inventory for Africa at high temporal resolution using a top-down method. It iterates on existing work, with the main contribution being the direct use of satellite-observed CO to estimate CO emission coefficients, as opposed to using aerosol optical depth to estimate total particulate matter (TPM). This approach is attractive because it eliminates significant uncertainty and variability in TPM emission factors.

The quality of the resulting inventory is assessed in two ways, a direct comparison of existing modeled emissions from a bottom-up approach (GFED4s), and a validation using the developed emissions to drive a chemical transport model and comparing the resulting mapped CO against independent satellite observations.

Overall, the work is of relevance to ACP community. The approach is valid and the writing and figures are reasonably clear. The advancements are incremental, but important. A few methodological details should be better explained, and qualitative language and an overemphasis on pointing out strengths over weaknesses should be toned down throughout.

Specific Comments:

Line 61: "The FEER and FREM approaches derive landscape fire emissions estimates directly from EO-derived FRE measures, removing the step requiring calculation of DMC and thus the uncertainties inherent in the calculation." While this is a true statement, it

does not mention the tradeoff in uncertainties from taking a different approach. There are uncertainties inherent in the estimation of the smoke emissions coefficient. If the authors would like to assert that the top-down approach has lower overall uncertainty, they will need to support that in the text.

Line 76: While the geostationary satellites provide higher temporal resolution, and it is explained why this is desirable, they also provide lower spatial resolution. Please explain the benefits and tradeoffs.

Lines 75, 96, 277, 292, 293, 307, 466, 558: Take out the word "far" entirely or replace it with something quantitative. This is overly qualitative and the reader should decide what is or isn't "far greater," "far higher," "far more consistent," etc.

Line 123: I would like to see at least a short description of the "small fire adjustment" factor used in the analysis, beyond just the reference. It appears to be important. Table 2 indicates a 50% difference with our without this correction. This makes it a core part of the method that should be discussed.

Line 173: Why was OLS used over ODR. I appreciate that the original work was recalculated using OLS for consistency, but was there a reason for the switch? The new values are 14% lower. Is that a better estimate? No justification was given for the switch so I'm not sure which version I should prefer.

Line 187: "TROPOMI CO plumes in the closed canopy forest biome were not sufficiently distinct from the background in this biome." Is this a shortcoming of TROPOMI CO, the method, or just particular to the region? In some regions, closed canopy forests are the primary source of biomass burning emissions. It would be good to get a discussion on how applicable this approach is to other parts of the world.

Table 1: I'm not sure I understand the need for C^{CO}_e calculated via C^{TPM}_e. What is the reason for showing this?

Lines 245-249: Does this discrepancy between FRP and CO emissions timing suggest that the relationship depends on the type of combustion? Are long-lived smoldering fires prevalent in this region? Do the authors have any hypothesis for this?

Figure 9. For intercomparison between the figures. it would be useful to have outlines of the ROIs on these maps. This would be more helpful to me than the city names, which are not included on Figure 8.

Technical/Grammatical Comments:

Line 30: Recommend removing allusions to a potential future product in the abstract.

Line 58: Remove "fully"

Line 62: Define "EO"

Line 211: "Figure 1" Is this a typo? I don't see how Figure 1 shows this at all.

Line 326: Remove parenthetical. Those details are given below on line 348.

Line 456: maybe --> may be

Line 598: will available --> will be available