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Comment on acp-2022-193

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Community 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-CC1>, 2022

General Comments:

This work uses a top-down approach to estimate CO emissions from biomass burning across Africa from 2004 through 2019. The main data underlying this emissions inventory is the geostationary SEVIRI FRP-PIXEL AF product, which contains a record of the estimated Fire Radiative Power (FRP; MW) emitted from landscape fires and detected by SEVIRI at 3 km spatial resolution and 15 min temporal resolution. Spatially and temporally integrating SEVIRI records of FRP yields an estimate of the Fire Radiative Energy (FRE; MJ) emitted from landscape fires burning within a given region and over a certain duration. Directly converting SEVIRI estimates of FRE into CO emissions (T_g) is accomplished through the application of 6 biome-specific emission coefficients ($g\ MJ^{-1}$) derived using a training dataset of coincident SEVIRI active fire observations and Sentinel-5P TROPOMI CO retrievals. Estimates of CO emissions – and by extension Dry Matter Consumption (DMC) and total carbon (TC) release – are presented and compared with the Global Fire Emissions Database (GFED) at a variety of spatiotemporal scales. Moreover, CO emissions estimated from the SEVIRI FRP-PIXEL AF product using the FREM_bCO method are input into WRF-CMAQ and the modelled outputs are compared to Sentinel-5P TROPOMI CO retrievals.

This work is relevant to the readers of ACP. Although this work leverages methodologies previously established by these and other authors, several issues should be considered prior to publication.

Specific Comments:

1. With some effort I was able to understand the methods and results, but not with any help from the nomenclature. In my view, the nomenclature used herein is internally inconsistent and may conflict with the nomenclature used in previous works describing older versions of the Fire Radiative Energy Emissions (FREM) approach. Please note the following:

- a. The "smoke emission coefficient" is introduced on Page 2, Line 63, as C_e^x , where the superscript x indicates the trace gas or aerosol of interest. The subscript e is never defined, though I suspect the subscript e stands for "emission" and the C stands for "coefficient". However on Page 3, Line 72, the subscript e in β_e stands for extinction.

Nevertheless, the emissions coefficients for total particulate matter (TPM) and carbon monoxide (CO) are appropriately written as C_e^{TPM} and C_e^{CO} .

- b. The emissions factor (EF) is introduced in Equation 1 on Page 3, Line 86. However for emissions factors, the subscript x , not the superscript x , indicates the trace gas or aerosol of interest. To better align with the nomenclature of the smoke emission coefficient, it seems to me that x belongs as a superscript in the nomenclature of the emissions factors. Also, if EF stands for emissions factor, then it seems logical to me that EC should stand for emissions coefficient.
- c. On Page 3, Line 87, it states: "Where is the biome-specific emission coefficient for trace gas species $\square\square$ ". Okay, so the subscript e doesn't stand for "emission" but rather e indicates the biome? Why not use a subscript b instead of an e to indicate the biome?
- d. Also on Page 3, Line 87, it states: " EF_x is the species $\square\square$ emission factor for that biome". However there is no subscript e or b in EF_x that indicates the biome?
- e. On Page 5, Lines 133-135, it states: "the FREM methodology derives a biome-dependent 'smoke emission coefficient' for a reference species $\square\square, C_e^y$] from the relationship between the thermal energy a fire radiates (i.e. the FRE in MJ) and the mass of the target compound $\square\square$ it emits (in kg or g)". What is the difference between the reference species *reference*, the reference species y , and the target compound y ? To the point, what is the difference between $C_e^{reference}$ on Page 3, Line 88, and C_e^y on Page 5, Line 134? What is the difference between a reference species and a target compound?
- f. On Page 8, Figure 2, all of the legends in the subplots contain C_{biome} = regression fit. This seems like new nomenclature, but not new if $e = biome$.
- g. Page 7, Lines 196-198: And here " b " is introduced as the base or reference? What is the difference between base and reference?
- h. Ultimately, it seems to me that Equation 1 could be written as follows:

$$EC_b^x [g.MJ^{-1}] = \frac{EF_b^x [g.kg^{-1}]}{EF_b^{ref} [g.kg^{-1}]} EC_b^{ref} [g.MJ^{-1}]$$

where EC is an emission coefficient, EF is an emission factor, the subscript b is the biome, the superscript x represents the trace gas or aerosol of interest, and the superscript ref represents the reference species.

2. Page 5, Section 2.2: The calculation of total plume CO [in g or kg] is not altogether clear to me and could be better described in the methods. I think the units stored in the Sentinel-5 precursor/TROPOMI Level 2 CO Product are mol m^{-2} , and so I'm assuming a conversion using molar mass is performed, but this is never stated. What is the buffer size? It seems to me that the plume boundaries were hand digitized, so is it possible that the buffer sizes are not constant but change from matchup to matchup? I'm assuming the minimum CO value was simply subtracted from every pixel within the plume buffer to yield the excess CO above the background, but this is never explicitly stated? I don't think "summing this excess over all plume pixels thus provided the total amount of fire-emitted CO in the plume for that matchup fire" is an accurate description. At some point you need to multiply the pixel value (mol m^{-2} or g m^{-2}) by the pixel size (m^2), correct? What is the actual pixel size? Is the pixel size constant or does it vary depending on the location of the pixel in the swath? I believe earlier in the archive it's $7 \times 7 \text{ km}$, correct? I also think the instrument settings for TROPOMI were changed on August 6th, 2019, which reduced the along-track pixel dimensions to $5.5 \times 7.0 \text{ km}$. Were the different pixel sizes between 2018 and 2020 taken into account?

3. Page 6, Section 2.3: Why the switch from ODR to OLS? Seems like 14% is a substantial difference just based on the selection of the regression method?

4. Page 7, Lines 186-194: What are the consequences of this limitation? How much CO is emitted from closed canopy forests relative to the total CO budget for Africa? I could not find contributions from individual biomes reported anywhere. Would it even be possible to complete the "FREM_bCO" emissions inventory without leveraging the "FEER-equivalent" value for closed canopy forests? Will this limitation appear again in other closed canopy forests across the globe when matching TROPOMI TCCO retrievals with different geostationary AF and FRP product.

5. Page 16, Lines 344-346: This sentence states: "Aerosol species emissions were generated through an analogues application of the updated FREM-TPM emission coefficients of Nguyen & Wooster (2020) (see Appendix A)". Given the title of this manuscript, I fail to see where this part fits into the FREM-Derived CO Emissions evaluation/assessment. Since this is not really related to the CO emissions inventory, and since all of the AOD results are compared in the Appendix and not in the body of the manuscript, I would strongly reconsider whether the TPM/AOD evaluation/assessment is necessary.

6 Page 19, Line 382: Results of the evaluation/assessment from this point forward are extremely confusing to me. I think part of the reason is that the authors use the term "daily summed TCCO". From my understanding there is one map of modelled TCCO and one map of S5P- observed TCCO generated daily sometime between 12:00 and 14:00 UTC. How can these be daily summed if there is only one map produced daily. I think the author's meant to say that the daily maps were summed over the extents of the ROIs. However this isn't a completely accurate description either as the TCCO values should be multiplied by the pixel area, correct? So in truth, per-pixel values of TCCO ($\text{g}\cdot\text{m}^{-2}$) were multiplied by their respective pixel areas (m^2) and then summed over the extents of the ROI's. If correct, I'm not entirely sure what this yields. Having the units of Gg, it seems to me that this is the total CO contained by the atmosphere in the ROI's? It's not the excess CO above background due to biomass burning since both the model and the observations also account for the background. So I guess it's just the total amount of CO contained by the atmosphere in the ROI's, but I guess I would have expected to see this presented in ppm, not in absolute terms like Gg. Also note that from Page 19, Line 382 onwards, the terms total column carbon monoxide (TCCO) and for a lack of a better term total carbon monoxide load (CO) are used interchangeably with interchangeable units of $\text{g}\cdot\text{m}^{-2}$ and g, leading me to question whether the author's themselves understand what they've calculated. For example, on Page 19, Line 382, TCCO is presented as Gg, which up until this point, including Figure 9 on the previous page, TCCO was presented as $\text{g}\cdot\text{m}^{-2}$. On Page 20, Figure 10, the total CO plotted in the left hand panels is presented as Gg, whereas Total Column CO plotted in the right hand panels is also presented in Gg. Finally, similar contradictions in terminology and units arise in Table 3 and Figure 11. I strongly suggest that the authors (i) define exactly what CO (in g) is supposed to represent, (ii) why the units of g instead of ppm are more appropriate, (iii) how CO in g differs from TCCO in $\text{g}\cdot\text{m}^{-2}$, and (iv) ensure that all remaining figures and tables are appropriately labelled with either CO or TCCO along with their respective units.

Technical Corrections:

- Page 1, Title: Seems a bit asymmetrical to me. Suggest changing "geostationary" to "SEVIRI" or vice versa changing "Sentinal-5P TROPOMI" to "polar orbiting".
- Page 1, Line 11: Likewise, to balance out "geostationary" in the first half of the sentence, I suggest changing "satellite observations of Total Column Carbon Monoxide (TCCO)" in the latter half of the sentence to "polar orbiting satellite observations of Total Column Carbon Monoxide (TCCO)."
- Page 1, Line 18: Instead of "spanning 16 years" maybe state the actual range of years, 2004-2019 correct?
- Page 1, Line 19: Suggest deleting "to derive CO emissions" from the end of the

sentence.

- Page 1, Line 20: Perhaps clarify that “per unit area” is “per unit burned area”.
- Page 1, Line 23: Here and elsewhere, I’m not sure that comparing outputs from the WRF-CMAQ chemical transport model with Sentinel-5P TROPOMI TCCO observations constitutes a “validation” of the FREM approach. It’s an assessment, perhaps, but not a validation.
- Page 2, Line 49: Suggest changing “actively burning fires” to “active fires” to coincide with the acronym “AF”.
- Page 2, Line 50: Suggest changing “relationship that relates a biome’s fire radiative energy (FRE) to DMC totals coming from GFED” to “relationship between a biome’s fire radiative energy (FRE) and DMC totals modelled in GFED”.
- Page 2, Lines 50-51: Suggest changing “The primary advantage over GFED” to “The primary advantage of GFAS”.
- Page 2, Line 53: Suggest changing “The main disadvantage is the fact that the relatively uncertain fuel load and combustion completeness assumptions, which introduce some of the most significant uncertainty to burned-area based fire emissions calculations, are incorporated into GFAS via this calibration” to “The main disadvantage is the fact that the relatively uncertain fuel load and combustion completeness assumptions, which introduce some of the most significant uncertainty to bottom-up fire emissions calculations, are also incorporated into GFAS via the calibration with GFED”.
- Page 2, Line 53: Granted the acronym FREM was defined in the abstract, but shouldn’t it also be defined in body.
- Page 3, Line 81: Maybe start a new paragraph?
- Page 3, Line 91: Suggest changing first part of the sentence to read: “Using Equation 1 to translate between emission coefficients...”
- Page 3, Line 93: Suggest moving “for example” to the beginning of the sentence.
- Page 4, Lines 125-126: Suggest revising the latter half of the sentence to read “We also apply the cloud cover correction used in the LSA SAF Meteosat FRP-GRID product (Wooster et al., 2015), though the effect of this adjustment is limited due to sparse cloud cover during the African fire season.
- Page 5, Line 144: Suggest replacing “temporally different” with “asynchronous”.
- Page 5, Line 145: This seems to imply that the 6 biomes were split into NHAf and SHAF to yield 12 relationships, which is not the case. I suggest rewording this sentence for clarification.
- Page 5, Line 153: Do you mean 50% of the observed FRP or 50% of the observed number of AF pixels?
- Page 6, Line 165: Were the AF pixels detected at a single SEVIRI timeslot or are they the cumulative collection of AF pixels detected from the start of the fire? Only two times are reported (11:24 and 11:30 UTC), which presumably correspond to VIIRS and Sentinel-5P, but what times were the AF pixels detected?
- Page 6, Line 183: What is the acronym “MAIAC”?
- Page 8, Line 200: Here the subscript represents the biome?
- Page 9, Lines 208 and 211: The CO smoke emission coefficients were derived from the data shown in Figure 2, not Figure 1, correct?
- Page 9, Line 208, Table 1: You set up the naming convention for the different inventories on lines 197-199 and then abandon this naming convention in Table 1. The first column reports the coefficients to be used in the FREM_bCO inventory and the second column reports the coefficients to be used in the FREM_bTPM inventory, correct? It would be helpful if they were identified as such in the table caption and/or the table header.
- Page 9, Line 221: Presumably the “s” in FREMs_bCO indicates a small fire correction, but this has not been mentioned up until this point.
- Page 10, Line 230: Here the “s” in FREMs_bCO is first defined.
- Page 11, Line 240: Here “small fire” is abbreviated “SF”?
- Page 11, Line 244: This sentence does not make sense to me. To me, if you’re

discussing seasonal fluctuations, then the two emissions inventories would track on a monthly basis. If you're referring to interannual variability, then the two emissions inventories would capture the same large fire years and the same small fire years. Not really sure which of the two temporal scales you're trying to describe here.

- Page 11, Line 247: Something seems strange about the use of the parentheses here.
- Page 11, Lines 253-254: How are the mean hourly CO emissions calculated from GFED4.1s? Using the monthly emissions and dividing by the number of hours in a month or using the daily/3 hourly fields? Not that it matters since total CO emissions from GFED4.1s should be conserved anyway, but in fairness I think it should be disclosed that it is possible to scale the monthly GFED emissions estimates to higher temporal resolutions.
- Page 14, Figure 5: Suggest removing "emissions" from the panel titles in the figure. Also, the abbreviation should be CAR not CAF.
- Page 14, Line 304: This is DMC per unit burned area, correct?
- Page 15, Figure 6: Random "SF" for small fire again?
- Page 15, Figure 7: Random "SF" for small fire again? As a stand-alone figure caption there is no indication that this is dry matter consumed per unit burned area. Note also that between 11S and 13S and between 31E and 33E is not Angola. Figure 7 is a map of somewhere in western Zambia near to Malawi.
- Page 16, Line 320: Again, can this really be considered a validation?
- Page 16, Line 322: GFED4.1s is misspelled.
- Page 16, 342-343: This sentence states that "Emission coefficients for all gas species used were calculated through the application of Equation 1 with CO as the reference species." Which other gas species were compared between the model runs and the FREM_bCO inventory? From here on forward I've only seen CO comparisons, which would not require the use of Equation 1. Moreover, this is not a strictly true statement either as the CO emissions coefficients for the closed canopy forests are based on FEER-equivalent values, correct? Also, it would be helpful to refer back to your naming convention. This is actually a part of the FREM_bCO emissions inventory, correct? Does this also include the SEVIRI small fire correction or not?
- Page 17, Line 365, Figure 8c: How are total carbon emissions being estimated? Via DMC? This is never described in the evaluation methodology, though I'm assuming that CO emissions are being divided by the CO emission factor to yield DMC which is then converted using the carbon fraction.
- Page 18, Line 371: It seems a little late in the manuscript to start referring to Senitnel-5P as S5P. I suggest either introducing this abbreviation earlier or do not adopt it at all.
- Page 18, Line 373: Presumably you are referring to Figure 8c?
- Page 18, Line 380: It would be helpful if the figure caption stated that the mean monthly TCCO maps were generated for June, July and August using observations and model runs collected between 12:00 and 14:00 UTC.
- Page 18, Line 380: So yes, according to panels (b), CMAQ was indeed fed with the FREMs_bCO emissions inventory. It would be helpful if this was stated somewhere in the methodology. See Comment 36.