CCs - Patrick Freeborn

We thank Patrick for the time taken and his valuable comments. We have taken these onboard and incorporated the suggested changes into the manuscript. We feel that this has greatly improved the clarity and substance of the work. Green highlighted comments have been addressed in the revised manuscript.

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^x_e, 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 *x*”. 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 *x* 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, is states: “the FREM methodology derives a biome-dependent ‘smoke emission coefficient’ for a reference species *y*, [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 *y* 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.

We greatly appreciate the comments regarding nomenclature, and on reflection agree this could be clearer – although we tried to keep with "normal" convention.

The use of C_e to denote smoke emissions coefficient was used in the submitted manuscript to align with the notation adopted in the first paper to show a top-down FREM-like emissions methodology. This was that which details the "FEER" fire emissions inventory (Ichoku & Ellison, 2014), and the same nomenclature was used by us in the papers describing the FREM technique (Mota & Wooster, 2018; Nguyen & Wooster, 2020). However, we do recognise that the discussion of how other species emissions are generated via Equation 1, and references to emissions factors (which are typically written EF_x, where x is the species of interest), has perhaps made this particular notation more confusing than helpful.

We have taken onboard the comments and redefined our notation as suggested by the reviewer. We adopt the notation EC_X^b, where b is the biome and x is the reference species. The smoke mass extinction coefficient remains β_e – and this is how is it most commonly referred to in the aerosol science literature.

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 7x7 km, correct? I also think the instrument settings for TROPOMI were changed on August 6th, 2019, which reduced the along-track pixel dimensions to 5.5x7.0km. Were the different pixel sizes between 2018 and 2020 taken into account?

Thank you for these valuable comments. The method used in our manuscript in relation to the calculation of total emitted CO is very similar to that used in the calculation of fire emitted total particulate matter (TPM) in our previous papers (Mota & Wooster, 2018; Nguyen & Wooster, 2020). As such, a more detailed description was not included initially – but we recognise now that inclusion of a more explicit description of how plume total CO is calculated would be valuable – so the below has now been added -- Section 2.2 (line 146):

"As introduced in Section 1, the FREM methodology derives a biome-dependent 'smoke emission coefficient' for a reference species x $[EC_x^b]$ from the relationship between the thermal energy a fire radiates (i.e. the FRE in MJ) and the mass of the reference compound x emitted (in kg or g) over the same time period. Focusing on CO, we derived EC_{CO}^b values from a set of matchup fires for which good observations of both FRE from the fire and TCCO from its plume exist. The derived EC_{CO}^b has units of $g.MJ^{-1}$ or $g.s^{-1}.MW^{-1}$ and can be used to generate CO emission rates (or totals) for a fire when multiplied by the FRP (or FRE) estimate for that fire. Sentinal-5P TROPOMI (S5P) TCCO retrievals are available from May 2018 until the present day, and we gathered our matchup data from joint S5P TCCO, Meteosat FRP-PIXEL, and VIIRS RGB imagery covering July to December 2018 and the full year of 2020. We studied both Northern and Southern Hemisphere Africa (NHAF and SHAF), which have asynchronous fire seasons. We derived EC_{CO}^b values for the six 'fire biomes' of Africa mapped by Nguyen and Wooster (2020), with this mapping based on re-classification of a 2019 landcover dataset generated from 300 m spatial resolution MERIS and PROBA-V observations as part of the European Space Agency (ESA) Climate Change Initiative (CCI) (https://cds.climate.copernicus.eu/cdsapp#!/dataset/satellite-land-cover). To provide further biome discrimination for *woodland savanna/open forest*, we made use of percentage tree cover information (above 5 m height), taken from a 2015 map of Vegetation Continuous Fields (VCF) generated from 30 m Landsat data (https://landsat.gsfc.nasa.gov/). EC_{CO}^b values were generated for the resulting six "fire biomes" - *closed canopy forest*, *low-woodland savanna/open forest, high-woodland savanna/open forest*, *grassland*, *shrubland* and *managed land*.

Plume selection was carried out based on S5P TCCO observations ($mol.m^{-2}$) and near co-incident VIIRS imagery (Error! Reference source not found.). Each match-up fire was selected and filtered by manually defining a polygon that encapsulated the smoke plume and responsible AF pixels. There were several criteria that match-ups had to comply with in order to be included for EC_{CO}^b derivation; cloud free observations of the fire and plume throughout the lifetime of the fire, determined from the FRP-PIXEL Quality Product detailed in Wooster et al. (2015); a relatively short period since the start of the fire (mean of 3.4 hours since first AF detection) to minimise plume dispersion and thus maintain an optimal background TCCO to in-plume TCCO ratio for calculating the fire's TCCO anomaly, this temporal limit also reduced the opportunity for oxidation of CO to affect these data; no other fires being present in the area during the day/days preceding the match-up fire being identified; some wind-driven dispersion of the plume to minimise the chance of thick smoke generated by the fire covering the location of some of the AF pixels and causing the area containing FRP to be incorrectly masked as cloud. A buffer of pixels surrounding each plume was included in each manually defined polygon such that the buffer easily encapsulated the high TCCO value pixels of the plume, plus a series of pixels outside of the plume from which the 'background' CO amount relevant to each plume was calculated. Only match-up fires for which a single 'fire biome' represented more than 50% of the observed AF pixels in the fire were retained for use in EC_{CO}^{b} derivation.

For each fire in the final match-up set we calculated i) the total plume CO [g] contained in the plume from the S5P TCCO retrievals, and ii) the total FRE [MJ] released by the fire from Meteosat FRP-PIXEL data – integrating FRP from the time of the fires first AF pixel detection on that day to the moment of the S5P overpass. The minimum TCCO value (mol.m^{-2}) from each plume buffer was taken as the appropriate CO 'background' value for that plume and subtracted from all plume pixel TCCO values. The resulting 'excess' TCCO pixel values were then converted to units of grammes by multiplying by the molecular weight of CO and by the pixels' area calculated from their geographic corner coordinates (thus accounting for the change in the along-track spatial resolution of S5P data in 2019 and any view-angle dependent pixel area growth). Summing these values across all plume pixels of a fire then yielded the total amount of fire-emitted CO in that plume, which was compared to the total amount of FRE the fire generated over the time that it released that CO. Each match-up fire FRE and total CO pair constitutes one datapoint on the relevant 'fire biome' scatterplot of **Error! Reference source not found.**. FRE and Total CO uncertainties were calculated from the uncertainty values provided within the FRP-PIXEL product and the TCCO product respectively, these are also plotted in the form of error bars on Error! Reference source not found.**.**"

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?

Thank you for this comment, a more in-depth discussion of this methodological choice has been added to section 2.3 (line 198):

"Matchup data for each 'fire biome' are shown in Error! Reference source not found. and were used to derive the set of biome-dependent CO smoke emission coefficients [EC_{CO}^{b}] listed in **Error! Reference source not found.**. Zero-intercept ordinary least squares (OLS) regression was used for this, rather than the orthogonal distance regression (ODR) used by Nguyen and Wooster (2020) during derivation of TPM smoke emission coefficients [EC_{TPM}^{b}]. OLS was used in this work for two main reasons. Firstly, although the ODR method considers the uncertainty in each of the variables, these uncertainties are themselves rather poorly constrained, with the known uncertainties only representing part of the total uncertainty sources. There are contributions to the uncertainty of FRP that are not quantifiable, for example due to variations in the amount of interception of a fire's FRP signal by any overlying tree canopy. We therefore deemed use of a regression method in which the slope is strongly driven by datapoint uncertainty to be unsuitable for use. Secondly, weighting based on uncertainty often resulted in undue weight being given to high value points (e.g. match-ups with high FRE and high plume-species amounts) due to them typically having lower relative uncertainties (see Wooster et al., 2015). Due to their typically being very few high value datapoints in each 'fire biome' due to the heavy tailed nature of fire size distribution (Freeborn et al., 2009), these few large fires were potentially being too strongly weighted in the resulting calculation of EC_x^b. For these reasons, we opted to use OLS regression, and to ensure a consistent methodology for emission coefficient derivation we also applied the same approach to the Nguyen and Wooster (2020) dataset to re-derive their EC_{TPM}^b values using OLS regression (see **Error! Reference source not found.**). The updated EC_{TPM}^b for closed canopy forest, managed land, grassland, shrubland, low-woodland savanna and high-woodland savanna are 26.07 $g.MJ^{-1}$, 12.23 $g.MJ^{-1}$, 9.39 $g.MJ^{-1}$, 9.88 $g.MJ^{-1}$, 10.65 $g.MJ^{-1}$, and 14.18 $g.MJ^{-1}$ respectively. On average these new values are 14% lower than those reported in Nguyen and Wooster (2020) derived via ODR, and are the ones referred to and used hereafter. The WRF-CMAQ model-based approach to evaluating our final CO emissions rates and totals in Section **Error! Reference source not found.** was also used to carry out an analogous evaluation of the TPM emissions generated from the updated EC_{TPM}^b values of **Error! Reference source not found.** (see **Error! Reference source not found.**)."

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.

We thank the reviewer for this valuable comment, we have added a section of text discussing the cause of this smaller sample size in closed canopy forest along with a more detailed discussion demonstrating that this limitation is observed in all top-down emission inventories to date. We have also referenced the FRE contribution of closed canopy forest compared to other fire biomes, which was calculated from 6 years' worth of SEVIRI FRP data in Nguyen and Wooster (2020).

Using only the current CO match-up dataset generated in this work, delivery of a FREMs_bCO inventory without use of any EC_{TPM}^b value for the closed canopy forest biome would not be possible. However, it is not essential to use a FEER-equivalent EC_{TPM}^b. The EC_{TPM}^b for closed canopy forest shown in Appendix A could be used instead to generate TPM emissions – and then CO values derived from this using Equation 1. We selected to use a FEER-equivalent EC_{TPM}^b as opposed to this latter approach for several reasons. Firstly, since FEER was generated from 7 years' worth of MODIS FRP data (compared to the 1 year of SEVIRI FRP data used for FREM EC_{TPM}^b) it includes a greater number of individual plumes from which an emission coefficient can be derived (Ichoku and Ellison, 2014). Additionally, the updated FREM EC_{TPM}^b values for all other fire biomes of Appendix A were all within 30% of their FEER-equivalent EC_{TPM}^b values, with the exception of closed canopy forest, which was 38% higher. It is also likely that FRP measurements from polar-orbiting sensors such as MODIS may be better suited to detecting fires in closed canopy forest due to their higher spatial resolution and varying sensor view angles available at the same location. Due to tree canopy interception of surface emitted FRP preventing the full FRP signal from reaching the sensor (Roberts et al., 2018), the detection of fires in closed canopy forest is optimal at angles closer to nadir. Therefore, FRP products with a lower minimum FRP detection threshold and which sample a location at varying view angles (unlike geostationary satellites) potentially are able to capture a more representative FRP measure from fires in closed canopy forest.

An increased number fire match-ups in the closed canopy forest biome, and ideally in general in other biomes as well, is indeed necessary. At the time of this research only 2 years' worth of S5P CO observations were available, limiting the plume match-up dataset further, even beyond the limitations set by the labour-intensive manual selection methods used. Some improvements to the methods are being invested in to speed plume detection in future work. Machine learning techniques could be used for plume identification and digitisation to produce a more automated method that could be enacted far quicker on large amounts of data than the time intensive manual methods used herein. This could also potentially make the detection of different plumes more consistent and allow the overall FREM method to be efficiently extended to other geographic regions, whilst minimising the number of 'low sample size' fire biome matchups.

Additional paragraph has been added at Line 218:

"For each of the six fire biomes at least 12 matchup fires were identified for derivation of EC_{CO}^{b}, apart from for closed canopy forest. Tropical evergreen forests (the primary type of closed canopy forests in tropical regions) are generally not very susceptible to fire, except during periods of extreme drought, due to the high humidity and low windspeed within the dense forest canopy and the limited amount of surface fuel available due to rapid decomposition of surface litter in these environments (Marengo et al., 2011; Tomasella et al., 2013). Fires in such tropical forests are most often the result of human land-clearing activity and are typically small in size, unless heavy machinery is involved in land clearing (Van Leeuwen et al., 2014). Furthermore, FRP observations in closed canopy forest can be affected by tree canopy interception of surface emitted FRP (Roberts et al., 2018). These factors result in a lower number of observable and identifiable fire match-ups for tropical closed canopy forest areas. Smaller fire sizes and fewer match-up fires being acquired in closed canopy forests areas relative to other biomes was also observed during previous applications of the FREM approach (Mota & Wooster, 2018; Nguyen & Wooster 2020) and the FEER approach (Ichoku & Ellison, 2014), even when using 7 years' worth of MODIS FRP and AOD data in the latter case. In this work the ability to identify small fires in closed canopy forest is further limited by i) the spatial resolution of the S5P TCCO observations which are at least 5 times lower resolution than the 1 km AOD product used in Nguyen & Wooster (2020) and ii) the limited availability of the S5P trace gas products which only became operational form mid-2018. An increased timeseries of S5P data and the exploitation of machine learning methods such as object recognition may aid in identifying a greater number of plumes in closed canopy forest - and this the subject of ongoing work.

Due to the FEER emission inventory exploiting a far larger dataset from which to identify fire match-ups (7 years of MODIS FRP and AOD) and it obtaining many more fire match-ups in tropical closed canopy forest (Ichoku and Ellison, 2014) we instead derive EC_{CO}^{b} for closed canopy forest from the 'FEER-equivalent' value. The method used to derive this is detailed in Nguyen and Wooster (2020), and essentially involves aggregating the FEER C_{e}^{TPM} emission coefficients of Ichoku and Ellison (2014) (https://feer.gsfc.nasa.gov/data/emissions/) to the relevant fire biome. Equation 1 was then applied to obtain a FEER-equivalent EC_{CO}^{b} , which was calculated as 156.7 $g.MJ^{-1}$ for the closed canopy forest fire biome. We generated FEER-equivalent EC_{CO}^{b} for each of the other five fire biomes to compare these to our directly derived EC_{CO}^{b} values, and found agreement within ±34% (see **Error! Reference source not found.**), somewhat justifying our use of the FEER-equivalent value in the closed canopy forest biome where a directly derived EC_{CO}^{b} value was not achieved. Further, Nguyen and Wooster(2020) showed that mean monthly FRE contribution from fires in closed canopy forests does not exceed 10% of the total monthly FRE coming from African fires, and thus its total is of relatively low importance to continental-scale CO emissions totals."

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.

It is true that the results of Appendix A are not directly used in the main text, but we believe these updates are important and highly relevant to this manuscript as they use the same underlying FREM methodology. We have however adjusted the title and abstract of the manuscript to be more reflective of the content as a whole. Thank you for this suggestion.

New title:

"Biomass burning CO, PM and fuel consumption per unit burned area estimates derived across Africa using SEVIRI Fire Radiative Power (FRP), Sentinel-5P CO and MODIS AOD observations"

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”. (Figure 10) 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 ($g.m^{-2}$) 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 $g.m^{-2}$ 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 $g.m^{-2}$. 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 g m^{-2}, and (iv) ensure that all remaining figures and tables are appropriately labelled with either CO or TCCO along with their respective units.

We thank the reviewer for his comments and recognise that our use of the terms TCCO and Total CO were not as clear as they could be. We have clarified these terms and added a description of how they are calculated. We have also amended figure captions, figures, and text to more clearly describe each variable in question.

As the reviewer rightly understands, Figure 10 shows the total amount of CO contained within the full domain or ROI on a given day (observed some time between 12:00 and 14:00 by S5P and an average of the CMAQ model output between 12:00 and 14:00 UTC). We chose to calculate total CO in a given area rather than to use concentrations for several reasons. Firstly, because the CMAQ model provides the model output variable Total Column CO, this can be directly compared to S5P TCCO (as it is in Figure 9). Secondly, since the model has a set number of layers and the S5P TCCO product simply gives a column total, assumptions about the atmospheric vertical distribution of CO would otherwise be needed to calculate concentrations. Comparing CO area-aggregated totals removes this need.

Lines added at 426:

"The CMAQ model produces a TCCO output (mol.m^{-2}) that could be compared to Sentinel-5P TCCO (mol.m^{-2}) measurement data from June to August 2019. None of the S5P observations used in this comparison were those deployed in the EC_{CO}^{b} derivation of Section **Error! Reference source not found.**. Prior to the intercomparison, both model and measurement datasets were converted to units of g.m^{-2} via multiplication by the molecular mass of CO. S5P acquisitions over the model domain occur daily between 12:00 and 14:00 UTC, and the resulting TCCO retrievals were combined and compared with the mean CMAQ TCCO output from the same two-hour period."

Lines added 461:

"Area-aggregated CO totals (Gg) were calculated by multiplying both CMAQ and S5P TCCO by their 0.1° × 0.1° grid cell areas to obtain a daily summed total CO timeseries (between 12:00 and 14:00 UTC) for the full domain extent and within ROI1 and ROI2 (labelled in **Error! Reference source not found.**). These are shown in **Error! Reference source not found.**, along with direct comparisons of these daily area-aggregated estimates. These CO totals show that temporal patterns observed by S5P are well replicated by the CMAQ modelling driven by the FREM-derived CO emissions. This indicates that (i) temporal trends in active fires are being well captured in the SEVIRI FRP-PIXEL product and (ii) the meteorological fields of WRF, particularly wind, are representing the real conditions sufficiently well."

Technical Corrections:

1. Page 1, Title: Seems a bit asymmetrical to me. Suggest changing "geostationary" to "SEVIRI" or vice versa changing "Sentinal-5P TROPOMI" to "polar orbiting".
2. 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)."
3. Page 1, Line 18: Instead of "spanning 16 years" maybe state the actual range of years, 2004-2019 correct?
4. Page 1, Line 19: Suggest deleting "to derive CO emissions" from the end of the sentence.
5. Page 1, Line 20: Perhaps clarify that "per unit area" is "per unit burned area".
6. Page 1, Line 23: Here and elsewhere, I'm not sure that comparing outputs from the WRF-CMAQ chemical transport model with Sentinal-5P TROPOMI TCCO observations constitutes a "validation" of the FREM approach. It's an assessment, perhaps, but not a validation.
7. Page 2, Line 49: Suggest changing "actively burning fires" to "active fires" to coincide with the acronym "AF".

8. 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".
9. Page 2, Lines 50-51: Suggest changing "The primary advantage over GFED" to "The primary advantage of GFAS".
10. 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".
11. Page 2, Line 53: Granted the acronym FREM was defined in the abstract, but shouldn't it also be defined in body.
12. Page 3, Line 81: Maybe start a new paragraph?
13. Page 3, Line 91: Suggest changing first part of the sentence to read: "Using Equation 1 to translate between emission coefficients…"
14. Page 3, Line 93: Suggest moving "for example" to the beginning of the sentence.
15. 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.
16. Page 5, Line 144: Suggest replacing "temporally different" with "asynchronous".
17. 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.
18. Page 5, Line 153: Do you mean 50% of the observed FRP or 50% or the observed number of AF pixels?
19. 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?
20. Page 6, Line 183: What is the acronym "MAIAC"?
21. Page 8, Line 200: Here the subscript represents the biome?
22. Page 9, Lines 208 and 211: The CO smoke emission coefficients were derived from the data shown in Figure 2, not Figure 1, correct?

23. 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 inverntory, correct? It would be helpful if they were identified as such in the table caption and/or the table header.
24. 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.
25. Page 10, Line 230: Here the "s" in FREMs_bCO is first defined.
26. Page 11, Line 240: Here "small fire" is abbreviated "SF"?
27. 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.
28. Page 11, Line 247: Something seems strange about the use of the parentheses here.
29. 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.
30. Page 14, Figure 5: Suggest removing "emissions" from the panel titles in the figure. Also, the abbreviation should be CAR not CAF.
31. Page 14, Line 304: This is DMC per unit burned area, correct?
32. Page 15, Figure 6: Random "SF" for small fire again?
33. Page 15, Figure 7: Random "SF" for small fire again? As a stand-along 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.
34. Page 16, Line 320: Again, can this really be considered a validation?
35. Page 16, Line 322: GFED4.1s is misspelled.
36. 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?

The CMAQ model requires other gas and aerosol species (beyond CO and TPM) to carry out the chemical and micro-physical processes of the model. Text has been added to make this more clear and to clarify that it is the FREMs_bCO emission inventory that is used as input (which includes a small fire correction).

37. 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.
38. 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.
39. Page 18, Line 373: Presumably you are referring to Figure 8c?
40. 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.
41. 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.