Anonymous Reviewer #1

We thank this reviewer for their input and comments, which raise important and useful points, we have incorporated their suggestions and feel the manuscript has been improved as a result. Comments that have been addressed are highlighted in green.

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 trade off 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.

This is certainly a true and valid point and we have included text to make this more clear. Lines added at line 76:

"Although uncertainties in the DMC conversion step are removed in these top-down approaches, other uncertainties are introduced - primarily from uncertainties in the satellite-derived datasets, and in the case of EC^{b}_{TPM}, the use of the mass extinction coefficient, β_e, used in the conversion of AOD to TPM"

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.

Also a very good point which was perhaps not as clearly stated as it should have been, Lines added 86:

"A drawback of using geostationary AF data is that, at present, operational geostationary satellites have a lower spatial resolution than do polar-orbiting sensors, resulting in the under-detection of 'small' or low-FRP fires. This 'missing' contribution to the FRE can be accounted for using a so-called 'small fire correction' (discussed in more detail in Section 2.1)."

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.

These have been removed

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.

Thank you for this input. Lines have been added discussing in more detail this adjustment. Line 134:

"Region-specific mean 'small fire' scaling factors were derived from comparisons of coincident and co-located aggregated FRP from the MODIS and SEVIRI active fire products. These were determined to be 1.67 and 1.46 for Northern and Southern Hemisphere Africa (NHAF and SHAF) respectively, and following Mota and Wooster (2018) these factors were applied to account for the average amount of FRP coming from fires burning below the SEVIRI sensor's minimum FRP detection limit."

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.

Thank you for this input, this choice could have been explained in more detail and text has been added to discuss the reasoning for this methodological choice. 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.**)."

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.

This is a very helpful and valid line of questioning. A discussion of this issue has now been added at Line 219:

"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."

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?

This has been included to demonstrate the difference in the predicted CO emissions that result from using a different reference species. This difference results from the combined contribution of the top-down derivation, which uses a different reference species satellite product (MODIS MCD19A2 AOD), and the application of Equation 1 which uses emission factors which are themselves not well constrained. Material has been added to the main text and caption to highlight purpose of this 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?

This is an interesting point and we thank the reviewer for noting this, we have added lines discussion the proposed reasons for this. Lines 319:

"However, as Mota and Wooster (2018) and Nguyen and Wooster (2020) noted for TPM emissions, the FREM methodology often predicts a slightly earlier peak in annual emissions in SHAF compared to GFED. This shifted peak agrees with findings showing that polar-orbiting based FRP measures also seem to peak in SHAF a month or so earlier than do BA measures. This appears, for example, in the work of Zheng et al. (2018) who compared GFED BA with GFAS FRP and who suggest that measured CO emissions actually lag BA derived CO emissions in Africa, based on MOPITT CO observations. They attribute this lag to a shift from flaming to smoldering combustion over the continent which is not accounted in the emissions factors applied in the GFED calculations."

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.

Indeed, the ROIs would be more valuable here and have been added to Figure 9. Site names have been retained for ease of comparisons between the CO and AOD spatial distribution results.

Technical/Grammatical Comments:

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

We feel this is not inappropriate as the technical development of this product is currently underway, however this has been re-worded.

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