Interactive comment on **"Global top-down smoke aerosol emissions estimation using satellite fire radiative power measurements"** by **C. Ichoku and L. Ellison**

Charles.Ichoku@nasa.gov

Detailed Responses to the comments of Anonymous Referee #1:

The study builds on the work by Ichoku and Kaufman (2005), introducing a new revised methodology to estimate global biomass burning emissions using a top-down approach based primarily on MODIS environmental satellite data. The author's reformulation of the previous methodology is clear, and the potential improvement was demonstrated throughout the manuscript. However, there are several limitations that must be addressed in order to make it appropriate for publication. There is a tendency to highlight the advantages of the current methodology (specially compared to bottom-up approaches) while minimizing, or in some ways neglecting, the limitations still involved in the approach. This is reason for concern as it could mislead less informed readers. Provided the study is properly phrased, the authors can maintain its advantages over the previous implementation while making the readers fully aware of the outstanding issues still plaguing the calculation of emission coefficients. Below the authors will find the list of specific comments:

Authors: We thank the Reviewer for this very objective assessment of the content and quality of our manuscript. We very highly appreciate his/her time and effort in reviewing our manuscript in detail. We have given serious consideration to each of the comments and have revised our manuscript accordingly. His/her contributions have certainly helped improve the quality of the manuscript, and we are very grateful for everything.

Page 27238 line 27: Should add range of Ce for Russian boreal forest as reported for other regions.

Authors: We have only one Russian boreal forest region, which does not allow us to report a range. Fortunately, the value for this Russian boreal forest region (24 g/MJ) conveniently falls within the range of the other Russian land-cover types (18–26 g/MJ), and we have combined all Russian regions into this latter range, and modified the relevant part of the phrase as follows: "…, and 18–26 g/MJ for the Russian boreal forest, croplands and natural vegetation".

Page 27239 lines 2-9: Consider breaking this one statement into separate sentences. A few other parts of the manuscript also require attention with the use of excessively long statements.

Authors: That sentence is now broken into two coherent sentences in a clean way.

Pages 27239-27332 (Introduction): Please incorporate additional citations – several sentences go without proper referencing (e.g. Pg 27239 line 19->20, Pg 27330 lines 2, 14, 24, among others)

Authors: We have included more citations where indicated as well as in other parts of the section where we found it necessary to do so.

Page 27330 line 3: replace "several decades" with "several years"

Authors: Since some of the earlier works on emission estimates include that of *Seiler and Crutzen* 1980, published 34 years ago, "several decades" seems appropriate here, and we have maintained it and added that citation to substantiate the use of this term.

Page 27330 line 9: must cite Kaufman et al. 1998 (already in reference list) and Wooster, Zhukov, and Oertel (2003; doi: 10.1016/S0034-4257(03)00070-1) as two of the first studies to introduce the FRP concept and it's relationship with biomass combustion.

Authors: We have now cited these recommended papers as well as several others that represent a cross-section of the satellite contributions to "biomass burning characterization and emissions estimation", of which Ichoku et al., 2012 is a review paper that includes very many more relevant references.

Page 27331 line 8: Replace "some serious adjustment" with "a bias correction"

Authors: Done. Thanks, 'bias correction' sounds better.

Page 27332 lines 8-10: This statement should be moved to the end of the previous paragraph (remove line separation). Then, start new paragraph with sentence "Section 2 provides…" (lines 10-20).

Authors: This has been done.

Page 27333 line 14: Replace "satellite measurements of fire and smoke" with "satellite measurements of fire energetics and smoke"

Authors: Yes, this sounds better.

Page 27333 lines 22-23: Replace "That Wooster et al. (2005) study indicated that…" with "Wooster et al. (2005) indicated that…"

Authors: The first "That" has been deleted.

Page 27335 line 1: Delete "when referring to MODIS data"

Authors: Deleted.

Page 27335 line 4: Use "Active fire observation products from MODIS..."

Authors: "Active" has been added.

Page 27335 line 10: The unit (MW) in this case should read W/mˆ2, unless you are assuming a 1kmˆ2 pixel area (the next paragraph eq. 3 is linked to Collection 4, which used W/mˆ2). Please rectify or else clarify the statement.

Authors: This has been corrected.

Page 27335 line 13: When referring to the MODIS Collection, I suggest you simplify it to "4" or "5" throughout the manuscript. The use of the two extra digits "00" is less commonly found in the literature (chances are the products won't evolve beyond the single digit versions anyway).

Authors: The leading "00" has been removed everywhere.

Page 27335 line 17: "data" is plural here and elsewhere whenever applicable. Found several sentences with incorrect use throughout the manuscript.

Authors: This has been corrected throughout the manuscript.

Page 27336 lines 15-17: I suggest adding a brief description of the main difference in overall global performance affecting the current study.

Authors: Since that is the main subject of Section 4.6, at which the analysis will be performed and results reported, we feel it would be premature to report the result in this Data section. Instead, we have added the following sentence: "Detailed analyses of the effects of this change in AOT data version and other factors on the computed C_e results are presented in section 4.6."

Page 27336 line 27: I suggest adding a brief explanation why you chose to work with the 850mb data and then point to Ichoku and Kaufman (2005).

Authors: We have explained that 850 mb height corresponds to the typical range of plume injection heights of a majority of the fires observed from satellite at ~1-km spatial resolution, with the exception of very large fires. We have cited several relevant references.

Page 27337 line 21: Replace "have had to be made" with "were required"

Authors: Replaced.

Page 27340 line 5: Add "... mass extinction efficiency derived from Reid et al. (2005)".

Authors: Added.

Page 27340 lines 8-11: It would be informative to have a histogram plot of T or at the very least some statistics describing the range of T values encountered. This will help readers interpret your results. For example, I assume the greater T gets the less representative your instantaneous FRP retrievals will become (basically the bulk of the plume will have originated several minutes earlier and therefore under different energy release and biomass consumption rates compared to what the instantaneous FRP retrieval provides).

Authors: We agree that this is one of the main issues with our method, although there is no simple way of avoiding this issue, since both the instantaneous FRP and the aerosol plume are observed from the same platform at the same time. However, we have addressed this issue in Section 4.2, and included a statement of related improvements needed in future versions. Also, as suggested by the reviewer, we have added a new figure showing the probability density functions (PDFs) of T for the main (11300) and supplementary (00000, 10000, 11000) filter configurations used in data selection, as follows:
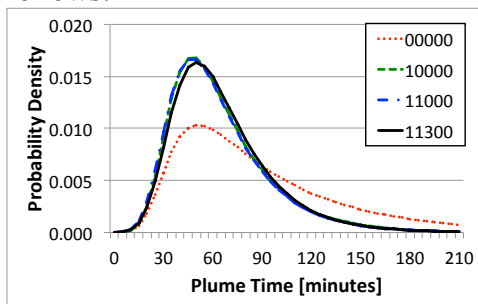
Page 27340 lines 16-18: How about those cases when active fires are also found within the downwind AOT pixels (or within upwind AOT pixels with smoke advecting over the fire pixel itself), how is the fire-emitted AOT calculated in those cases? With 10x10 km cells and a 3x3 sampling window I would expect to see many such cases in areas of high fire activity where multiple land-use fires co-exist in a relatively small area (e.g., Africa, Amazonia, South East Asia).

Authors: This is something that we had to seriously consider in deriving this algorithm. We initially tried to eliminate those cases, as can be observed in the F_pcounts* settings in Table 2 (refer to Table 1 for the definitions of "F_pcounts", etc.), and found that the remaining available data was significantly low (Table 3), but the measure did not provide any significant improvement. Therefore, we settled for an alternative method in dealing with the contamination. The new method (described in the preceding section 4.2) takes

the background AOT value as the average value of the upwind pixels instead of the minimum value. It is a more robust method of accounting for external fires as it deals not just with fires in neighboring pixels, but also with fires outside of the 3x3 scene. Admittedly though, this method does not deal with added smoke from downwind pixels beyond the central pixel. The "F_pcounts_DW3" category was created to deal with this situation, but again from Table 3 (setting 11302), we were very uncomfortable with using only 3% of the total available data, which hardly seemed to cover much of the globe at all and did not offer much improvement in r^2 values. Using the setting one level before that (11300) gives a much better percentage of data (10%), which we feel is sufficient to use (see the coverage for this setting in Figure 6.) However, we could reconsider incorporating the 11302 setting for a future version of this product. We have added a fairly extensive paragraph in section 4.2 to discuss the potential impacts of this uncertainty.

Page 27341 lines 5-25 (ending on next page line 5): There is no real need for this brief intro to the following sections. It provides superficial information only and could serve as a distraction to the reader (to me it looks like the authors are saying "if you don't want to read all the details here is a brief summary for you"). This is a recipe for disaster as it could lead to misinformed users of your product.

Authors: This segment is not a summary of what is discussed in Sections 4.3 and 4.4. The entire Section 4.1 lays out in a coherent manner the basic principles (with associated equations) employed in deriving the Ce product. Then, Sections 4.2, 4.3, and 4.4 discuss the practical implementation details of the different stages (with associated peculiarities of the specific data used and unique handling requirements), without needing to explain the basic principles behind each stage again. If this segment of Section 4.1 is removed per the Reviewer's comment, the reader may not comprehend the basis of these different stages. On the other hand, trying to incorporate this segment within the respective subsections they represent might compromise the coherence of the basic principles in Section 4.1, with the equations currently coordinated therein distributed into different sections.

However, we have modified this segment to some extent to make it look less like just a summary of the subsections that follow.

Page 27342 line 17: High latitude plumes tend to be large and well-defined. Once again I wonder what might happen in Africa, Amazonia, and South East Asia where the bulk of biomass burning occurs, and where multiple fires can be found within close proximity. Please elaborate adding more information to the readers.

Authors: This is definitely a valid point, as we mentioned on Page 27343 line 26 and following. Due to the fact that this part of the analysis is very labor intensive, involving visual inspection for each case, we had to focus our attention on one test region, Siberia, which has a fair mix of large and small fires, to obtain a representative sample. We observed that there were fires in surrounding pixels 64% of the time, and have now indicated so in the manuscript. Thus, our algorithm evaluation against MISR data as presented in section 4.2 is already based on many cases where fires are in close

proximity. However, as indicated in our previous related response above, future versions of our product will explore improvement of this aspect of our analysis.

Page 27342 line 20: Should it read "... measure plume heights (MISR) ..." ?

Authors: The statement has been much clarified.

Page 27342 line 25: The assumption is correct, i.e., that the proportion of cells contaminated by background smoke or haze is regionally dependent. I would add that such proportion should be higher in the same global hot spots mentioned above (Africa, Amazonia, South East Asia). The authors should make that clear and also discuss the implications involving the application of the current methodology to those regions.

Authors: This has been clarified in the text, by indicating that regions with typically smaller fires, such as the African savannas, are likely to be more impacted. This statement is placed in such a way that it is followed up by the mitigation measures taken to limit such contamination.

Page 27343 lines 1-2: Should this statement read "we augmented (or improved) the sampling of AOT representing the plume to include the four downwind pixels, ...". The original sentence ("we restricted the sampling...") doesn't seem to properly describe the direction of change from the previous version of the method, which used the maximum AOT retrieval only. Basically you relaxed (as opposed to restricting) the method in order to include a larger sampling of the plume's AOT.

Authors: Thanks. The Referee is right. We have rectified the statement accordingly.

Page 27343 line 9: In other words, the majority of the retrievals were in areas of widespread burning, where background smoke was detected and therefore subtracted from the fire's AOT. This result highlights the need for improved description of the multiple-fire scenario in both upwind/downwind cells (see comment above).

Authors: We have addressed this topic in response to previous comments above, but have also provided further elaboration in the manuscript on how we have improved the sampling approach relative to Ichoku and Kaufman (2005) for the selection of upwind and downwind pixels rather than the use of minimum and maximum values from the 3x3 aerosol-pixel matrix.

Page 27343 line 16-17: What about wind speed used to estimate the rate of smoke release (eq.s 11/12)? Isn't that also a requirement?

Authors: The main objective at the analysis stage described here is the correct identification of the downwind quadrant. Wind speed becomes important at the stage of estimating the emission rates, and thresholds are applied to the wind speed (as indicated in Table 2 and described in section 4.3) to avoid ambiguity. We have endeavored to use one of the most optimal global wind data available. MERRA reanalysis wind data and

other meteorological variables are global and very widely used by the research community, and we considered it appropriate to use these data in our research. In future versions, we plan to always continue to find and use the most optimal global wind data available.

Page 27344 line 1: Delete "certain"

Authors: Deleted.

Page 27344 line 2: Can you list a few examples where much lower/higher injection heights are found?

Authors: Examples have been cited, with appropriate references.

Page 27344 lines 13-14: Wouldn't it be more appropriate to consider the distance to the center of the downwind pixels instead? Statistically speaking that should better represent (on average) the location of the plumes detected by the AOT product. I am just wondering whether using the distance to the outer edge of the 3x3 window could add any bias to your estimates. Did you estimate the average plume location within the 10km AOT cell by looking at the available 250m RGB data or MISR plume data? What does that show?

Authors: The plume length is based on the distance between the location of the 1-km fire pixel and the outer edge of the downwind aerosol pixel. Since the AOT value includes the aerosol within the entire 10-km downwind aerosol pixel, limiting the distance to the center of that pixel will fail to capture the entire plume within the boundaries of the 3x3-pixel configuration analyzed. Therefore, the method used is optimal. The location of the 1-km fire pixel within the central 10-km aerosol pixel determines the location of the plume source.

Page 27345 line 13: I believe Fig 3 should be numbered "2" and vice-versa. The two are out of order the way they appear in the manuscript.

Authors: They are not out of order. Fig 2 was first mentioned on Page 27342 line 1.

Page 27347 line 25: So is the global emissions analysis built on 10% of the available data? I am concerned this could describe a highly skewed sample. This is worrisome. I think the authors need to elaborate further on this and address potential limitations more clearly. This is the only sentence I could find in the manuscript stating such limitation with the analyses.

Authors: The fact that a preliminary analysis shows that ~10% of the available data would be used for deriving the emissions is because the rest of the data fails to meet the requirements that have been specified for robustness. For instance, to avoid ambiguity, cases where wind speed was ≤2 m/s or where any of the 3x3 aerosol pixels did not have aerosol retrieval were filtered out. These precautions have only resulted in using a sample

set that is much smaller than the population size, but do not make the analysis skewed in any direction. In fact, since the data analyzed are daily observations from two satellite sensors (MODIS on Terra and Aqua) over an 8-year period, even 10% is still ample data for the required purpose, given that only 12 days of observations (first day of each month in 2010) from Aqua-MODIS alone amounted to 43,211 data points (Page 27347 lines 13–15).

Page 27348 line 1: "Using all available data" in reality means "the 10% of valid data", correct? Please clarify.

Authors: This has been clarified to: "using all available MODIS data for the period of 2003–2010 after filtering as described in section 4.3".

Page 27351 lines 16-17: This is a highly subjective decision and a questionable one as you are admittedly replacing a first (and potentially higher confidence) retrieval with a lower confidence one in order to increase your overall r^2. It's like being right for the wrong reason. This kind of decision must be properly discussed making the potential consequences clear to the reader. Please elaborate.

Authors: We acknowledge that gap filling of data by ways other than the most accurate observations is not ideal, but sometimes needs to be done where such observations do not exist. In our case, we use observations albeit with lower confidence, and have employed what we consider a reasonable logic to determine optimal values to use. We have rationally assigned quality flags (QA) to help the user determine whether or not they need to filter out the lower confidence data depending on their application. This is discussed in detail following the gap-filling algorithm. Any further elaboration would lengthen the discussion without necessarily providing more helpful information than is already there. However, we have added the clause "In order to account for cases of low data availability…" to make it clear that our motivation for this procedure is not to get higher r^2 values, but to ensure that our Ce values are based on a sufficient number of data points.

Page 27352 line 5: The dominant land cover may not describe the actual fire-prone classes although in the next sentence the dominant land cover type is used anyway. This needs some further consideration and or justification.

Authors: The next sentence actually clarifies that the higher-resolution (1-arc minute) land-cover data were used to determine the dominant "**fire-prone** land cover type, which is used in the following analysis." (see Page 27352 line 8).

Page 27352 line 10: A 15x15 degree (?) window is a huge area to sample/extrapolate. Chances are you are mixing different fire regimes as opposed to "identical fire-prone land cover type". More clarification and or warning flags are required so that the reader is fully aware of the limitations involved with the current methodology.

Authors: We acknowledge that our description about the 15x15 window size was confusing, as explained in response to the next comment.

Page 27352 line 17-19: I don't understand why the sample window is configured in such a way, decreasing from the initial 15x15 to 3x3 size. It would make more sense to start with a small sample window (3x3), and gradually grow it (to a max of 15x15) until you reach the minimum number of valid data. Please clarify. Also, the eight cell QA>QAmin requirement seems to conflict with the minimum 3x3 sampling window size. You have a total of eight cells around your target cell (the one to be filled), right? Assuming one cell provides your minimum QA value, there can only be seven more cells to meet the QA>QAmin requirement (also assuming the entire window sample is composed of the same dominant land cover type). Am I missing something?

Authors: Please accept our apologies for the confusing way we described this. The confusion was in our explanation of QAmin selection, which was first done using the 15x15 window, and then the sampling starts at 3x3 and goes outward. We have reworded the paragraph so that it is clear that we begin with a 3x3 window, and expand out to 15x15 *only* as necessary.

Page 27352 line 28: Fig 7 itself does not provide enough information to the reader. For example, what is the percentage of cells with QA 0,1,2,3,4? A graph (or table) showing QA levels on one axis (rows) and the r^2 on the other axis (columns) and the percentage/frequency describing each data point (similar to Table 3) would help. For example, for QA 0 x% shows an r^2 between 0-0.2, y% shows an r^2 between 0.2-0.4, and so on for the entire r^2 range and QA classes.

Authors: We have now added a table, exactly as suggested by the Reviewer, in the manuscript. Thus:

QA	r^2					TOTAL
	0.7-1	0.5-0.7	0.3-0.5	0-0.3	N/A	
4	1745	0	0	0	-	1745
3	0	1646	773	182	-	2601
2	548	420	321	155	-	1444
1	378	395	291	119	-	1183
0	243	470	921	1556	3756	6946
TOTAL	2914	2931	2306	2012	3756	13919

Page 27353 line 4: Replace "rough" with "low confidence"

Authors: Thanks, sounds better.

Page 27353 line 9: I assume "them" refers to FEER.v1 gridded product? Please rephrase.

Authors: "them" has been replaced with "the new FEER.v1 C_e data"

Page 27353 line 11: I think the authors should organize this section better. You start the first sentence suggesting that a quantitative "evaluation" is required to determine the product's suitability for application. However, you begin with a comparison with the previous implementation of your methodology. That serves no "evaluation" purpose as IK05 builds on a similar methodology and does not constitute a validation data set. Please consider rephrasing.

Authors: Since there is no equivalent "ground truth" to validate the new FEER.v1 C_e product, our aim here is to determine where the values of this product stand in the spectrum of existing comparable products/parameters. The only such existing product happens to be that of IK05. We still consider that what we have done constitutes a type of "evaluation" of our FEER.v1 C_e product, although not in the strict sense. Therefore, the Reviewer is right that "quantitative evaluation" is an overstatement for what we have done. Thus, we have modified the title to "Relative Evaluation …", which is more realistic, and have done some rewording in the text to reflect this.

Page 27353 line 13: The authors must properly introduce the reader to IK05. The text implies that the reader has seen it and is familiar with the previous study, when that may not be true.

Authors: Ichoku and Kaufman (2005) was referenced multiple times prior to this point in the manuscript, especially while the methodology in this study was being described as building upon that one. Here, we have introduced "IK05" as a short form of reference for that paper. We indicated that the values we are referencing are in Table II of IK05 and repeated the same values in Table 4 of the current manuscript. However, we have now changed the placement of Table II in this sentence to show that it actually refers to the IK05 paper.

Page 27353 line 21: Add "with the exception of East Kazakhstan where they are practically equal".

Authors: Done.

Page 27354 line 18: Add "In other words,..."

Authors: Done.

Page 27355 line 17-19: Please be more specific: MODIS data should be available for longer periods (as demonstrated in the current study) and so should the NCEP re-analysis data.

Authors: We simply took the output from the data analysis used for the IK05 paper, which only covered a short time period (essentially Jun–Dec 2002), as listed in the manuscript. Although some of the old MODIS data sources and versions still exist, we did not take the extended effort and time to find all available MODIS Collection 4 data

and run them through the original IK05 code to generate an output spanning a longer time period, which would not be of much benefit. We have now reworded the phrase to be clearer.

Page 27356 line 6: replace "transitions" with "transition"

Authors: Done.

Page 27356 line 25: I am having trouble understanding why the MODIS FRP in collections 4 and 5 would cause such an impact when they are essentially the same (the major exception being the pixel area multiplication). Could the authors have incorrectly used that data set with IK05?

Authors: The reason is because MODIS Collection 4 FRP was not multiplied by pixel area and was used in IK05, whereas Collection 5 was multiplied by fire-pixel area. That made the difference. We have now inserted "(i.e. without and with multiplication by fire-pixel area, respectively)" right after "… from Collection 4 to 5 …" to make it clearer.

Page 27357 line 3: I am assuming "fire-generated" and "fire-emitted" are the same. Please use one definition only.

Authors: Harmonized to "fire-emitted" throughout the manuscript.

Page 27357 line 5: Should it be "IK" to conform to Table 5?

Authors: The "IK" in the first row of headings of Table 5 simply means that both "IK05" and "IKu" are represented in the columns beneath. The "IKu to FEER.v1" on Page 27357 line 5 is still valid because that is what is plotted in Fig. 8b.

Page 27357 lines 8-9: Attributable to what algorithm change? Please cite the appropriate study which documents the mean global increase observed by the AOT product's science team going from Collection 4 to 5.

Authors: We are very thankful to the Reviewer for pointing this out, as we acknowledge that our description in the text sounded confusing. We were only referring to the algorithm for calculating the fire-emitted AOT, which we have now specifically identified with τ^f_{a550} in the text. It is now easier to follow.

Page 27357 lines 9: Are you suggesting there was a two fold increase in AOT values between MODIS collections 4 and 5? Please clarify.

Authors: No. We have explained this in response to the previous question.

Page 27357 line 11: "algorithmic changes relating to AOT" may at first be interpreted as MODIS AOT algorithm changes. Please clarify.

Authors: You are right. We have now clarified it.

Page 27358 line 17: I am wondering what effect might result from using monthly average FRP values without proper consideration of diurnal variations in fire activity. Errors could be large. A few MODIS FRP data points (assuming multiple detections by Terra and Aqua for the same fire) may not provide a good sample to derive the time-integrated FRE for a fire. Chances are it won't work any better for large regional samples. Please elaborate.

Authors: We have inserted the following statement in the text, highlighting the likelihood of large uncertainty in the GFAS.v1 FRE, and providing justification for why we used the data: "Such derivation of monthly average FRE based on only four or less MODIS fire observations a day (from Terra and Aqua satellites) cannot capture the fire diurnal cycle, thereby resulting in high uncertainty. However, that is currently the only feasible way to obtain FRE globally. Higher frequency (sub-hourly) observations from a few available geostationary satellite sensors that measure FRP have different characteristics and produce an average of 17%–38% underestimation relative to MODIS (Roberts et al., 2005; Xu et al., 2010). Moreover, a combination of these geostationary FRP data still does not provide global coverage, as some large biomass-burning regions, including Siberia, Central Asia, and India, are left uncovered (Zhang et al., 2012). Since the GFASv1.0-based FRE data are global, publically available, and being used in the European Union's Monitoring atmospheric composition & climate (MACC) project (http://www.gmes-atmosphere.eu/about/project_structure/input_data/d_fire/), they were considered appropriate for use in deriving emissions using the FEER.v1 C_e product to enable comparison with existing emissions inventories, as described below."

Page 27359 line 26-27: This should be of no surprise as all products are – either directly or indirectly - governed by input MODIS fire counts. Please make that clear to the reader.

Authors: Thanks. We have added the following sentence: "This may be due at least in part to the fact that all products are influenced by MODIS fire-pixel counts, either directly or indirectly."

Page 27360 line 26: Add "FEER.v1"

Authors: Sure, thanks. Corrected.

Page 27361 line 16: Use "burned biomass estimates to calculate emissions"

Authors: Substituted.

Pages 27361-27365: A number of previously defined acronyms are described in full again. Please avoid that.

Authors: Duplicate definitions have been removed.

Page 27361 line 17: Add "used to multiply time-integrated satellite measurements of fire radiative power ...". Must highlight the challenges associated with retrieving representative daily FRE totals. Makes it sound artificially simple.

Authors: Replaced "direct" with "time-integrated".

Page 27361 line 25: Not necessarily true - 10% of usable input data reported by the authors and the need to fill in grid cells using reference land cover products indicate otherwise.

Authors: This issue was addressed under a previous question.

Page 27362 lines 15-16: "there are still possible sources of uncertainty" is clearly an understatement. Several limitations still exist, many of those were highlighted above and should be properly acknowledged here and elsewhere in the manuscript to avoid misleading readers/users.

Authors: Statement has been adjusted to "…the FEER.v1 global gridded C_e product may still contain several limitations and uncertainties…"

Page 27362 line 18: Add "uncertainties in the satellite retrievals of AOT and FRP (including the time-integration methods to derive the required FRE data), ..."

Authors: We do not need to add the parenthesized statement because the presented list of sources of uncertainty applies to the FEER.v1 global gridded C_e product, which does not involve time-integration of FRP data. FRP is time-integrated only when required to multiply with the C_e product in order to derive total emissions for a given time period.

Table 4: Add a label (e.g., "Region Average") to identify the corresponding bold values in the table

Authors: We have deleted the bold values, as they are not really region averages but only averages of values from similar ecosystems, which may not even belong to the same region, as in the case of the Boreal forests that includes North American and Russian regions.

Table 5: Should it read "... deriving the predicted changes in Ce from IK to FEER.v1 according to ..."?

Authors: No. The following explanation has been added to the caption: "The "IK => FEER.v1" on the first header row indicates that both the IKu to FEER.v1 and IK05 to FEER.v1 transitions are included in the columns beneath"