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Comment on acp-2022-120

Anonymous Referee #1

Referee comment on "Optimizing 4 years of CO_2 biospheric fluxes from OCO-2 and in situ data in TM5: fire emissions from GFED and inferred from MOPITT CO data" by Hélène Peiro et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-120-RC2, 2022

The authors designed a two-step inversion approach to study the impact of fires on the inversion estimation of net carbon sources and sinks. In the first step, CO fire emissions are constrained with MOPITT CO data, and these optimized emissions are translated into CO2 emissions using biome specific emission factors. Subsequently, these optimized CO2 fire emissions are used as input in a second inversion step - alongside a rebalanced prior NEE that fits the global atmospheric growth rate - to constrain regional NEE with OCO-2 and in situ CO2 data. Despite much information is provided and carefully investigated, I do have a number of concerns about the results presented. The main text also requires additional editing and a thorough proofread to improve the general readability. In particular, I would like to see a shortened results section with a focus on the main findings and a smaller number of figures. The lengthy descriptions in each section distracts from the main points of this original work. Perhaps some of detailed descriptions, figures and comparisons can be moved to a supplementary document. The points below must be addressed before the paper can be accepted for publication in ACP.

Main concerns:

1. Reported optimized fire CO emissions:

After reading the first part of the paper, a number of things struck me regarding the CO emissions. Figure 5 provides an overview of the prior (GFED4s) and posterior CO emissions. I find it curious why the Northern Tropical and Southern Tropical African fires are scaled down so much. This contradicts other recent studies of African fires. Zheng et al. (2018), which you have included in the reference list, did a similar kind of inversion for the African continent with MOPITT CO data. For the period 2005-2016, they found more or less similar emissions for Northern Africa compared to GFED4s. But for southern Africa, they found that GFED4s underestimated the emissions by about 62%. Your results show the opposite; GFED4s overestimate emissions for Africa. Of course, there are differences in the time period studied between the two papers, but nevertheless it looks surprising. I think that this difference in outcome should be noted in the paper and, if possible, also

explained. Is this African signal driven purely by MOPITT CO? If so, can you demonstrate this by showing world maps with annual mean concentrations of prior CO (from GFED4s), posterior CO (from optimized fires) and MOPITT CO? And how does the seasonal cycle of these fires look like for Northern and Southern Africa? This can be shown with a figure similar to Figure A4. And finally, is there independent data that support your results? The 'lack' of fire emissions in Africa could explain the appearance of the large compensating natural CO2 source in Northern Tropical Africa in your OCO-2 inversions (Figure 11).

In addition, I would like to inform the authors of a recent PNAS paper by Ramo et al. (2021) which shows with new 20-m burned area data from Sentinel-2 that the GFED4s emissions for Africa are probably greatly underestimated.

Ramo et al. (2021), https://doi.org/10.1073/pnas.2011160118

A similar contradiction arises in Indonesia. In Figure 5a, strangely enough, lower posterior emissions are reported for the different fire types during the intense El Nino fires of 2015. This is the opposite of what Nechita-Banda et al. (2018) found in their MOPITT CO inversion study. They found emissions up to 120 Tg CO, about 1.5 times greater than what GFED4s provided. This difference should also be addressed and explained in your paper. Besides that, it also contradicts with what is shown in the lowest panel of Figure 9 of your paper. The blue (GFED4re) and red (MOoptre) bars are (almost) identical for 2015 (and the other 3 years). And finally, this result contradicts with the statement at line 694: "the fire estimated from MOPITT CO emissions are stronger than with GFED4 emissions". Again, according to Figure 5a, MOPITT CO emissions are actually smaller.

Given these two major inconsistencies, I wonder how much we can trust the results of the fire inversions for the other regions. For example, the strong reduction in fire emissions in the boreal regions is also striking and is somewhat inconsistent with the record-breaking fires we have seen in recent years in these regions.

I also noticed that the breakdown of fire emissions in Figure 5 does not agree at all with the reported CO emissions by GFED4s. As an example, if I look at Southern Tropical Asia in panel 5a, the breakdown between GFED4s agricultural, deforestation, savanna, and peat fires is roughly: 5, 38, 25 and 22 Tg CO/yr, respectively. However, if I look at the published GFED4s emission tables for 2015 for the EQAS region (roughly similar to your STA region) I get a completely different set of emissions per fire type: 0.7, 31, 3.8 and 74.2 Tg CO/yr, respectively. So in words, GFED4s reports a much larger source of CO from peat fires and a smaller source of CO from savanna/grasslands than you. Therefore, please carefully check Figure 5 for possible errors for all the regions. Similar issues also apply to Fig. A3. The contributions from the different fire types do not agree with the reported values in Fig. 5.

GFED4s tables: https://www.geo.vu.nl/~gwerf/GFED/GFED4/tables/GFED4.1s_CO.txt

To better understand the reported posterior CO fire emissions it would also be helpful if the TCCON comparisons for CO are provided to the reader (in a supplementary document).

Finally, I would like to reiterate Meinrat Andreae's comments that the emission factors should indeed be replaced by the updated ones reported in Andreae (2019).

2. Role of fire emissions on the optimized NEE

How can we benefit from this more tedious 2-step inversion approach? The answer is somewhat lost in the lengthy description of the results. If I simply look at Figures 11, 12 and 13, the differences in NEE between the different inversion experiments (with the same data constraints) are small. The largest differences originate from using either OCO-2 or in situ CO2 data for reasons discussed in the paper (like differences in data coverage). However, with either data constraint, the optimized CO2 fire emissions seem to have a small impact on the NEE in comparison to the unoptimized CO2 fire emissions. As described in the abstract, both MOPITT derived fires and GFED4s fires provide larger net sources in the tropics.What specifically did we learn from the MOPITT experiment? Also the comparison with independent TCCON data in Table 6 does not show significant differences in comparison to the other inversions if you use the optimized fire emissions in a NEE inversion. A more in depth discussion about the wider application of this methodology is necessary and should be mentioned in the abstract.

3. Readability of the paper

The paper would benefit from some additional editing. Mainly to correct and shorten the long (and sometimes awkward) sentences and to trim down some of the lengthy descriptions. In particular, the Results section should focus more on the main results of the paper and be presented in a more concise and logical way. Now I find it difficult to quickly get the gist of the paper in the whole list of comparisons between the inversions and between the different data constraints used. The fact that it is a two-step inversion makes that even more difficult. The posterior of the first inversion becomes the prior of the second inversion, and that blurs the distinction between ‘prior’ and ‘posterior’ labels. There is so much work in this paper that I wonder if it wouldn’t be better to split the paper into two separate papers: one for the estimation of fire emissions with MOPITT CO constraints, and a follow-up study on the impact on NEE with OCO-2 and in situ data, but that's something up the authors to decide. You can perhaps choose to move some the detailed comparisons to a supplementary document where the reader can find additional information about the main results.

I also noticed at lot of inconsistencies between labels reported in the caption, in the figures and elsewhere in the tables and the main text. For example, I came across multiple names for the same inversion type: CMS-GFED3 (in Table 5), CMS (e.g. in Fig. 12) and OCOcms (in Fig. 5a). Another example: MOPITTopt (e.g. line 455), MOoptre (e.g. Fig. A4), and MOre (e.g. Table 4). I find this distracting and should be fixed in the revised manuscript.

Sometimes you interchangeably use the inversion name (e.g. MOre) when you refer to a flux and vice versa. For example at line 454 you write: "The prior categories shown are fire, NEE and net fluxes for the prior GFED4, GFED3, MOPITTopt and CMS-GFED3".

If you follow your own Table 5, this should change to: "The prior categories shown are fire and NEE for GFED4re, GFED3re, MOre and CMS-GFED3".

I also believe the number of figures should be reduced (currently 14 excluding the Appendix figures). You can merge a number of figures into a single figure. For example, Figure 8 and 9 should be combined into 5 squared panels. That provides an overview of all emission estimates on a single page. Similarly, Figures 11, 12 and 13 can be combined into a single page filling figure with 8 panels.

Finally, shortening of the region labels with No and So in the figure titles is often not necessary, is confusing, and it doesn't look very pretty. If there is enough space write the labels in full. For example, So Trop So America should become Southern Tropical South America.

In the specific comments below I give more examples of long sentences, wrong usage of inversion names, and other specific errors.

Specific comments:

Line 6. This part is not clear. In the end are the CO2 NEE and ocean fluxes optimized with OCO-2 and insitu data or only NEE? This should be stated very clearly in the abstract.

Suggestion:

These optimized CO2 fire emissions (FIREMo) are used to re-balance the Net Ecosystem Exchange (NEEmo) and respiration (Rmo) with the global CO2 growth rate. Subsequently, in a second step, these rebalanced fluxes are used as priors for an inversion to derive the NEE and ocean fluxes constrained either by the Orbiting Carbon Observatory 2 (OCO-2) v9 or by in situ CO2 data.

Line 11. Be consistent throughout the paper with labels. Use either CASA-GFED3 or GFED3.

Line 12. "Results show…" Unclear what this sentence is trying to say. Does "Results" refer here to the evaluation with TCCON? Or does it refer to the flux estimates?

The way I read it is that the posterior flux estimates (whether you mean NEE or fire is also unclear) are more robust (i.e. similar) than the different prior flux estimates.

Lines 16-20. I find this short recap of the main results quite hard to read because so many geographical regions and elements of 2-step inversion approach are compressed in 2 sentences (GFED4s, MOPITT CO, OCO-2, insitu data). Please rewrite.

A suggestion:

Slightly larger net CO2 sources are derived with posterior fire emissions in the OCO-2 inversion, in particular for most Tropical regions during 2015 El Nino year. Similarly, larger net CO2 sources are also derived with posterior fire emissions in the in-situ data inversion for Tropical Asia.

Line 21. Use either: 're-balanced posterior simulation' or 're-balanced posterior simulated concentrations'

Line 21. 'very close **to** each other'

Line 34. Be aware that since 2017 GFED4s emissions are not based anymore on direct burned area datasets, but instead based on relationships between MODIS active fire detections and GFED4s emissions for the period 2003-2016. This is because the underlying burned area dataset has been upgraded in the meantime from Collection 5.1 to Collection 6, making it incompatible for usage in GFED4s. GFED4s emissions from 2017 onward are therefore called GFED4s-beta emissions.

See: https://www.geo.vu.nl/~gwerf/GFED/GFED4/Readme.pdf

Line 51. Induce

Line 81. There are many examples you write double plural "emissions sources", "emissions estimates", "emissions inventories". Although I'm not an English native speaker, I think it's grammatically better if you write it as "emission sources" or "emission estimates".

Line 98. "and the post-event" I suggest "and the subsequent years"

Line 106. "The importance of these results for conclusions …". I think this is grammatically incorrect. Please rewrite. Something like: "The importance of these inversion results are discussed in Section 4."

Line 110. Please provide a clearer breakdown of the 2-step approach. A suggestion:

Our inversions are performed in sequence: (1) we assimilate total column CO retrievals from the MOPITT v8 products to produce optimized CO fluxes, which are used to update the assumed CO2 fire emissions, and then (2) we assimilate either total column CO2 from OCO-2 version 9 retrievals or CO2 in situ data to produce optimized CO2 NEE fluxes

Line 113. validation (singular)

Line 124. "…allowing a well-understood of its continuity and consistency ". Please rewrite.

Line 177. Awkward sentence. Perhaps start it like this: "Despite the known shortcomings (biases) of satellite data, several studies have preferred to use satellite data over the Tropics to take full advantage of the improved spatial coverage."

Line 182. at smaller spatial and temporal scales

Line 214. suggestion: "…shows the site locations over the globe."

Line 226. Change to "the global in situ network"

Line 233. …the corresponding satellite and in situ data.

Line 236. R covariance structure is not discussed in the paper.

Line 262-264. These 2 sentences should be merged to the same paragraph.

Line 272. Change to "fire carbon emissions with 11%"

Line 293-296. This part needs some editing. Also, I'm not sure what you trying to say here. Do you mean perhaps if an optimized CO flux for a pixel becomes twice as large (after inversion with MOPITT CO) you scale up the fluxes of the underlying vegetation types with a factor of 2?

Line 296-297. Suggestion: "Figure 3 shows for instance the GFED vegetation type for each year, where each pixel represents one or several vegetation types."

Line 299. As mentioned Andreae. Use the new updated EFs published in Andreae (2019)

Line 309. "type per grid box". This is at 3x2 resolution? If so, state that explicitly.

Line 311. "in balance with fire estimate". You mean in balance with the atmospheric CO2 trend, or not?

Line 318. Unclear. Are the ocean fluxes also optimized or not?

Line 323. You mean CASA-GFED3

Line 326. "gross ecosystem exchange". Is this the correct terminology? Should it not be the Net Primary Production (NPP)? NPP is equal to the sum of gross primary production (GPP) and autotrophic (maintenance) respiration (Ra). See below.

NEE = GPP+Rh+Ra=Rh+NPP

NPP = GPP+Ra

eq. 2. What is the meaning of number 3? Because it is based on CASA-GFED3?

Line 328. What is the meaning of MOPITTOpt and RMo? First time these parameters are mentioned.

Table 3: Something is off with the calculated values of FIRE4 (GFED4s). When I calculate the emissions myself from the official GFED4s tables I calculate for 2017, 2018 and 2019 very different emissions than what is reported in Table 3 (highlighted here in bold). I take the global emissions from https://www.geo.vu.nl/~gwerf/GFED/GFED4/tables/GFED4.1s_CO2.txt and subsequently convert them to carbon emissions with 12/44 ratio.

FIRE4 1.882.091.73**1.78 1.69 2.13**

It would also be helpful to see the estimates of NEEre3, NEEre4 and NEEreMO, and CMS-Carb in Table 3.

There is also another issue with FIRE4 and FIRE3 estimates in Table 3. They do not match with the blue and green bars in Figure 7. All estimates in Fig. 7a exceed 2 PgC/yr, which is not the case in Table 3.

Line 380-423. Very difficult to read. A lot of inconsistencies in region labels, and redundant information in the text. I suggest to keep it simple and concise by briefly discussing the results in separate short paragraphs for each continent. Just keep it simple.

Highlight that the optimized fires are generally smaller than prior fires.

This section also omits important large differences between GFED4s and the posterior fires (see my earlier concerns). Especially the differences for the boreal regions, Africa, and Indonesia are striking and inconsistent with the recent literature. This should be addressed.

Figure 5. - Put labels (a) 2015, (b) 2016, (c) 2017 and (d) 2018 on top of each panel

Typo in legend: moppit should be MOPITT

In the legend make MOPITT and GFED4s transparant white, not grey (as this color is already used for agri).

I suggest to include a bar graph in each panel that shows the global emissions for the different land types.

Line 428. You mean "The first one"

Line 438. temperate without capital

Figure 7: Please put the experiment labels in the middle of each grouping of bars. In addition, label the 4 panels with a, b, c and d. Similar comments apply to the other bar plots.

Section 3.2.1. Overall I believe this section can be trimmed down and made more concise.

Figure 8 and 9 should be combined into 5 squared panels.

Line 474. "global fires emissions" => "global fire emissions"

"We can also observed" => "We can also observe"

Line 477. "FIREMo observes less emissions" => e.g. "FIREMo yields less emissions"

Line 485. Example of a somewhat tedious number of sentences that should be condensed to one sentence.

"The larger emissions with FIREMo compared to FIRE4 over tropical Asia comes mainly from some specific vegetation. The main vegetation type in this region is savanna and we can observe that for the CO2 prior emissions, FIREMo has the higher flux for Northern tropical Asia (Southern tropical Asia) compared to FIRE3 and FIRE4 (FIRE4 respectively) for savanna but also for agriculture and deforestation (see Fig. A3)."

Besides the sentence structure, I disagree with what it says. Figure 9c shows FIREmo emissions are of similar magnitude as FIRE3 and FIRE4.

Line 493. capture => captured

Line 495 and several other lines. "the GFED" => "GFED"

Line 498. Smokes => smoke

Line 510. "…between OCO-2 and IS inversions the larger ensemble…" => "…between OCO-2 and IS inversions detailed in Crowell et al. (2019) and Peiro et al. (2022)"

Line 517. "…for the Tropics with larger sources for the OCO-2 inversions…". Is this not simply because FIREmo is much smaller in tropics than FIRE4, and thus we see a compensating effect by increasing the NEE source?

Line 517. "…opposite behavior for the Tropics" This part should be the beginning of a new sentence.

Line 525. "we can again observe a consistency in OCO-2 across the priors "

You do perhaps mean " consistency between OCO-2 and the priors" ?

Line 526. "(with sources for OCO-2 inversion)". I don't understand what this means.

Line 530. "This could then explain why we observe stronger sinks with in situ than OCO-2 posterior NEE emissions. " Which figure can we observe this?

Line 532. "different with a Tropical sink in all years except in 2015 and 2016 ". Where can I see this? In figure 10?

Line 522-544. This part needs to be rewritten. I feel that the key message is somewhat lost in this extensive summary of differences between inversions and regions. Make sure the following items are expressed in a concise manner:

- Sinks of OCOcms and IScms are generally weaker than the other inversions. Suggest sensitivity from the imposed AGR.

- Global sinks are larger with in situ data, which is largely driven by larger sinks in the tropics. Possible culprit: sparse data coverage.

- While OCO2 inversions show larger sources over the tropics and larger sinks over SH Ext. Could be a compensating effect for the scaled down fire emissions in the tropics.

Line 531. At least from Figure 10 I conclude that the in situ inversions yield for all 4 years a tropical sink, not only 2017 and 2018 as you write. I also see sinks up to 2.5 PgC/yr. How does this relate to the reported sink of -0.5 PgC/yr?

Line 548. "…we can see that the OCO-2 inversions have deeper net sinks over the Boreal regions than with OCO-2…" Perhaps you mean OCO-2 inversions have deeper net sinks in comparison to IS?

Figure A6. Panel titles do not agree with the caption labels. E.g. first panel shows North America, but caption says Boreal North America, and the second panel shows North Trop South America but the caption says temperate North America.

Line 554. Clarify what you mean with "drop off sinks"

Line 555. This is an example where you can be more concise and to the point: "…is balanced by the Tropical Asia (North and South) where net fluxes go from sources to sinks."

You can write this as: "…is balanced by sinks in Tropical Asia (North and South) "

Line 555. An example of a sentence with inconsequent use of tense: "2015 was a large net sources of carbon (due to intense fires) while 2016, 2017 and 2018 are deeper sinks with IS over Northern Tropical Asia and sinks with OCO-2 over Southern Tropical Asia. "

I would write it like: In 2015 there were large net carbon sources (due to intense fires), while in the other years there were larger sinks over Northern Tropical Asia (with IS) and Southern Tropical Asia (with OCO-2)

Line 557. An example of unnecessary sentence. "At the same time, posterior fluxes in Europe are anti-correlated with posterior fluxes over Northern Tropical Africa." The sentence that follows already makes the point of anticorrelations between Europe and Northern Tropical Africa.

Line 559. "where the post ENSO period has smaller sources in Northern Tropical Africa linked with smaller sinks in Europe (Fig. 11). "

I don't think this is true. The deepest sink for Europe appears in 2017, not in 2015.

Line 560. "more in line with…" more in line than what? IS inversions? Be clear please.

Line 561. Remove "carbon per year". It is redundant

Line 563. "no estimate that can be refuted at present. " Do you mean: there is no reliable benchmark for comparison?

Line 565. by Houweling et al. (2015)

Line 568. "We can see that our inversions here are within the estimates observed in the study of Peiro et al. (2022)." You can skip this sentence. You already say something similar in lines 566-567.

Line 570. Another example of a some text that is very difficult to read

"Our re-balanced priors give the deepest sink in 2017 (in 2016 for CMS prior) which is observed as well in the posteriors net fluxes using OCO-2 and it is in opposition of the OCO-2 inversions of Peiro et al. (2022) which have deeper sinks in 2016. This is due to stronger fire emissions in 2017 compared to the other years balanced with the respiration, and the differences between the two studies could be due to the re-balanced respiration. "

Can you not write something like:

A major difference between this study and Peiro et al. (2022) is that the rebalanced priors and posterior fluxes provide the largest sink in 2017, as opposed to 2016. This is likely a consequence of the larger fires and the subsequent rebalanced respiration that was derived in this study.

Line 575. "but an agreement across priors within each observational constraint." What agreement across priors are you referring to? Both types of constraints (OCO-2 and IS) use the same set of prior emissions. So I don't understand this sentence.

Maybe if you write it like this it becomes clearer what you try to say:

Between all inversions the largest differences in fluxes appear between IS and OCO-2 constraints. However, across the different fire emissions we observe a split; on one hand inversions using FIREmo are similar to FIRE4, while inversions using CMS are more similar to FIRE3. That means fires have a larger impact on the posterior solution than the rebalancing of prior NEE to match the global AGR.

Line 579. "are balanced with higher sources for the other regions that have net sources, regions mainly over the Tropics" please rephrase.

"are balanced with larger sources in other regions, mainly over the Tropics."

Line 586. "lag between flux in the Tropics and observation by the in situ network" Why is there such a lag in the tropics and not in the extra-tropics? Is that because the distances are longer between the measurements sites and the major source/sinks regions? Please explain in the main text.

Line 589. "FIREMo and FIRE4 drop off for 2017 but FIRE3 driven fluxes do not."

Please be clear. Do the fire emissions become smaller or the inferred NEE fluxes become smaller? Which region?

Overall I think section b needs to be edited. The main points should be addressed in a more clearer and concise way. The current summary of results is very extensive and long which makes it difficult for the reader to extract the key points.

The key points from section b that needs to be highlighted in a more concise manner:

- Between IS and OCO-2 inversions there are persistent differences in posterior NEE

- Some of these differences are caused by differences in data coverage, lag between flux and observation, cloud fraction, etc.

- Larger sinks with OCO-2 in North America and Europe, while larger sinks with IS in Asia.

- Independent of observational constraints: the sinks in the tropics are generally smaller while there are larger net sinks in the NH Ext.

- Independent of observational constraints: Generally smaller sinks during El Nino in the tropics.

Line 638. "Additionally, for the 2015-2018 period, the posterior biases were ☐ 7 ppb underestimated TCCON values while the priors were ☐ 13 ppb overestimated TCCON values." Change to: "In comparison to TCCON, for the 2015-2018 period, the posterior biases were underestimated by 7 ppb, while the priors were overestimated by 13 ppb"

Line 644. "over the Northern latitudinal" change to: "over the Northern hemisphere"

Line 654. "In Southern Hemisphere, MOre prior has smaller biases than GFED4re." Are these differences significant at all? All lines seem to be on top of each other in Figure 14. Are the differences significant in comparison to the measurement precision of TCCON CO2? Please elaborate on this.

Table 6. I suggest to rename the labels to FIRE3, FIRE4 and FIREMo

Line 694. Please discuss the discrepancy between your inversion study and Nechita-Banda et al. regarding the 2015 Indonesian emissions. They found emissions of 0.5 PgC, which is not only more than GFED4, but also more than the GFED3 estimate reported in Fig. 9 of your study.

Line 695. If GFED4s is able better capture small fires then please explain why GFED3 predicted larger emissions for Indonesia in 2015. Is this related to higher fuel loads in the older model?

Line 702. "where IS4re and ISMore have sources of carbons compared to the IS constrained with the GFED3 fire, showing then higher net sources with GFED4 and MOPITT than with GFED3 fires"

change to "where IS4re and ISMore derive carbon sources in contrast to IScms that derives a carbon sink with GFED3 fires."

Line 704. "the CO2 posterior emissions using MOPITT CO information were able to catch the seasonality"

Please clearly state that you referring here to the CO2 fire emissions and not NEE. The GFED emissions also show seasonality. Is there independent proof MOPITT derived fires show a better seasonality? I would think the smoke could have hampered the MOPITT observations just as much as the MODIS observations. Please elaborate on this.

Line 706. "It is thus important to include CO fire emissions over this region to improve estimates and constrain CO2 NEE and Fire emission with both OCO-2 and IS data constraints "

I don’t think this sentence covers your methodology correctly. I suggest to rephrase it differently: “It is thus important to use CO observations to constrain estimates of CO2 fire emissions, and subsequently constrain NEE with OCO-2 and IS observations”

Line 709. “finer enough” to “fine enough”

Line 710. “Additionally, the emission factors used in the emission ratio are characteristic of vegetation type but are not dependent of spatial or temporal scales. ”

I think you try to say that emission factors lack spatial and temporal variability to account for the full dynamics range of combustion characteristics. That is different than saying “not dependent of”.

Line 713. “between the priors using CO fire emissions and the other prior fire emissions ”

I don’t understand this comparison. All your experiments use fire emissions. Do you mean comparing optimized fire emissions (FIREmo) with non-optimized fire emissions (FIRE4)?

Line 713. “Further works are needed ” to “Further work is required”

Line 716. “this tropical region”. This is a new paragraph, so which region are you talking about now?

Line 730. “The IS results suggest a very strong sink in North Asia ” Do you think this is mostly an inversion artefact due to low data coverage here?

Line 746. “OCO-2 measurements are globally distributed, but seasonally varying coverage.”, Another difference is that OCO-2 represents a column density as opposed to a concentration in the lower boundary layer.

Line 771. Looking at the posterior fluxes and the TCCON comparisons in Table 6, I hardly see any differences in performance between MOre and the other inversions (GFED3re, GFED4re and CMS). So the added value of optimizing fire emissions before optimizing NEE is not very apparent. On the contrary, your results seem to be very insensitive to the optimized fire emissions. This outcome should be presented much clearer in your discussion and conclusions.

Line 776. "We found that a priori CO2 flux uncertainties are substantially reduced when matching the NOAA AGR as well as CO/CO2 ratio but not strong enough compared to a re-balanced GFED and GFED4.1s NEE, and suggest hence for future work the development of joint CO-CO2 inversions with multi-observations for stronger constraint in posterior CO2 fire and biospheric emissions. "

This is a key sentence as it wraps up your paper. However, even after reading the paper I have difficulty to fully understand it.

I tried to rephrase it in three separate sentences. Is my interpretation correct?

"We found that CO2 fluxes are more robust if the NEE and fire emissions are rebalanced in order to match the NOAA AGR as well as the satellite-based CO constraints. However, a more reliable NEE is obtained if we utilize in situ and satellite-based CO2 constraints. This opens new avenues for future research for the development of a joint CO-CO2 inversion framework that uses multiple streams of data to improve the fire and biosphere emissions."