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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spatial and temporal variability in the ratio of trace gases emitted from biomass burning" *by* T. T. van Leeuwen and G. R. van der Werf

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We greatly appreciate the positive, constructive, and very thorough reviews of our paper. A new version of our revisited paper is attached as supplement.
Our main changes based on the comments of reviewer 2 are summarized below:

- We shortened Section 2.1 by 46% and modified Section 2.2.
- While still focusing on the database of A&M2001-2009, we now also include a comparison with the EF database of Akagi et al. (2010) for MCE and different environmental parameters.

- We expanded the discussion on the different correlations we found for the whole emission factor (EF) dataset, compared to results from individual studies.
- We changed Figure 3, and quantified the amount of emissions and EF measurements in peak fire month and the shoulder of the fire season.
- We expanded the discussion on the different environmental parameters we used (Section 4.1).
- We shortened section 4.3 by 35%.
- We developed a continuous classification scheme for EFs, and constructed monthly MCE field, shown in the (new) Figure 6.
- Using the continuous classification scheme, we explored regional differences in the savanna biome.

Please find a detailed response for reviewer 2 below.

Kind regards,

Thijs van Leeuwen

Specific comments

1) P23560, L15: I suggest replacing "datasets thought to drive" with "measurements of environmental variables that may correlate with"

The text was replaced following the reviewer's suggestion (line 24).

2) P23560, L19: I suggest replacing "driver datasets" with "remotely sensed data"

We replaced “driver datasets” with “remotely sensed data” in line 28.

3) P23560, L20: I suggest inserting “for EF” after “techniques”

We inserted “for EF” in line 29.

4) P23560, L21: I suggest inserting “EF” before “measurements”

We inserted “EF” in line 30.

5) P23560, L21-24: The comparison of consumption weighted EF to simpler arithmetic means is worth presenting, but the difference is not large compared to typical global uncertainties. It would be useful to also note in the paper (and summarize here) how much the climate-window-specific EF depart from the arithmetic means (and how much data these departures are based on)

We have modified the text so it now more clearly shows that there are basically three levels of calculating average emission factors (Section 3.5 and 3.6) and acknowledge that the difference is probably not significant. Please see comment 55 for a more thorough answer. Further, we used the derived equations that relate different environmental parameters with EFs in combination with GFED emission estimates, to give estimates of regional EFs (Section 3.6). Please see comment 66 for a more thorough answer.
In the abstract (lines 32-35) we added a sentence on regional differences we found for MCE when using relations between the different environmental parameters:
“When using relations between the environmental variables and EFs to extrapolate to regional scales, we found substantial differences, with for example a ~19% lower CO EF for savanna and grasslands in Australia compared to southern hemisphere South America.”

6) P23561, L26: I would replace “, or alternatively” with “and/or”

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We replaced “or alternatively” with “and/or” in line 37.

7) P23561, L3: change “on” to “to” or “for” or “in”

We changed the sentence into:
“its importance for atmospheric chemistry“ (line 42).

8) P23561, L23: delete “campaigns of ”

We deleted “campaigns of”.

9) P23561, L26: Could delete one of two overview references for SAFARI 92 and add one for SAFARI 2000: Swap et al., 2002.

One reference for SAFARI 92 was deleted (Andreae et al., 1996a), and the paper of Swap et al. (2002) was added as a reference for SAFARI 2000 (line 63).
(“Andreae et al., 1996b” now becomes “Andreae et al., 1996”).

10) P23562, L1: “southern Africa”

Text has been corrected (line 66).

11) P23562, L14: Could add: “Akagi et al., (2010) produced a compilation and averages for EF by fire type that is weighted toward field measurements in fresh smoke.”

The paper of Akagi et al. (2010) was added as a reference in line 72, and we now also made a comparison between the MCE and different environmental parameters for the fresh-smoke EF dataset of Akagi et al. (2010). In lines 472-482 of Section 3.4 we state:
“For MCE we performed a similar analysis using the dataset of Akagi et al. (2010), which is based on EF data measured in fresh plumes only, which have not undergone

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significant photochemical processing. Overall, the correlations with the different environmental parameters did not improve compared to the EF dataset of A&M2001-2009; a maximum correlation coefficient of 0.55 was found using all environmental data combined. This is not an indication that one dataset is preferred above the other one; for CO and CO2 it does not matter whether fresh or aged smoke is sampled. The differences could be the result of a larger number of samples in the A&M2001-2009 dataset. When translating our findings on MCE to other trace gases or aerosols, it may be preferable to use the Akagi et al. (2010) dataset because it consistently only takes those measurements focusing on fresh smoke into account, better representing initial emissions."

12) P23562, L24-25: Many small fires do not show up as hotspots or burned area and remote sensing products often have low sensitivity to CO and other BB products in the boundary layer, thus agreement between estimates using the two different approaches suggests, but does not prove, that they are converging on the right number.

We now address these uncertainties more specifically, still arguing though that the uncertainty in emission factors is often overlooked when bottom-up estimates are constrained using top-down approaches. This section is rewritten to:
"New burned area products (L3JRC [Tansey et al., 2007], MODIS [Roy et al., 2008; Giglio et al., 2010], GLOBCARBON [Plummer et al., 2006]) allow for a better characterization of the timing and locations of fire, although the quality of these burned area products varies and they may have difficulties in capturing small fires (Chang et al., 2009; Roy and Boschetti, 2009; Giglio et al., 2010). When accounting for errors in transport and chemistry as well as uncertainties in satellite retrievals of trace gases and aerosols, combining bottom-up (such as GFED) and top-down methods potentially allows for an assessment of the magnitude of emissions as well as their spatio-temporal variability (Arellano et al., 2004; Edwards et al., 2004; Gloudemans et al., 2006). This requires a thorough understanding of the relations between biomass combusted and emission of the trace gases or aerosols that are used as top-down constrains, most

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often CO.” (lines 89-99).

13) P23564, L18: suggest changing “gassed out” to “outgassed” Section 2.1: I think it could be cut by a factor four and still get the solid, main ideas across. Examples of some possible changes if the section is retained are given next.

We reduced section 2.1 by 46%. The retained section was changed according to the reviewer’s assumptions below.

14) P23565, L6-7: Suggest replacing “Several different compounds, with theoretically every possible molecule,” with “Many different compounds”

We followed the reviewer’s suggestion (line 154).

15) L9: I think K and C are being confused here. 450K (150C) sounds too low for pyrolysis to be exothermic. I think it’s closer to 700K, but it is a known number that can be looked up.

This part had been deleted when shortening section 2.2.

16) L10: Glowing is from 800-1000K.

This part had been deleted when shortening section 2.2.

17) L10-11: The complex, flammable mixture (known as “pyrosolate”) is produced even at 500K; it does not begin at 800K as might be inferred from this sentence. I would delete this sentence as the release of flammables was already introduced on lines 6-7. Just a general statement that the gases evolved from the solid biomass by pyrolysis are flammable and get oxidized if they enter a flame could suffice.

This part had been deleted when shortening section 2.2.

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18) L14-16: I would omit this sentence as the CO is partly from glowing and the CH4 is partly from "aromatization." Not sure it's known (rather than speculated) how important the flame chemistry is.

This part had been deleted when shortening section 2.2.

19) L26: "often referred to as" should be "which is related to" and combustion efficiency should be defined.

We changed "often referred to as" into "which is related to" and define the combustion efficiency in lines 168-171:
"The amount of substances emitted from a given fire and their relative proportions are determined to a large extent by the ratio of flaming to smoldering combustion, which is related to the combustion efficiency (CE), defined as the fraction of the fuel C burned converted to CO_2."

20) P23566, L6: It would be difficult for Oxygen to be in short supply in an open fire. I think the key to extinction is that, for the fire to propagate, the fuel geometry has to be such that about 5-10% of the energy released from a unit amount of burning fuel element is transferred to (and ignites) a unit amount of nearby fuel. The most common causes of extinction are a physical gap in the fuels that prevents sufficient heat transfer to additional fuels, rainfall, or fire spread into wet fuels.

This part had been deleted when shortening section 2.2, but to address the reviewer's concern we added the following sentence in lines 161-163:
"The most common causes of extinction are a physical gap in the fuels that prevents sufficient heat transfer to additional fuels, rainfall, or fire spread into wet fuels."

21) Section 2.2. General: this section might be shortened a great deal and left rather vague. Some speculation in older papers about the impact of environmental variables

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has been proven wrong. Other older papers reached conclusions based on a limited amount of data that are in conflict with subsequent studies. I suggest redoing this section with one summary sentence per variable in a short paragraph. I also make a few comments on the existing text.

Following the reviewer's suggestion, we shortened section 2.2 by 30% and rewrote most of the text, according to the reviewer's assumptions in the comments below. To emphasize that different studies are in conflict about the role of specific environmental variables on EFs, we highlighted the uncertainties with regard to this throughout the section.

22) L14: I suggest changing: "In general, the four most important factors controlling EFs of CO, CH4, and CO2 are vegetation characteristics, climate and weather, topography, and fire practices" to: "Important factors influencing EFs of CO, CH4, and CO2 likely include vegetation characteristics, climate and weather, topography, and fire practices". I am not sure if the true importance of the many possible factors influencing EF has been determined and also, in some sense, five factors were listed.

We appreciate that our statement was too strong and scientifically not defendable and have modified the text following the reviewer's suggestions.
We changed "In general, the four most important factors controlling EFs of CO, CH4, and CO2 are vegetation characteristics, climate and weather, topography, and fire practices " into "The exact physical relations between environmental variables and EFs are not well understood, although recent laboratory studies have aimed to quantify how, for example, moisture content impacts EFs (e.g. Chen et al., 2010). Qualitatively, important parameters that partly govern the flaming / smoldering ratio and thus EFs include vegetation characteristics, climate, weather, topography, and fire practices." (lines 174-178).

23) L19: "burning efficiency" is not defined. Is it combustion completeness (the fraction *of available biomass that burned) or combustion efficiency (the fraction of fuel C burned converted to CO2)?*

With "burning efficiency" we meant the "combustion efficiency". To be consistent throughout the paper, we changed "burning efficiency" into "combustion efficiency". A definition is given in lines 168-171:
"The amount of substances emitted from a given fire and their relative proportions are determined to a large extent by the ratio of flaming to smoldering combustion, which is related to the combustion efficiency (CE), defined as the fraction of the fuel C burned converted to CO2".

The role of fuel moisture is not really known yet. For instance, crown fires spread at high rates with huge flames burning fresh foliage of extremely high moisture. Wet fuels can ignite if a sustained ignition source is applied. So this is an example of these issues being far more complex than scientists guessed they would be years ago.

We addressed the role and uncertainty of fuel moisture more specifically in lines 179-185:
"A variable that may affect both the behavior and the emissions of a fire is the water content of the vegetation. The water content partly determines whether a plant or tree can ignite and what the combustion efficiency will be. Water in plants or trees has the capability to either stop a fire completely or to slow down the burning process (to a low smoldering stage). However, also wet fuels can ignite if a sustained ignition source is applied. For instance, crown fires spread at high rates with large flames burning fresh foliage with high moisture content."

24) Another general issue affecting section 2.2, other sections of this paper, and science in general is worth mentioning here. Often, a relationship that holds true in initial work is noted, which makes sense, but then future work does not support the relationship. An example of that involving fires follows. In Yokelson et al., (1996) and Goode et al., (2000) good correlation for NH3/NOx vs MCE was found for all studies published

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at that time. However, McMeeking et al., 2009 constructed a similar plot (their Figure 10) with the available studies at the later date and there was no useful level of correlation. Similarly, Figure 4 of McMeeking et al shows a very weak dependence of MCE (a function of CO and CO2) on fuel moisture that contrasts the more limited results of Lobert et al. Thus I think that mentioning the results of Lobert et al at the top of page 23567 – as well as a detailed discussion of how environmental variables affect EF - is not that useful given the limited amount of current knowledge. In general, there is very little hard data on how environmental variables effect the emissions from real fires and the limited amount of lab data is often conflicting and inconclusive. That limited amount of lab data is hard to find because it is buried in papers mainly focused on other topics. For example, in addition to the studies quoted above, there is information suggesting that fuel consumption is effected more by fuel spacing than by fuel moisture: Bertschi et al., 2003 McMeeking et al., 2009.

Based on the reviewer's comments we decided to shorten and rewrite the section, and highlight the large uncertainties. In lines 219-223 we summarize:
"In summary, both the combustion process and its inter-relationship with the environment are very complicated. At present, literature focusing on how environmental variables impact EFs from real fires is limited and data from laboratory studies is often conflicting and inconclusive. Nevertheless, empirical relationships between satellite observables and EF may exist and are further explored here."

25) P23567, L4-14: Efficient heat transfer between fuel elements is very important and not mentioned (see last reference above). In deforestation fires, huge logs can be consumed mostly by flaming combustion (Christian et al., 2007). Again, I think a summary statement that these variables impact emissions in complex poorly understood ways is sufficient at this point.

We acknowledge the importance of efficient heat transfer between fuel elements, and added the following sentences in lines 197-199:

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"However, with an efficient heat transfer between fuel elements even large logs in deforestation fires can be consumed mostly by flaming combustion (Christian et al., 2007; McMeeking et al., 2009)."

26) P23567, L10: "oxidized" should be "pyrolyzed" Section 2.2.2: Just saying that vegetation and the length of the fire season are influenced by climate should suffice. Not sure what is meant by "settlement and ecological support for" vegetation. The seasonal trend in emissions for savanna fires in Korontzi et al was actually just for Dambo fires and no trend was seen for Miombo fires, which account for a larger amount of biomass burned.

We changed "oxidized" into "pyrolized" in line 195, and reduced the part of the text where we describe the influence of climate:
"Climate also plays an important role in the existence and settlement of vegetation, and thus determines the availability of fire fuel (Lobert and Warnatz, 1993). Fire frequency and the fire season are also partly determined by climatic factors." (Lines 200-202).

27) P23568, L2: fuels are not always "oxidized" – should be "consumed."

This part of the text was deleted.

28) L4 "burning efficiency" = ?

With "burning efficiency" we mean "Combustion efficiency". We changed this throughout the text, see also comment 23.

29) L3-6: Lobert had a good idea to do this, but it was a very limited investigation of this in the lab, I don't know of any field verification of these effects.

This part of the text was deleted while shortening section 2.2, following comment 1.

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30) Section 2.2.3 Again a few simple lab trials may not be relevant. The key variable is that heat rises and an upslope fire therefore achieves better heat transfer from the burning fuels to the unburned fuels, which makes it spread faster if all else is equal.

We added a sentence on the key variable for the local topography in lines 208-211: "The local topography can also change the burning behavior of a fire; heat rises and an upslope fire therefore achieves better heat transfer from the burning fuels to the unburned fuels. If all other conditions are equal, this leads to fires that spread faster."

31) P23568, L17: suggest replacing "has become for a large part" with "is mainly" since new analysis of the Antarctic ice core indicates that historical burning by humans may have been higher hundreds of years ago.

We replaced "has become for a large part" with "is mainly" in line 212.

32) L19-24: Slash and burn fires may be less intense than fires featuring windrows, but it seems to mainly affect the particle emissions? It might be more important to mention that the fuels consumed in understory fires (e.g. shrubs and leaf litter - common in the Miombo) are different from the fuels consumed in deforestation fires (mostly large trunks) and whether or not these different types of tropical forest fires can be identified from space.

This part of the text was deleted. To address the reviewer, we think that Miombo fires are usually classified under savanna fires. The tropical forest fire experiments we have included in our analyses were mainly deforestation fires. With our new approach, we hope to better capture the range from open grassland fires to closed savanna fires (including Miombo), which were treated the same in the past.

33) L24-26: The key here is variable relative consumption of the peat and forest overstory. In summary I recommend reducing section 2 to a few summary statements

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showing that both combustion and how it is affected by the environment are very complicated, almost completely unstudied, and poorly understood. Nevertheless, empirical relationships between satellite observables and EF may exist and should be explored.

The part of the text where we discuss the role of relative consumption of peat and forest overstory was deleted.
In general, we reduced section 2 by 30% and emphasized in lines 219-223 that the combustion process and the way different variables influence this are very complicated: "In summary, both the combustion process and its inter-relationship with the environment are very complicated. At present, literature focusing on how environmental variables impact EFs from real fires is limited and data from laboratory studies is often conïňĆicting and inconclusive. Nevertheless, empirical relationships between satellite observables and EF may exist and are further explored here."

34) P23569, L3-4: The more recent compilation of EF by Akagi et al., (2010) likely includes additional data of value to the authors.

We used the EF database of A&M2001-2009 (updated until 2009). This updated version already included most of the field measurements of CO, CH4, and CO2 EF presented in the database of Akagi et al. (2010).
A comparison with the more recent compilation of EF by Akagi et al. (2010), focusing on EF measurements in fresh smoke plumes only, was done in section 3.4, lines 472-482:
"For MCE we performed a similar analysis using the dataset of Akagi et al. (2010), which is based on EF data measured in fresh plumes only, which have not undergone significant photochemical processing. Overall, the correlations with the different environmental parameters did not improve compared to the EF dataset of A&M2001-2009; a maximum correlation coefficient of 0.55 was found using all environmental data combined. This is not an indication that one dataset is preferred above the other one; for CO and CO2 it does not matter whether fresh or aged smoke is sampled. The

differences could be the result of a larger number of samples in the A&M2001-2009 dataset. When translating our findings on MCE to other trace gases or aerosols, it may be preferable to use the Akagi et al. (2010) dataset because it consistently only takes those measurements focusing on fresh smoke into account, better representing initial emissions."

35) P23569, L10-11: I think that the EF values for some categories for some species such as glyoxal in A&M2001 are from lab studies.

The reviewer is right, and to emphasize that we focus on the measurements of CO, CH4, and CO2 EF we changed the sentence into:
"In the work of A&M2001-2009, laboratory measurements were also excluded for calculating biome-averaged EFs for CO, CH4, and CO2." (lines 235-236).

36) P23569, L11-12 and 22-27: I thought the EF were already calculated in A&M2001. If the authors recalculated them, they may want to explain why they did that.

We did not recalculate the biome-averaged EFs of A&M2001-2009, and explain in the text how A&M2001-2009 calculated their EFs. To make this clear in the text, we changed lines 237-238 into:
"Most of the EFs in the database of A&M2001-2009 are measured using the C mass balance (CMB) method (Ward et al., 1979; Radke et al., 1990)."
And line 248-249 into:
"When the emission data were given as molar emission ratios, A&M2001-2009 used the molecular weights of the trace and reference species to calculate the EF."

37) P23569, L20: The more detailed study of Susott et al., (1996) suggests a global average C fraction for biomass closer to 50%, but with a considerable range. I am not suggesting recalculating the EF again, but acknowledging the uncertainty in %C is relevant.

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We used the EF database of A&M2001-2009, and therefore also adapted the way that they calculate different EFs. Since we did not recalculate the EFs, we added the following sentence in lines 243-247 to address the reviewer's comment:
"A&M2001-2009 adopted a C content of 45% when this information was not given in literature cited. However, a detailed study of Susott et al. (1996) suggests a global average C fraction for biomass closer to 50%, with a considerable range, which would indicate an additional 10% uncertainty in addition to other uncertainties."

38) P23570, L20-27: This is an important point. Indochina and other areas could be added to list.

By adding Indochina to the list in line 275, we mentioned an additional important area that is undersampled when overlying GFED fire emissions and the measurement locations as in Figure 1:
"Most locations with both CO and CO_2 EF measurements are in North America, the arc of deforestation in the Brazilian Amazon, southern Africa (South Africa and Zambia), and northern Australia (Figure 1). While these areas are all major biomass burning regions, several other important regions lack measurements. These include Central Africa (e.g. Congo, Angola, but also regions further north such as Chad and southern Sudan), Siberia, Indochina, and Indonesia, although laboratory studies for Indonesian fuel samples exist (Christian et al., 2003). (lines 271-277).

39) P23570, L29: change "the three" to "three"

"the three" was changed into "three" (line 286).

40) P23571, L8: change "all in-situ" to "all these in-situ"

"all-in-situ" was changed into "all these in-situ" (line 292).

41) P23571, L11-15: Here is an example of what I mean by expanding the core discus-

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sion. It is interesting that "r" goes down when limiting to extratropical forest. Examining this further may be useful. Also the slopes for the biomes here could be compared to the previously determined slopes for those biomes in the cited studies. That would yield insight into how correlations evolve as more data is added. If section 2 is greatly reduced (as suggested earlier) the implications of this could be explored a bit instead since it is the new work.

We examined our results further by comparing the correlation coefficients and slopes of the regression for different biomes with findings from other individual studies in lines 300-322:
"Although lowering the number of EF studies in general decreases the correlation coefficient, several individual studies focusing on a selected number of measurements found higher correlation coefficients than the ones reported above. Yokelson et al. (2003) found a correlation coefficient of -0.93 (EF(CH4) = -48.522 $\times$ MCE + 47.801) for 8 African savanna fires. Korontzi et al. (2003) also found higher correlations and a slightly different slope for the regression of southern African savanna measurements - grasslands had a correlation of 0.94 (EF(CH4) = -43.63 $\times$ MCE + 42.951) and for woodlands a correlation of 0.98 (EF(CH4) = -58.214 $\times$ MCE + 56.710) was found. Both vegetation types combined gave an overall correlation of 0.94, and a trendline of EF(CH4) = -47.948 $\times$ MCE + 47.068. For the tropical forest biome, Yokelson et al. (2008) found a correlation coefficient of 0.72 for 9 fire-averaged MCEs and CH4 EFs. The slope of this regression was significantly more gentle (EF(CH4) = -47.105 $\times$ MCE + 48.555) than the slope for this biome using all measurements in the AM2001-2009 database. In older work, comparisons between the CE (which correlates well with the MCE) and CH4 EFs was presented. Ward et al. (1992) showed a correlation of 0.96 and a slope of EF(CH4) = -82.1 $\times$ CE + 78.6 for a regression of 18 deforestation fires in Brazil. We are not aware of any recent comparisons between MCE and EF CH4 for fires in the extratropical forest biome, but in older work of e.g. Ward Hardy (1991) and Hao and Ward (1993), an overall higher correlation (r>0.8) is found for extratropical forest measurements. The slope of the regression lines of these individual studies was

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more gentle than the slope we found for the whole dataset. Lab experiments (Christian et al., 2003; McMeeking et al., 2009; Burling et al., 2010) also show overall higher correlations between MCE and EF CH4 than our results for all data for the different vegetation biomes combined."
We then summarized these findings as follows in lines 323-333:
"Overall, higher correlation coefficients and flatter slopes for the EF CH4 and MCE relationship were found for individual studies focusing on a relatively small number of EF measurements, compared to the whole EF database of A&M2001-2009. Possible explainations for these differences between the whole dataset compared to individual studies are discussed in section 4. Individual studies (e.g. Hao and Ward, 1993) have shown that the linear relationships between the MCE and EF of CH4 are quite different for individual biomes, for reasons not fully understood. This is also apparent from Figure 2; the slope and intercept of the savanna and extropical forest biome compare very well, but the regression line of CH4 EFs and their MCE derived for tropical forest biome shows a steeper slope and larger intercept. Most variation and therefore lower overall correlation coefficient was caused by the extratropical forest measurements."

42) P23571, L21-P23572, L4: interesting and worth including any quantitative data that may be in references. L26: add "per unit area" after "consumed"

We added "per unit area" after "consumed" in line 341, and changed Figure 3:
we now quantify emissions in the peak and shoulder of the fire season (see comment below (43) for a more extended comment on the changes we made).

43) P23572: general. I am confused by Figure 3. It's a good idea to show the cycle of biomass consumption shifted so the peaks match up and also superimpose the measurement cycle. But why is there one vertical axis? Which line is which? The text seems to say the solid line is measurements, but the figure caption seems to say the solid line is fire emissions. I guess MOM stands for "month of measurement." This *discussion is worth including and expanding, but needs clarification. For instance, what is the relative amount of fuel consumption in the off-peak months compared to the peak month? Is the fuel consumption symmetrical about the peak so that roughly equal amounts occur before and after the peak? Is the peak month in the middle or near the end of the local dry season? These are important questions that the authors are very well qualified to address. Quantifying the off-peak biomass consumption could help build momentum for an off-peak measurement campaign.*

It is important to quantify the emissions in the shoulder of the season, and to compare this to the emissions in the peak fire month(s). We changed Figure 3 to further highlight this and make the figure easier to interpret, and now show the GFED3.1 fire emissions for the peak fire month (PFM) and the shoulder of the fire season for the savanna (Figure 3a) and tropical forest (Figure 3b) biome. The number of EF measurements for both biomes in these specific months is also shown.
In lines 347-363 we quantify the emissions in the PFM and outside the PFM for the savanna and tropical forest biome, and address the questions that are posed by the reviewer:
"We explored the seasonal variation of the fire emissions for all EF data where a detailed description of the location and date of measurements was provided. To investigate whether the available measurements captured the fire seasonality we compared the number of EF measurements conducted in a specific biome with the seasonal variation in C emissions according to GFED3.1 (Figure 3). Only the $0.5^{\circ}\times0.5^{\circ}$ grid cells enclosing the locations where EF measurements were conducted for CO, CH4, and CO2 were used, and the seasonal cycle in each grid cell was normalized to its peak fire month (PFM). Figure 3a shows the seasonality of the number of EF measurements and the GFED3.1 fire emissions for all the EF measurement locations in the savanna and grassland biome for the PFM, and the months before and after the PFM. Results for the tropical forest biome are shown in Figure 3b. For EF measurement locations in the savanna biome, 46% of the total annual amount of C was emitted by fires in the PFM, and 78% when also including the month before and after the PFM. For the

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tropical forest biome, this was 66% and 84%, respectively. The percentage of EF measurements conducted in the PFM was 23% for both the savanna and tropical forest biome, and respectively 71% and 88% when also including the month before and after the PFM. In other words, the current body of measurements have undersampled the peak fire month with especially the tropical forest fire measurements sampling earlier than desirable."
We added the new Figure 3 in the supplement.

44) P23573, L2: I would change "explain" to "model" since any reasonably, accurate, predictive, empirical relationship is valuable.

We changed "explain" into "model" (line 368).

45) P23573, L3: I would change "of the" to "in our"

We changed "of the" into "in the" instead of "in our" as suggested by the reviewer (line 369), since we refer to the database of A&M2001-2009.

46) P23573, L11: I totally agree with the justification given on lines 18-19 for using FTC, but I am not sure I understand what was done here. Many papers give the latitude and longitude of the fires sampled and the observed vegetation type for each fire sampled. It might be useful to compare the FTC at that lat/long to the observed vegetation type and the EF. It might also be interesting to compare mapped to observed vegetation type or the FTC before and after the fire? But was the FTC deresolved first? That may be necessary, but does it also prevent inspecting potentially useful information? In any case it should be clarified.

Indeed, many papers give the observed vegetation type but the definition is rarely standardized. In addition, our major goal was to move from a discrete classification to an approach that captured variability within biomes and could be used in a global modeling approach. This is also our reason to choose coarse resolution maps ($0.5^{\circ}\times0.5^{\circ}$)

instead of the native 500-meter maps. While the latter would suffer less from the role of landscape heterogeneity, several other issues such as the time of burn (before or after the fraction tree cover map was build?) etc. would complicate this assessment. For all these reasons we have used the coarse resolution FTC maps and have further expanded on the description of the methodology to prevent confusion.

47) P23573, L21: Useful correlation may exist between monthly precipitation and EF and should be pursued, but that is different from the precipitation "dependence" of EF. For example, dead fine fuels dry within 1-several hours after rainfall or reduc- tions in RH. Also, in developing the 1978 US National Fire Danger Rating System , precipitation duration was more important than precipitation amount as excess water simply runs off the fuels during in- tense storms.

To not confuse the reviewer, we changed "precipitation dependence with EFs" into "correlation of precipitation with EFs" (line 389).
We recognize that by using low-resolution data only, we may miss parameters that may play an important role in the variability of EFs on a high spatial and temporal resolution, like e.g. precipitation duration and daily high temperature. To emphasize that the monthly and mean annual precipitation we use may not capture all the EF variability, we added the following sentence in lines 401-403:
"Since we explored large-scale relations between EFs and the monthly and mean annual precipitation only, we may miss variability related to synoptic scale precipitation."

48) Again if section 2 is greatly reduced, more time could be spent exploring/ clarifying the effect of the resolution used for FTC, precipitation, and temperature. One difference between tropics and temperate zone is that the daily variation in temperature generally exceeds the seasonal variation in temperature in the tropics. Fires are generally lit after 11-12 AM and out by 3-5PM in tropics. Would fires correlate better with the daily high if available, etc?

We reduced section 2 by 30%, and expanded the discussion on the different environmental data we used, and their main advantages and disadvantages (Section 4.1 and 4.2).
We certainly recognize that our approach is a first step and more detailed analyses should be done (for this review step we included the mean annual precipitation and mean annual temperature). However, this is currently not possible due to the lack of both reliable input data at the global scale and a lack of detail in several older EF papers where often no exact location or time of measurement was given.
With expanding data availability (either from 'classical' EF campaigns or using simultaneous satellite measurements of trace gases [as is currently done with IASI for example: http://www.atmos-chem-phys.org/9/5655/2009/acp-9-5655-2009.html]) this situation may improve in the near future."

49) P23575, L2: Potentially related to the authors work there was some earlier work relating savanna fire emissions to PGREEN by Hoffa et al that was also used in Ito and Penner. In general there are older papers using non-vegetative classification schemes that could be compared to and/or discussed in more detail.

We now (shortly) discuss the work of Hoffa et al. (1999) and Ito and Penner (2005) in section 1., lines 113-118:
"In the literature only a few papers on regional emissions estimates considered seasonal and/or spatial variability of EFs into account. Hoffa et al. (1999) related fire emissions in Zambian grasslands and woodlands with PGREEN, defined as the proportion of green grass biomass to total (green+dead) grass biomass. Ito Penner (2005) applied three different EF scenarios that accounted for both seasonal and spatial variability. Both studies confirmed that a spatial and temporal varying EF can have a significant impact on regional emissions estimates." Further we also refer to their work in lines 541-542:
"Following the work of Hoffa et al. (2003) and Ito and Penner (2005), we developed a non-vegetative classification scheme for EFs, driven by various environmental param-

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eters."

50) P23575, L4: For length of the dry season it should be the consecutive number of months with low precipitation not the total out of 12. Should specify if this is the case.

We did use the consecutive number of months with precipitation lower than 100mm/month within 6 months (instead of 12) before the measurement. Therefore we changed the sentence and added "consecutive" in the text (lines 432-435):
"The length of the dry season for the EF measurement locations was defined by counting the number of consecutive months in the 6-month period before the measurement was conducted with precipitation rates below 100mm/month (GPCP $1^\circ \times 1^\circ$ for the 1997-2008 period, and GPCPv2.1 $2.5^\circ \times 2.5^\circ$ for 1979-1997)."

51) P233575, L11: There are many references on the time lag for dead fuel moisture to approach equilibrium in the environment. E.g.: the 1978 Fire Danger Rating System, or Ralph Nelson's chapter in "Forest Fires" by Johnson and Miyanishi.

We added the following reference for the 1978 Fire Danger Rating System in line 440: Bradshaw et al., 1984

52) P23575, L12: I would change "driver" to "remotely sensed environmental" and then if the authors like, on line 13 explain that they are calling these remotely sensed environ- mental observables "driver data" or just "environmental data"

We changed "driver data" into " remotely sensed environmental data" (line 443), and now use "environmental data" throughout the paper.

53) P23575, L14: recommend changing "all EF data for the" to "all our EF data for"

We changed "all EF data for the" into "the EF data of A&M2001-2009" (lines 445-446), since we used the unmodified EFs of the A&M2001-2009 database.

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Section 3.4: The correlation in Table 1 with EF could be compared to the correlation with vegetation type shown in Figure 2. This type of analysis is useful and is the core contribution of the paper. However, the relatively small sample size in the plots in Figure 4 should be noted as well as the need for more data.

We are not sure whether we fully grasped the reviewer's suggestion. We had inserted Figure 4 to provide some confidence that relations between broad-scale vegetation and climatic parameters did exist, albeit for selected measurements. In lines 483-499 we note that the sample size was small:
"In general, repeating the calculations but focusing on each individual biome yielded lower correlations than with all measurements lumped together. However, some of the relations found when using the full suite of data were still valid. For example, also within the savanna and grassland biome we found a negative correlation between FTC and MCE (or positive correlation between FTC and the CO emission factor) with an almost identical slope and offset as when using all measurements. Correlations between the EFs and the environmental data for the extratropical forest were very poor. Possible explanations for these poor correlations are discussed in section 4. Higher correlations between EFs and the driving variables were found when focusing on specific locations, although it must be noted that the sample size of these correlations is relatively small. Figure 4a, 4b, and 4c show correlations for respectively Brazilian deforestation fires and savanna fires in Australia (FTC vs. MCE), Brazilian deforestation fires (FTC vs. CH4 EF), and boreal fires in Alaska (precipitation vs. CH4 EF). A similar pattern occurred when focusing on vegetation types: correlations between MCE and CH4 EF were relatively low when using all data lumped together (Figure 2), and higher correlations were found in different individual studies, using a smaller sample size. Also, the extratropical forest data showed overall lower correlations than data for the savanna and tropical forest biome."
To look more into discrete vegetation types instead of the continuous fields we used, as we think is suggested, would defeat the purpose of this paper.

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54) P23576, L21: Yokelson et al., (2009) and Akagi et al., (2010) found that EF for tropical forest fires in the Yucatan were similar to those for tropical forest fires in Brazil.

To address this comment, we added the following sentence in lines 506-511:
"For example, nearly all tropical forest measurements are made in the Brazilian Amazon and Yucatan province of Mexico (Figure 1), while information from other deforestation hot spots such as Bolivia and Indonesia is lacking. Different regional deforestation practices could in principle lead to variations in EFs, something that cannot be taken into account at the moment due to a lack of measurements"

55) Section 3.5: I think what the authors are doing here is stating that a biome average EF using all EF from that biome may not represent the average smoke for the whole biome. Therefore, they create climate windows and place the EF for each biome within their respective climate window. Then they compute a weighted average EF for the biome where the weighting factor is the GFED fuel consumption for that climate window in that biome. It should be mentioned that the GFED fuel consumption by climate window has some uncertainty and there are also other issues besides relative fuel consumption that affect the representativeness of the sampling. The new weighted-average biome EF are not very different from the straight average biome EF when compared to typical global uncertainties. Therefore, it might be interesting to reveal the range of climate-window-specific EF retrieved for the various climate windows. This could imply significant regional or seasonal variation. It should be recalled that ultimately one is limited by the fact that many compounds were measured in only a handful of studies.

We have modified the text so it now more clearly shows that there are basically three levels of calculating average emission factors:
1)Arithmetic mean as is usually done
2)Weighted mean as we present using the climate windows, weighting each measurement location with the amount of biomass combusted as computed in GFED.

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This was done to eliminate biases in the relative importance of locations, e.g. were location with low emissions not oversampled? These did not differ much from the arithmetic mean and we present more clearly that simply taking the average from the available literature does not introduce large biases compared to the more sophisticate approach. However, this approach may still give biased results if the measurement locations are not representative from a regional perspective (e.g. are African savannas oversampled compared to savannas in South America?), therefore we have now a third level:
3) Mean emission factors (or MCE) based on the equations we derived and the GFED biomass combusted estimates.

Using the multivariate regression equation based on the different parameters, we also calculated regional differences in lines 571-580:
"Regarding the savanna and grassland biome: we found the highest MCE in Australia (0.9466), followed by southern hemisphere Africa (0.9432), northern hemisphere South America (0.9403), and southern hemisphere South America (0.9386). Although differences in MCE are relatively small, they have a substantial influence on the amount of CO and other reduced trace gases released. For example, the small difference in MCE between Australia and southern hemisphere South America (0.9%) may imply a relatively large difference in the amount of CO emitted (16%) if the total amount of C emitted as CO and CO_2 is kept constant in both regions. An important next step is to implement these spatial and temporal EF and MCE scenarios into GFED, and quantify regional differences in trace gasses emitted."
To address the uncertainty in GFED-measurements, we added the following sentence in lines 531-537:
"Overall, our new calculated weighted averages for CO, CH_4, CO_2 EFs and MCE do not deviate much from the arithmetic mean of A&M2001-2009, and are well within the range of uncertainty, especially when also taking the substantial uncertainties in the GFED fuel consumption estimates into account. This indicates that the measurement

locations were representative with regard to emissions strengths. However, it does not provide information of the representativeness of the measurement locations for the whole biome, which will be addressed next."

56) P23577, L12: "a large body of " before "available" as some studies are missing.

We added "a large body of" before "available" in line 583.

57) P23577, L20: "campaigns" is the correct spelling.

We use the correct spelling now in line 591.

58) Section 4.1: Is it possible to estimate uncertainties for the space-based products?

No formal uncertainty analysis exists for many space-based products. Although we cannot quantify the uncertainty, we do emphasize in the text that satellite products may have a large uncertainty (lines 605-607):
"Here we have not included uncertainties in these environmental parameters because they have not undergone an official error assessment, with the exception of the precipitation data."

59) P23578, L7-10: It is not known how wind and topography affect EF for real fires. See my intro for ideas on other variables that influence EF that are not explicitly included among the available remote sensing data.

We rewrote this sentence in lines 610-613:
"Although other environmental data (e.g. precipitation duration, fuel spacing, wind, and topography) may play an important role in fire characteristics and thus in the partitioning of trace gases emitted (e.g. Lobert et al., 1991), we could not take these factors into account because reliable information is not available from global datasets (see Section 4.1)."

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60) Section 4.3: I recommend eliminating this section for reasons given below and then expanding section 4.1.

Although differences between measurement techniques are more important for sticky or reactive gases, we decided to keep this section because we found some examples where the use of different techniques may have caused variation in CO_2, CO, and CH_4 EFs measured in specific experiments. Since these data were used in the EF dataset of A&M2001-2009, we discuss the different measurement techniques shortly (we did reduce this section by 35%, because not all the information was relevant enough). Section 4.1 was expanded by 90%.

61) P23578, L17-21: CO_2, CO, and CH_4 are fairly easy to measure by NDIR, GC, and FTIR, because they are not sticky or reactive. Differences between measurement techniques are more important for sticky or reactive gases. Yokelson et al., 2007 found good agreement between their airborne measurements for these gases and the tower-based measurements of Ward et al., 1992. And both experiments probed the convective phase of the fire. Residual smoldering combustion, which can last for weeks, does contribute emissions that are sometimes significant, but rarely sampled.

Please see comment 60.

62) P23579, L10: Is there a reference for increasing MCE with sample height?

This part of the text was deleted.

63) P23579, L20: The tower based measurements underestimated particle emissions by a factor of 2 in Brazil (Babbitt et al., 1996), but was OK for some gases. Ground-based sampling of savanna fires with long poles does not sound safe. Babbitt et al., 1996.

This part of the text was deleted.

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64) P23580, L11: "fase" should be "phase"

We changed "fase" into "phase" (line 660).

65) P23581, L6: There is large natural variation in the relative amount of fuel consumed by flaming and smoldering. Also Yokelson et al., (1996) showed that delineating smoldering and flaming phases causes errors in estimating the true fuel consumption by process. It might be interesting to explore if the GFED fuel consumption, which is highly specific, can be used to estimate the flaming to smoldering ratio.

The basis for the GFED data is the CASA biogeochemical model which estimates biomass in different classes: wood, coarse wood debris, leaves, and fine litter (in addition several other soil classes are available which are not relevant here). The model may be simpler than the reviewer assumed. The most basic approach would be to assume wood and coarse woody debris burn in the smoldering phase, while leaves and fine litter burn in the flaming phase. The results of this exercise would be similar to using solely fraction tree cover because this dataset drives the allocation of primary production to the different fuel classes, and we have therefore refrained from including this analysis.
In Section 3.3 we added a sentence on how GFED and FTC are linked:
"We used the fraction tree cover (FTC) product regridded to $0.5^{\circ}\times 0.5^{\circ}$ resolution for the year 2002 to represent the vegetation density and the ratio between herbaceous and woody fuels in the EF measurement locations. In the GFED modeling framework, FTC is the key control on the fraction coarse fuels that burn predominantly in the smoldering phase (e.g., stems, coarse woody debris) as opposed to fine fuels burning mostly in the flaming phase (leaves, grass, fine litter) in a grid cell." (lines 376-381).

66) Section 4.5: The same climate window could produce different EF in different locations e.g. Russia and Canada. The weighted means presented here are almost identical to the arithmetic means, thus the range may be more interesting than the

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difference in average values.

Please also see our answer to comment 55.
Using the derived equations that relate different environmental parameters with EFs in combination with GFED emission estimates, we can now give regional EFs. In Figure 6 our new calculated MCE is shown. Since our approach is probably best in capturing variability within the savanna biome we have focused on this and mention, for example, how Australia differs from South America (lines 571-580):
"Regarding the savanna and grassland biome: we found the highest MCE in Australia (0.9466), followed by southern hemisphere Africa (0.9432), northern hemisphere South America (0.9403), and southern hemisphere South America (0.9386). Although differences in MCE are relatively small, they have a substantial influence on the amount of CO and other reduced trace gases released. For example, the small difference in MCE between Australia and southern hemisphere South America (0.9%) may imply a relatively large difference in the amount of CO emitted (16%) if the total amount of C emitted as CO and CO2 is kept constant in both regions. An important next step is to implement these spatial and temporal EF and MCE scenarios into GFED, and quantify regional differences in trace gasses emitted."

67) P23582, L10-12: It seems that pine-oak forests high in the mountains of Mexico could be classified as extratropical? However, the measurements available so far were impacted by urban deposition for some nitrogen-containing gases.

Measurements high in the mountains of Mexico cluster more with other boreal forest measurements, so they could indeed be classified as extratropical. We did not exclude these measurements, since our focus is on CO, CO2, and CH4, and we do not take nitrogen-containing species into account.

68) P23582, L15: A temperate forest biome was added in Akagi et al., (2010).

We added the reference in lines 717-719:

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”For a more specific EF average, it could be helpful to expand the amount of vegetation types, for example by adding a ’temperate forest’ and/or ’chaparral’ biome as in the Akagi et al. (2010) database.”

69) P23582, L16-19: Rather than simply pose the question here, perhaps the authors can address/answer it. For instance, how does the correlation with vegetation type shown in Figure 2 compare to the correlation with FTC and other variables they explored. I think they can at least start to answer this question, which could be a useful additional contribution of the paper.

This comment is related to comment 53 and we cannot properly address this one because we may not fully grasp the question. There are only three vegetation types (savanna, tropical forest, extratropical forest) thus we cannot calculate, for example, the correlation between an EF and the vegetation type. Our main goal was to switch from a discrete classification to a continuous one, introducing spatial and temporal variability, also within vegetation types.

70) P23582, L26: I am not confident that burned area is better known than many EF; and in any case such a statement should be accompanied by references to that effect.

Since this is disputable, and references on the uncertainty of burned area compared to the partitioning of biomass burned are lacking, we decided to delete “such as burned area” in line 742.

71) P23583: L14-17: Excellent point about a standardized, accurate protocol being needed for measurement of fuel and environmental variables and it could be combined with more “boots on the ground” satellite validation (hotspot detection efficiency, burned area accuracy, etc). (“campaigns” is the correct spelling.)

We added a sentence on the importance of satellite validation in lines 760-762:
“This requires a more multi-disciplinary approach and calls for combining campaigns

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aiming to quantify biomass loads, combustion completeness, EFs, and satellite validation of e.g. hotspot detection efficiency and the accuracy of burned area."
In line 761 we changed "campains" into "campaigns."

72) Section 4.6: I would just collapse this into two sentences in the conclusions that ideally we need 1) more EF measurements in off peak months and key, un-sampled geographic areas, 2) more measurements of environmental variables accompanying the field EF measurements, 3) more validation of satellite products.

Following the reviewer's suggestions, we rewrote this part of the conclusion section into (lines 800-811):
"Currently, most of the literature describing emission factor measurements lack a detailed description of the measurement site and ambient conditions during the experiment. This information is crucial to better understand the differences between the various measurements, and be able to understand the representativeness of large-scale satellite data and ambient conditions as done in this study. In addition, to better facilitate our understanding and ability to model MCE or EFs, more EF measurements should be performed in the peak fire months and in unsampled geographic areas. The development of a more uniform sampling protocol for the sampling and measurements of EFs in different vegetation types is another crucial step to better compare different measurements. For example, the database of Akagi et al. (2010), that compiles EFs based on a more uniform and accurate sampling protocol consistently only takes those measurements focusing on fresh smoke into account, better representing initial emissions."

73) L9: Add 10% for uncertainty in fuel C

This is an important factor that may account for part of the remaining variability, and therefore we added this uncertainty in lines 777-780:
"Uncertainties in driver data, the range of fuel C content, differences in measuring tech-

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niques, assumptions on weighting ratios of flaming and smoldering contributions, and insufficient information on the measurements may account for part of the remaining variability."
In section 3.1 (lines 245-247) we also mention the uncertainty in fuel C:
"However, a detailed study of Susott et al. (1996) suggests a global average C fraction for biomass closer to 50%, with a considerable range, which would indicate an additional ~10% uncertainty in addition to other uncertainties."

74) L18: There were some available papers that were not included

Using the EF database of A&M2001-2009, we count 70 different papers of CO, CH4, and CO2 EFs measurements. Lab experiments and papers with a focus on the EF of other trace gasses or aerosols were not taken into account.

75) L21: "uncharted" means "unmapped," "unsampled" would be better.

We changed "uncharted" into "unsampled" (line 806).

76) L25: EF based on a more uniform and accurate sampling protocol were summarized in Akagi et al., (2010).

We refer to the Akagi et al. (2010) paper in our conclusion section, lines 806-811:
"The development of a more uniform sampling protocol for the sampling and measurements of EFs in different vegetation types is another crucial step to better compare different measurements. For example, the database of Akagi et al. (2010), that compiles EFs based on a more uniform and accurate sampling protocol consistently only takes those measurements focusing on fresh smoke into account, better representing initial emissions."

Please also note the supplement to this comment:

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http://www.atmos-chem-phys-discuss.net/10/C14158/2011/acpd-10-C14158-2011-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 23559, 2010.

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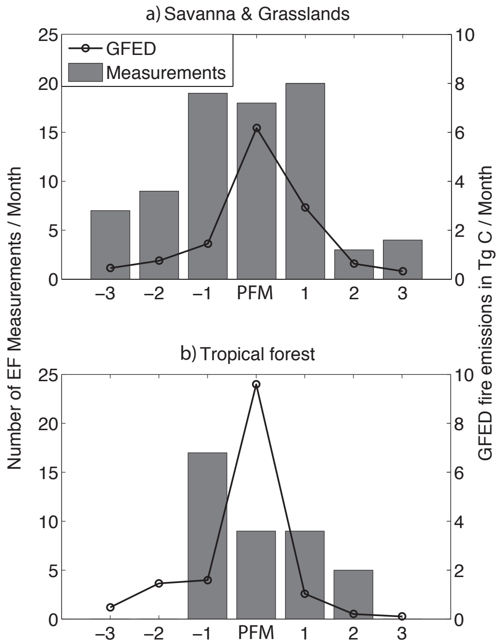

Fig. 1. New Figure 3

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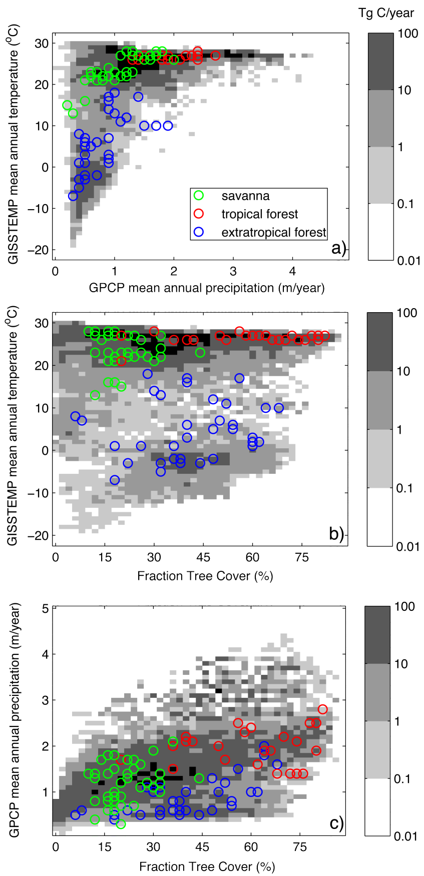

Fig. 2. New Figure 5

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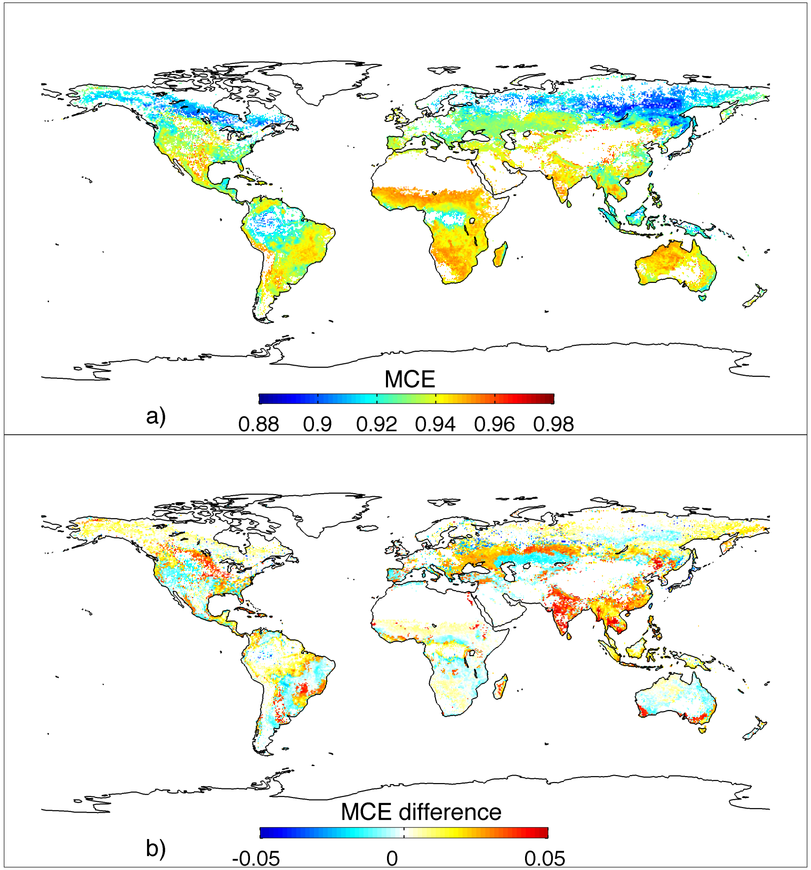

Fig. 3. New Figure 6

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