Authors response to referee comments

O'Shea, S. J., Allen, G., Gallagher, M. W., Bauguitte, S. J.-B., Illingworth, S. M., Le Breton, M., Muller, J. B. A., Percival, C. J., Archibald, A. T., Oram, D. E., Parrington, M., Palmer, P. I., and Lewis, A. C.: Airborne observations of trace gases over boreal Canada during BORTAS: campaign climatology, airmass analysis and enhancement ratios, Atmos. Chem. Phys. Discuss., 13, 14069-14114, doi:10.5194/acpd-13-14069-2013, 2013.

We are very grateful to both referees for their insightful and helpful comments about our manuscript and for recognising the important scientific utility of the study. We have made use of those comments to improve the manuscript in our revision submitted with the response below. We now address the comments individually. For clarity, referees' comments are coloured red and the responses are coloured black.

Referee #1

The paper describes a set of aircraft measurements performed in Eastern Canada aiming at the investigation of biomass burning plumes from boreal forest fires. Biomass burning plumes are identified using a set of trace gases. The presentation of a comprehensive data set makes the paper a valuable scientific contribution, however, the way the data are presented should be improved prior to final publication. Often the data analysis is lacking structure, e.g. when moving between different aspects and concepts of varying complexity rather than taking the reader along from the discussion of measurements to a scientific interpretation using elaborate models and concepts such as for example MCE and NEMR.
In particular, section 3 is in parts poorly organised, switching back and forth between different aspects of data discussion and interpretation. The paper would greatly benefit from reordering the structure of data presentation, e.g. in section 3.1. , 3.3.1., 3.3.2 These sections contain partly technical aspects of data such as different time resolution of data sets and plume identification strategies that mix up with data interpretation.
While the introduction gives appropriate credit to previous reserach on biomass burning, the discussion lacks references to other studies of biomass burning plumes, both at high latitudes and in other regions.

We agree that the structure of Section 3 could be confusing. To this end, it has been extensively re-ordered so that descriptions of analysis techniques are separated from the data interpretation. The description of BB plume identification is now discussed earlier in Section 2.2. The comparison with previous measurements has been given its own separate section (3.2) and all terminology used in the BB case studies (ER/EF/MCE) is now described more appropriately at the beginning of Section 4.

Also, a number of new references have been added to the manuscript, which discuss and recognise previous work on BB in other regions. These include:

Crounse, J. D., DeCarlo, P. F., Blake, D. R., Emmons, L. K., Campos, T. L., Apel, E. C., Clarke, A. D., Weinheimer, A. J., McCabe, D. C., Yokelson, R. J., Jimenez, J. L., and Wennberg, P. O.: Biomass burning and urban air pollution over the Central Mexican Plateau, Atmos. Chem. Phys., 9, 4929-4944, doi:10.5194/acp-9-4929-2009, 2009.

Korontzi, S.,Ward, D. E., Susott, R. A., Yokelson, R. J., Justice, C. O., Hobbs, P. V., Smithwick, E. A. H., and Hao,W. M.: Seasonal variation and ecosystem dependence of emission factors for selected trace gases and PM2.5 for southern African savanna fires, J. Geophys. Res., 108(D24), 4758, doi:10.1029/2003JD003730, 2003.

Mauzerall, D. L., J. A. Logan, D. J. Jacob, B. E. Anderson, D. R. Blake, J. D. Bradshaw, B. Heikes, G. W. Sachse, H. Singh, and B. Talbot, Photochemistry in biomass burning plumes and implications for tropospheric ozone over the tropical South Atlantic, J. Geophys. Res., 103(D7),8401–8423, doi:10.1029/97JD02612, 1998.

Wooster, M. J., Freeborn, P. H., Archibald, S., Oppenheimer, C., Roberts, G. J., Smith, T. E. L., Govender, N., Burton, M., and Palumbo, I.: Field determination of biomass burning emission ratios and factors via open-path FTIR spectroscopy and fire radiative power assessment: headfire, backfire and residual smouldering combustion in African savannahs, Atmos. Chem. Phys., 11, 11591-11615, doi:10.5194/acp-11-11591-2011, 2011.

Yokelson, R. J., I. T. Bertschi, T. J. Christian, P. V. Hobbs, D. E. Ward, and W. M. Hao, Trace gas measurements in nascent, aged, and cloud-processed smoke from African savanna fires by airborne Fourier transform infrared spectroscopy (AFTIR), J. Geophys. Res., 108, 8478, D13, doi:10.1029/2002JD002322, 2003.

Yokelson, R. J., Christian, T. J., Karl, T. G., and Guenther, A.: The tropical forest and fire emissions experiment: laboratory fire measurements and synthesis of campaign data, Atmos. Chem. Phys., 8, 3509-3527, doi:10.5194/acp-8-3509-2008, 2008.

Yokelson, R. J., Crounse, J. D., DeCarlo, P. F., Karl, T., Urbanski, S., Atlas, E., Campos, T., Shinozuka, Y., Kapustin, V., Clarke, A. D., Weinheimer, A., Knapp, D. J., Montzka, D. D., Holloway, J., Weibring, P., Flocke, F., Zheng, W., Toohey, D., Wennberg, P. O., Wiedinmyer, C., Mauldin, L., Fried, A., Richter, D., Walega, J., Jimenez, J. L., Adachi, K., Buseck, P. R., Hall, S. R., and Shetter, R.: Emissions from biomass burning in the Yucatan, Atmos. Chem. Phys., 9, 5785-5812, doi:10.5194/acp-9-5785-2009, 2009.

Yokelson, R. J., Andreae, M. O., and Akagi, S. K.: Pitfalls with the use of enhancement ratios or normalized excess mixing ratios measured in plumes to characterize pollution sources and aging, Atmos. Meas. Tech., 6, 2155-2158, doi:10.5194/amt-6-2155-2013, 2013.

Specific comments

Figures

In the current version, labels and annotations in most figures are barely readable. For final production please enlarge font size. Figure 1 has a unpleasant aspect ratio of height and width, it looks rather distorted. Consider colorcoding with a tracer mixing ratio. It might enhance understanding to refer to this figure later in the text more often when discussing individual flights. Since it is stated several times in the text that data from the transfer flights was not included in the analyses, these flight tracks can be omitted. Showing only the campaign region will make it easier to distinguish individual flights.

The size of all figures has been increased. This will be checked again at the production phase and we ask that the copy editor advise us again on this at that stage. Also, the transit flights have been excluded from Figure 1 to improve the coordinate scaling and clarity of the key Canadian area flights and a further plot has been added that has been colour-coded for CO concentration, as a marker to visualize spatial sampling of BB-influenced airmasses. References to this Figure have been added where needed throughout the text.

Abstract

ACP allows paragraphs in abstracts, and I suggest tomake use of this to structure the abstract.

We agree and have also shortened the abstract by removing the mean CO_2 and CH_4 campaign-climatology concentrations, which as reviewer 1 also notes, need accompanying explanation, which can only be provided in the body of the text (section 3).

P 14071, L 21 change "By examining individual case studies" to e.g. "Examining individual cases".

Agreed. This has been changed.

See also detailed comments on text, in particular on section Large Scale Distribution.

Introduction

P 14072, L 18–21 This statement mixes the inherent episodic character of biomass burning events with the difficulties of complete monitoring. "Coupled" is not a good choice of word here as it implies an interaction.

Agreed. This has been removed.

P 14072, L 24/25 "Fires ... composition". This wording does not make any sense to me. What is meant by "Fires ... attributed ... to changes". Fire to be the cause of observed changes? Or atmospheric changes to cause fires? Please clarify.

We were suggesting that fires are the cause of the observed changes. This has been clarified.

P 14073, L 12 emission - emissions ?

Agreed. This has been changed.

Aircraft Sampling

P 14074, L 22/23 "based out of" appears an odd choice of word; "based in" or "operated out of"

This has been changed.

P 14075, L 22/23 What were the actual mixing ratios of the used standards? Please identify the respective scales (WMO-X2007 and NOAA 2004 ?).

In the manuscript we direct the reader to O'Shea et al. (2013), which provides an extensive description of the calibration standards used. However the manuscript has been updated to include the name of the calibration scales here for completeness.

Analysis

P 14076, L 1/2 What do the given percentages refer to? Total Emissions?

Yes, they refer to total emissions. This has been clarified in the revised manuscript.

P 14076, L 12–16 How did you deal with the PTRMS data that as stated above has a time resolution f 9–20 s when merging the data, especially for the 3-merges?

PTRMS data was not used in the 3 s data merge. This is now explicitly mentioned in the revised manuscript.

P 14076, L 23 ff A complicate argument for an obvious fact.

Agreed. This discussion has been shortened.

Large scale distribution

This subsection is not well organised and therefore difficult to follow for a reader not familiar with the data analysis procedure. The heading implies an overall discussion of the data but the section mixes bulk statistics with plume identification. I suggest to split it into two parts, separating the bulk statistics and comparison with published data from other projects from the issue of plume analysis.

Agreed. The comparison with previous projects is now in a separate section (3.2).

In general, I think, there is not much information in mean values of all the data (also applies to numbers given in abstract). It was described above how the PBL differs from the free troposphere, but mean values for only the PBL are not given. Averaging all altitude levels does not consider the observed changes in the PBL, and it does not take into account the frequency at which individual altitude levels were probed, nor the altitude distribution of plume encounters. Thus, the comparison with previous campaigns are rather meaningless.

We agree with this to some extent. It is not possible to convey the necessary accompanying discussion and context to these mean values in the abstract and they are therefore removed there. We have added further discussion of the campaign climatology and its utility in Section 3. We feel that the campaign climatology is a useful statistic/result to include, in line with those reported for other studies and it is useful to modellers etc who may wish to use broader process-independent representative data. We now discuss the differences between the PBL and free troposphere and the nature of any outliers and how these influence the climatology.

The data presentations lacks a discussion of the spatial plume distribution. Where and at which altitudes were plumes probed? Was ojn any occasion one plume probed several times at different altitudes or at different ages to document plume aging? In Figure 2 outliers on the high end show up in all altitude bins. Is there a layer where plumes occur more often than in others?

We now describe that regions of enhanced CO, CH_3CN and HCN were distributed relatively evenly across all altitude bins, this is shown in Figure 2. The plume locations are now shown more clearly in the map in figure 1 and this is described in detail in section 4. Also now described in section 4 is the occasion when a single plume was sampled multiple times at different altitudes.

It was stated above that two different merges of the data were produced, but it is not always clear which one was used in a particular step of the analysis.

This is now described in Section 2.2.

P 14078, L 10–18 Comparison of station and ground data are difficult in general. Comparing free tropospheric altitude data to a mountain background station may be useful to do, the same applies for selected low altitude flight data and ground stations. Comparing the ground station data to the mean value of all altitude does not give meaningful information unless explained in more detail. How do PBL values measured from aboard the aircraft compare to Sable Island mean vales?

We have now included a comparison with the BORTAS PBL values as well.

P 14078, L 21 ff Did ARCTAS and ABLE-3B cover a similar altitude range? At which altitude were plumes encountered in this study and the previous ones?

All three projects covered a similar altitude range and this is now explicitly mentioned in the revised manuscript. Also the three projects sampled biomass burning plumes throughout the troposphere. The large sample and data distribution from the 3 projects provide only a statistical overview useful to modellers etc and we are clear that this is all that can be taken from such an aggregate dataset. The transient and variable nature of individual fire plumes (across 3 campaigns) mean that like-for-like sampling frequency at exactly the same altitudes and locations would not be of any additional advantage and our statistics are instructive in the climatological sense only as we point out.

P 14079, L 9–14 Move this paragraph to the above discussion of figure 2.

Agreed. This paragraph has been moved.

P 14079, L 15–28 This paragraph does not belong here, it would only make sense if the data was presented without the plume encounters. Consider moving all aspects of plume identification to a separate subsection or to the methodology section 2.2

We agree and have moved this paragraph to section 2.2

P 14080, L 7 Correlation of what with CO? CO2 and CH4?

This has been clarified: "CO_2:CO R^2= 0.09 and CH_4:CO R^2= 0.12".

P 14080, L 12-14 "The flights ... influenced by BB" - This is a very important, though also very general statement and should be moved up.

We agree and have moved it towards the start of the section.

P 14080, L 15 Which merge (10 s or 3 s) was used for this?

As is mentioned in section 2.2 a 10 s data merge is used for the bulk statistics.

P 14080, L 19/20 Why is this correlation not shown in Figure 3? Consider replacing panels a and b by the respective ones showing the data after removing plumes.

Since out scatter plot in Fig. 3 has been colour coded by CH_3CN we feel that it is relatively easy for the reader to identify the airmasses in our sample that haven't been influenced by BB.

P 14080, L 20 orthogonal regression or least square?

We now make clear in section 3.3 that all regression slopes are orthogonal distance regressions.

P 14081, L 4–6 Not clear what this sentence means, please reword.

Agreed. This has been reworded to:

"However, C_2H_6:CH_4 correlation for background measurements was noted to range widely flight-by-flight with a peak of 0.95 during flight B624 (for 8 samples) and a minimum of 0.05 for flight B622 (for 5 samples)."

P 14081, L 8–13 How representative were the samples of the plumes? Were plumes represented to a similar extent as in the high resolution data set? Especially narrow plumes might me missed when taking flask samples. Please specify what you mean by "specific but irregular". How were the times/places for sample collection determined. Was sampling automated or manually?

This has been clarified in the revised text. Samples, locations and times were arbitrarily chosen based on real time measurements to reflect both plume and background airmasses.

P 14081, L 24 What does near-field mean here in terms of distance to sources and age of the plumes?

Based on the fact that the fires were directly overflown within the boundary layer we estimate that plumes during this flight were less than 1 hour old (since emission). This is now made clear in the revised manuscript.

MACC comparison

P 14083, L 10 The altitude binning in Figure 4 is different from Figure 2. Please comment on the choice of altitude bins.

The BORTAS dataset is binned to the MACC model altitude levels. This is now explicitly mentioned in the revised manuscript.

ER an EF

P 14083, L 26 Skip first sentence, using emission ratios and factor is common practice for this type of data analysis.

Agreed. This has been removed.

P 14084, L 15 I don't think NEMR is a widely used concept, therefore explain.

A NEMR or enhancement ratio for a species X is equal to the regression slope of the measurements of X in a plume versus the simultaneous measurement of a tracer species. In aged plumes this may not be equal to the emission ratio. We note this to be a commonly used term in the literature cited. However, in the revised manuscript we have expanded its description and added several new references where it has been used.

Near-field

P 14085, L 12 How old and how distant from the region of origin is "fresh"?

This information is now included in the manuscript. This flight consisted of boundary layer flying directly over the fires. The sampled plumes were estimated to be less than 1 hour old.

P 14085, L 20 ff The issue of data availability on different time scaled needs to be discussed above in the context of data merging. The method presented here seems to be a compromise to deal with the lower resolution PTRMS data. Was it only used for the data for that one flight? How much better are the results with the first method?

In section 2.2 we note that PTRMS data is not used in the 3 s data merge due to its relatively slow times response. As we describe it Section 4.1 we examine the sensitivity of calculated the EFs to the definition of the plume used. Using the methods we tried the calculated EF did not vary by more than 10 %.

P 14086, L 10 "with **a** standard deviation"

Thank you. This has been changed.

P 14086, L 12/13 A lack of measurements can hardly have a meaning. Change to "Due to a lack of measurements the distribution . is not well known"

Agreed. This has been changed.

P 14086, L 20 What do you mean by "grouping" ? Using just the point marked red in the figure?

This means that we calculate a single EF/ER using all the enhancements that were observed during low level flying. This has been rephrased in the text. Only red points designated as BB were used in the calculation. The text now reads:

“Since the flight traversed across a small region several times we include all BB plume measurements to calculate a single ER/EF per species for this period.”

P 14086 L 17) comment on the "blue" outlier at about 400 of CO ?

Both CO and HCN were used to determine whether sampling was in a BB plume. If there was a missing data point for either of these species the corresponding CH_4/CO_2 data point was not used in the ER/EF calculation. We believe that this a minor point and have not altered the text.

P 14086 L 24) Discuss the "good agreement" in more detail, especially the difference between fresh and aged plume. EF for CO and CH4 for fresh plumes are markedly different from literature values. Does this allow conclusion on the fire characteristics the burning phase fires were in, ...

The EF's given in Table 2 are averaged literature values, which are comparable with this study. The CO_2 EF's are all in agreement within reported uncertainty. As mentioned in our manuscript "van Leeuwen and van der Werf (2011) grouped CH_4 EFs in the literature based on biome and the fires MCE. The EF calculated for B626 shows an excellent agreement with the amount of CH_4 emitted for a fire with its combustion efficiency as predicted from their work."

P 14086 L 26) Which threshold do you refer to here?

This is the threshold used to classify whether sampling in is plume. This has been rephrased in the revised manuscript.

P 14087 Is there a relation between the determined ER/EF and plume age and the MCE given in the table.

The relationship between EF and MCE is now discussed in section 4.2. In section 3.1 we state that fire activity was reduced when sampling in the near field and therefore a lower MCE could be expected. The following paragraph has been added:

"For all CH_4 EFs we find a regression slope of EF =-47 x MCE + 47 (R^2=0.54). A similar relationship was found by several previous studies, such as Yokelson et al. (2008) for fires in a tropical forest (EF = −47×MCE+49, R^2= 0.72), Yokelson et al. (2003) for savanna fires (EF = −49×MCE+48, R^2= 0.86) and Korontzi et al. (2003) also for savannah fires (EF = −48×MCE + 47, R^2= 0.88). However, van Leeuwen and van der Werf (2011), who synthesised literature values, found a steeper relationship for extra-tropical forests (EF = −59.992×MCE + 60.967) though with weaker correlation (R^2=0.27)."

Please also see our response to the question on P 14091 L 4–7.

P 14087 The MCE concept is applied to all plumes and should therefore not be part of the discussion of the plume on 26 July, but be moved to a more general section of the text.

Agreed. This has been moved to the beginning of Section 4.

Far-field

Give a brief summary on NEMR before using the concept so intensively. The NEMR discussion is mixed up with the trajectory analysis.

This is described in the beginning of section 4. This has now been re-ordered so that a description of the plume and the trajectory analysis is instead given before the NEMR discussion.

P 14088 L 17 ff / Figure 6 It's difficult to see details in the trajectory plots. Are the trajectories shown for the complete flight or only for those section that have been identified to be within a plume?

The trajectories are for the whole flight (plume and background). The plots have been enlarged. This will be checked again at the production phase.

Figure 7 a A discussion of the CO2 time series seems to be lacking. Why do the low CO2 mixing ratios not occur during the low altitude flight sections? The second low CO2 phase corresponds to high CH4 but lower CO and HCN inside the plume. Did the aircraft fly in an out of the plume? Maybe use shading in the Figure to illustrate which parts of the flight were identified as being inside plumes and which not.

We agree and have implemented the reviewer's suggestion regarding using shading to illustrate in plume sampling. In the text we now state that "Runs were repeatedly performed into and out of the same plume at different altitudes (Figure 1)." We also include the following sentences about the CO_2 time series:

"Correlation between CO_2 and CO is very weak (R^2=0.03). This suggests that the majority of the BB CO_2 signal for this plume has been removed during transport due to a combination of biospheric uptake and mixing with other airmasses"

Figure 8 Labels a,b,c,d are difficult to see in the figure, move outside maps. In Figure 6 labelling was clock-wise, here it's counter clock-wise.

Labels have now been enlarged for all plots. The clarity of the plots will be checked again at the production phase.

P 14089 L 6–8 Unclear grammar, please reword.

This has been reworded to:

"NAME column integrated sensitivity plots are shown in Fig. 8. These show airmasses that were associated with BB passed over the same fire region in northwest Ontario as indicated by the HYSPLIT trajectories (Fig. 6 and 7)."

P 14089 L 6–8 NAME is driven by HYSPLIT trajectories, so isn't it by default that it traces back the airmasses to the same region of origin as HYSPLIT does?

NAME is not driven by HYSPLIT. NAME and HYPLIT use different (yet highly similar) meteorological wind data - UM and NCEP, respectively. Also, although both models are Lagrangian, NAME is dispersive and HYSPLIT is not. The former gives insight into mixing over the airmass history and the latter does not. Using the two methods allows us to make more informed discussion of airmass history and influence than either would alone.

P 14089 L 10 "However **,** ..." – general comment: in many longer sentences a lack of commas makes it tedious to extract information.

Agreed. This has been changed.

P 14089 L 12–14 In Figure 8b the maximum sensitivy seem to be south east of the active fires detected by MODIS. However, according to the color scale (which is difficult to see give the size of the figure) there still seems to be significant sensitivity in that region. Is it possible to illustrate the better agreement for case 8d by quoting values of the sensitivity footprint for the region of interest in comparison to 8b? I can't follow the later statement that this indicates that NAME points to "more distant" sources. Looking at the sensitivity map it rather seems to point to sources more close to the region where the aircraft was operated.

We agree that "a more distant" source is confusing, we have changed this to read:

"For B622, NAME guides us well in the surface attribution of BB sources, while for B623, if we were not otherwise expectant of dynamics such as active pyroconvection (Gonzi and Palmer, 2010; Glatthor et al, 2013), then NAME would suggest a non-combustion surface source."

As suggested we also now quote values of the sensitivity footprint:

"For B622 (Fig 8d), there is a footprint in close proximity to the fires detected by MODIS (relative sensitivity to North Ontario fires = 1.9 %). However, for B623 (Fig. 8b), there is little evident surface contact over the 5-day history in regions with significant fire activity (relative sensitivity to North Ontario fires = 0.1 %)."

Figure 9 Labels a,b,c missing.

They have now been included.

Figure 9 According to the text the figure provides a comparison betwen IASI and BORTAS data. According to the caption all three panels show IASI data. Please clarify. I suggest to include aircraft tracks in these figure.

Figure 9 only shows IASI data. We would prefer not to include the flight track since this would obscure the IASI data and the flight track is already been given in Figure 1, where it is more easily viewed.

P 14090 L 10–23 These two paragraphs contain introductory information for this section and should be moved up. Without illustration the descriptions of in and out of plumes are difficult to follow.

Agreed. These paragraphs have now been moved to Section 3.2.2. Flight tracks are also shown in Figure 1 and are more visible now that the transits have been removed from the plot.

P 14090 L 24 Such basic data treatment issues should be discussed prior to going on to more sophisticated interpretation tools such as trajectory analysis.

This has now been moved to the start of the section before the trajectory analysis.

P 14090 L 26 There has been no information yet about plume altitude during the campaign. How did you do the partitioning here?

In the previous two paragraphs we now give altitudes for the plumes and these are also shown in Fig. 2 and now Fig.1 as well. The partitioning was performed by selecting each period when there was no change in altitude by the FAAM BAe-146. These periods were then analysed separately.

P 14090 L 28 Check table reference.

This should be table 2

What is meant by "runs and profiles"?

Runs are periods when the aircraft does not change altitude and profiles are when there is a change in altitude. This has been added to the description of Table 2.

Again, the discussion switches back to an issues that was in part discussed above. Please streamline and discuss the table, including the difference between the fresh plume and the aged plumes in one place.

The paragraph now follows on directly from the previous discussion.

P 14091 L 4–7 higher MCE/higher flaming contribution in aged plumes – is this by chance or is there a cause for his?

This is most likely due to the time period when the plumes originated. In section 3.1 we state that overall fire activity was reduced when sampling in the near field and therefore a lower MCE could be expected as the fires end. The following sentences have been added to the text:

"However, as mentioned in Sect. 3.1, the aged plumes originated from a period of younger fires and more intense activity. Such fires would typically have a higher MCE (e.g. Wooster et al, 2011), which is representative of a larger proportion of flaming combustion and therefore a lower proportion of reduced compounds, such as CH_4."

P 14091 L 13 What do you mean by "straight-and-level run"

We realise this may be confusing to those not involved in aircraft sampling. This has been removed.

P 14092 L 17/18 ? Those airmasses that spent the most time over the HBL (50 hours, large blue circles) have lower CH4 mixing ratios.

We are suggesting that this is likely to be a mixed (to some degree) air mass of contrasting sources, not purely containing wetland CH_4 or biomass burning CH_4 as it is not observed to be correlated with other BB tracers As such concentrations cannot be determined with confidence using a single variable

P 14092 L 28 Check Figure reference.

This has been changed to 10b.

Referee #2

1. Summary
This paper presents a detailed analysis of several flights during the BORTAS experiment dedicated to the study of boreal forest fires in summer 2011. I have retained three major conclusions for the paper. First, the observed CH4 and CO2 concentrations are comparable with observations made during previous campaigns in the same region/season. Second, emission factors for CO and CH4 based on observations in a fresh sampled plume are higher than the values given in the literature. Third, the calculation of emission factors based on aged plume can lead to important biases and requires a careful discrimination of the biomass burning origin.

2. Overall evaluation and recommendations
This paper is an interesting contribution to the limitations of the current methodologies used to derive emission factors of biomass burning chemical species. The paper is well written. The methodology is sound. The figures are clear and concise.

We thank the reviewer for their insightful comments and for recognising the scientific significance of our study.

3. Details supporting the evaluation

Could the authors provide a recommendation to the modelers concerning the emission factors of CO2, CH4, CO? Table 2 shows higher EF for CH4 and CO then the literature.

As we discuss in the manuscript, biomass burning EFs show a wide degree of spatio-temporal variability as might be expected. We cannot claim that the EF we calculate is the only one that should be used in chemical transport models. Rather it should be added to the existing literature so that true variability and controlling factors can be better understood by end users of the outputs of this research.

I would recommend that the authors list the studied chemical species in the abstract. The sentence "and other biomass burning tracers and related trace gases" does not reflect the contents of the paper which only illustrates and discusses CO, CH4, CO2, HCN, CH3CN.

We agree and have changed this in the revised manuscript.

The legend in Figure 4 needs to be completed (what do boxes / lines represent ?)

This information has been added to the figure description.

In Figure 4, why are they less model levels for CO2 than for CO or CH4?

The CO_2 and CH_4 MACC values were determined using different versions of the MACC model, so as to correct for a known bias in the MACC CO_2 data. One of the differences between the models was the number of atmospheric pressure levels on which it was run, hence the differnce between the model levels. This has been made more explicit in the text.'

The fraction of C in the fuel is taken equal to 500g/kg. The authors may indicate that this value is only valid for boreal forests.

This has been included in the revised manuscript.

5-days backtrajectories in Figure 7b shows trajectories arriving from the boundary layer in the US south-east sector (30/40_N, -90/-80_W) where active fires were detected. These trajectories are not anymore in Figure 7c, which, if I understand it correctly, indicates that the associated flights points did not exhibit enhanced CO and HCN. If the trajectories are correct, the aircraft should have sampled these fires plumes. Could the authors comment on that point in terms of trajectory uncertainties or discrimination using CO/HCN tracers?

This is a careful and correct insight. Our sampling of air corresponding to trajectories arriving from the SE USA did not meet our thresholds for defining a BB plume and we cannot and should not therefore include them in our analysis. We postulate that this was because the fires seen in MODIS in the SE USA were not predominantly BB fires and may be hot point sources along the Eastern Seaboard such as large power stations etc, which have been noted elsewhere as an artefact of MODIS remote sensing. The low brightness temperature of those fires seen in MODIS is consistent with this postulation. We have added this discussion to the text.

Figure 8: Would it be possible to extract the legend bars from the figures and put them on the right side of the panel plot?

This has been done. We have also increased the size of labels and annotations for all figures as suggested by referee 1.

P14090: Table 1 ! Table 2

This has been changed.

p14091: Fig 9b ! Fig10b

This has been changed.