

## ***Interactive comment on “Field measurements of trace gases and aerosols emitted by peat fires in Central Kalimantan, Indonesia during the 2015 El Niño” by Chelsea E. Stockwell et al.***

**Chelsea E. Stockwell et al.**

bob.yokelson@umontana.edu

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Response to Referee #2

We thank the Referee for their encouraging assessment and constructive suggestions, which will improve the paper. The Referee comments are reproduced below followed by our detailed response.

Anonymous Referee #2

Review of “Field measurements of trace gases and aerosols emitted by peat fires in Central Kalimantan, Indonesia during the 2015 El Niño”, by C. E. Stockwell et al., 2016. The manuscript by Stockwell et al. presents measurements from 2015 peatland fires

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in the Indonesian province Kalimantan. The findings presented in this manuscript both add to and modify previous lab-based measurements of peat combustion, amending a handful of key EFs that were previously only available from laboratory studies, while confirming the validity of laboratory studies for estimating EFs for species that are not easily measured in the field. The paper is well-written, cohesive and thorough, and my assessment is that it merits publication in ACP after the following issues are addressed.

Specific Comments

**R2.1:** Page 2, line 19 – “(2012)” - is that a reference? It should be included properly here.

**Authors:** We found “(2012)” on P2, L8. The year of the recent measurements was included because our field data did not agree well with the lab measurements made in 2001 that IPCC uses, but did agree well with lab measurements made in 2012. It's probably safe to delete the year and we have done so.

**R2.2:** Page 4, line 32 and Page 19, line 10 – I don't think it's necessary to put “in preparation” here – it is included in the reference itself. However, please include a full reference for this work if possible, including a full author list and title.

**Authors:** We removed “in preparation” throughout and updated the reference as suggested.

**R2.3:** Page 5, lines 11-12 – please explain what  $n=1$  is in reference to, or simply state, if this is the case, that there was only a single sample of each type analyzed in the lab study.

**Authors:** Within both parentheses we changed “ $n=1$ ” to “one sample”

**R2.4:** Page 5, line 33 – it would be helpful to the reader to direct them to Table S1 at this point, rather than making them wait to find out about the table until the next page.

**Authors:** Good suggestion and we now call out Table S1 earlier on P5, L27.

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**R2.5:** Page 6, lines 4-25 – a map/diagram of the sites would help put this entire sampling description into context.

**Authors:** A site map has now been provided as Supplementary Figure 2 (now referenced on P6 L4).

**R2.6:** Page 12, lines 20-25 – where are the ERs if I want to look at them? A lot of this discussion is very qualitative and vague (i.e. “seven of these nine cases agree: : :” – what about the other two? And what were they? It would seem reasonable to spell that out or offer something specific about the differences between the FTIR and the WAS sample that would alleviate the reader’s concern that there is something we should know about the alleged differences. I understand that the analysis were not set up to evaluate differences, and yet to just allude to it but not give us anything further is more suspicious.

**Authors:** This is an excellent comment and we considered adding this comparison to the supplement in response. For background, we had two WAS cans that the field notes indicated were filled from the FTIR cell. While changes could theoretically occur to some species during storage in the FTIR cell, this seemed like a valid opportunity to compare the data for the overlap species fairly directly. Unfortunately, in attempting to further clarify this comparison, we have now realized that the FTIR cell was actually re-filled after the FTIR measurements and before the cans were filled. Thus, the comparison included an unknown contribution from natural plume variability and was not semi-rigorous after all so we have deleted this text. Ideally we would be able to compare overlapping techniques at least semi-rigorously and often we have in previous studies. For instance, most recently in Hatch et al. (2016), the WAS vs FTIR slope for overlap species that were nearly the same as in this study was  $1.01 \pm 0.001$ ,  $r^2 = 1.0$ . Unfortunately, the resources we could import to the field were limited and we did not perform conclusive tests. We have made the following changes to the text.

Old text: “There were nine instances when the same gas was measured by both WAS

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and FTIR in nearly the same place and seven of these nine cases agree within the combined uncertainty. The other two cases are less close, but this experiment was not well-designed for comparison. We have noted excellent WAS/FTIR agreement previously under more rigorous, but drier conditions (Christian et al., 2003) and we found that these 2015 field WAS results compared well with on-line measurements during FLAME-4 peat fire sampling for many major species as discussed later in the paper.”

New text: “This experiment was not well-designed for comparison, but we have noted excellent WAS/FTIR agreement previously under more rigorous, but drier conditions (e.g. Christian et al., 2003; Hatch et al., 2016) and we found that these 2015 field WAS results compared well with on-line measurements during FLAME-4 peat fire sampling for many major species as discussed later in the paper.”

Hatch, L. E., Yokelson, R. J., Stockwell, C. E., Veres, P. R., Simpson, I. J., Blake, D. R., Orlando, J. J., and Barsanti, K. C.: Multi-instrument comparison and compilation of non-methane organic gas emissions from biomass burning and implications for smoke-derived secondary organic aerosol precursors, *Atmos. Chem. Phys. Discuss.*, doi:10.5194/acp-2016-598, in review, 2016.

**R2.7a:** Page 12, lines 33-35 – many of the uncertainties are unreasonably precise – i.e.,  $0.867 \pm 0.479$  and  $0.860 \pm 0.433$ . Please round these to make them more reasonable for reporting.

**Authors:** We have rounded uncertainties in cases where the level of significance did not match (e.g. propylene  $1.07 \pm 0.531$  now changed to  $1.07 \pm 0.53$ ). However we like to report our results with any potentially useful amount of digits for several reasons that include minimizing round-off error if the number is used by others. The variability should be propagated for all terms used in any calculations done with these numbers and reported along with the result to avoid misleading uncertainties.

**R2.7b:** Page 13, lines 2-3 the “overlap” isn’t very surprising, considering that the range

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in your work is from 0.3 to 1.44. Overlap isn't hard, and likely shouldn't be emphasized like this. "Are consistent", perhaps.

**Authors:** We did not mean to imply overlap between a variable data set and a single point is difficult, but it is a typical reality check in comparing sets of data. A lack of overlap would imply serious questions about the relevance. We changed: "which overlaps with" to "roughly consistent with" – hopefully the point that significant emissions of large alkanes likely occur from real peat fires is made.

**R2.8:** Page 13, lines 10-15 – I'm not fond of the idea of alluding to something that should be done, and then just saying "we haven't attempted this yet." Why bring it up? Or why not attempt to include the analysis here?

**Authors:** Referee #1 also commented on this. Our thought in including this was that the reader would wonder if peat characteristics correlated with emissions and that therefore mapping peat characteristics would improve estimates. We wanted to point out that we don't see a way forward. However, as the Referee points out we can just delete it this text, which is what we have done.

**R2.9:** Page 14, lines 5-16 – I have issues with this plot, and with the implication that the overlap in time is so fortuitously going to take something with 7 points, eliminate 3, and leave you with a four point plot that has an  $r^2$  of 0.674, and that you're going to give it any actual credence. I don't think you "confirmed" the MAC near 0.1 at all. You just eliminated points until the remainder of your points came slightly close to giving you a line. This either needs far more justification, or it shouldn't be included.

**Authors:** Figure 3 shows that the emissions may change with time so looking at the ratio of absorption to mass from data collected at the exact same time is important to consider and gives the value from the plot of  $0.071 \pm 0.03$ . If we relax our time-overlap restriction to tap into more data and compare all data as a straight average (a plot would have low  $r^2$ ) we get nearly the same number  $0.09 \pm 0.08$ . These values overlap within uncertainty and because the mass measurement has a larger size cut-off, both

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approaches imply a MAC value "near 0.1" – note that in this case we restrict ourselves to one significant figure as we are not quoting a measured uncertainty. As with the MSE estimate, we get similar values using just the perfectly overlapped data as we do with using all the data. To remind the reader why we considered temporal overlap in a dynamic environment we made the following change:

Old text: "If instead we plot  $EF B_{abs-405}$  versus  $EF OC$  just for the four plumes sampled over the exact same time period (but different size cutoffs; blue points in Fig. 6) we get a slope of  $0.071 \pm 0.03 \text{ m}^2/\text{g}$ ."

New text: "Keeping the dynamic nature of the emissions chemistry shown in Fig. 3 in mind, if we restrict our analysis to the same four plumes where sample timing was identical (but different size cutoffs; blue points in Fig. 6) and plot  $EF B_{abs-405}$  versus  $EF OC$  we get a slope of  $0.071 \pm 0.03 \text{ m}^2/\text{g}$ ."

**R2.10:** Page 14, line 36 – please comment on the differences between the Liu et al. paper using SSA 781 and you using SSA 870 nm, and what kind of linearity you expect for the two different locations, and how that affects your comparison. Also, this comparison of the observed aerosol parameters with literature values would benefit from having a table like S3 included.

**Authors:** At both 781 and 870, the absorption by BrC should be very small so the high SSA is consistent with minimal BC emissions. The amount of data available to compare here is limited and so we have elected to present this in the text rather than adding a very small table in the supplement. We did reformat the text to make it easier to read.

Old text: "Turning to optical properties, Liu et al. (2014) reported some SSA values and the AAE for smoldering Kalimantan peat (Fire 114) from FLAME-4: MCE (0.74), SSA 405 (0.94), SSA 781 (1.00), and AAE (6.06). These are very consistent with our data (Table 2) and especially with our lowest MCE field sample: MCE (0.726), SSA 405 (0.941), SSA 870 (0.997), and AAE (6.23)."

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New text: “Turning to optical properties, Liu et al. (2014) reported some SSA values and the AAE for smoldering Kalimantan peat (Fire 114) from FLAME-4: MCE (0.74), AAE (6.06), SSA-405 (0.94), and SSA-781 (1.00). These are very consistent with our data (Table 2) and especially with our lowest MCE field sample: MCE (0.726), AAE (6.23), SSA-405 (0.941), and SSA-870 (0.997).”

The requested comment on the comparison was added a few lines further down: “BrC absorption is very small at both 781 and 870 nm so the high SSA at the long wavelengths in both studies and similar AAEs are consistent with minimal BC absorption and dominant absorption by BrC.”

**R2.11:** Page 15, line 36 – “and other factors” is very vague. Please expound.

Authors: Quantifying the amount of fire emissions is extremely challenging, especially for peat fires and in SE Asia, as discussed at length in the cited reference by Reid et al. In bottom-up approaches, fires and burned area are missed due to cloud cover, which approaches 90% on average in SE Asia and also due to an aggressive “cloud mask” that rejects smoky pixels. MODIS only scans areas near the equator 2 out of every 3 days and at the edges of the scans the resolution is degraded to about 6 km, which misses numerous small, smoldering peat fires even if clouds (and canopy) don’t interfere. In top-down (inverse modeling) approaches, fires and burned area can be underestimated due to the same factors above and general uncertainty is added due to problems such as uncertainty in air mass factors, emission factors, smoke injection altitude, meteorology, and the evolution of species used as constraints. Thus, most often the initial amount of fire emissions in inventories needs to be increased in models by factors of ~1.5-10 to match observations. Some of many examples of this adjustment procedure include Lu and Sokolik (2013), Reddington et al. (2016), and extensive references cited within these papers. Rather than a long digression in the paper, we have edited this sentence and added more references showing that a-priori fire emissions are usually too low.

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Old text: On the other hand, burned area is likely underestimated in inventories since they rely on remote sensing data that misses hotspots, burned area, and the fire products used in top-down approaches. This is due to high regional cloud cover; orbital gaps; rapid green-up, which is strongly associated with shallow burn depth (Cypert, 1961; Kotze, 2013); and other factors (Reid et al., 2013). Thus, overestimating burn depth and underestimating burned area tend to cancel.

New text: On the other hand, burned area is likely underestimated in inventories since they rely on remote sensing data that misses some of the hotspots and burned area used in bottom-up estimates, as well as some of the fire products (e.g. CO, aerosol) used in top-down approaches. The information gap is caused by high regional cloud cover; orbital gaps; rapid growth of new vegetation, which is strongly associated with shallow burn depth (Cypert, 1961; Kotze, 2013); and other factors (Lu and Sokolik, 2013; Reddington et al., 2016; Reid et al., 2013). Thus, overestimating burn depth and underestimating burned area tend to cancel when coupling these terms to estimate fuel consumption.

Lu, Z., and Sokolik, I. N.: The effect of smoke emission amount on changes in cloud properties and precipitation: A case study of Canadian boreal wildfires of 2007, *J. Geophys. Res.*, 118, 11777–11793, doi:10.1002/2013JD019860, 2013.

Reddington, C. L., Spracklen, D. V., Artaxo, P., Ridley, D., Rizzo, L. V., and Arana, A.: Analysis of particulate emissions from tropical biomass burning using a global aerosol model and long-term surface observations, *Atmos. Chem. Phys. Discuss.*, doi:10.5194/acp-2015-967, in review, 2016.

**R2.12:** Page 16, line 13 – you “interpolated” between two points to find something. How did you do this? Was it linear? Why? How do you know?

**Authors:** We don’t have any data or theory to support anything other than a simple linear interpolation and we now specify that this was what we did.

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Old text: This is close to our 530 nm value if we interpolate between 870 and 405 nm (0.981).

New text: We can estimate an SSA at 530 nm by linear interpolation between 870 and 405 nm and obtain a similar value (0.981).

**R2.13:** Page 15-16, Section 3.3. This section feels very hand-wavy. I would like to see a more quantitative and step-wise analysis presented for the comparisons mentioned to previous studies and other kinds of peat BB observations in this paragraph. Some of the comparisons mentioned are presented with little defense as to their relevance and/or the validity of the comparison (i.e., the interpolation mentioned above.)

**Authors:** There are very little data to compare to so we compared to everything with any relevance. In addition, we have now added comparisons to two more papers that came to our attention after we submitted our paper (and we updated the text and Table S3 to reflect this). Hopefully by addressing the specific comments and adding these new comparisons this section will be more useful. Even if we cannot address the comparisons in this section as conclusively as might be liked, representativeness is an important issue and we have attempted to raise and discuss it to the extent possible.

P16, L30 (end of section 3.3) added text: “Two very recent studies probed peat fire emissions during the 2015 El-Niño. Huijnen et al. (2016) measured three EFs for peat fires also near Palangkaraya. Their “peat-only” EFs are  $255 \pm 39$ ,  $1594 \pm 61$ , and  $7.4 \pm 2.3$  g/kg for CO, CO<sub>2</sub> and CH<sub>4</sub>, respectively. Their means are all within one standard deviation of our means and their EFs are within +1.9, -13, and -22% percent of ours, respectively. Not many details of the measurements are given, but the agreement is good. Parker et al. (2016) report three space-based measurements of the ER for Kalimantan fires in Sept-Oct 2015 for CH<sub>4</sub>/CO<sub>2</sub> ranging from 0.0062 to 0.0136. This is lower on average than the CH<sub>4</sub>/CO<sub>2</sub> ERs reported for peat combustion in the in-situ studies cited above (range ~0.011 – 0.035). The difference is consistent with our expectation noted above that some flaming-dominated consumption of surface

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fuels likely contributed to regional emissions in 2015. However, a glance at Figure 6 in Parker et al. (2016) shows that some of highest retrieved levels of these gases, which they attribute to fires, are far off-shore and/or upwind of the fires. Thus, more evaluation is clearly needed to determine if space-based approaches can accurately measure CH<sub>4</sub>/CO<sub>2</sub> ERs (e.g. Agustí-Panareda et al., 2016).”

Agustí-Panareda, A., Massart, S., Chevallier, F., Balsamo, G., Boussetta, S., Dutra, E., and Beljaars, A.: A biogenic CO<sub>2</sub> flux adjustment scheme for the mitigation of large-scale biases in global atmospheric CO<sub>2</sub> analyses and forecasts, *Atmos. Chem. Phys.*, 16, 10399-10418, doi:10.5194/acp-16-10399-2016, 2016.

Huijnen, V., Wooster, M. J., Kaiser, J. W., Gaveau, D. L. A., Flemming, J., Parrington, M., Inness, A., Murdiyarso, D., Main, B., and van Weele, M.: Fire carbon emissions over maritime southeast Asia in 2015 largest since 1997, *Scientific Reports*, 6, 26886, doi:10.1038/srep26886, 2016.

Parker, R. J., Boesch, H., Wooster, M. J., Moore, D. P., Webb, A. J., Gaveau, D., and Murdiyarso, D.: Atmospheric CH<sub>4</sub> and CO<sub>2</sub> enhancements and biomass burning emission ratios derived from satellite observations of the 2015 Indonesian fire plumes, *Atmos. Chem. Phys.*, 16, 10111-10131, doi:10.5194/acp-16-10111-2016, 2016.

**R2.14:** Page 17, line 33 – “the lab value is actually the sum of isomers compared to a single isomer from the GC analysis: : :” please explain this more, including references to the table, in which I see no evidence of a difference between a sum of isomers and a single isomer. Is this for a particular compound or set of compounds? Be specific.

**Authors:** This is a very helpful and important comment. One thing that was clear in the FLAME-4 papers, but we failed to specify in this paper was that the mass spec assignments in the lab study were “nominal” in some cases: or in other words a best guess at the most abundant species when many isomers could contribute. For example, mass 137.132 in the lab studies was calibrated with alpha-pinene and shown as that compound in Table S3, but there are numerous isomers that have the same exact

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mass (Hatch et al., 2015; 2016). In addition, fragments of higher masses can contribute to the mass spec signals. Thus it is not surprising that the lab MS value for all of mass 137.132 is much larger than the field WAS value more specific to alpha-pinene. In addition, one of the FLAME-4 PTR-TOF-MS calibrations was just revised based on the work described in Hatch et al., (2016). Thus, we have revisited this in detail. We have revised the text and the species impacted by isomers are now clearly flagged in Table S3.

P17, L30: Old text: “Table S3 compares all 31 gases measured for Kalimantan samples in both the lab (FLAME-4) and the field. The average of the two lab EFs is within a factor of two of the field mean for 20 of 31 species, which is adequate given that a factor of two is essentially also the field coefficient of variation ( $n = 35$ ). In 7 of the 11 cases with more than a factor of two difference, the lab value is actually the sum of isomers compared to a single isomer from the GC analysis of the field WAS samples. For the remaining 4 species the lab values tend to be higher for unclear reasons. For instance formic acid is higher in the lab where an open-path system was used instead of the closed cell system in the field, which could be subject to sample losses. However, HCl and NH<sub>3</sub> are likely more prone to adsorption than formic acid (Yokelson et al., 2003) and they were higher as measured with the field system suggesting the Teflon sample line and coating on the closed cell were effective in limiting line losses. The lab average for NO is higher, but NO was below detection in one lab fire and high in the other where flaming briefly occurred. The one field fire where flaming was briefly observed (Plume C, Table S2) had a higher EF for NO than the lab fire where it was detected. Thus, further comparisons with more lab fires will clearly be useful, but it appears the trace gas EFs from the lab are reasonable proxies for the EFs for species that have not been measured in the field”

New text: “Table S3 compares all 31 gases nominally measured for Kalimantan samples in both the lab (FLAME-4) and the field. (We clarify the need for the term “nominal” below.) Due to the natural high variability in the field data, the low number of lab mea-

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surements (two), the use of different peat samples, etc., we start by proposing that the lab measurements provide useful EFs for species not measured in the field if the average of the two lab EFs is within a factor of two of the field mean for species measured in both locations. Next, we find in the right-hand column of Table S3 that 15 of 31 species fail this initial factor-of-two test (ratios shown in red). However, this result is somewhat misleading since the lab data for 8 of these species (shown in blue) is actually comparing a best guess at the identity of the most abundant isomer for an exact mass measured in the lab to a WAS-based analysis for a specific isomer. Thus, these ratios could be larger than two because of contributions from other isomers (or fragments) to the mass spectrometer signal, higher than normal sensitivity in the mass spectrometer, WAS error, or unusually high variability for some species; with no way of knowing the individual contribution of these factors. We do note that a generally good comparison of the WAS and mass spectrometer was obtained when they were compared more directly in peat smoke in the lab (Hatch et al., 2016). Thus, only 7 out of 23 compounds fail the factor-of-two test, if we eliminate species that are ambiguous due to isomers. Of these 7 species, three are very close to the factor-of-two cutoff and are of less concern (ammonia, acetaldehyde, and hydroxyacetone). For the remaining 4 species (formic acid, NO, 1,3-butadiyne, styrene) the lab values tend to be higher for unclear reasons. For instance formic acid was higher in the lab where an open-path FTIR system was used instead of the closed cell FTIR system in the field, which could be subject to sample losses. However, HCl (below detection in lab) and NH<sub>3</sub> are likely more prone to adsorption than formic acid (Yokelson et al., 2003) and they were higher as measured with the field system suggesting the Teflon sample line and coating on the closed cell were effective in minimizing losses and sampling losses were not the source of the discrepancy. The lab “average” for NO was more than four times higher than the field value implicating high variability. NO was below detection in one lab fire and “high” in the other lab fire where flaming briefly occurred. The one field fire where flaming was briefly observed (Plume C, Table S2) had an even higher EF for NO than in the lab fire where it was detected. The other two species of concern are styrene and

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1,3-butadiyne. These two ratios could be high due to decay in the canisters, fragments in the mass spectrometer, or perhaps other less likely reasons. In summary, more lab/field comparisons should be carried out, but our rough analysis suggests that trace gas EFs measured in the lab are useful estimates (i.e. within a factor of two) for the emissions of most gases not yet measured in the field.”

**R2.15:** Page 18, line 14 – you should reassure the reader that the 37% of unidentified or tentatively assigned mass peaks of the NMOG mass is not going to negatively affect your assumption that you are measuring all the carbon to be factored into your EF calculations. I’m sure it’s not significant, considering the major non-NMOG carbon species, but this should be recognized.

**Authors:** P19 L 33-35 we added: “The missing NMOG mass in the field measurements is not large enough to cause significant error in our field carbon mass balance, but would impact estimates of secondary formation of aerosol and O<sub>3</sub> (Yokelson et al., 2013; Hatch et al., 2015).”

**R2.16:** In Table 1, Table 2, Figure 1, etc., there are numbers that are both too precise considering the standard deviations reported, and I dislike the excel-style presentation of numbers with exponents written as (e.g.) 1.67E-3.

**Authors:** We have changed the tables so they are no longer in the excel-type format. . The number of figures in the table values is to avoid round-off error, though we have changed values where the significant figures did not match: e.g. CH3I, 0.0125(0.00448) corrected to 0.0125(0.0045).

**R2.17:** Re: Table S1 – there are a handful of things that would make this table easier to digest, without having to search out other information.

**Authors:** We have italicized the Referees suggestions in the rest of this comment and reply point by point in un-italicized text.

*Instead of “Y/N type” or “Y/N what” as a header, eliminate Y/N and just include the*

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*type/what or put “none” or “unknown” where applicable.*

Au: Done.

*Also, please spell out here what the peat fuel types are, so that I don’t have to go back and find that in the paper (in the footnote would be fine.)*

Au: They are now spelled out in each cell.

*“day-mon” should be “DD-Mon”.*

Au: Done.

*Why are there some plumes included that aren’t lettered? These don’t seem to add anything to the paper.*

Au: The unlettered plumes represent occasions when it was possible to quickly collect a WAS canister, but not deploy all the instruments. These samples add important emissions information.

*“seec” is not a word or a shortform (as a direction).*

Au: “seec” was changed to “sec”

*For Depth of Burn, “site avg” is redundant. “site avg” is fine.*

Au: We changed to “approx. site avg” and deleted the “~” where they appeared before entries.

*Why is so little known about site 6?*

Au: The team member collecting site data was unable to participate on the last day of the field campaign. The emissions team was able to collect some data.

*Re: Winds – “av, max, dir” implies you’ll have numeric values below. Maybe leave the “av, max, dir” part out, and just consider it a verbal description of the winds.*

Au: Done.

C14

*Be consistent with spacing and vertical cell centering.*

Au: This was checked.

*Also, for all three supporting information tables (S1-S3), please be consistent about font size and styles and remove bold settings.*

Au: The journal has requested that we use 9 pt Times New Roman with bold headings for our print tables and we assumed they want that for the supplement as well. However, Table S1 was clearly outside the normal journal format so we changed the font and font size to optimize readability.

*Table titles should all be uniformly sized.*

Au: Please see above.

*For all supplement tables, if these are being submitted as they are now in an excel file, a san serif font is likely best for readability. If you're preparing a printed document, a simple serif font is also acceptable (i.e., Times New Roman.)*

Au: We have been asked to use Times New Roman 9 pt for tables by the journal so we did that. On Table S1 we used a more readable font since it is clearly a non-standard table.

### **Referee #2 (Continued) Technical Comments**

**R2.18:** Page 5, lines 4-5 – don't us semicolons in place of commas.

**Authors:** We separated the items in the list with semi colons because one item has multiple terms separated by commas: "... ; charred logs, char, and ash from previous burns;"

**R2.19:** Page 5, line 26 – remove the hyphen from "at six-different peatland: : :"

**Authors:** removed

**R2.20:** Page 6, line 10 – no need for "(#2)" after "This site: : :"

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**Authors:** deleted

**R2.21:** Page 11, line 12 – there is a missing or extra parenthesis here.

**Authors:** The first "(" was deleted and now reads "1/(1+ $\Delta$ CO/ $\Delta$ CO<sub>2</sub>)"

**R2.22:** Page 18, line 28 – "0.35  $\pm$  .1 x 10: : :"

**Authors:** fixed

**R2.23:** Page 19, line 1 – "Six of the nine: : :"

**Authors:** changed to "six of the eleven"

**R2.24:** Page 19, line 11 – the Putra et al. paper in preparation needs to be included in the reference list.

**Authors:** added

**R2.25:** Tables 1, S2 and S3: "ethyne", "ethene", "propene" (and in the text and Figure 1, where applicable).

**Authors:** We think the common name for these species and many others (e.g. formaldehyde, formic acid, methanol and many more) is more or reasonably common, but added the requested formal names in the parentheses with the formula at the first appearance of these species in the text (P7).