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Atmospheric Chemistry and Physics Discussions

ACPD

15, C8295–C8299, 2015

Interactive Comment

Interactive comment on "Biomass burning emissions of trace gases and particles in marine air at Cape Grim, Tasmania, 41 S" *by* S. J. Lawson et al.

S. J. Lawson et al.

sarah.lawson@csiro.au

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We thank the reviewer for the very helpful suggestions and additional references which in almost all cases have been incorporated into the manuscript

Responses to specific reviewer comments are given below (responses denoted by > before text)

This paper presents an extensive set of opportunistic measurements of bushfire emissions made when a bushfire impacted the Cape Grim station during a campaign aimed at studying particle formation in the clean marine environment. The paper is well written and contains significant new information on the emissions from fires in a poorly sampled region of the world. Thus I recommend publication after a number of minor issues are addressed.

Page 17605 line 18: “fresh and diluted BB plumes” - rephrase please- (the degree of dilution may vary but both are diluted by ambient air).

> in response to comments made by reviewer 1 we are no longer referring to Period B as a diluted plume (due to a lack of CO enhancement observed). We have therefore changed the sentence to remove reference to dilution:

“In this study we have investigated the chemical composition of fresh BB plumes in marine air at the Cape Grim Baseline Air Pollution Station”

2. Page 17606 line 10, please define whether the _ symbol refers to standard deviation? - if so at what confidence interval?

> has been defined as $\pm$1 std dev.

3. Page 17611 line 12, do you really need to use the acronym “nss”? You probably do not use it enough for it to be necessary.

>Have removed acronym

4. Page 17613, end of line 3 “particle” should be “particles”?

>yes, corrected

5. Page 17615, line 1 “produce” should be “produces”?

>this text has been removed in response to comments from reviewer 1

6. Page 17617-17618 and page 17627 line 25: you imply that there is a change in the absolute magnitude of the emissions from the fire (as well as the emission ratios) as a result of rain/changing combustion efficiency but I am not convinced that you present sufficient evidence for this. The concentrations increase dramatically at the

measurement site but the amounts reaching the site depend both on the emissions and on the degree of mixing. A change in meteorological conditions (accompanying the rainfall) could significantly alter the degree of (e,g. vertical) mixing and produce greater concentrations at the measurement site. A change in the emissions from fully oxidised products (like CO2) to partially oxidised products (like CO), would be fully expected with a reduction in combustion efficiency due to rain, nevertheless the changes in the ratios of acetonitrile and black carbon to CO are very interesting.

>we agree and we have modified the text in this section, as well as in the conclusion and abstract, to state that we see a change in emission ratios due to rainfall and decreased combustion efficiency. We have removed any reference to absolute emissions magnitude of emissions changing as a result of the rainfall/decreased combustion efficiency.

7. Page 17619, the comparison of number concentrations from different sites should also point out that the degree of mixing will be a major factor in the concentrations measured.

> comparison of particle number concentrations with other studies has been removed, due to similar comments raised by reviewer 1

8. Page 17620: (or somewhere else!) Somewhere you should add a sentence saying that it is assumed that the enhancement ratios measured are unaltered from the original emission ratios because of the short transport time to the measurement site.

> the following additional sentence has been added after the following paragraph:

Existing text:

“During the selected time period, wind speeds of 16 m s-1 meant that the plume travelled the 20 km to Cape Grim over a period of about 20 minutes, which allows the plume to cool to ambient temperatures but ensures minimum photochemical processing of the plume (Akagi et al., 2011). Advection of the plume to the site occurred primarily at night

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so minimal impact of photochemical reactions on the plume composition is expected (Vakkari et al., 2014).

Additional sentence:

"It is therefore assumed that the enhancement ratios measured at Cape Grim are unaltered from the original emission ratios."

9. Page 17621: why do you remove background amounts and then force the straight line fit through the origin? The slope of the regression should be the same regardless of what the background values are. This probably doesn't make a great deal of difference but you are likely adding unnecessary uncertainty to the results.

> as stated in Yokelson et al 1999 (page 30,117), and in agreement with the statement of Reviewer 2 above, there are several different methods of calculating ER to CO, which produce essentially the same result. The method used here was used successfully by Yokelson et al 1999 and found to agree closely with alternative methods.

10. Page 17621 last paragraph: CO and CO2 are often poorly correlated when sampling a fire plume if the combustion efficiency of the fire varies during the measurement period. Thus poor correlation in itself should not be a problem, if you can determine the actual enhancement in CO2 and CO as you can simply sum the total enhancements of each throughout the fire. The single grab sample measurement for CO2 every 40 minutes may be more problematic when attempting to do this, so I don't have an issue with the use of a literature value for the emission factor of CO if you are really not confident that you can obtain a trustworthy one from your own data. However you do not explain the choice of the EF from Akagi et al. This seems like an odd choice to me when you point out on page 17604 "EFs from NH coniferous forests are unlikely to be representative of Australia's temperate dry sclerophyll forests". Why not use the EF from Volkova et al?? If you don't want to recalculate - just explain the choice and/or maybe comment on how much (or little) difference a different choice of EF for CO would make to your results.

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>thank you for this advice. As mentioned in author response to reviewer 1, we have calculated the EF based on the carbon mass balance method as suggested by both reviewers. Due to the realistic MCE obtained, and agreement between ER and MCE, we believe we have reliably measured the excess CO and CO2 during the fire.

>The EF calculated using the CO EF from Akagi et al are now reported for comparison in supplementary material and are not the focus of the paper. However, to respond to the comment above, we selected the Akagi et al CO EF because this was an average temperate forest EF, calculated from several independent studies, and we believe be a robust average value. We could have used an Australian EF from a single study as suggested but were unsure about the representativeness of this value.

11. Page 17624 line 11: delete “a factor of” before “almost a factor of”

>This detailed comparison of EF with other studies has been removed from manuscript in response to comments from Reviewer 1

12. Page 17626: insert “for” before “NH temperate forests”

>This detailed comparison of EF with other studies has been removed from manuscript in response to comments from Reviewer 1

13. Page 17626: consider changing section title to “summary and future work” ???

>changed to ‘Summary and future work’ as suggested

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