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Discussions

Interactive comment on "Emission ratios of trace gases and particles for Siberian forest fires on the basis of mobile ground observations" *by* Anastasia Vasileva et al.

Anonymous Referee #1

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Emission ratios of trace gases and particles for Siberian forest fires on the basis of mobile ground observations

Vasileva et al

Review for ACP.

The paper provides a detailed examination of emission ratios of typical gases and aerosol information from two different fire complexes measured from a train in 2005-2007. This is a valuable contribution to constraining the variability and average emission ratios of species from biomass burning. The analysis was carefully thought out, and on the whole the paper is in good shape. However, I do have some concern about

instrumental issues highly relevant to the observations (specifically for the aerosol and BC data), and some suggestions for slightly modifying the analysis/presentation. These are outlined below.

Primary comments:

1) A serious problem is that there is insufficient information about the uncertainties of the measurements – especially the aerosol and black carbon measurements. Essentially all background information about instruments and measurement uncertainty are only referenced via citations, and the citations are insufficient to demonstrate the level of control over the measurements necessary to provide convincing evidence of its value (both because they are hard to find, and because the ones I found did not have enough information). a. For example, Kopeikin 2008 was cited for the GRIMM PM measurements. The entirety of its discussion was: "The nephelometer "Dust Indicator and Tunnel System" designed by GRIMM Corporation (Germany) with the concentration measurement range from 0.01 to 15 ug/mˆ3 was used in expeditions in 2004-2007."

Then there is some discussion of a different system (with limited calibration information) for different missions that are not relevant here. This is entirely inadequate. For BC, I was not able to get the corresponding Kopeikin paper via inter-library-loan, but I saw that it did not contain any references that were relevant to major corrections and uncertainties typically applied and associated with Aethalometer measurements. The current ACPD paper: Comparison of different Aethalometer correction schemes and a reference multi-wavelength absorption technique for ambient aerosol data, by Jorge Saturno et al., gives a good introduction to these issues, which must be dealt with before the data can be considered final. 2) The focus of the paper is on coarsely segregated averages of the two plumes measured, with variability within each section treated more as a "uncertainty source" than as an important feature in its own right. I suggest that the authors attempt to share more information about the variability, perhaps merely via slightly increased discussion, and highlight that component of the results as valuable in their own right to expand the value of these within the whole. In this vein, I wonder if Figures 3 and 4 could be made more useful by showing the ratios, in addition or instead of the simple concentrations. 3) The connection between the observations of fire state by the scientist on the train, and the actual (mixed) state of the fires at the positions and times actually sourcing the pollution sampled is extremely weak. I did not understand how the line-of-sight of the observer were relevant to large and wide-spread fires. Hence, unless this can be strengthened, I suggest removing the conclusions about lack of connection between flame state and MCE. 4) I did not understand the rationale to omit the CO2 data from a large segment of the F1 plume. How do the authors know that this is not some variability from the fire? Why would CO2 be differently mixed than other trace gases? Unless this is more clearly supported, it seems CO2 should be included for this segment.

Secondary comments:

1) The work of the authors in analyzing the relationships between different species using different fitting techniques was welcome to see. However, the results (and supporting literature) support the idea that the orthogonal distance regression is both the most appropriate approach, and pretty much represented the mean between the other two linear regressions. Once this was clear, I thought the paper might benefit from this discussion being moved to supplemental material, and the graphs simplified by omission of the two simple linear regression lines. After all, this is not the main thrust of the paper. 2) The authors present PM/CO correlations in units of ng mˆ-3/μg mˆ-3 "for easy comparison to other studies". However, this makes it difficult to compare the other ERs and PM3 to CO correlations, and makes PM/CO a "odd man out". Perhaps it is clearer to leave PM/CO in ng mˆ-3/ppm, and adjust the other studies. 3) In figures 8 and 9 I suggest referring to the new analysis as "this work" rather than Vasileva et al., 2017. 4) The paper is clear and well written, but has numerous small English errors (mostly missing "the"s or extra "thes". Here are some examples: a. Page 2 line 3: "in THE future" b. Line 13: "alter the OXIDATIVE capacity . . ." c. Line 14: "and disturb THE background chem. . ." d. Line 27: ". . . can SERIOUSLY DETERIORATE the .. "

e. Line 28: ". . . and contribute to Arctic haze" ("the" should be left out). These, and others, should be corrected.

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