Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-1067-RC2, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evidence for a larger contribution of smoldering combustion to boreal forest fire emissions from tower observations in Alaska" *by* Elizabeth B. Wiggins et al.

Anonymous Referee #2

Received and published: 12 February 2020

Review criteria: Scientific significance: Excellent (4) Scientific quality: Good/Excellent (3.5) Presentation quality (2)

General comments: The discussion paper presents important research into the characteristics of wildfire emissions using established techniques, but novel analysis. Teasing apart the contributions of various fire events and the combustion stage (Flaming vs. smoldering) is a new and valuable way to understand nuances of boreal fire relevant to many needs, such as human health, carbon cycling, and smoke planning. However, the paper falls short in many ways, and will need some extensive modification to reach its potential. I strongly suggest a re-focus on a more relevant outcome from

Printer-friendly version

Discussion paper

the work (rather than the fact that previous work was not catching smoldering as well as they could), a fully revised Discussion (some ideas below), and some attention to references (see notes below). This work is very important, and when presented well will make a great contribution to the literature on this subject. Specific comments:

1. The title will need modification. It is unclear what "larger" refers to – larger than what? Than previous studies (yes, but I know that only when I get to the end of the Abstract). It could be larger than flaming combustion. The point is that having an unreferenced comparative adjective can be troublesome, especially in a title where you want to be clear. The title could be the same, but with the first four words dropped: "Contribution of . . .". Also, it is my opinion that, while this may show larger contribution than previous studies, this work has a lot of other implications and contribution that could be highlighted in the title. In some ways the community would not be too surprised to learn that the smoldering fire signal has not been captured in previous studies, so highlighting this part of it is not needed to make this an impactful paper/study.

2. The comparison to previous studies would more naturally go into the discussions, rather than the introduction/background. I suggest revising to put Table 1 into the discussion where you can make the case more directly, rather than introducing the previous work without yet seeing your results.

3. There is a blatant and concerning misuse of terminology on Page 2, line 34: The sentence "Smoldering combustion can be defined as combustion with a degree of combustion completeness, or modified combustion efficiency, less than 0.9 [Urbanski 2014]." First, MCE and combustion completeness (CC) are very different things. CC is the proportion of fuels consumed/combusted, while MCE is defined as the proportion of a gas to CO2. Second, the Urbanski paper puts MCE of 0.65 to 0.85 as "smoldering", and references Akagi et al. 2011 so I don't know where the 0.9 figure comes from. The choice of the thresholds stated on page 5 lines 1-4 need to be better justified.

4. I found a couple of instances where the citations used are inappropriate. While I mention only 2 here, I would suspect others, so the citations need to be fully vetted for appropriateness. First: “Rogers et al. 2015” in Page 1 line 33 is not a review of boreal fire regime. It may mention this, but it is not what that study provides to the literature. Second: “Bertschi et al. 2003” in Page 7 line 34 is of laboratory experiments and work in savannah ecosystems, not boreal forest fires. In both of these cases, it could be argued that no reference is needed. If you do include a reference, it needs to be a paper or resource where the statement made is shown or studied, not where it was stated. I suggest the co-authors assist with improving the citations.

5. The discussion would benefit from more regarding the implications of the results. What is the data showing us that is relevant? Some possible ideas to highlight/discuss (these need to be discussed with co-authors, so are only representative):

a. Figure 5 (Page 6 line 9) shows a linear relationship between CH4 and MCE. Provide a short discussion of this in the discussion – what does this mean for using the data?

b. Page 6 line 22 – “. . .attributed to boreal fire emissions.” – As opposed to what?? Or why? A bit of discussion on what other factors contribute to the signal, and why there are some difference in the model will help non-atmospheric modelers better understand why these results are so powerful

c. The temporal distribution data (Fig 10) is very interesting and could be helpful for exposure assessment for health studies. (although PM, rather than CO would be of interest).

d. Page 7, line 24: I am not sure I see a temporal trend in the old data, and I am not sure why this would be something to note. This statement is best dropped. Table 1 presents past results that are collected in a variety of settings, so (in my assessment) represents some data on the range of variability, not a record of change over time.

I hope these comments inspire the authors to revise the manuscript for a more useful

product.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-1067, 2020.