Anonymous Referee #2

We thank the reviewer for their constructive comments on our manuscript. Here we provide our initial responses to these comments and will provide a modified manuscript after the discussion is closed.

Received and published: 29 May 2019
Davidson and colleagues present a study in which variability in methane emissions have been quantified for different burn severity classes and microtopographic positions in a boreal fen in Canada. The research begins to answer important questions regarding interactions between fire disturbance and methane cycling in the context of boreal carbon cycle feedbacks to climate. The authors find that fire generally reduces methane emissions and, for at least several years following fire, eliminates relationships between water table depth and methane emissions. The paper is based on a relatively small but important data set that is appropriately analyzed. The paper will be suitable for publication after a few relatively minor revisions.

We thank the reviewer for their positive comments on our study and we outline the detailed reply to their comments below:

I have several overarching comments followed by more specific ones. This manuscript is rather short, which isn't a bad thing, however I do think there is room to expand and add some additional details, especially in the discussion.

For example, permafrost is invoked as a potentially important driver in the Introduction, but then is not mentioned in the discussion.

We do not mention permafrost in the discussion as our study site is not underlain by permafrost and we did not want to cause confusion to the reader.

The main point regarding the wildfire overriding hydrological controls on CH4 emissions comes through. But the secondary point that these effects are understudied and may vary with ecosystem type could be developed more.

I would encourage the authors to archive their data and code in open access repositories, ideally where they would be citable with a doi.

We can include a Data Availability Statement and state the data that support the findings of this study are available from the corresponding author upon reasonable request.

It is a little confusing to have CH4 flux numbers reported normally in the text of the results, and then log-transformed in the figures. It would be best to back-transform for the figures if possible.

As the statistical differences reported in the text are based on analyses using the log-transformed data, we believe it is acceptable to present this data in the figures.

However, we now include figures with the untransformed data in the supplementary information for comparison:

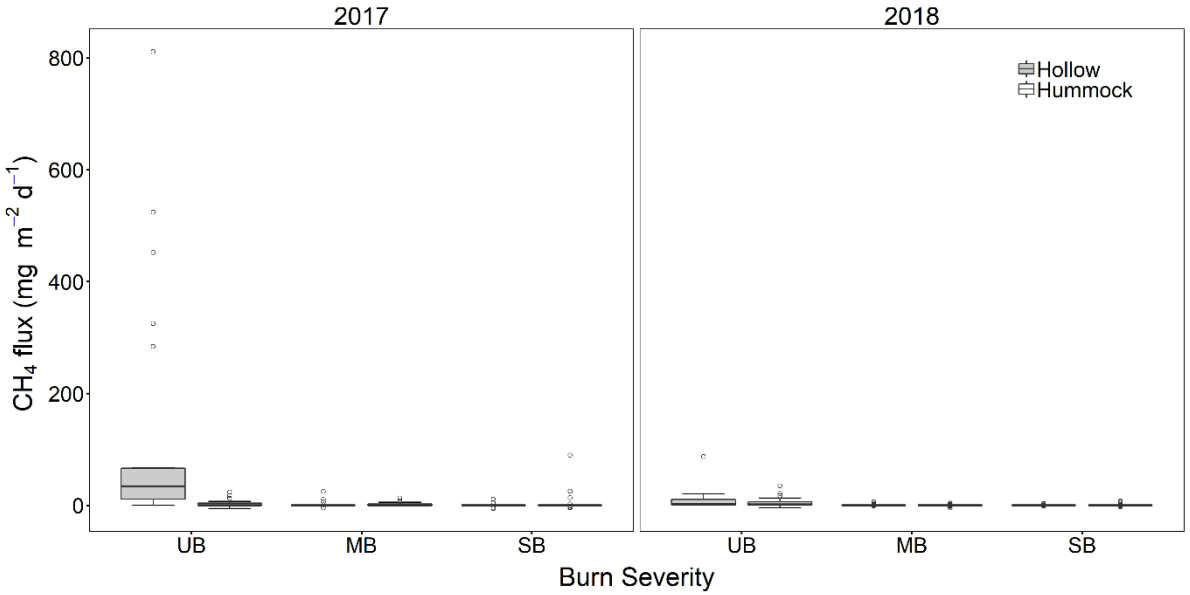

Figure S3. Methane (CH_4) emissions at each microform type across the peat burn severity gradient for 2017 and 2018. UB is unburned, MB is moderately burned and SB is severely burned.

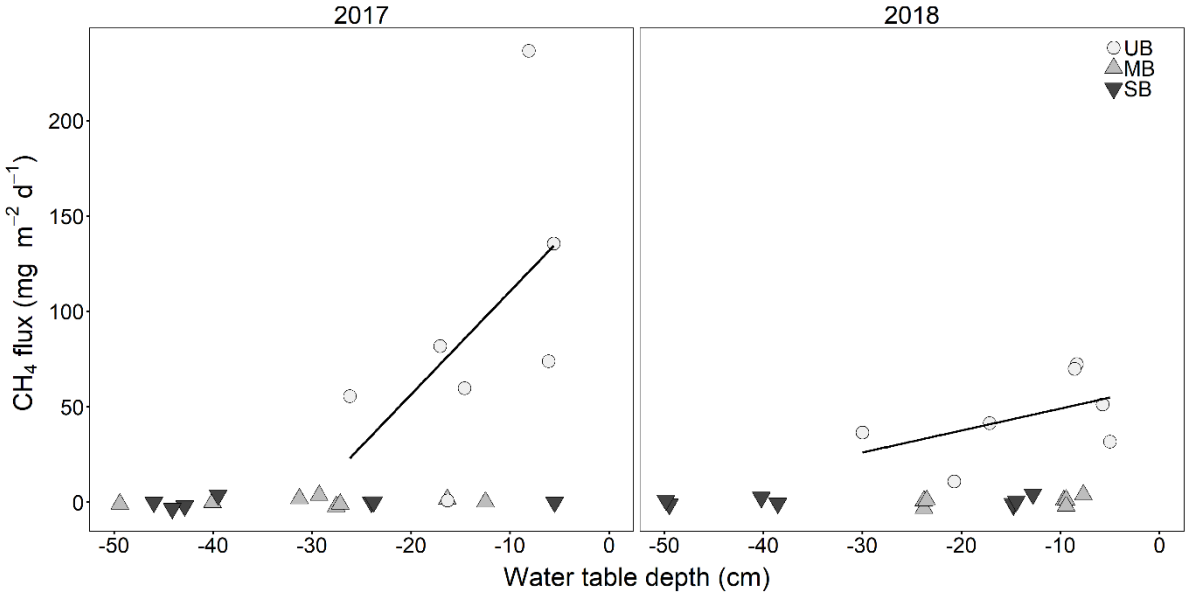

Figure S4. Seasonal mean methane (CH_4) flux at each collar across the peat burn severity gradient plotted against seasonal mean water table (WT) depth. UB is unburned, MB is moderately burned and SB is severely burned.

P1 L1: Different disciplines may understand peatland methane emissions in different ways. Why not use "Wildfire overrides hydrological controls on boreal peatland methane emissions" or something similarly specific?

We agree with this comment and have changed the title accordingly.

P1 L8: Are you referring specifically to a negative carbon cycle feedback to climate, or any negative feedback (e.g. surface energy partitioning)

We are referring to any negative feedback including impacts on carbon cycle and surface energy partioning for example.

P1 L19: This sentence seems a little odd since you have not yet mentioned any argument regarding the overriding influence of fire. Maybe reword, or set it up better.

We have removed this sentence for clarity.

P2 L13: It would be nice to have the briefest of descriptions of hummocks and hollows, I think I know what they are.

We included descriptions of hummocks and hollows on Pg 2 new lines 21-27:

"Microtopography across peatlands can be impacted through fire, by increasing the prominence of hollows (low lying areas close to the water table; Belyea & Clymo, 1983) on the landscape through altering elevation (Benscoter et al., 2015), and often hollows will have a higher severity of burn compared to other areas across the landscapes (Mayner et al., 2018; Benscoter et al., 2005). Conversely, hummocks (mounded topography, approximately 0.2 m or higher above the water table; Belyea & Clymo, 1983) are generally resistant to fire, namely due to moisture retention differences between the different moss species present at both microform types, as Sphagnum spp. is much more resilient to fire than feather moss (Kettridge et al., 2015)."

Also what is the mechanism by which fire alters elevation, causing hummocks.

The fire can remove vegetation and substrate within the hollows much more readily than the fire-resistant Sphagnum hummocks due to the moisture retention differences of the mosses species found at each microform type (Pg 2, new lines 25-27). This vegetation and substrate removal may indeed make the hummocks look more prominent on the landscape, but the fire does not create hummocks.

Lastly, it would be interesting to know how prevalent hummocks vs. hollows are on the landscape – are there any papers out there with numbers you could site (e.g. hummocks make up XX% and hollows XX% of typical fen's in this region).

At Poplar Fen, it is estimated that the landscape consists of 47% hummocks and 53% hollows (Gabrielli, 2016, Graduate Thesis, Wilfrid Laurier University) now included on Pg 3, new lines 20-21.

P2 L17: Can you say specifically which moss species is found at each microform type?

The dominant hummock forming moss species at this site is *Sphagnum fuscum* which we state on Pg 6 new lines 4-5.

P2 L31: Is your site underlain by permafrost? I don't think it's reported in the methods. That could be useful to know.

No, our site is not underlain by permafrost.

P3 L1: A single site in both Canada and Russia?

We have clarified in the text that we meant multiple studies by the same author (Köster et al. 2017 and 2018; one in Canada and one in Russia), Pg 3 new lines 4-5.

P3 L18: Please give a one or two sentence description of the DOB protocol. The reader should only be obligated to look at other refs if they want all of the juicy details.

We have included a summary of the DOB protocol on Pg 3 new lines 25-29.

"The DOB was determined following the protocols used by Lukenbach et al., (2015a), van Beest et al. (In Review). In summary, this method assumes a pre-fire flat surface between multiple reference points across the site, including adventitious roots in the burned sites and unburned reference points. A string is attached between two reference points and ten measurements were taken along the length, from string to burned ground surface, giving an estimate of the depth of the burn."

P30 L30: Is it possible that these cover variables could have changed of the course of your study? Particularly moss colonization, but also water, which I imagine could change with the weather conditions. Also, could you briefly describe your percent cover approach; since some sites have more than 100% I assume you are looking at over story and ground cover?

We agree that there is a possibility that vegetation cover changed between 2017 and 2018, however we think the changes would be small and are confident with our vegetation cover values.

We did look at ground cover and over story (bryophytes vs. vascular plants) but also would like to highlight the MB and SB sites contained burned areas and bare ground, hence the amount exceeding 100%.

P4 L12: Please include justification or reasoning for the -5 mg Ch4/m2/d threshold.

We use the -5 mg CH4/m2/d threshold is because we believe it is unlikely for this system to have consumption of methane greater than

this. This resulted in only a loss of 2% of the data (6% loss overall after all data checking).

Now added to Pg 4 new lines 21-23.

P6 L25: Do both of these studies indicate both of these things, or is each point from one of the studies?

We apologise for the confusion. We have clarified in the text that each point is one of the studies, Pg 7 new lines 3-5;

"These results contrast with other studies looking at CH4 emissions post-fire at peatland sites, with Danilova et al. (2015) indicating that fire across an ombrotrophic bog could decrease CH4 oxidation due to removal of the methanotrophic community, while and Grau-andrés et al. (2019) note a potential increase CH4 emissions due to increased graminoid cover."

P7 L1: What do you mean by an addition here?

We have changed the wording to presence instead of addition, Pg 7 new line 13.

P7 L4: Please expand the discussion of fire effects on water table depth. A more in depth process level discussion would be nice here, perhaps also with some specifics on the variability within this fen that you allude to.

We expand on the link between water table, fire and methane production/emissions on Pg 7, new lines 16-31.

"Higher emissions at the UB site could result from overall shallower WT at this location compared to the MB and SB sites (Table 1), which were located at the fen margins. Poplar Fen has a highly variable connection to groundwater (Elmes et al., 2018) and the hydrogeologic setting of Poplar Fen likely contributed to the limited effect of the wildfire at this location, but could also result in higher CH_4 emissions than would have occurred naturally at the burned sites prior to the fire. However, the comparison of our results to emissions measured between 2011 and 2014 at another location in Poplar Fen burned during the fire indicate there was no significant difference in CH_4 emissions. Interestingly, we see no relationship with CH_4 emissions and WT depth at the burned sites. This switch in the typical understanding of the relationship between CH_4 emissions and WT further strengthens our argument on the overriding influence of fire. Even under suitable hydrological conditions, there is a lack of CH_4 production, as shown in the incubation study. Removal of vegetation and soil organic matter can lead to drier conditions (Thompson & Waddington, 2013), with a lower water table creating a larger aerobic zone, potentially leading to lower rates of CH_4 production and potentially greater rates of CH_4 consumption. However, fire can also cause a higher water table, which could potentially lead to larger anaerobic zones and potentially higher CH_4 emissions. However, this is dependent on the severity of the burn, where a low severity fire

which only removes vegetation and does not impact the microbial community and organic matter content of the soil may still allow for CH_4 production. Conversely, a high severity burn which has removed these communities and organic matter may no longer allow for CH_4 production, even with suitable hydrological conditions."

P7 L13: The resistance of S fuscum to what?

We have clarified on Pg 7 new line 33 that we meant resistance of S. fuscum to fire.

P7 L14: Chemical changes in the soil substrate? Can you be a little more specific here?

We have clarified on Pg 7 new line 33 that we meant chemical changes in the soil substrate.