Biogeosciences Discussions

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Comment on bg-2022-144

Anonymous Referee #2

Referee comment on "Carbon emissions and radiative forcings from tundra wildfires in the Yukon–Kuskokwim River Delta, Alaska" by Michael Moubarak et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-144-RC2, 2022

The following comments pertain to the manuscript "Yukon-Kuskokwim River Delta 2015 fire burn depth measurements and unburned soil and vegetation organic matter and carbon content collected in 2019". Overall, I think the findings presented in this manuscript are of high scientific importance and I applaud the authors for conducting this work. Having said that, I have a few major issues with this manuscript. The first one concerns the language of this manuscript. If my understanding of this manuscript is correct, the findings presented here stemmed from field data collected in a single burn scar in YKD. While these findings are important in their own rights, the authors (mostly in the results and discussion sessions) made a lot of statements that are obviously overly strong. Tundra wildfires, while being overall understudied, vary substantially based on climate, terrain, species composition, etc. As a result, while this study shows that the YKD fire that it looked into had a similar emission rate to the Anaktuvuk River Fire (ARF), they still represent two cases out of many more tundra fires, many of which are much less severe than the ARF. Therefore, the authors should be very careful when they make statements concerning "tundra fires". You can indicate that the YKD fire and the ARF likely represent the upper level of the impacts of tundra fires (in terms of carbon emission and radiative forcing), but you can't just hint that these are representative situations.

My second major issue with this manuscript is the fact that while it has a substantial discussion session, it fails to discuss the sources of uncertainty introduced by the methods that were used. For example, this study used dNBR as the indicator for burn severity, however, there have been studies that show that dNBR may not be a very good burn severity indicator in the tundra (Loboda et al 2013) and that there may be better indicators (Chen et al 2020). Another major potential source of uncertainty stems from the fact that the emission factors the authors used were from boreal fires, which as the authors state in the method section may lead to an overestimation. However, this overestimation wasn't further discussed in the discussion.

Additionally, in terms of dNBR, there have been a lot of studies showing the caveats

associated with dNBR when it is used in high northern latitude settings (eg, Chen et al 2020). The author failed to take these into account completely. Additionally, the authors never showed the audience how the YKD fire that they picked compared with other tundra fires. This is a piece of critical information that is needed for a wider audience to understand the scientific importance of your work.

Loboda, T. V., French, N. H., Hight-Harf, C., Jenkins, L., & Miller, M. E. (2013). Mapping fire extent and burn severity in Alaskan tussock tundra: An analysis of the spectral response of tundra vegetation to wildland fire. *Remote Sensing of Environment*, *134*, 194-209.

Chen, Yaping, Mark Jason Lara, and Feng Sheng Hu. "A robust visible near-infrared index for fire severity mapping in Arctic tundra ecosystems." *ISPRS Journal of Photogrammetry and Remote Sensing* 159 (2020): 101-113.

Chen, D., Loboda, T. V., & Hall, J. V. (2020). A systematic evaluation of influence of image selection process on remote sensing-based burn severity indices in North American boreal forest and tundra ecosystems. *ISPRS Journal of Photogrammetry and Remote Sensing*, *159*, 63-77.

Here are my more detailed comments.

Line 38 “AK”: Spell out Alaska.

Line 64 “North slope”: North Slope

Line 97: More information about how this particular fire compares with other tundra fires (both from YKD and other tundra regions in Alaska) is needed (in terms of size, overall dNBR, etc).

Line 105: Where did you get the burn scars? This needs to be specified.

Line 181 dNBR’s full name has already been given previously.

Line 181 “Key and Benson 2006”: This reference should be provided when dNBR was first mentioned.

Line 183: CFMASK algorithm: this needs a citation.

Line 191 “We excluded estimates derived from tussock measurements because burn depth estimates from tussocks correlated negatively with remotely sensed fire severity.”: Is this a part of the analysis that isn’t included in the manuscript? More information needed.

Line 199 “low severity burn”: Unburned islands almost certainly exist within the burn scar that the authors focused on. Since there is no "unburned area" class, the "low severity" class most likely includes unburned areas, which is why this class should be called something like "unburned/low severity".

Line 239 “similar to the framework employed in Randerson et al. (2006) and Huang et al. (2016).”: Even though you provided the references for the method you used in this study, a basic description of the said method should be provided in this manuscript since the audience, including the reviewer, should be able to understand your method without going to the source materials. Also, when directly apply the methods described in previous studies, you need to briefly explain the settings of those studies so that you can show the audience that the methods are actually applicable.

Line 240 “these equations”: Similar to my previous comment, here "these equations" sounds really strange since no equations were actually given in this paper.

Line 307-310: Throughout this paper, there are many paragraphs like this that consist of a few sentences. This not a good scientific writing practice and these paragraphs should be rewritten to be merged into bigger paragraphs.

Line 352 “Both models were corrected for spatial autocorrelation between transect locations.”: how?

Line 410-418: As i pointed out previously, your low severity class includes unburned areas, as a result, you likely have overestimated the emissions here.

Line 458 “here we … frequent tundra fire regimes”: this statement needs to be modified significantly. what you can confidently say is that the particular wildfire that you visited emitted a lot of carbon and has a warming effect. However, with the field data that you

collected, you can't make a statement indicating this is the case for all tundra wildfires. In fact, this is very likely not the case for many, if not most tundra wildfires, since tundra wildfires can be quite low in severity.

Line 463: I don't like how this statement is worded because it incites misinterpretation. You should just say that the emission per unit area is similar between this fire and ARF. That is fair. Additionally, you are comparing this fire with ARF (which is also fair), but you didn't give any other comparison between the two (such as size, species composition, burn severity). These contexts are important for readers to understand the scientific importance of your findings.

Line 473”Carbon loss per area from tundra wildfires are within the range of total above- and belowground carbon loss from boreal wildfires, approximately 0.5 to 4 kg m-2 (Walker et al., 2020a; Walker et al., 2018b; Rogers et al., 2014)”: This is another instance of overgeneralization. While this fire and ARF may have carbon emission per unit area that is similar to that of boreal wildfires, you can't say so for all tundra wildfires.