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The Cryosphere Discussions

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Interactive Comment

Interactive comment on "Uncertainties in the global temperature change caused by carbon release from permafrost thawing" *by* E. J. Burke et al.

Anonymous Referee #1

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Overall Evaluation

This manuscript represents a very useful study of evaluating potential uncertainties in estimating global temperature changes associated with various issues affecting the atmospheric release of carbon exposed by permafrost thaw. The analysis is in many ways a first order approximation of the positive feedback to global temperature of this release in that it uses a simple decomposition model and a simple energy balance model to calculate the change in global mean temperature caused by the exposure of carbon from the thawing of permafrost. The real strength of the analysis is in partitioning the uncertainties in the global mean temperature response among uncertainties in climate scenarios, soil organic carbon content, carbon quality, and soil physiology issues. This is quite useful for the global modeling community, and I think will help foster interactions with the permafrost carbon community which can potentially work to reduce uncertainties in the latter three issues. The analysis in this paper also complements some other recent analyses on the permafrost carbon feedback. In general, the manuscript is well organized and well written. However, there are a few issues that I think need attention in a revision revision (see my comments below). In particular, there are some issues of conceptual uncertainty that were not dealt with in the analysis that need to acknowledged.

Specific Comments

(1) I was really puzzled by the sentence in the abstract between lines 11 and 14 as I really didn't understand why the estimate that 50% of the uncertainty in global temperature response is associated with indicates that the effects of permafrost thaw is currently controllable by mitigation measures. I have several problems with this statement. First is the logic – why does 50% vs. say 0%, 25%, 75%, 100% of the uncertainty attributed to climate scenarios lead to the this conclusion? Second, why doesn't the absolute range of global temperature to temperature have anything to do with respect to the ability of mitigation to control the response. Finally, this manuscript is not about mitigation, a very complex topic involving policy actions and the timelines for implementation those actions. The manuscript only discusses mitigation in one sentence between lines 24 and 26 of page 1387, and it is essentially the same sentence that appears in the abstract. My suggestion to remove this sentence related to mitigation from the abstract unless a more thoughtful discussion of the issue later in the paper can justify its placement in the abstract as a primary conclusion of the paper.

(2) Page 1369, line 21: Change "thus" to "this".

(3) Page 1370, line 11: Change "an accumulative" to "a cumulative".

(4) Page 1371, lines 24-25: I think you first need to have some description of the ap-

proach that HadGEM2-ES uses to simulate soil temperatures before you describe how you diagnosed the zero degree isotherm. I'm assuming that HadGEM2-ES treats soils in northern latitudes as if they are mineral soils and does not consider the thermal and hydrological properties of organic soil horizons and live moss above the mineral layer in simulating soil thermal dynamics. These issues are key to the stability of permafrost as organic soil horizons and moss insulate permafrost from air temperature during the summer. Assuming that the model has not dealt with these issues, it would be useful to get a sense for potential biases in simulating soil temperature in northern high latitudes. You acknowledge the limitation of the 4-layer model and compare it with a 70 layer model to correct for biases in the 4-layer model. However, it would be good to document the performance of the 70 layer model, perhaps by comparing the output to data from the circumpolar active layer monitoring (CALM) network. I don't have any problems with there being a bias in the analysis, but I do want to get a sense of the degree of bias.

(5) Page 1372, line 10: Reference to Fig. 1. I don't see any units on either of the axes in Figure 1.

(6) Page 1372, lines 13 and 14: See my comment number 4 about the issue of accuracy. It would be good to establish the degree to which the 70 layer model is accurate vs. "assuming" it is accurate.

(7) Page 1372, line 18: I noticed that Burke et al. (2012) is "in preparation". It doesn't likely that the cited in preparation manuscript will be published prior to a decision on this manuscript.

(8) Page 1372, line 24: Change "interpolated" to "interpolation".

(9) Page 1373, line 27: It seems that there should be a comma after "pedons".

(10) Page 1374, line 13: "Turbles" should be "Turbels".

(11) Page 1375, section 2.3: One assumption that is being made here is that there are

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no new inputs of carbon into the thawed permafrost layers. I think you need to make a statement that the methodology does not consider this issue, and it would be good to also return to it briefly later in the paper, perhaps in section 5.

(12) Page 1375, line 15: Change “immediately the permafrost” to “immediately as the permafrost”.

(13) Top of page 1378: When I got to the end of section 2.3.2, I was surprised to not see any explanation of the last two parameters in Table 1. I see that the explanation of these parameters come in the Results section, but I was wondering I they could be explained in the methods, and it seems most appropriate to do this at the end of the full paragraph on page 1374.

(14) Page 1379, line 10: Change “at each year” to “for each year”?

(15) Sentence spanning lines 26-29 of page 1381: I really can’t figure out what was done “by perturbing the mean SOCC for each soil by 0.75 of the standard deviation and using . . .)”. Please rewrite this and justify this methodology better.

(16) Page 1382, line 25: “Schaeffer” should be “Schaefer”. It also seems that you may want to cite Zhuang et al. (2006; CO2 and CH4 exchanges between land ecosystems and the atmosphere in northern high latitudes over the 21st Century. Geophysical Research Letters 33, L17403, doi:10.1029/2006GL026972.) along with Schaefer, Schneider, and Koven.

(17) Page 1384, line 15: “Eq. (3)” should be “Eq. (4)”.

(18) Page 1385, lines 21 and 22: I assume you mean “high latitudes” instead of “lower latitudes”.

(19) Page 1394: For the “soil reduction” parameters in Table 1, perhaps rename them to be “soil SOC reduction” to be more descriptive.

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