

Interactive comment on “Projecting the release of carbon from permafrost soils using a perturbed physics ensemble” by A. H. MacDougall and R. Knutti

Anonymous Referee #1

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Comments on the manuscript by MacDougall and Knutti “Projecting the release of carbon from permafrost soils using a perturbed physics ensemble” submitted to Biogeosciences Discussions.

Overall Evaluation This manuscript presents the results of a study that uses a new version of the UVic ESCM to conduct a parameter uncertainty analysis to report uncertainties in the release of carbon from the permafrost region to the atmosphere for four RCP scenarios through the year 2300. In general, the study finds that the mean response and range of uncertainty of the new version of the UVic ESCM are more in line with other recent syntheses. Some of the conclusions are similar to those of pre-

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vious studies: (1) the permafrost carbon feedback is most important for scenarios with substantial mitigation, (2) that permafrost soils are expected to release carbon for a very long time because of the long time lag between forcing and response. The analysis does identify among the parameters considered that better constraints on the size of the non-passive soil carbon pools and on the equilibrium climate sensitivity of the model will substantially reduce uncertainty of responses of carbon in the permafrost region. Finally, the analysis includes an analysis of permafrost carbon responses out to year 10,000.

In general, I like the design of this study, and the analyses are quite competent. The parameter sensitivity analysis is very welcome and valuable with respect to building on the recent data synthesis of Schadel et al. (2014) on the quality of soil carbon in the permafrost region. However, there are three shortcomings in presentation and discussion that I think should be addressed in a revision: (1) a clear communication in the Introduction of the objectives/questions of this study is needed, (2) what about uncertainty with respect to parameters not considered by the analysis, and (3) better justification of the deep future analysis. Below I provide more of my thoughts on these issues followed by other specific comments in the manuscript.

What are the objectives/questions raised in this study? The Introduction is very vague with respect to communicating the key objectives/questions of this study. The Introduction has paragraphs on uncertainty in soil carbon quality, methods for analyzing model uncertainty, and multi-millennial simulations of anthropogenic climate change. However, these are somewhat disjointed and the Introduction needs to tie them together more effectively and communicate the key objectives/questions of the analysis after these “motivation” paragraphs. A key deficiency along these lines, is that the key take home of the parameter uncertainty analysis with respect to Schadel et al. (2014) didn’t even make it into the abstract.

What about uncertainty with respect to parameters not considered in this study. The manuscript needs to better justify why it focused on the 6 parameters it chose vs. other

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parameters it could have chosen. For example, a large component of uncertainty of the application of earth system models to analyzing the permafrost carbon feedback concerns the NPP response to increases in atmospheric CO₂, yet this was not even mentioned in the discussion. I'm not suggesting that the study conduct analyses of additional parameters, but that it adequately discuss the relevance of the parameters it chose to include vs. those it chose not to include in the analysis.

Better justification of the deep future analysis? By the end of the paper, I wasn't convinced that the "deep future" analysis was very insightful. It was somewhat interesting to read through, but its relationship to mitigation in the near future didn't come across to me. It just seemed glommed onto the rest of the paper to me. I suggest either better justifying it in the Introduction and more effectively discussing its relevance, or dropping it from the paper.

Specific comments

Title: I think the analysis of uncertainty is the most important aspect of this study, but "uncertainty" doesn't appear in the title. Also, why a "perturbed physics ensemble"? Don't some of the parameters analyzed represent biological phenomena? Wouldn't a "perturbed model ensemble" better wording?

Abstract, Page 19500, Line 17: You need to define "common era" for the reader. I could only guess at what was meant by the term.

Introduction, Page 19503, Line 19: Again, "common era" needs to be better defined.

Methods, Page 19504, line 25: "organic matter content" is mentioned, but does the model consider organic horizons? Note that Schadel et al. (2014) analyzed carbon quality for both mineral soils and organic horizons, so this is the reason why I'm asking. Also, I'm wondering about the role that organic horizons play in the soil thermal dynamics of the model. I think all of these issues should be elaborated upon in the Methods.

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Methods: Nothing is mentioned about inputs into the soil in the model description. How is NPP calculated and what are its sensitivities.

Results, Page 19511, Line 4: What do you mean by release of carbon from permafrost soils? Do you mean net changes in soil carbon, do you mean net loss of carbon from previous frozen soils, do you mean net changes in ecosystem carbon? How is the permafrost region defined in Figure 1 (it differs from Hugelius apparently). Please clarify.

Figure 6: Should the X axis be labeled “transformation” instead of “transmutation”?

Page 19513, Lines 18-19: Is this true of Hugelius’s map, or just the UVic map? Isn’t the issue that more permafrost carbon is exposed at the southern boundary because the thaw is deeper?

Page 19516, Line 4: Change “Incorporating this new data” to “Incorporating these new data”.

Page 19517, Lines 1 and 2: Change “effect” to “affect”? Rewrite sentence so that it doesn’t end in the preposition “for”.

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