

Interactive comment on “Improved estimates show large circumpolar stocks of permafrost carbon while quantifying substantial uncertainty ranges and identifying remaining data gaps” by G. Hugelius et al.

Anonymous Referee #2

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Hugelius et al. provide a detailed, thorough summary of the state of permafrost carbon stocks using updated circumpolar database information. This paper is a useful product that should interest many researchers, including carbon-cycle scientists, boreal and polar researchers, and other groups. What follows are some suggestions for improvement and some general comments.

One of the major improvements in the paper is the inclusion of uncertainty estimates. Because the topic plays such an important part in what makes the paper novel, the authors should add some description and material on this topic. To begin with, the

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ranges of uncertainty differ by class and geography. For 0-3m stocks, CIs for 95% and 99% are given. For Yedoma deposits, the values selected are the 16th and 84th percentiles. This discrepancy is confusing and should be revised. If the authors choose to include the 16th and 84th percentile estimates, for comparability to Strauss's paper, they should also provide at least a 95th or 99th estimate as well. What additional sources of error not included in the manuscript do the authors think are likely to be most important? Do the authors have data for adjacent soil sampling to look at intra-site variation? What are the instrument errors associated with their analyses? The uncertainty estimate does not include mapping uncertainty or, to my knowledge, these other uncertainties. A similar issue arises in the CIs for the delta estimates: “For most of the deltas included in this estimate, no field-observations of alluvium depth or SOC content are available”. I think this issue deserves greater emphasis. Furthermore, is the statement consistent with on from later in the manuscript: “While the new estimate uses many sources of data that differ from the initial estimate (Table 5), the main cause for the significant reduction is that including new field data greatly reduced the estimated mean SOC content (kgCm^{-3}) in alluvial deposits.” The reduction in carbon content is substantial compared to the 2009 estimate compared with 91 Pg).

The paper would benefit from clearer discussion of what causes differences with previous and alternate approaches. For example, do differences arise because of differences in carbon density per square meter, from depth extrapolations, or from land classifications? A clearer summary statement would provide a stronger et of future research needs to reduce uncertainties.

Similarly, a clearer breakdown of “thin” and “thick” categories would be helpful. There isn't a dichotomy, of course, but a continuous distribution across the two categories. In fact continuous rather than categorical treatment is something that a revised or follow-up manuscript could tackle.

I would like to see more discussion of the location of the pedons in Figure 1. As much of an improvement in additional profiles that this manuscript represents, the vast majority

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of Asia in particular remains unsampled. This shortcoming is particularly clear for the blue “Continuous” area in Figure 1.

The main text should include a map of the locations of the river deltas applicable to the estimates (see Table 5 and Figure 1).

The figures, particularly 2 and 3, are not legible. The print is too small to read easily. These should be revised to be more easily accessible.

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