

Interactive comment on “Ideas and perspectives: why Holocene thermokarst sediments of the Yedoma region do not increase the northern peatland carbon pool” by G. Hugelius et al.

Anonymous Referee #2

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Reviewer comments The results of Walter Anthony et al. (2014; hereafter WA) provide an interesting and provocative rough estimate of how thermokarst-lake processes in yedoma re-sequester large amounts of C into the upper portion of the land surface (variable depth but average ca. 5 m), assuming moderate longevity of lakes after formation. There are two potential issues with the WA estimates. First, the internal study methods WA used could be flawed in regard to choices of upscaling approach, calculation of C content, etc. Second, and the subject of this paper, their comparisons with other estimates of the stocks of C in permafrost, specifically peat carbon, are not correct, and the estimated large increase in potential C storage is an artifact.

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To make the argument clearer it may be useful to alter the focus of the paper in two ways: i) add more information about the calculations summarized in Figure 2, and provide enough information so that the reader does not have to plough through WA or the database papers to figure out where the numbers come from; ii) divert the focus somewhat from the issue of peatlands terminology to the carbon pool in general. After all, the main point of the paper is that the new C pool described by WA may be overestimated, no matter what it is called.

1. The first argument is that WA “primarily studied mineral lake sediments, and these do not match widely used international scientific definitions of peat or organic soils”. WA encountered a range of sediments in alas deposits, including sediments that would be described as peat (e.g., in-situ layers formed by subaqueous aquatic mosses) and sediments that are clearly not peat (lake sediments derived from minerogenic sources plus organic detritus and production). The majority of material in sections is the lake sediments (which are named “stratified muddy peat” by WA). Therefore, I agree that the terminology is confusing. The thawed yedoma (taberal sediments) and lake deposits that underlie alases are largely not peat, and thus technically cannot be added to the peat carbon pool. However, this is a semantic argument (as the authors themselves acknowledge): while it remains unfortunate that WA published using confusing terminology, the point that a large store of (possibly uncounted) OC has been identified is not discounted by this argument.

2. The next argument is that “independent field data and geospatial analyses show that the Siberian Yedoma region is dominated by mineral soils, not peatlands. Thus, there is no evidence to suggest any systematic bias in the NCSCD field data or maps”. When studying soil profiles in alases, the authors note there is not that much the way of extensive peat cover (as observed from the surface down). Here, one could raise questions about what constitutes an alas, or indeed a thermokarst lake, or a yedoma thermokarst lake. The landscapes brought under the umbrella of “yedoma” are variable, as are the lakes in them and the developmental trajectories of the lakes. Figure

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1 shows that in the lower Kolyma and adjacent coastlands, quite a few independently surveyed alases support peatland (peat growing on the surface), but in study regions to the west, none of the surveyed sites were classified as peatland. It may be that for whatever reason (developmental, climatic, etc.) alases in some regions tend not to develop surface peat. From the WA profiles, the same variability characterizes the way in which thermokarst sediments were deposited through the Holocene within their region of study. Yet there are few stratigraphic data from other regions, and this may have implications for the way the WA upscaling was done (which is a new argument). I agree, given this regional variability, there is nothing obvious that indicates the soil/peat mapping, based on the occurrence of surface peat, contains an unrecognized source of error.

3. The final point is that there is “spatial overlap between these Holocene thermokarst sediments and previous estimates of permafrost soil and sediment OC stocks. These carbon stocks were already accounted for by previous studies and cannot be added to the permafrost OC.” This is the main point of contention. Here, I worry the reader is not given enough information as to how the deeper sediments are characterized in either a) or b) of Figure 2. I do not think the reader should be expected to delve deeply into the various cited publications to find this out; thus the key numbers and calculations could usefully be clarified. From Figure 2, and as discussed in the text, the two estimates of Holocene sediments, a) and b) are similar (excluding how unchanged yedoma is dealt with (which also needs explaining further). The thermokarst sediments of a) are estimated to be greater than those of b), which are the focus of the enhanced carbon argument. I was expecting the soil-database estimates to be lower. IF these calculations are supportable, then the WA argument –that the WA estimate greatly increases C values over previous estimates – seems to be refuted. But we need more information to judge the argument effectively.

Other comments: 1. As regards Fig 1: Please explain further. Is the pale colour (0 of 4) actually the distribution of yedoma? (If not, the yedoma distribution should

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be shown; if it is, it should be more clearly labelled.) I cannot see any of the black (dark) colour on the map (4 of 4). Should one assume there are no 4 of 4 localities? The site location dots are so large they obscure possibly important map cover data. Possibly separate these two sets of information into two more effective figures? 2. page 18094-25 “In brief, earlier studies did not include a pool of OC stored taberites, an in situ thawed, diagenetically altered Yedomia deposit, and applied bootstrapping approaches to calculate OC stocks while Walter Anthony et al. (2014) use arithmetic means.” I think this needs re-writing. . . . In brief, earlier studies did not include a pool of OC stored in taberites, an in-situ, thawed, and diagenetically altered Yedomia deposit. Further, they applied bootstrapping approaches to calculate OC stocks while Walter Anthony et al. (2014) use arithmetic means. It would be helpful to explain why this difference of approach is significant. 3. This paper is largely well written, but there are quite a few minor typos and spellings, which are corrected in the attached reviewer version of the MS.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/12/C8888/2016/bgd-12-C8888-2016-supplement.zip>

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