

Interactive comment on “Sources and characteristics of terrestrial carbon in Holocene-scale sediments of the East Siberian Sea” by Kirsi Keskitalo et al.

Anonymous Referee #3

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General Comments:

In many ways, this manuscript is similar to the Tesi et al 2016 (Nature Comms) paper (which was about the Laptev Sea) but this is for the East Siberian Sea. It is a reasonable contribution to furthering our knowledge about this part of the East Siberian Sea during the Holocene. Towards the end of the manuscript, several arguments are made that are not supported by the data, and need to be changed or fixed. There are some gaps in the methods that need to be filled.

This manuscript is fine. It is a bit dull, but it is fine. I can tell that a great deal of work went into the sample analysis; and this research group is well known for their extensive

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and detailed use of biomarkers in sediments, this aspect of the manuscript is a great display of their talents. The worst part of this manuscript is some acute form of over-citation disease, and high levels of self-citation, at times choosing a self-citation even when it is the incorrect citation. The introduction alone has 65 citations—and about 37 of those references are to the same research group that produced this manuscript. I am well aware that this group is active in this research area, but many of these seem unneeded. The over-citation is part of what makes the manuscript dull to read.

I believe the manuscript can be made suitable for the journal, but there needs to be considerable cleanup first.

Detailed Comments:

line 39-40: I don't believe it is necessary to cite 7 papers for the permafrost carbon feedback. Also, the 3 Shakhova et al references are primarily about methane emissions from the sea, and are absolutely NOT primary sources for describing the permafrost-carbon feedback, and should definitely not be cited at this point.

line 43-45: Are 14 references really needed to prove the statement that there have been recent studies on carbon cycling in/between the sea and land in the Arctic? Again, I'm well aware this group is active in this research area, but at 14 references, it starts to look like h-index padding. Besides, this reviewer is aware of plenty of additional papers on the topic—many co-authored by authors of this manuscript—which are NOT listed here. I am of the opinion that actually ZERO references are needed at this point.

line 48: "profoundly destabilized"? How is that different from "destabilized"?

line 66-72: Are you writing a paper about the ESS or Laptev Sea? I'm not sure what the internecine citation battle on these lines, showing conflicting and agreeing results from the same group in the Laptev Sea has to do with the ESS. I think most of this can be removed. Besides, the same topic is covered adequately beginning on line 371. No need to repeat it here.

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line 84: I don't think you need to redefine ESS again here.

line 104: "Today the period with less sea ice in the ESS"... less than what? do you mean the ice-free season?

line 110: I/B not i/b line 110: This reader would rather know when the core was collected than the months of the entire cruise.

line 121-122: I'm a little surprised that there is no acknowledgement to the Swedish Museum of Natural History.

line 129: Is "Stackebo, Sweden" a company or a place?

lines 138-141: The Pearson et al 1998 reference is basically how NOSAMS works. That's fine, but you need to say more here about molluscs. How were these molluscs retrieved? How were they processed and prepared BEFORE being sent to NOSAMS? What was actually sent to NOSAMS? (And I'm fairly certain these are mollusc shells, not complete molluscs). Also, the Pearson et al reference isn't about dating cores, or anything, with molluscs—please provide a reference for that. The entire analysis and conclusions rely on this mollusc dating, and the authors have unfortunately breezed over it as if they were seashell collecting.

line 144: include a reference here for Marine13 calibration curve: Reimer et al 2016, https://doi.org/10.2458/azu_js_rc.55.16947

line 147: just make it easier to read, say "(calendar years before present)" and provide the abbreviated form (cal yrs BP) if needed.

line 175: trimethylsilyl is misspelled.

line 206: "effectively estimate" doesn't make any sense, unless you're trying to say that you were not able to make an estimate. I think "estimate" is adequate here.

line 207: Wasn't this method was used in a number of other studies from the SAME research group before Tesi et al 2016a—indeed, even before Anderson et al 2015. Al-

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though Anderson et al 2015 is an adequate reference.

line 212: this is an odd reference for the endmembers, because the end members are not first used or defined in the Bröder et al or Tesi et al paper; they are USED in these papers, but the end members come from earlier work, at least that is my understanding.

line 214: " -26.3 ± 0.63 ‰" Perhaps sometime (perhaps not in the present manuscript), this research group could explain how *all* the carbon in ice complex deposits has such a precise, narrowly-constrained $\delta^{13}C$? That seems spectacular.

line 221: hexametaphosphate is misspelled.

line 236-237: "Although any actual sediment transport"... is not a complete sentence.

line 241: The ice scouring argument would be more convincing if the authors provide an estimated sea depth at the time of the putative scouring event. Shallow water depths make it more likely, correct?

line 247, line 260, line 268 etc: "East Siberian Sea" – should be ESS throughout manuscript once it has been introduced.

line 259: needs a comma. "period, but has a similar"

line 276: "These compounds have been widely used in recent studies of terrestrial OC in the Arctic"—curious selection of citations here. There are so many possibilities here that I wonder why you chose these four? Also, this is simply saying "everyone is doing it, so we did the same thing"—popularity isn't the same thing as a method being "good" or suitable. So skip that sort of argument—better to cite the original method papers.

line 313: I would say it is a matter of opinion whether the parameters shown in figure 3 are "near-continuous" across the 6500 year hiatus. They don't all look that way to me. Another possibility besides bioturbation is that the values are similar because...they are similar. That simple explanation is not outside the realm of possibilities.

line 322-330: so why bother to tell the reader about lignin phenols acid/aldehyde ratios,

only to conclude with saying that they're not a useful degradation proxy? (This also applies to Figure 4, which seems to be based on this proxy that we were just told is not useful.)

line 335-336: "The only source of 3,5-Bd in the marine environment is from brown algae which are not common in the study area (Goñi and Hedges, 1995; Tesi et al., 2014)." Tesi et al 2014 is NOT an appropriate reference here, as that paper ONLY mentions brown algae ONCE, and then only when referencing Goni and Hedges 1995.

line 348: "The longer distance from the coast allows more time for organic matter to degrade before burial". Why, exactly, is it surprising that it takes longer to transport organic matter farther from the coast than near the coast? This seems self-evident.

line 354: "suggests that with longer transport time lignin degradation is more extensive"... This is again self-evident, right? With longer transport time, pineapples also degrade more. I'm well aware that this research group has written papers about this, but what I don't understand is what's the opposite scenario the authors are arguing against here? That shorter transport time could somehow result in more degraded lignin? Why would anyone even consider that?

line 366: "East Siberian Arctic Shelf" – you defined this as ESAS a long time ago.

line 367-368: "The proportion of old terrestrial organic matter might also be greater in Arctic sediments due to generally low primary production in the area (Stein and Macdonald, 2004)." This is really confusing. This sounds like you're saying that primary production is low (in the sea?). But this is about the core, so it's really the sea in the past, correct? And what about this paper: East Siberian Sea, an Arctic region of very high biogeochemical activity, Leif Anderson et al 2011, Biogeosciences. Or are you arguing that Stein and Macdonald say that the part of the ESS where the core was collected is different than that discussed in the Anderson et al paper?

line 382: "When the shoreline was farther seaward during the early Holocene, the core

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PC23 from the Laptev Sea experienced"

No, the core didn't exist then. You mean that the location where the core was collected experienced...

line 385-389: "Although the record of GC58 does not go back in time to the glacial-interglacial transition at the very onset of the Holocene, our results suggest that coastal erosion was likely the dominant process affecting the permafrost carbon supply and deposition also at that time. This seems likely, especially when considering the location of the core GC58 in between the rivers, and as has been observed in modern day shallower sediments in the East Siberian Sea (Bröder et al., 2016b; Vonk et al., 2012)."

This would be an interesting argument, but the results of this study don't suggest this! As is plainly stated, the GC58 core does not extend to the glacial-interglacial transition! Therefore results from that core cannot suggest anything about coastal erosion at that time. This needs to be removed or changed.

line 418: I see the regime shift, but there could have been many regime shifts in the missing years on both sides of 8400 years BP.

line 421: "The source apportionment data highlights the importance of coastal erosion as a terrestrial carbon source to this region of the ESS throughout the Holocene."

"throughout the Holocene"? The core is missing 6500 years of the Holocene (more than half!) This core is not suitable for any "throughout the Holocene" pronouncements. Remove or change this statement.

line 441: IB/RV is not a ship designation. Try "I/B Oden" or just IB.

line 507: please update if this paper has been accepted or published.

Tables and Figures

Table 1: What is "NOSAMS Accession Number" and why is it important to include here? I see that these numbers appear on Figure 2, but WHY? How is this important

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to the study?

Line 717: again here, you mean mollusc shells, not molluscs (presumably)

There is general sloppiness in the figures. For instance, Figure 2 uses a serif font, while others are sans-serif. At least use the same font style throughout. No reason to use italics in Figure 1. Figure 5 has bold axis labels—which is fine, but none of the other figures are boldface. Be consistent.

Figure 2 is going to have unnecessarily minuscule text unless it is printed or viewed at full-page size. Increase the font size.

Also in Figure 2 caption, tell the reader what the tiny curves are on the figure. I think I know what they are, but explain it, or remove it.

In Figure 4, the more degraded- less degraded triangle is ugly; the same thing is implemented much more elegantly in Karlsson et al 2016—from the same research group.

These figure problems do not change the content, but give a bad impression to the reader, as if the manuscript was prepared in a hurry.

Line 764: "Lignin composition of the sediment core GC58 (black circles)." There are no black circles in this figure! (blue circles?)

Line 769: "and with an orange square (\pm standard deviation)" There are no orange squares! (red square?)

Line 776: "Yedoma" — yedoma is not a proper noun and should not be capitalized.

Line 777: NO, these end members values are NOT from Bröder et al 2016b and Tesi et al 2016a! Those are simply two recent papers from this research group that used the same end members! The "literature" these are based on is a whole different set of papers. Figure 7: Why is the green arrow jagged? Why not just a straight arrow? Or did I miss something?

Figure 7: the ICD-PF error bars extend BELOW D14C=-1000 per mille. This is not physically possible.

Figure 7: It should be made clear that PC23 is the core from Tesi et al 2016, NOT THIS MANUSCRIPT.

Supplement

Line 41: "An estimation of the lateral transport time of sediments shown as ..." I think you mean "An estimate of lateral transport time ..."

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2017-20>, 2017.

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