Clim. Past Discuss.,
https://doi.org/10.5194/cp-2017-20-AC4, 2017

Climate of the Past
Discussions

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

Kirsi Keskitalo et al.

keskitalo.kirsi@gmail.com

Received and published: 8 August 2017

We are grateful to all the three reviewers for their comments on the manuscript. These constructive and overall positive comments have improve the manuscript during revisions. The referee comments are given first and our response follows. All references to line numbers refer to the revised track-changes document. Please find the revised manuscript and supplementary information as track-changes document attached (one .pdf file).

Reviewer #3, anonymous

Printer-friendly version

Discussion paper

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 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 overcitation 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.”

RESPONSE

Thank you for your comments. To the best of our knowledge, Tesi et al. (2016) is the first sedimentary record studied in detailed to understand the land-to-ocean remobilisation of PF-C during the last deglacial period. The current paper is thus only the second one available to provide this type of new information, and from a different setting/shelf sea. Of course, there are similarities in methodology and study design between our study and the study by Tesi et al. (2016). We believe this is a plus as it is thus allows to compare the outcomes from these two systems. We also find this East Siberian Sea (ESS) study to be a worthy contribution as, to the best of our knowledge, there are no previous studies from its shelf area involving gravity/piston cores. We have found a completely different signal in terms of carbon fingerprint compared to Tesi et al. 2016 study. Thus, our results are valuable. We have gone through the citations and decreased their amount (in total 12 were removed), especially to studies performed by us and/or collaborating partners. Hopefully this will make the manuscript more pleasant to read while still giving appreciation to the relevant literature.

We have also made improvements to the figures and tables by matching the font styles and revised some of the figure captions.

SPECIFIC POINTS

1) “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 permafrostcarbon feedback, and should definitely not be cited at this point.“

The number of citations has been reduced to four as the three papers by Shakova et al. have been removed.

2) “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.”

Thank you for your opinion. Here we wanted to give examples of recent studies that have been done on carbon cycling. We are also aware that there are many excellent studies about this topic that could have been cited here which is why the current number of citations was as high as 14. We have removed all the references from here as the text works also without any citations.

3) “line 48: "profoundly destabilized"? How is that different from "destabilized"?”

We understand that it is not easy to define where is the difference between destabilised and profoundly destabilised but referring to the large remobilisation of permafrost that followed the destabilisation we chose to use the word profoundly to emphasise the extent of permafrost destabilisation at that time.

4) “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.”

To keep the focus on the ESS we have removed the last phrase of the paragraph that was covering only the Laptev Sea (L71–73).

5) “line 84: I don’t think you need to redefine ESS again here.”

Text has been changed.

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

The word less was referring to the period when there is sea ice present. We have rephrased the sentence to make it clearer (L110–112).

7) “line 110: I/B not i/b“

The ship designation has been changed to I/B.

8) “line 110: This reader would rather know when the core was collected than the months of the entire cruise.”

The collection month of the core has been specified in the text (L116).

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

We are very grateful to Karin Wallner from the Swedish Museum of Natural History who helped us with the 210Pb analysis and she has been acknowledged for this. We have now added her affiliation to the acknowledgements as well as affiliations of the other technical personnel who have assisted us in our work (L469–470).

10) “line 129: Is "Stackebo, Sweden" a company or a place¿‘

Stackebo refers to a mine in Västergötland province in Sweden from where the reference material has been retrieved.

11) “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 molluscsâAËŸTplease provide a reference for that. The entire analysis and ËĞ conclusions rely on this mollusc dating, and the authors have unfortunately breezed over it as if they were seashell collecting.“

Thank you for your comment. We have clarified in the text that we have analysed mollusc shells and not complete molluscs. The mollusc shells were handpicked from the core, rinsed with MilliQ water and sonicated before sending them to NOSAMS for 14C analysis. It is common practice to analyse these kind of samples at the NOSAMS facility which is why we have not specified the process in detail. Correct, Pearson et al. (1998) paper is about accelerator mass spectrometry (AMS) measurements at NOSAMS which is why we think it is a suitable reference here. We do explain the age model construction (using the results from the 14C analysis of the shells) for the core GC58 in the following paragraphs of the same section (Sect 2.4).

12) “line 144: include a reference here for Marine13 calibration curve: Reimer et al 2016, https://doi.org/10.2458/azu_js_rc.55.16947 “ Thank you for pointing out the lack of this reference. This reference has been added accordingly to the text (L151) and also to the Table 1 caption (L769) and to the Figure 2 caption (L787).

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

The text has been changed accordingly (L154).

14) “line 175: trimethylsilyl is misspelled.”

Thank you for pointing this out. The spelling has been corrected.

15) “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.”

We have removed the word effectively from the text.

16) “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. Although Anderson et al 2015 is an adequate reference.”

Correct, the source apportionment method has been used in several studies before but the studies by Anderson et al. 2015 and Tesi et al. 2016a use Markov Chain Monte Carlo method for the source apportionment as the previous studies are based on random sampling.

17) “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.”

Thank you for pointing this out. To give credit to previous and original work the citation has been changed to a paper by Vonk et al. (2012) which compiles end-member data from several studies for the ICD-PF and topsoil-PF and Smith et al. (2002) for the Marine OC end-member. Also, we noticed that the end-member values were reported slightly incorrectly here and have changed them to the actual values that were used in the analysis. This did not affect any of the conclusions.

18) “line 214: "=-26.3±0.63 ‰sometime (perhaps not in the present manuscript), ÂÍ this research group could explain how *all* the carbon in ice complex deposits has such a precise, narrowly-constrained d13C? That seems spectacular.” We agree that a discussion of the precision of the δ13C end-member does not fit the scope of this manuscript. This range reflects a compilation of individual samples (n=374) with an average value of -26.3‰ and a standard deviation of 0.63‰ (Vonk et al., 2012). With growing knowledge on ICD-PF we also hope to learn more about the variability of its δ13C values

19) “line 221: hexametaphosphate is misspelled.”

Thank you for pointing this out. The spelling has been corrected.

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

This sentence has been rephrased (L245–246).

21) “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?”

The water depth of the coring site in ~1,700 cal yrs BP would have been similar to the current water depth (52 m) and around 34 m in ~8,500 cal yrs BP (Lambeck et al., 2014). According to Ananyev et al. (2016) most ice scouring events happen in <40 m sea depth though ice scour marks have been observed in greater depths as well. We have now changed the wording to present ice scouring as one possible, but not the exclusive explanation as the age gaps could have been caused by some other event as well (L249–251). Also the respective water depths have been added to the text.

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

The changes have been made accordingly throughout the manuscript.

23) “line 259: needs a comma. "period, but has a similar".”

Thank you for pointing this out. The missing comma has been added to the text (L270).

24) “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.

Thank you for your comment. The idea behind the argument was to point out that the method has been widely used as it is a good and working method to study terrestrial OC, not so much to say that it is popular. This also regarding the citations, they are meant to present a few recent studies that have successfully used the method. We have rephrased the sentence (L287–288).

25) 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.”

Thank you for your opinion. This is indeed also a possibility and it has been added to the text as an explanation (L327).

26) “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.)”

We have shortened the text regarding the acid to aldehyde ratios and moved the Figure 4 to Supplementary information (Supplementary Figure S3). Even though the proxy might not be useful in our study, the data could be useful in future study designs regarding which proxies to use and not to use. It also supports the argument from the studies of Goñi and Hedges (1995) and Tesi et al. (2014). In addition, we want to show the degradation proxy data as these kind of data have not been reported in Holocene scale from the ESS.

27) “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.”

We have left only the citation to Goñi and Hedges (1995) to the text.

28) “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.”

The longer distance here is linked to the previous phrase about how the coast line has shifted during the past ~9,500 yrs due to the sea level rise thus making the transport time longer. The statement could be self-evident but yet we want to underline it to explain why the amount of lignin declines in the core.

29) “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?”

This part maybe self-evident, but it may nevertheless be the best explanation why the lignin in the core is more degraded at the top of the core than at the bottom. We state how we interpret our data and do not argue against opposite scenarios.

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

We have revised to abbreviate East Siberian Arctic Shelf to ESAS both here as well as on line 247.

31) “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?”

With low primary production we meant that the primary production in the Arctic regions is relatively low in a global scale regarding its seasonality, long sea ice cover and cold temperatures. We see that the phrase is a bit ambiguous and have removed it.

32) “line 382: "When the shoreline was farther seaward during the early Holocene, the core PC23 from the Laptev Sea experienced" No, the core didn’t exist then. You mean that the location where the core was collected experienced... “

Thank you for pointing this out. The wording has been changed to refer to the location instead of the core (L397).

33) “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."

We have changed this paragraph. We suggest that coastal erosion was likely an important process at glacial-interglacial transition supplying permafrost carbon to the ESS. This is presented as a hypothesis which has been made clear in the text (L400–405).

34) "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."

It is correct that we do not know what happened in the period from ∼8,200 to 1,700 cal yrs BP and from ∼9,300 to ∼8,500 cal yrs BP. We have changed the text (L432–433).

35) "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."

This statement has been changed to be more accurate regarding the Holocene time periods that we are referring to (L436–438).

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

The ship designation has been changed accordingly here (L458) and also in L467.

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

This paper has not been accepted or published yet.

Tables and Figures

38) "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 to the study?"

The NOSAMS Accession Number is used for archiving and traceability of samples analysed at NOSAMS. It might be useful in the future if there is a need to go back to the raw data reports from the NOSAMS laboratories which is why we report it.

39) "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."

Thank you for your comment. The word mollusc have been changed to mollusc shells both in the Table 1 as in its caption (L767 and 770) and also in the Fig 2 caption (L785). The italics in Fig 1 have been changed to normal font. The bold axis labels in Fig 5 are not boldface anymore.

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

The font size has been increased.

41) "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."

The Figure 2 caption has been updated (L785–791).

42) "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."

Thank you for your opinion. We have changed the color of the triangle and present two triangles instead of one to make the figure more pleasant to look at. Also, the figure has been moved to the Supplementary Information (Supplementary Figure S3).

43) "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."

Thank you for pointing out the inconsistencies within some of the figures. We have made revisions and minor changes to make them more pleasant for the readers to look at.

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

Thank you for pointing this out. The text has been corrected to blue circles (L843).

45) “Line 769: "and with an orange square (±standard deviation)" There are no orange squares! (red square?)”

Thank you for pointing this out. The text has been corrected to red square (L848).

46) “Line 776: "Yedoma" âĂŤ yedoma is not a proper noun and should not be capitalized.”

Thank you for the grammar correction. The word yedoma is not capitalised in the text anymore (L836).

47) “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.”

The citation has been changed to a study by Vonk et al. 2012 (ICD-PF and topsoil-PF) which compiles end-member values from different studies and Smith et al. 2002 (Marine OC) (L837–839).

48) “Figure 7: Why is the green arrow jagged? Why not just a straight arrow? Or did I miss something?”

The arrow follows the direction of the dual isotope values in the GC58 core from the bottom of the core to the top. The arrow is jagged to show that there is a drop in the Δ14C values in the middle of the core.

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

Thank you for pointing this out. We have corrected the ICD-PF error bar. We have also changed the y-axis so that it ends at -1000 ‰

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

A reference to Tesi et al. (2016a) has been added to the caption to clarify that PC23 is from a different study.

Supplement

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

Thank you for pointing this out. The word estimation has been changed to the word estimate.

ADDITIONAL CHANGES

In addition to the comments from the referees we have made the following changes during our revision process.

Tables and Figures

Regarding Table 1 we have specified that the calibrated age is a mean age and present 2 sigma error instead of 1 sigma. We have also added the median calibrated age. To the caption of the Table 1 we have added the ΔR value and the calibration curve used. We have changed the order of the Figures 5 and 7 (now 5 and 6) so that the Figure 5 presenting dual-carbon isotope composition of the sediment cores GC58 and PC23 comes before Figure 6 presenting lignin composition of the core GC58. These figures were presented in a wrong order in the original manuscript.

References

Ananyev, R., Dmitrevskiy, N., Jakobsson, M., Lobkovsky, L., Nikiforov, S., Roslyakov, A. and Semiletov, I.: Sea-ice ploughmarks in the eastern Laptev Sea, East Siberian Arctic shelf, Atlas Submar. Glacial Landforms Mod. Quat. Ancient, Geol. Soc. London, Mem., 46(1), 301–302, doi:10.1144/M46.109, 2016.

Goñi, M. A. and Hedges, J. I.: Sources and reactivities of marine-derived organic matter in coastal sediments as determined by alkaline CuO oxidation, Geochim. Cosmochim. Acta, 59(14), 2965–2981, doi:10.1016/0016-7037(95)00188-3, 1995.

Lambeck, K., Rouby, H., Purcell, A., Sun, Y. and Sambridge, M.: Sea level and global ice volumes from the Last Glacial Maximum to the Holocene, Proc. Natl. Acad. Sci., 111(43), 15296–15303, doi:10.1073/pnas.1411762111, 2014.

Smith, S. L., Henrichs, S. M. and Rho, T.: Stable C and N isotopic composition of sinking particles and zooplankton over the southeastern Bering Sea shelf, , 49, 6031–6050, 2002.

Tesi, T., Semiletov, I., Hugelius, G., Dudarev, O., Kuhry, P. and Gustafsson, Ö.: Composition and fate of terrigenous organic matter along the Arctic land-ocean continuum in East Siberia: Insights from biomarkers and carbon isotopes, Geochim. Cosmochim. Acta, 133, 235–256, doi:10.1016/j.gca.2014.02.045, 2014.

Tesi, T., Muschitiello, F., Smittenberg, R. H., Jakobsson, M., Vonk, J. E., Hill, P., Andersson, A., Kirchner, N., Noormets, R., Dudarev, O., Semiletov, I. and Gustafsson, Ö.: Massive remobilization of permafrost carbon during post-glacial warming, Nat. Commun., 7, 13653, doi:10.1038/ncomms13653, 2016.

Vonk, J. E., Sánchez-García, L., van Dongen, B. E., Alling, V., Kosmach, D., Charkin, a., Semiletov, I. P., Dudarev, O. V., Shakhova, N., Roos, P., Eglinton, T. I., Andersson, a. and Gustafsson, Ö.: Activation of old carbon by erosion of coastal and subsea permafrost in Arctic Siberia, Nature, 489(7414), 137–140, doi:10.1038/nature11392,

2012.

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2017-20/cp-2017-20-AC4-supplement.pdf

---

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