

Interactive comment on “Carbon mineralization in Laptev and East Siberian Sea shelf and slope sediment” by Volker Brüchert et al.

Anonymous Referee #2

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The manuscript "Carbon mineralization in Laptev Sea and East Siberian Sea shelf and slope sediment" of Brüchert and co-workers describes interesting sediment data from 19 different stations in the East Siberian Sea. The authors measured depth profiles of geochemical data such as DIC concentrations and their stable isotope signatures, oxygen, sulfate and ammonium concentrations as well as process data of sulfate reduction and oxygen consumption. Furthermore, they used the profile data of manganese and iron to model manganese and iron reduction rates. Based on DIC stable isotope signatures and the fraction of DIC from organic matter mineralization they derived the contribution of marine and terrestrial organic matter to overall organic matter decomposition using a common endmember model. Finally they upscaled their data to the outer Laptev Sea and the outer East Siberian Sea. This is an impressive data set from a region, which is only hardly accessible and of which only few data are available. The

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presented data are of great interest for readers of Biogeosciences and worth being published.

However, the presentation and interpretation of the data need substantial improvement. The manuscript is very long and contains substantial reiterations. But more importantly, a part of the manuscript has the classical structure of Introduction, M&M, Results and Discussion but a large fraction of Methods and Results is presented in the Discussion only. Some results are even discussed before Methods and Results are presented, which makes the manuscript hard to read. I suggest thoroughly rearranging the manuscript according to the classical structure of research manuscripts and shortening the manuscript by removing reiterations.

In particular, the description of the authors approach of partitioning total organic carbon degradation into terrestrial and marine sources needs substantial attention and should be clearly divided into a description of Methods, Results and Discussion (see below). In this context, I missed the carbon concentrations in the sediments and their stable isotope signatures. I assume the authors measured them and they will help to interpret the results of the "modelled" $\delta^{13}\text{C}$ signatures of respired organic matter.

Most of the data seem being related to anoxic carbon degradation processes, excluding aerobic organic matter turnover, although oxic processes are responsible for most of the organic carbon decomposition in the studied sediments. The authors should clarify throughout the whole manuscript if they relate to total, oxic or anoxic carbon decomposition.

The conclusions are very long and mainly a reiteration of the results and the discussion. It should be shortened substantially.

Specific comments:

L 29: Please give the depth used for integrating

L 48: These C amounts are stored in soils of permafrost landscapes. In the permafrost

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itself only 800 Pg are stored, see Hugelius et al. 2014. Please rephrase.

L 56: “qualitatively different rates” is unclear, please rephrase.

L 146 omit one “dissolved”

L 154-155: This is a reiteration from 2.1; please remove one of the descriptions.

L 176 – 200: Please indicate where the measurements were done (on the ship, in the home lab) and how the samples were transported.

L221-223: Reiteration from 2.1, please remove one of the descriptions.

L256: Please describe how texture was determined.

L 257: The designation of colors throughout the sediment description is not unambiguous. Please use an accepted color system such as Munsels.

L 296: These data are not presented in Table 3 and it seems they are not presented in the manuscript at all.

L 341-342: Please identify the station.

L 352-356: Here the presentation of the carbon concentration data are needed. Furthermore, to better illustrate changes in organic matter reactivity, SRR should also be presented normalized to organic carbon.

L 360: Decrease or increase of DIC? Furthermore, data of Station 50 should be presented in Fig. 4 if they are given in the text.

L 368: Please indicate where the rates of sedimentation are shown.

L 381-387: This are results that should be presented in the Results section.

L 395: This is a discussion of results that were not shown. Please show these results in the Results section before discussing them.

L 418: Table 4

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L 488-502: This is a description of methods, which should go to the Methods.

L 498: Please clarify “no or very minor”. Was isotope fractionation considered or not?

L 500: What means minor. Please show the formula for the calculations.

L 502-506: This should go to the Results.

L 505: -35.8‰ is a very low stable isotope value for organic matter, even lower than found for terrestrial organic matter in the hinterland of the Laptev Sea. To assess the significance of this value the stable isotope signatures of the bulk organic matter are needed. Is it possible that methane oxidation contributed to DIC? Are methane concentration values available and can they be referred to? L 525- 538: This is a method description and should go to the Methods

L 539 – L542: These data should be presented in the results.

L 545 – 570: This paragraph again contains mostly a description of methods. The description was also not completely clear to me. Better present the respective formula used for partitioning degradation rates into terrestrial and marine sources. Furthermore, I understood that the authors only considered organic matter degradation via sulfate reduction. If this is the case, it should be made clear, that this approach gives no information on most of organic matter degradation in the sediments (aerobic processes), which likely consume the most labile fraction of deposited organic matter.

L 571 – 573: I cannot see that the data in Fig. 7 shows the “influence of offshore transport of terrestrial organic matter”. The figure rather shows decreasing O₂ consumption rates and SRR from the shelf down the slope. Please clarify. Furthermore, the remineralized DIC ¹³C data from Table 3 rather show increasing terrestrial influence down the slope. How does this relate to the data in Fig. 7?

L 606: outer Siberian shelf sediment

L 609: Please show the carbon data in the Results.

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L 622 – 627: This should go to the Methods. Please clarify how the “degradation rate constant of organic matter” (L 620) can be determined by the anaerobic carbon mineralization (L 622) if latter only contributes 4 to 26% to total organic matter turnover in the sediments (Table 4).

L 635: anoxic degradation rate measurements?

L 648-650: This sentence should be rephrased since it is unclear. Which implications? If only anaerobic degradation rates are used in the assessment, isn't it obvious that no information on aerobic decomposition can be derived?

L 654 – 656: These results should go to the Results section.

L 654 – 661: Where are these data shown? Only in text of the discussion?

L 659: This sentence is unclear. Regression line of which data? How do you come from a slope of 5.5 to 18%?

L 663 – 664: The numbers for the contribution of anaerobic organic matter decomposition to total organic matter decomposition are given in Table 4 and are generally lower than 18%. This should be discussed.

L 664 – 666: I cannot follow this conclusion. If the contribution of anaerobic organic matter decomposition is only slightly lower (L 661) this means only that (relatively) more organic matter is degraded aerobically but I do not see any information on “highly reactive marine-derived organic material”. Please rephrase.

L 711 – 712: Can these data please be presented in the Results?

L 725 – 728: As I understand this relates only to anaerobic organic carbon mineralization. Please clarify.

L 748 – 151: This sentence is unclear. The manuscript did not present any data on priming. How would priming be assessed by this dataset? How can priming be “deduced from the dual contribution of terrestrial and marine-derived organic matter

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to DIC”? I suggest omitting any reference to priming or show a dataset that relates to priming.

L 1127-1129: Please quote the respective reference.

Table 2: Please give mean values also for SRR and O₂ uptake at the East Siberian Shelf and standard deviations for all mean values. Furthermore, indicate why a part of the data are missing.

Table 5: please explain TEAP

Figures 2-5: please give the legend at least in one of the panels.

Fig. 4 + 5: The δ -symbol in the axis name is missing

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