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Discussions

Interactive comment on "Seasonal and interannual variability of sedimentation and organic matter distribution in the Buor Khaya Gulf – the primary recipient of input from Lena River and coastal erosion in the SE Laptev Sea" *by* A. N. Charkin et al.

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General Comments This paper presents results for suspended solids and surface sediments collected from the Laptev Sea to the east of the Lena River delta. The measurements include particle size distribution, organic carbon content and δ13C, and are supported by other water-property measurements (e.g., T, S). As noted by the authors, the region suffers from a thin data base, such that an extensive set of collections from

multiple years provides a welcome set of geochemical data from which to infer sediment processes in this region. Furthermore, given that accelerating climate change is being manifest in these northern regions, it is even more important to understand the dynamics surrounding organic carbon, and how the storage or remobilization is being impacted. Accordingly, the data collected at $\sim$250 station points provides important opportunities to discuss this region. However, I think there are three major problems with the manner in which this manuscript delivers its findings. 1) Other work relevant to this paper seems woefully neglected. This is true both in the sense of previous work on organic carbon and δ13C measurements (see, for example, some of the references listed below), but also in the sense of prior publications from this general region investigating processes important to organic carbon including particle production and transport. For example, to discuss what happens under the ice in late winter, it would seem appropriate to draw from the Eicken et al. 2005 paper and its schematics. Likewise, in talking about POC distributions and transport, it would seem useful to bring in connections with Lalande et al. (2009) who present vertical flux data for the same time period. The lack of consideration of these and other papers actually weakens the presentation and isolates this paper. 2) The graphic presentation relies heavily on a series of areal displays which are hard to inter-compare, at least quantitatively Furthermore, where comparisons are made by reference to the figures, no estimates of confidence are possible (i.e., statistical comparisons based on t-tests or regressions). This manner of presentation then leads to frequent statements of difference not supported by any rigorous treatment. Given the amount of data available, this should not need to be the case. Much of the discussion could be better supported by a few well-chosen vertical sections to show distributions of properties and, where possible, property-property plots. No matter how the data are displayed, points at which data have been collected need to be included to give an idea of data distribution and density underlying the displays. 3) Finally, the application of δ13C as a tool to distinguish different sources is done in a haphazard fashion that leads to seemingly contradictory statements throughout the text. The result is that although there appear to be many

data available, no clear image of what they are saying emerges. The authors would do better to produce a coherent discussion of δ13C (sources and processes), which would include the limitations of this measurement in the present discussion, and then ensure that later statements do not over-interpret this parameter, or interpret it differently in different sections. As the paper stands, it is predominantly descriptive and, although there may be several years of data collection presented here, the database remains thin in the context of proposing interannual variability. That said, it is a valuable effort to try to make sense of widely different years, as the authors have done.

Specific Comments

P1918, Line 27. The sentence implies that marine biogenic carbon can be found in winter, but not summer. Here and in much of the text that follows, the potential for marine organic carbon to contribute to δ13C appears to be practically ignored from the discussion. What role does it play in the POC in this region? This sort of carbon is eventually discussed, but there needs to be a treatment of its properties and potential contribution much earlier in the text. See, for example, Vinogradov et al., 2000; Lalande et al., 2009; Sakshaug, 2004). P1919, Lines 13-15. I would agree that this important area of the Arctic Ocean has a shortage of investigations. But that makes it all the more important to provide an idea of what has been done, and to set the current findings within the context of other data and interpretations. Commencing here, and following through the text, the authors practically ignore other organic work on this shelf. I am not a Laptev Sea specialist, but I am well aware of related studies dealing with organic composition (%C, δ13C) and other relevant correlates by, for example, Peulve et al., 1993; Boucsein and Stein, 2000; Boucsein et al. 2002; Fahl et al. 2001; Stein and Fahl, 2004; Mueller-Lupp et al., 2000; Viscosi-Shirley et al., 2003; Lobbes et al., 2000; Petrova et al. 2008 and so on. One result of this neglect is that the authors have isolated their findings from the context of what is already known or proposed about the functioning of the inner Laptev Shelf In my view, this then leads to a surficial, descriptive interpretation of the new data, relying almost entirely on areal mapping, with

the result that the paper falls short of its potential. P1919 line 18-20. Where do these statistics come from? It would be helpful to provide some numbers here (e.g., typical input of the Lena for solids, freshwater and organic carbon set against input for the years data were collected). P1919 line 26. With regard to the uncertainty of how much sediment reaches the Laptev Sea, the reader is left hanging. Does this uncertainty remain? Do the present data help resolve this issue? Is the budget put together in Stein and Macdonald (2004) right or does it need revision? P1920 Line 10. Again, how much sediment (t/yr) is involved in this retreat? How does it compare with the river input? P1920 lines18-21. The statement of objectives should be clarified. The term "shed light" does not shed any light for this reader. P1921 lines 1-8. It is stated that 250 stations were occupied, presumably as marked in Figure 1. But were every one of these identical stations occupied during every mission including the one in late winter? This brief description neglects important details of sampling strategy (why, where, how many times, what was measured). The authors might consider presenting this partly in the form of a table. Also, some of the geographic locations used (e.g., Muostakh Island) are not marked anywhere, leaving most readers in the dark. Page 1921 lines 15-23. The precision/accuracy of the methods is very poorly presented. We are told only that accuracy and reproducibility are $\pm$ 0.1‰ for δ13C, without any basis for how they were determined, and nothing for any other measurement. Page 1922 lines 4-22. The discussion of water stratification and the subsequent distribution of suspended solids would benefit from the presentation of a representative section out from the coast. As it is, the paper relies almost entirely on comparisons of surface contour plots, which are difficult to read/compare and do not provide a sense of quantitative agreement between properties. Page 1922 line 14. Storm surge. How much, how measured? Page 1922 line 26. Where does the 'normal hydrometeorological condition of $\sim$10cm s-1' come from? Also if the authors mean to say that the normal current velocity is $\sim$10 cm s-1, then say that rather than presenting it in cumbersome jargon. Page 1923 line 7. Again, a sectional plot illustrating the nepheloid layer would be helpful to the reader. Page 1923, line 11-12. Figure 5 does not show the relationship between OC and sediment

size distribution. It would be better to provide a plot directly showing this relationship (e.g., PSD vs δ13C or POC) if that is what is desired, rather than forcing the reader to compare two areal contour plots. Page 1923, Lines 18-22. Discussion here and later would benefit from presentation of sectional views comparing the two regimes rather than text, which is difficult to visualize. Reference to pertinent work like that of Eicken et al., 2005, would likely also help. P1924, Lines 3-6. This paragraph is confusing, and it is very unclear where the assertions come from. Why do fluvial sources hover at -29 ‰ while there is a plume of -23.6 ‰ opposite the Bykovskaya channel? This seems contradictory. What is this plume? Page 1924, Lines 17-25. Again, this would be much better presented with a representative section(s). It is very difficult to visualize the differences between the sampling periods. P1925, Lines 5-6. What is the basis for this generalization? Citations? P1925, Lines10-14. The ragged presentation of δ13C results in this section seems very incoherent to me. Why is -29 ‰ given as indicative of fluvial sources above, then but then -26.4 ‰ is found here near the river mouth, with -29 to 32 ‰ found in the offshore. There needs to be a more coherent approach to the δ13C data, perhaps best done in one place in the discussion. A problem that must be faced with δ13C here, and everywhere, is that there are multiple sources of organic carbon, and each source has variability in its composition. This limits the application and requires a very careful treatment. It might help the discussion if C/N ratio were also brought into the discussion, but perhaps N was not measured. It might also help if the δ13C data were plotted against, for example, water depth, distance from shore, POC, psd, etc. This would do two things. It would give the reader a better notion of the range in data and their density, and also how they arrange themselves in space, or between years. If the highly depleted POC δ13C is from primary production based on pre-depleted terrestrial DIC, then that would suggest that autochthonous production might be an important component of the POC distributions. It also would mean that marine (or algal) carbon could be anywhere from -16 ‰ (ice algae) to -32 ‰ making δ13C of dubious value for source discrimination. And yet, this is hardly discussed at all. To this point in the manuscript it is as if the terrestrial POC dominates everywhere.

P1925, Lines 24-25. The reasoning here is unclear. It seems to me that there are potentially three main sources of POC in the system; river particulates, coastal erosion, authochthonous production. Both river and coastal erosion can have ancient components in their POC, but marine primary production would be modern. The discussion so far does not touch on the issue of carbon sources with any clarity or in a convincing manner that would suggest 14C could be applied to distinguish between organic carbon coming from rivers and from erosion. See for example Goni et al., 2005. P1925 Lines 28 onward. There might well be two regimes in this system depending on whether or not wind/wave energy is getting into the water column as proposed. This finding would not be surprising and there are many other papers that discuss the effect of storms and surges on particle transport processes. The presentation of the two schemes in Figure 7 could be improved by giving a better caption that explains the difference between the regimes. I'm not at all sure what is being shown in the small picture panels to the right. There needs to be text highlighting to the reader what the significance is of these panels to the findings being presented. P1926, Lines 7-15. It would help if some quantitative and specific context were provided. For example, how stormy were these periods (and how was this measured), how long did the storms last in a given year before sampling, what was the ice cover for various years, and how strong were consequent storm surges (m)? The generalizations are of little help. P1926, line 14. What does 'considerable' mean and which 'pressures' are being referred to? P1926, Lines 17-27. I have no argument with the conditions needed for resuspension, but did these conditions occur for the period in question, and if so, how were they measured? E.g., did currents exceed 25 cm s-1. By how much? For how long? P1927, Line 2. The points being made here toward the schematic in Figure 7 would be far better illustrated by sectional views than by comparisons between surface maps presented in Figures 2, 3. Page 1927, Line 8. Where did these observations of wave height come from? When were they made? Likewise what is the next statement regarding what is usual for the Buor Khaya Gulf based upon? Page 1927, Lines 15-17. Large volumes of terrigenous material? How much, how was this determined? Page 1927, Lines 20-25. I've no doubt

that these islands have disappeared, but Fig 1 does not inform me much about where they were. Page 1928, Lines 10-15. Again, this would be helped by sectional views. It's very difficult to visualize the comparisons from the present set of Figures. Page 1929. Although I think I understand what the authors mean about 'accumulation only', one has to ask how accumulation can occur without sediment supply. The authors need to clarify exactly what they mean here. Page 1929, Line 15. Discussion here would be helped by sectional views and/or references to appropriate literature on the functioning of the system in winter (e.g., Eicken et al., 2005) and in summer under low winds. Page1930, Lines 14-16. I'm unclear why Figures 1, 5 show that the Lena River delta has been growing. To do that, you'd need a temporal sequence I would think. Page 1930 Lines 17-19. What are normal summers? What is the basis of comparison? How do you know there was less input if no numbers are available to compare? Page 1930, Lines 27-28. -29 ‰ is proposed as lighter than normal, with no explanation of how normal is defined and, again inferred to indicate a river source. The problem with the scattered commentary on δ13C values in the manuscript is that no clear discussion is given anywhere on the end-member compositions for δ13C, their variability in time and space and how they have been selected. Nor is any discussion given of results provided by other studies on this parameter and other markers of terrigenous carbon that might help with the interpretation. Page 1931, Lines 16-18. We are now told that -26.4- -28.5 ‰ is typical for the Lena and for eroding ice. What happened to the -29 ‰ given earlier? The discussion around δ13C is confused and unconvincing. And, in all of the discussion so far the production of organic carbon by ice algae (which can be very heavy in δ13C) and marine algae has been completely ignored. Page 1932, Lines 1-5. Finally, marine carbon is brought up. The problem with the discussion is that there is river POC, coastal erosion, marine organic carbon (pelagic and ice algal) and algal carbon produced in the rivers, all of which have different and variable δ13C. The discussion of δ13C in this paper does not make contextual sense out of these varied sources. Of course with just δ13C measurements, it is not possible to distinguish 4 potential sources. These problems must also be considered in the discussion of δ13C

in the subsequent text on pages 1932-1933.

Typos and smaller items (not a complete list) P1918, Line 7. 'first recipient of the 16 overwhelming. . .' I don't get what the authors are saying. P1918, Line 11. The 250 stations. Were these sampled every year? Here and in the body of the paper, the number of samples used for various tasks (Table 1, Figures) is unclear. P1918, Line 16-20. Awkward sentence that might better be broken into two sentences. Page 1922 line 18 criteria should be criterion P1924, lines 4-5. Fluvial sources of POC are. . . P1924, lines 8-9. Is this statistically significant? This statement and a number of others are presented without any basis being given for the assertion. P1924 Lines 13-14. What is the normal discharge (and variation)? P1925, L12 has should be have.

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