

Interactive comment on “Sedimentary organic matter variations in the Chukchi Borderland over the last 155 kyr” by S. F. Rella and M. Uchida

Anonymous Referee #2

Received and published: 3 May 2011

General Comments:

This paper provides a much needed window into full glacial cycle paleoceanography in the western Arctic Ocean, an area that is still critically understudied considering its potential to influence, and be influenced by, anthropogenic climate change. In this context, such high resolution investigations of biogeochemical cycle changes accompanying large, rapid climate oscillations in the past are uniquely valuable as predictors of the future and thus warrant timely dissemination to the scientific community. Moreover, the paper is generally well-written and concise, incorporating broad ideas of oceanic and atmospheric linkages to paint a very interesting picture of Arctic rim dynamics over the past 155kyr. Some facets of their record and its corresponding interpretation need to be further refined, however, including the fidelity of their age model, the seemingly strict

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assignment of terrestrial organic carbon and calcite input to lateral transport from the Russian and Canadian margin sources respectively, and the uncertainty associated with the C/N proxy, all of which are addressed in more detail below. I'm also unsure that the subject material (a sediment record of climate-ocean dynamics) is best suited for this particular journal. I therefore recommend reconsideration pending revision.

Specific Comments:

As the other reviewer noted, my first concern lies with the age model. While I tend to agree on the assignments of major low frequency events like MIS 2 and 6 (even with a wary eye on the latter, as the tie point between LR04 and PC1 could be equivocally defended at the ca. 690 vs. 620 cmbsf CaCO₃ dip), robust D/O correlation simply cannot be supported by any constraint other than visual wiggle-matching. Indeed, the relatively constant ~5cm/kyr sedimentation rate calculated by the authors may actually be cause for concern, as I would otherwise expect large fluctuations at a site so heavily influenced by glacial dynamics. Nevertheless, I'm unsure what additional metrics could be used to support the age model. Radiocarbon is almost certainly not the answer here- the dating resolution at D/O timeframes is poor and of course useless beyond ca. 60ka, but the bigger problem would probably lie in the temporal offsets of both TOC and CaCO₃ if most of their sedimentary inventories are in fact allochthonous. Spectral analysis of the PC1 record relative to that for its NGRIP ice core d18O counterpart might offer additional insight as to whether the existing age scale is at least in the same ballpark. In the absence of further age constraints, however, this uncertainty should be more explicitly acknowledged in the manuscript.

On a related note, it's a rather big leap to link changes in the concentrations of TOC and CaCO₃ to ocean current and/or iceberg scouring in the absence of any other supporting information. For example, the flux of TOC could remain relatively constant (thus implying a constant eastward TPD etc.) whilst the flux of CaCO₃ changed as a function of strength of the Beaufort Gyre. Moreover, such antiphase behavior in TOC and CaCO₃ concentrations is a common feature of many marine sediment sequences as

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one tends to dilute the other as a function of seasonal production changes in overlying waters, i.e. without the need to invoke lateral input. Thorium-230 normalized sediment accumulation rates and focusing factors would go a long way toward underpinning or contesting the central hypothesis of the paper and are therefore highly recommended.

Given these uncertainties, I would also like to see a better discussion of why the Canadian and Russian margins should be considered as sources of mostly CaCO_3 and terrigenous OC respectively. While the authors do cite some supporting literature, not all of the existing biomarker and isotope work is fully referenced and incorporated into the story. In fact, this manuscript provides an ideal opportunity to compile such previous data into a contoured overlay of modern pan-Arctic sediment OC and calcite content in Figure 1, which would serve to better frame their interpretation.

And on the topic of proxies, the very low C/N ratios do indeed suggest a significant contribution from inorganic nitrogen which, as the other reviewer also indicates, can be estimated by a cross-plot of TN versus TOC. I'd like to make a further suggestion though: if the correlation coefficient is high (indicating a relatively constant background of mineral nitrogen), the contribution denoted by the y-intercept can effectively be subtracted out of the record, potentially affording an opportunity to use the resulting organic C/N ratios to better constrain marine vs. terrigenous input. If there is wide scatter however (reflecting a highly variable background), then the existing C/N record offers no reliable information relative to sediment source and should be removed from the manuscript entirely.

Measurements of clay abundance and type, on the other hand, could provide a much less ambiguous signature of terrigenous OC input and geographic provenance respectively. This can be relatively easily measured at very high resolution by elemental profiling using XRF core scanner. Indeed, the methods section mentions that a x-radiograph of the core was obtained- was this part of a XRF scanner system? If so, please include these powerful data; if not, I strongly recommend their acquisition.

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Technical Corrections:

Pg 2260 line 7: Change to ‘...a region that potentially responded sensitivity to...’

Pg 2262 lines 27-28 through Pg 2263 line 6: Unclear whether the authors are still describing glacial conditions or modern oceanographic dynamics. Please use correct tense.

Pg 2264 line 6: Change to ‘...atomic weight of calcite and carbon.’

Pg 2264 line 23: Do they mean 602 cmbsf? And why not extend the high CaCO₃ interval to ca. 430 cmbsf with two negative excursions? That would seem like an equally valid description, again reflecting the somewhat arbitrary nature of feature assignments on this age model.

Pg 2272 line 1: Change to ‘...particularly in light of a good correlation...’

Figure 1: Out of curiosity, where is the 120m isobath? Instead of invoking ocean current dynamics, could more direct fluvial input to the coring site during glacial sea level lowstand account for the PC1 record? I think this should be plotted in Fig. 1 and discussed in the text.

Figure 2: This is also shown in Figures 3 and 4. Thus, Fig. 2 is redundant and can be removed.

Interactive comment on Biogeosciences Discuss., 8, 2259, 2011.

BGD

8, C786–C789, 2011

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