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Climate of the Past
Discussions

Interactive comment on "Bering Sea surface water conditions during Marine Isotope Stages 12 to 10 at Navarin Canyon (IODP Site U1345)" *by* Beth E. Caissie et al.

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The paper presented by Caissie and her colleagues details the development of oceanographic conditions in the Bering Sea prior, during, and immediately following Marine Isotope Stage 11. The focus on MIS 11 is timely due to its environmental similarities to predicted near-future climate change, and the Bering Sea geography provides an environment that is of broad interest across many disciplines. This study is also one of only a handful that documents marine environmental change in the high-latitudes during MIS 11, which adds to this study's timeliness. The authors use a combination of diatom and calcareous nannofossil micropaleontology, bulk sediment geochemistry, and grain-size analyses to show the evolution of this interesting period though changes

in the marine ecosystem, sea ice conditions, and water column nutrient cycling.

There are several elements of this study that are great, including detailed environmental changes associated with the various phases of the MIS 12-10 transition, and an honest assessment of the age control. I also particularly liked seeing the application of modern species richness indices to the diatom data in this paper.

Some issues the authors need to address include:

1) Better integration of these new Exp. 323 results with the other recent papers that have resulted from the cruise [e.g., d15N studies of Schlung et al. (2013) and Knudson & Ravelo (2015); opal productivity studies of Kanematsu et al. (2013) and Kim et al. (2014)]. While these studies do not provide as detailed an analysis of MIS 11 as the current paper, they do provide a good background for assessing glacial/interglacial background changes in the Bering Sea that are relevant to the current study.

2) To better assess the relative contributions of terrigenous versus marine organic matter to the dataset, cross-plots of the organic matter d13C, sedimentary d15N, and molar N/C ratios (see Perdue and Koprivnjak (2007) for explanation of N/C instead of C/N for % terrestrial calculations) need to be presented. See Walinsky et al. (2009)'s Figure 9 for a good example. It might also be worth considering breaking the data into groups based on the time intervals introduced in the discussion.

3) During the time periods associated with low sea-level stands in this paper, the mouths of the Yukon and Kuskokwim Rivers (and other smaller rivers that currently drain into the Bering Sea) would have been greatly advanced across the exposed shallow continental shelf. Are these the "glacial meltwater rivers" that are suggested in Section 5.1? It is difficult to dismiss them as potential sources of terrigenous material, especially given the evidence that they contributed an enormous sediment load to the glacial Bering Sea [as evinced at the Meiji Drift, see VanLaningham et al., (2009)], as well as cut some of the largest submarine canyons in the world during these low stands (e.g., Scholl et al. 1970 and subsequent work). Additional explanation for why

Site U1345 appears to be devoid of this terrigenous material seems warranted.

4) As written, the entire Discussion section is tough to follow. There are quite a few time overlaps between the various subsections that are confusing, plus the added details from the contemporaneous North Atlantic and Antarctic regions add further complexities. I recommend re-organizing the Discussion into 2 major sections – (1) the MIS 12-10 transitions as seen at U1345 [subsections for each time interval (without time overlaps), which is similar to what has already been written], and relating the U1345 variability to other regional/global records.

5) Since the original premise of this study was intended to present the Bering Sea MIS 11 paleoceanographic variability as an analogue for future conditions, perhaps a small section at the end of the discussion should address this?

6) I'm skeptical about the nature of the deposit that is attributed to being evidence of the Bering Strait Current Reversal (Subsection 5.3.1). When I first saw the grain-size data, I thought turbidite, and the enrichment in P. sulcata [a common diatom marker of redeposition and/or downslope transport due to its highly silicified morphology; see Sancetta (1982)] seems to support that idea. However, the authors discount the turbidite mechanism on account of no visible sedimentary structures that are normally associated with turbidites. However, the authors make a good point about illite being an additional potential Arctic Ocean flow marker (Lines 767-771), as well as being a potential way to explain the anomalous N data. I highly recommend the authors do a little XRD analysis on the sediments in this interval (and immediately preceeding/succeeding) to determine presence/absence of illite in this interval. It is pretty easy, and the lead author's institute has an appropriate instrument (housed in ISU's Office of Biotechnology; www.marl.iastate.edu/xrd.html). This will serve as both an additional line of evidence to support the idea of an Arctic Ocean inflow, as well as help to explain the N data (since the low d15N values suggest an increase in the relative proportion of terrigenous organic matter, not necessarily inorganic N hosted in clays).

7) The idea that the Nome River Glaciation started during peak warmth in MIS 11 is a bit counter-intuitive; I think a better treatment of the extant Nome River Glaciation sites (and in particular, their respective age controls) is required to support this idea. Also, while the authors do introduce the "snow-gun" hypothesis near the end of Subsection 5.3.2, I think re-organization to increase clarity and introduce the snow-gun idea sooner will greatly improve the readability here.

There are a few minor issues as well: 1) Overall, the mean d15N = 6.4‰ for the full dataset, and from looking at Fig. 7, it looks like there might be values that exceed 8 or 9‰Ṫhese high values are suggestive of denitrification, yet this process isn't considered in the N cycle discussions spread throughout the paper.

2) Because many of the figures are very data-rich, in many cases axes have been truncated, which makes it difficult to assess extreme data points (which are often very important, such as the extremely low d15N values associated with the 406-402 ka event). I would recommend that, instead of cutting axes ranges, they should instead be offset so that the full axis range can be indicated. I'm specifically thinking of Figure 7, but this could apply to many other figures, too. There are also several instances where it is difficult to determine which line goes with which axis; perhaps color coding or additional labels are necessary.

3) I am also providing a PDF copy of the manuscript that I have made several grammatical corrections to; please review in detail.

In conclusion, I would like to recommend this article for acceptance, pending the minor revisions I've indicated here, as well as the editorial revisions on the attached manuscript. If any of my notes are not clear (or legible), I recommend the authors contact me directly with any questions they may have.

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Please also note the supplement to this comment:

http://www.clim-past-discuss.net/cp-2015-184/cp-2015-184-RC2-supplement.pdf

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