

Reviewer 2: J. Addison

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 through 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., $\delta^{15}\text{N}$ 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.

These records will be assessed and integrated into this manuscript. Thank you for pointing out their omission.

2) To better assess the relative contributions of terrigenous versus marine organic matter to the dataset, cross-plots of the organic matter $\delta^{13}\text{C}$, sedimentary $\delta^{15}\text{N}$, 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.

Thank you for this suggestion. This will help frame the discussion about contribution of terrigenous matter and organic matter as well as help us interpret sub-millennial scale variability.

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.

This is an excellent point, we did not intend to dismiss the sedimentation from these rivers. In light of the new clay mineralogy data (see below), this interpretation has been revisited. Pelto et al, in revision, shows a decrease in the input of sediment to a nearby site from the Yukon as sea level rose during the Deglaciation. Our work on this older time period (MIS 11) can reference Pelto (in revision) for additional context.

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.

This suggestion, combined with the suggestions of reviewer 1 should make the discussion shorter and much more readable.

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?

This will be added to the final paper in addition to a short summary of the question about the length of MIS 11.

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 preceding/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 $\delta^{15}\text{N}$ values suggest an increase in the relative proportion of terrigenous organic matter, not necessarily inorganic N hosted in clays).

Thank you for asking us to take a closer look at this interval. The nature of this anomalous deposit remains unknown, though a turbidite is certainly possible. However, the site's position was chosen on an interfluvial to avoid turbidites as much as possible. Moreover, the typical graded layers, from coarse sand and microconglomerates in the bottom to silt and clay in the top (Bouma sequence) are missing from this site. Instead we see very poorly sorted terrigenous fragments mixed together. It's unusual to get just one layer like this. We would expect a turbidite to work in the same place for a prolonged period. If the low $\delta^{15}\text{N}$ suggested an increase in terrigenous organic matter, we would expect to see a change in C/N and/or $\delta^{13}\text{C}$ as well. There is nothing remarkable about either marker during these low $\delta^{15}\text{N}$ excursions.

In addition, as suggested, we have analyzed the clay mineralogy in 10 samples across MIS 11, including several in the proposed glacial advance/throughflow reversal interval and found no evidence of illite in the core, so an Arctic Ocean influence is unlikely at U1345. This is in direct contrast to the results of Kim et al., 2015 who saw large amounts of illite nearby in U1343. It may be that the currents were such that the same events are not recorded everywhere, though this seems unlikely. Our revision examines these two interpretations and addresses possible spatial variability of the Bering Strait current.

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.

We agree that it is counter-intuitive to find that the Nome River Glaciation began during peak warmth, however, we believe that there is significant terrestrial evidence that supports this not only

occurred during MIS 11, but also during MIS 5e (Kaufman and Brigham-Grette, 1993; Kaufman et al., 2001; Pushkar et al., 1999). What these terrestrial studies lack is an accurate chronology. We think that our findings can add support to the onshore chronology and provide an important means of linking land and sea responses. We will rewrite this section to make this more clear. Additionally, we recognize that this hypothesis is not adequately tested yet but rather it provides an opportunity for future work.

There are a few minor issues as well:

1) Overall, the mean $\delta^{15}\text{N} = 6.4\%$ for the full dataset, and from looking at Fig. 7, it looks like there might be values that exceed 8 or 9%. These high values are suggestive of denitrification, yet this process isn't considered in the N cycle discussions spread throughout the paper.

Yes, thank you for pointing out this omission. You are likely correct that denitrification is happening throughout much of the record. This will be expanded upon and the new analyses you suggested above should clarify the isotope results.

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 $\delta^{15}\text{N}$ 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.

We tried hard to make the figures as readable as possible. Thank you for these very specific suggestions to help us improve.

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.

Please also note the supplement to this comment:

Noted, thank you for the detailed comments.