Reviewer 1 (T. Cronin):

Thank you for this very helpful review. Following these suggestions, we are working on a much shorter manuscript, which includes pulling out a section to develop a separate publication. The most significant revisions that you will notice include streamlining the hypotheses (1. Orbital scale variability; 2. Millenial-scale variability; 3. Direction of Bering Strait throughflow) and better focusing the paper.

Below we have included specific responses to the general issues that Dr. Cronin raised. Please note that all minor, line-by-line suggestions will be completed in the final paper.

This paper reconstructs paleoceanography for an important glacial-interglacial cycle, MIS 12-11 in an important region in the Bering Sea using IODP cores. MIS 11 is an especially important interglacial due to pre industrial-level CO 2 concentrations but higher-than-present sea level and in many regions, significantly warmer air and sea temperatures. The study uses sediment, geochemical and micropaleo proxies (diatoms & calcareous nannofossils) for marine productivity, sea ice and land ice reconstruction. So I think a high-latitude marine record of the MIS12-11 period like this one is sorely needed to go with Lake E, Lake B and others.

Some general issues I think the authors should deal with in a revision:

1) the introduction tends to be unfocused and too long. Please shorten it giving the main hypotheses to be tested. I also think the methods and results sections tend to be long. In section 4.2.2, in the diatom ecology section, can the main taxa or assemblages used for productivity and sea ice be emphasized? Much of this section is taken care of in the table. In fact, it really constitutes a review of high latitude diatom ecology going back to Sancetta, Koizuma and other pioneers; this is useful but it merits its own paper in a micropaleo journal.

Thank you for this fabulous suggestion. The diatom ecology section has been removed from this paper and a separate manuscript is nearly ready to be submitted based on this (and other relevant) work. Additionally, the introduction, methods and results have all been shortened quite a bit.

2) The methods section is long and could be put in a supplement, in fact some of it is, but the Supplementary Materials is not cited until Page 28. Likewise, Section 5 is too descriptive and does not focus on the key patterns that address the hypothesis about suborbital variability.

Much of the methods section has been pulled out of the main text and put into a new methods supplement. This supplement also contains additional information requested by the second reviewer.

3) related to # 1, I sense some of the text and references are not quite up to date [interglacial sea level papers, Bering Sea sea-level section 2.2 [where is Keigwin 2006 paper?), modern Bering Sea oceanography, see section 2.3)

Section 2.2 intentionally left out the Keigwin paper because it was intended to be more general than the most recent deglaciation and also focused more on terrigenous input rather than timing of sea level rise, however, the Keigwin paper is important, so we've added it. We will of course also make reference to the sea level compilation of Kaufman and Brigham-Grette, 1993 in the context of the compilation by Rohling et al. 2014 in Nature. In addition, the following papers, particularly Bering Sea interglacial papers that were not yet in press when we first submitted this paper, have also been added: δ^{15}N studies (Schlung et al., 2013; Knudson and Ravelo, 2015); opal productivity (Kanematsu et al., 2013; Kim et al., 2013); clay mineralogy (Kim, 2015), and the diatom study from Teraishi (2015).

4) The NADW discussion does not belong under a section called Bering Sea hydrology. Later in the paper, Section 5, there is again NADW discussion in the context of late MIS 11 Bering Sea reversed flow. In general, I don't think the Bering-N Atlantic links are well established mainly due

to chronology/correlation issues, which I believe are discussed in a 2009 paper by L&R on Atlantic Pacific diachroneity of O18 records.

Discussion of NADW was included in this section because of its hypothesized influence on the direction of flow through Bering Strait, however we agree that this discussion is misplaced and you are correct that it should not be possible to determine millennial-scale synchronicity between the Atlantic and Pacific. The association with NADW has been removed from the paper and the issue of the direction of throughflow is included later in the paper when this hypothesis is tested.

5) I wonder if the suggested correlations of this study's IODP core records with emerged Quaternary marine deposits [this is mentioned in several places] are warranted given age uncertainty of the onshore deposits?

The chronology of the onshore deposits is certainly less accurate that a marine core will ever be. But we would like to argue that Kaufman and Brigham-Grette, 1993 and Pushkar et al., 1999 provide the most likely interglacial age for the Nome River Glaciation. The stratigraphy there places important constraints on early ice build up in local mountain ranges before global sea level drops. Kaufman et al., 2001 have the same advance in the Bristol Bay region. Our findings can add support to the onshore chronology.

6) The 404 ka ice-rafting event discussed on page 28 seems speculative and not up to date on ice-rafting processes in the Arctic and subarctic. This section evolves into a mechanistic explanation, covering the "snow gun" hypothesis and alternatives [turbidites, sea ice etc]. I think this section should be rethought and rewritten. As with the issue of Bering Sea flow reversal in an earlier section, are these central to the question of patterns and causes of variability within MIS11?

This section has been significantly updated and simplified to include the comments of Reviewer 2, who asked us to more fully explore the question of a turbidite during this interval. While we agree that the hypothesized glacial advance is likely not adequately tested, there are advances during MIS 11 and 5e/5d transition (the latter not important here), which do provide an important means of linking land and sea responses. This is something the Arctic Ocean records cannot do. We suggest that we reframe the discussion about this aspect into a speculative section that could drive new work to explore the sources of the IRD.

7) The paper uses both cores - U1345 & 1343 - although Kim published on U1343 using different proxies but the same O18 for tuning, is there any way to integrate results from both cores better to provide a more robust pattern of MIS paleoceanography?

This is an excellent point. Kim's 2013 and 2015 low resolution opal and clay mineralogy papers will be incorporated.

Is the main focus of the study on orbital glacial-interglacial timescales or millennial timescales (that is, stadials and interstadials within MIS 11, see section 2.1 on sea level, or abrupt reversals like DO events ? The 15-meter thick MIS 11 record [line 199] ought to allow millennial-scale events to be seen. I have concern with the authors statement, in their discussion of the age model and tuning to LR04 and the other site U1343: "we urge caution when interpreting millennial scale changes at the site or comparing our record to others that examine MIS 11 at millennial scale resolution or finer". I got the impression in the introduction there would be more definitive conclusions reached on within-interglacial climate variability. Plots in Figures 5-8 don't really show me DOlike or Heinrich-like variability, which could be an important new conclusion, given our ideas on what causes such events in at least the N Atlantic region.

There are 3 main hypotheses that this paper seeks to test:

1. **Productivity and sea ice extent are primarily controlled by orbital-scale forcing MIS 11**
2. **Millennial-scale changes in sea ice occur throughout MIS 11**
3. **Throughflow through Bering Strait temporarily reversed after Termination V**

Additionally, we speculate that continuous marine records in the Bering Sea include records of glacial advance that can be used to explore land-sea linkages, but this section is significantly shortened and not treated as equal to the above three hypotheses.

We understand your concern about our caution about age model error, however we think it is important to recognize the limitations of the age model, especially in light of questions raised by Liesiecki and Raymo (2009) about synchronicity (or lack there of) of the isotope stack between the North Atlantic and Pacific. We think that the age model allows a reasonable estimate of sedimentation rates in the core, and the ages are likely fairly precise, however, the error in LR04 is 4 kyrs for sediments younger than 1 Ma. This means that it is not possible to say that an event that happened in U1345 at 412.4 corresponds with an event at 412.4 in a distal core. However, we can certainly resolve events at millennial scales within this core. Perhaps it makes the most sense to keep the caution about interpreting millennial-scale events BETWEEN cores, but remove the line about interpreting millennial scale changes within U1345.

In sum, I rated the paper as accept after minor revisions, some changes I am suggesting might take major text-shortening, but the science presented is sound, it is just not clearly packaged or presented.

Specific Comments

Line 27 comma before however
Line 28: This confuses me as the paper is in the Bering Sea, not the N Atlantic: led to "lowered productivity in both the northern Atlantic and the northern Pacific. "
Line 48 proper citation of IPCC 2013
Lines 55-56. Fix grammar in second part of sentence.
Line 58 – which coastal region were these glaciers?
Line 70 – do you mean little is known from North Pacific Ocean region incl. Bering Sea?
Line 74. Is this marginal zone sea ice?
Line 114 E Antarctic ice was stable. . .
Line 196 "and" no italics

Section 4.2.1. Authors begin to use "ka" in discussions of diatoms but absolute years were not discussed in the age model-tuning section. So please tell readers earlier in the paper, at Table 2 reference, which should be in age model section, about the ages of MIS12, MIS 11 per the tuning to LR04. See line 586- the section title should say how old oldest sediments are, not "beginning of record"

We agree with these changes.

Section 5.1 includes early deglaciation but section 5.2 is on Termination V, which is the deglaciation. Line 625, the debate about the duration of MIS 11 should be mentioned, embodied in papers by Masson-Delmotte, Ruddiman and others. See line 710 on this topic. I became confused about the MIS11 duration and the number of substages. Many authors do NOT use the substage terminology and the LR04 and the Antarctic ice core records show only two MIS 11 peak warm periods. One reason this is critical is this study of MIS 11 in the Bering Sea is one of the most detailed available. So it should shed light on the issue.

We will address this issue in the revisions and appreciate this important point.

Figure 1 in the caption, mention both U-cores plotted in the map.

Figure 3 is a little complicated but it is critical. Consider dividing into 2 figures. The U1345 curve, red line, certainly looks different from that for U1343 – why so? Cook and Kim age model papers might be summarized in the text, in fact it would be useful to reproduce the O18, tie points data for the entire period covered by their tuning study.

A table will be added.

Figures 5-8 are fine and do show what the study sought to accomplish: variability in diversity, lithology and microfossil assemblages. A more succinct treatment of these data in the text and better summary in the conclusions would help readers not familiar with these proxies. Why is MIS 11 split on the left in Fig 7 but not in others? Label horizontal colored panels in the figures for clarity. Figure 7 what is the source of N Atlantic stadials? Are they really relevant to this study?

We will correct the figures.