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Climate of the Past Discussions

Interactive comment on "Rapid waxing and waning of Beringian ice sheet reconcile glacial climate records from around North Pacific" *by* Zhongshi Zhang et al.

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The paper by Zhang et al (submitted) is an important attempt to reconcile evidence from numerous authors for the existence of some type of ice sheet over the East Siberian Sea during past glacial intervals, prior to MIS2. The concept of a Beringian Ice Sheet was initiated by Grosswald and Hughes (e.g., QSR 2002) for the Last Glacial Maximum (LGM, 20ka) but the physical evidence for that ice sheet has been missing for the LGM (see recent community map compilation by Bond, 2019). Zhang et al. however regurgitate arguments from decades past while trying to approach a modern problem.

I suggest they restructure the argument.

I think we all can agree that there is, yes, compelling evidence for some type of grounded glacial ice in the past off the Chukchi Cap and Arliss Plateau in the Arctic Ocean based on papers by for example, Niessen et al. (2013) and Joe et al. (2020, and references there in). Basilyan et al. (2010) show glacial ice shoving onto the New Siberian Islands before 70ka and more likely during MIS 6. The challenge, however, has been to determine what that ice sheet looked like and when was it present (Brigham-Grette, 2013). This remains a challenge because stratigraphic records both on and offshore, well-dated glacial moraine sequences in Chukotka and Alaska and regional paleoshorelines demonstrate a rich Pleistocene history without continental-scale glaciation. This paper takes a completely wrong approach to the problem.

There is no doubt that glacial ice seems to have been grounded on the outer shelf of the East Siberian Sea during earlier glacial episodes. While mammoths roamed Wrangel Island from 48 ka to nearly 3400 years ago (Vartanyan et al., 1993) there are erratics on the islands plateaus that prove it was glaciated earlier than that (Gualtieri et al. 2003). But the ice sheet that produced the glacial erosional and depositional features observed does not need to look like the ice sheets proposed in this paper. Rather, the reconstructions from this paper violate the physical evidence from many dozens of field-based reconstructions published over the past 3-4 decades. Their disregard for the field evidence is negligent in trying to get to a real issue. For example, Figure 7 has a caption suggesting that their simulation of a Beringian Ice Sheet for MIS 4 and MIS 6 "agrees nicely" with field based work cited, but this is simply not true. Supplemental Figure 5 demonstrates that their simulation does not even get the Laurentide or Eurasian ice sheets correct (note here that in S.Fig 5i they show the southern limit of the ice sheet in MIS 6 terminating in the center of Hudson's Bay, labeled "The North American east coast" in the caption).

The authors are asking the right questions but do not have the right approach. I suggest the authors first demonstrate how their simulation gets the Laurentide, Cordilleran, and

Eurasian ice sheets correct for MIS 2; produces glacial ice extent for MIS-2 Beringia as outlined in the map by Bond (2019). There is wide spread evidence for aridity in much of Beringia during the LGM. So they could address the question why MIS 6 and especially MIS 8, 10 and 12 were wetter (more extensive valley glacier systems). Next start with a small ice sheet in the East Siberian Sea. They could ask "how large can the model grow an East Siberian ice sheet without violating the geologic record. Felzer (2001) tried this experiment with a GCM that looks primitive by today's standards perhaps, but the approach respected the Beringian Pleistocene history known at the time. While cold-based glacial ice can miraculously preserve ancient landscapes, ice sheets still have to weigh something, and leave a record of glacial isostasy (see arguments in Brigham-Grette and Gaulteri, 2004)

There are three solid reasons why this paper should be rejected, but the most obvious is the mismatch with physical data:

1. This paper suggests central Chukotka, home to Lake El'gygytgyn, was repeatedly covered by glacial ice sheets nearly 2 km thick during the last several glacial cycles. This idea is ludicrous but central to their faulty glacial reconstructions. It is clear from Melles et al. (2012) and Brigham-Grette et al. (2013) that the central part of Chukotka was never covered by continental scale ice sheets over the past 3.6 million years. Absurdly, Zhang et al. suggest that Lake El'gygytgyn has been repeatedly covered by 1.5 to 2 km of glacial ice during the times when Melles et al. 2012 and notably dozens of subsequent papers support the interpretation that Lake El'gygytgyn was covered by a perennial lake ice cover during glacial times, much like perennially ice-covered lakes in the Antarctic Dry Valleys today with moats in summer. The "glacial" facies outlined by Melles et al. (2012) and Brigham-Grette et al. (2013) describes lacustrine (not glacial) sediments deposited under the lake-ice cover (not an ice sheet), consistent with the presence of cold and arid pollen assemblages and 10% biogenic silica produced by diatoms living in the water column under the lake ice. The authors of this present paper have completely ignored the meaning of glacial /interglacial/super-interglacial

change in the El'gygytgyn record. This undermines the premise of their paper from the start.

2. This paper ignores decades of research that documents the extent of glacial ice cover in Beringia during the LGM about 22 ka ago, and for earlier glacial intervals MIS 4,6, 8 10 etc. If you look at their figures Supplement #6 and 7, it is clear that most of their reconstruction violates field evidence (Brigham-Grette and Gualtieri, 2004, provides background). See also Dyke et al. (2002).

3. The argument of using ICE6G does not follow on their figures showing model outputs. And the argument that Devils Hole must be reconciled with the ice sheet extent seems like a huge stretch. Why should a record from Nevada say anything about a Beringian Ice Sheet? What is the physical teleconnection? The so-called missing ice sheet issue accounts for 10 meters of sea level but this is well within the error of models for glacial isostatic loading (GIA) and dynamic topography. This part of the paper seems weakly argued. The statement that "early warming is a better match to the paleoclimate data" is wrong given that the glacial onset and reconstructions are incorrect.

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