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Interactive comment on "Middle and Late Pleistocene climate and continentality inferred from ice wedges at Batagay megaslump in the Northern Hemisphere's most continental region, Yana Highlands, interior Yakutia" *by* Thomas Opel et al.

Anonymous Referee #2

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The manuscript presents stratigraphy, geochronology, and ice-wedge stable isotope data from the Batagay megaslump- a remarkable bluff of late-Middle- and and Late-Pleistocene sediments in interior Yakutia exposed by a spectacularly-large thaw slump. They add some new radiocarbon dates to the emerging chronological framework for this site, and provide some new stable O and H isotope data for a small number of ice-wedges ranging in age from MIS 6(?) to the Holocene. Focusing on the broad MIS 3 interval, the authors conclude that winter temperatures during MIS 3 were colder at

this site in interior Yakutia, relative to a compilation of purportedly MIS 3 ice wedges from mostly coastal sites.

In principle I think data and discussion from this kind of proxy archive are a good fit for the scope and audience for Climate of the Past. I also think this site is really a remarkable find, particularly because of the potential for preservation of pre-MIS 5 relict ground ice. The writing and figures are mostly clear. But several factors make me unable to recommend publication: 1 Relatively low number of analyses 2 Poor chronology that inhibits meaningful comparison between sites 3 Speculative nature of the paleoclimate discussion.

I elaborate on these points below, with more specific comments at the end.

Note: Though the title of the manuscript alludes to Middle and Late Pleistocene climate and continentality, my main points of concern are limited to the MIS 3 part of the story because the authors acknowledge that the data from younger and older parts of the sequence are equivocal (p14/line10).

1. The Batagay megaslump headwall exposure is over 1 km long, yet the conclusions re: MIS 3 rest on data collected from only two ice wedges at a single measured section. The discussion and conclusion around Holocene climate is similarly based on analyses of only a single ice wedge. I realize that field work on sites like this is difficult and potentially dangerous, but the chainsaw sampling is rapid and contemporary analytical techniques allow for hundreds of samples to be analyzed in relatively short order. Rather than limited and equivocal results from a reconnaissance visit to the field site, my sense is that this topic deserves "high-resolution systematic sampling and dating", as the authors point out in their conclusion.

2. Most critically, I question if the available data support a meaningful conclusion re: MIS 3. In other words, what does it actually mean to compare a single probably-MIS3 ice-wedge from one site to another single probably-MIS3 ice-wedge from another site (as is done in Fig 7 and Table S2), since this interval spans ~30,000 years and includes

some pretty high-amplitude multi-millennial-scale climate oscillations in high-resolution proxy records? The time interval is also notoriously difficult to date accurately with 14C methods, and many purportedly finite ∼35-45 14C ka BP dates in the literature ought to be viewed with a strong dose of skepticism (notably, for example, the purported MIS 3 chronology for Mamontova Gora - an important comparative site Fig 7/Table S2). Seven out of the 18 sites in the Fig 7/Table S2 ice-wedge compilation are unpublished, so readers can't assess the reliability of these chronologies for themselves. The authors mention the issue of dating and a long MIS 3 (p12/line26) but do not really address it in a way that justifies the approach. One example of the interpretive difficulties: Novaya Sibir, Belkovsky, and Kotel'ny are all above 74 degN in the New Siberian Islands, yet only Novaya Sibir has relatively depleted isotope composition. Is the between-site difference in isotope composition due to differing age or some site-specific factor? Either way, the lack of good chronological control inhibits meaningful comparison.

One last point of criticism on the comparison of different sites: why was the compilation/comparison limited to just one "MIS 3" ice-wedge from each site? In the context of this analysis, would it not be more useful to compare the average isotope composition of multiple ice-wedges from a particular stratigraphic interval (e.g. the 10 ice wedges with dD and d18O data attributed to the yedoma ice complex in Opel et al 2017)?

3. I acknowledge that quantitative paleoclimate reconstruction from this type of archive is highly uncertain, but the climate implications presented here are vague. Differences in isotope composition between areas are quantified, but then unsupported paleotemperature interpreations are made (e.g. "significantly lower [temperatures]" p14/line7; "extremely low winter temperatures" p11/line23 vs "very low winter temperatures" p11/line26). These distinctions need to be defined.

The authors have not really addressed the issue of paleogeography, nor potential differences in moisture source both through time and for different sites. For example (assuming for a moment that it's possible to meaningfully compare MIS 3 IWs at the Bata-

gay site to those compiled in Fig 7), there's an interesting spatial pattern whereby the Novaya Sibir Island site also has highly depleted IW isotope composition during MIS 3. What are the paleogeographic implications of Late Pleistocene sea level change, with respect to continentality? What are current and modelled MIS 3 moisture sources for that site and the Batagay site? Is there paleoceanographic proxy evidence (e.g. from planktic forams) for changes in surface water isotopic composition at likely moisture sources? All of these points would likely provide useful context for evaluating the data presented in the manuscript.

Other points: This manuscript, which includes many co-authors on earlier papers that document the chronostratigraphic framework for the site, introduces yet another unit-stratigraphic nomenclature for the Batagay megaslump headwall exposure. For example, at least by my interpretation, "upper Ice Complex" (this ms) = Unit III (Ashastina et al. 2017) = Unit 4 (Murton et al. 2017). Given the potential importance of this site, and since all the key players are co-authors on this manuscript, it would be very useful to the community if the authors could reconcile these different frameworks here in this manuscript.

This group is highly experienced in stable isotope studies of ground ice. Nevertheless, it would be useful to provide some additional methods data. Is the quoted lab precision for dD and d18O 1sigma or 2sigma? What are the summary statistics for the internal quality control secondary standard? And most importantly (p4/line24), how exactly did the authors decide which samples to exclude from further analyses? The description in the manuscript is very vague (please clarify, with a citation or two, what exchange processes are being invoked between IW and pore ice), and I would strongly prefer that the authors present ALL the ice-wedge stable isotope data first and then justify to readers why some data should be excluded from further consideration.

Specific comments:

Referencing: There are points in the introduction and discussion where it would be useful and appropriate to include some citations to relevant work in North America, where there is a long tradition of stable isotope work on ice wedges (e.g. Fraser and Burn; Michel) and the stratigraphic complexities of Middle/Late Pleistocene permafrost exposures (e.g. Péwé, Westgate; Fraser & Burn; Froese, Reyes).

Title: Given the substantial interpretive and chronological uncertainty re: the lower sand and upper unit ice wedge data, I suggest removing "Middle and Late Pleistocene" from the title and replacing it with something more specific

Section 1 in the slump floor: Why is the one sampled wedge from the lower sand collected away from the exposed headwall, as indicated in Fig 3? Can authors reject the possibility that the sampled section is actually younger material displaced into an apparently lower stratigraphic position by slumping? And I'm troubled by the rejection of the 14C age on hare droppings from the ice-wedge. If this was a pristine, freshly-exposed ice-wedge, how would younger material be incorporated into the wedge itself? Surely the outermost surface of the wedge ice is removed prior to sampling? And if material "entered into or later froze onto the surface" (p8/line24), doesn't this also imply possible reliability issues with the isotope data from that wedge?

p8/line18: Do you mean "....different stratigraphic contexts"? This would make more sense. Also, the next few sentences of this paragraph are pretty vague and not particularly useful. I think it's pretty obvious now that adequate dating of these sediments is going to be a major challenge. There's some mention of alternative approaches in the conclusion section, which really should be moved into the discussion and properly addressed.

wood layer and the thaw unconformity (p. 8/9 transition): I assume you mean "...situated above the lower sand unit and BELOW the upper Ice Complex....." on p8/line29, since you reasonably attribute the wood layer to the last interglacial?

14C dating (Table 2 and p5): I appreciate the details on pre-treatement and analysis. Please clarify if smaller blanks were measured for background correction of the many

samples with low mass of organic C (Table 2).

Fig. 5: The changing vertical scale is confusing. Since the ice-wedge morphology is important in this context, I recommend a stratigraphic diagram to scale, with additional panels showing the photographs that are currently relegated to the Supplemental file.

Fig 6. The blue inverted triangles for upper sand ice wedges are very hard to distinguish.

References: Ashastina et al. 2017 Climate of the Past 13: 795-818. Murton et al. 2017 Quat Res 87: 314-330. Opel et al. 2017 Climate of the Past 13: 587-611.

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