Review of: Ashastina et al., Batagay Megaslump Exposure, Climates of the Past: Discussions

General Comments

This paper describes the stratigraphic sequence exposed by the Batagay mega slump headwall in the Yana Highlands region of Siberia. Several sections representing different positions in the exposure were described and sampled for paleobotanical, cryostratigraphic, and geochemical analysis. The dating is based on nine radiocarbon ages, and three OSL dates. The section represents at least 142 ka of discontinuous deposition based on the lowest OSL age and several erosional unconformities. The authors discuss the geomorphic and paleoclimatic implications of this record, and how this section relates to other permafrost exposures in Siberia.

In general, I question whether this paper is appropriate for publication in *Climates of the Past: Discussions* because it's main focus seems to be the detailed interpretation of the depositional environment at the Batagay sequence. The climatic connections are not strongly made until well into the discussion, and need to be developed much further if this record is to be published. For example there is very little introduction to the paleoclimate issue being addressed in this study.

In addition, I found there to be a lack of description of the sedimentary facies in the section, which could greatly aid in the interpretation and back up some of the claims the authors make. I suggest that the authors outline the methods and references used for identifying different depositional environments. These should be based on modern-day analogues with pictures of the modern-day depositional processes and their facies compared with these corresponding facies in section. In general the final section of the Discussion makes several assertions about the origin of the sediment without such comparisons.

I also find the uncertainties behind the low-resolution dating, 14C age reversals, and poorly constrained erosional unconformities to prevent the firm connection of this record to global climate proxies and regional sequences that are dated at higher resolution and have continuous deposition.

One of the main themes of this paper is to address the ongoing controversy of what depositional process generated the yedoma ice complex. I think this is a very interesting and relevant issue, but I don't think this journal is the appropriate venue for this. If the editor feels that this is not the case, then I highly suggest that the interpretation of the depositional environments at Batagay be developed much further with more evidence from modern environments and a better description of the sedimentary facies in the exposure. I elaborate on many of these points in my detailed comments below.

Detailed Comments

Page 1 Line 14: Late Pleistocene should be capitalized.

Page 1 Line 18: '…sought climate record.' Should be reworded.

Page 1 Line 19: 'close by the pole of cold' I am not sure this name is well known.

Page 1 Line 25: '8° C colder than today' What is this quantified reconstruction of MIS 8 based on? Are they talking mean annual temperature? See below for more on this.

Page 1 Line 28: 'proves again' should be reworded.

Page 1 Line 30: 'In the Holocene cover….' I think the authors mean in the Holocene unit.

Page 2 Lines 6-10: This final statement of the abstract is one side of an ongoing controversy about the origin or processes that generate yedoma deposits. If the authors are going to interpret such deposits as being formed by nival and proluvial processes, then I think they should briefly describe the basis for this interpretation.

Page 2 Lines 22-25: This portion of the introduction describes the controversy of what geomorphic process is the cause of the yedoma ice complex. The authors are questioning whether aeolian deposition was primarily responsible because 'there existed a diversity of habitats, including aquatic'. I do not understand why the existence of aquatic habitats precludes the aeolian interpretation. Permafrost can perch the water table near the surface and this can create aquatic habitats in otherwise dry environments. I think the authors may want to describe better the basis for why they are questioning the aeolian hypothesis, and how this study can address this controversy. In addition, this controversy seems to be the main theme of the paper, and not in line with the Climates of the Past Discussions Journal

Page 3 Line 32: 'Globally greatest temperature gradient' should be reworded.

Page 4 Line 4: 'Accepted as the lowest temperature in the Northern Hemisphere' If there is a citation for this, then it should be called here.

Page 4 Line 11: 'Resembling' should be 'Similar'

Page 5 Line 12: 'Possible reservoir effects as a result of the accidental use of freshwater aquatics…' This does not make sense. How did the authors know the macrofossils were aquatic? Did they identify them as such or did they infer this based on the $\delta^{13}C$ values. In addition, the authors should indicate what ^{14}C calibration curve was used.

Page 5 Line 16 to Page 6 Line 7: I am not an expert in OSL dating, but the methods described here seem to follow standard techniques in the literature.

Page 6 Line 10-16: It seems unnecessary to describe, in detail, how the thaw slump is positioned and behaves to start off the results section. This section does not seem to have much bearing on the main points of the paper. If this section needs to be included in the paper, then I suggest it go in the Study Site section. If the point of this is to say that the depths of different sections of the slump cannot be compared because some of them are not vertical, then this could be reduced to a few sentences.

Page 6 Line 22: I assume these meter calls are being measured from the top of the slump. This should be specified here.

Page 9 Line 26: The authors think that the sedimentary transitions of the different units represent erosional unconformities. Do they see cut and fill or other features to back this up? I do not doubt this interpretation, but it would be useful to describe the reasoning behind this. In my opinion this interpretation can be in the Results.

Page 9 Line 31: I find the 300-year BP ^{14}C age on plant material that is 1.15 m below the surface to be suspect. Is there loess deposition in this region today? How could 1.15 m of sediment accumulate in 300 years without incredible rates of productivity, a mass movement above the section, or high rates of loess deposition? Bluff-top sequences of loess in section often have reworked loess that blew onto the ground surface as the cut-bank neared the site of the section. Is this a possibility? It would seem more likely that this date represents modern material from rooting or cryoturbation from the current vegetation mat.

Page 10 Lines 1-8:

> Unit II corresponds to the YIC. YIC deposits can form only under extremely cold winter conditions. They are thus indicative of cold stage climate in a continental setting. Our dating results confirm the assumption that the YIC was deposited from at least >51 ka BP to 12 ka BP, thus during the last cold stage and including the MIS 3 (Kargin) interstadial period. Huge syngenetic ice wedges and high segregation ice contents are the most typical features of Yedoma sequences. The structure of ice wedges intersecting sediment columns are evidence for the syngenetic freezing of the ice-wedge polygon deposits. The ice wedges were 4.5 to 6.5 m wide, which indicates the impact of an extremely cold climate during their formation and also indicates aridity (Kudryavtseva, 1978). The thermokarst mounds (baidzherakhs) appearing in staggered order 4.5-6.5 m apart on the upper southeastern part of the YIC support this hypothesis.

The dating results from this study do not necessarily indicate that the YIC was deposited continuously from 51-12 ka. This is because there are only a few 14C ages from this unit and they seem to be subject to reworking. Could an alternative view be that YIC accumulated only episodically or during a fraction of this time period because the plant remains were reworked or the deposits are too coarsely dated to infer continuous deposition.

Page 10 Lines 25-27: The authors say that the 14C age reversal could be due to a ground squirrel stashing food underground, which would bring younger C down below older C in section. The two dates are 26.2 and 33 ka. The younger date is 2.55 meters below the older one. Because ground squirrel food caches are limited by permafrost (they have not been observed to burrow into frozen ground), this would suggest that the active layer at this site was at least 2.55 meters deep. This does not seem plausible. The authors should discuss this further if they think it to be possible.

Page 11 Lines 1-5: The authors describe how the erosional unconformity probably corresponds to a thermal erosion event during the warm times of the Pleistocene Holocene transition. This may be true, but it should be acknowledged that the ^{14}C dates that bracket this erosion event seem to span around ~25.7 ka. The Bolling Allerod and early Holocene warm interval were millennial-scale events. I think it should be acknowledged that this correspondence is highly speculative given the age control.

Page 11 Line 22: The authors say that the climatic conditions were insufficient for ice wedge growth, but climate is only part of driver for ice wedge growth. The type of depositional environment and grain size of Unit IV could also prevent ice wedges from forming or being preserved. The authors should rule out whether non-climatic factors contributed to the lack of ice wedges in this unit.

Page 12 Line 1: The authors state that the presence of ice wedges in Unit V indicates that the mid-Pleistocene was characterized by extremely cold winters. This statement does not seem to be based on any dating, and relies on stratigraphic order. It should be acknowledged that just because the ice wedges are below the MIS 5 paleosol that this Unit V does not necessarily represent the Mid-Pleistocene. Similarly, the authors state that the ground ice in Unit V survived multiple interglacial warm times, but they only show that the ice survived MIS 5.

Page 12 Line 25-30: The authors state that the only mechanisms for the deposition of >50% sand in Unit II come from proluvial, nival, or periglacial processes, but give no citation. The authors do not think that aeolian processes could be responsible for depositing this unit. In many aeolian settings, silt and sand can be deposited together depending on sediment availability, wind energy, and surface roughness. It is not uncommon to have sand sheet interbedded with loess deposition. A more detailed report on the sedimentary facies in the section could constrain whether aeolian processes are at play here.

Page 13 Lines 13-15: A citation call would be useful to back up the interpretation that changes in magnetic susceptibility is a prerequisite for aeolian deposition here. I think there could be little change in MS under varying aeolian processes.

Page 13 Lines 17-20:

genesis, as was discussed for subunits IIa and IIc. Nevertheless, the high percentage of the silt fraction in the GSD of subunit IIb cannot be interpreted as an exclusive indicator of aeolian deposition, because high silt content in the sediment composition can also result from cryogenic disintegration of quartz due to repeated thawing and freezing cycles (Konishchev and Rogov, 1993; Schwamborn et al., 2012).

It is not clear to me why free-thaw action on quartz grains excludes the possibility of aeolian deposition here. Wouldn't freeze-thaw action be prevalent in this region regardless of the climate or depositional environment?

Page 13 Lines 29-30: The authors call MIS 5a the last glacial period. This seems too similar to the common name for MIS 2, which is often called the last glacial period. I suggest another name.

Page 14 Line 12: The mean annual ground temperature is only partly driven by climate. Surface processes, like the thermal conductivity of different soils and the thickness of the insulating snow layer, should be discussed as these features were likely different in the past.

Abstract in General:

In general, the abstract is too long. It should be cut in half to describe the main motivation, approach, and points of the study.

The order of the abstract is counterintuitive to the study. First, the authors introduce the site, and units with some specific temperature reconstructions. Then the authors describe detailed methods that they used including the sampling interval. These methods shouldn't be in the abstract, and certainly should not come after the main points of the paper are described.

Similarly, details about organic carbon magnetic susceptibility etc. do not need to be in the abstract if they are not contributing anything about the main points of the paper.

I ask that the authors reconsider describing the Siberian lowlands as a maritime climate. Potential evapotranspiration exceeds precipitation in much of the Arctic. Perhaps the authors mean that the region is less continental today than it was during glacial intervals when this yedoma deposit formed. The lowlands are also described as maritime in the Introduction.

Introduction in General:

The authors describe the climate of the Siberian lowlands as maritime and the study area in the Yana highlands as more continental. I suggest the authors include the mean climatic specifications to show how different the two regions are.

I also question whether these two sites *were* climatically different when they formed during past glacial periods. Because eustatic sea level was much lower, and permanant sea ice more extensive the whole region would have been more continental, and the lowlands would have been included in this. Therefore, the authors should describe how much different these areas were in the past.

The final few paragraphs in the introduction are better suited for the Study Site section as they describe the study site.

Study Site in General:

I suggest that the authors briefly describe the modern-day vegetation, and major geomorphic processes occurring in the region today aside from the slump.

Results in General:

In my opinion, it is not necessary to describe the angles of the bluff and sections at various depths. These are subject to change within a few days of being described and do not add much to the interpretation.

The results would read much better if this section were to be broken up into different units instead of different techniques. The authors could easily describe the lithology, chronology, organic geochemistry, paleobotany, etc of Unit I and then proceed to Unit II. This provides a narrative for what these units are composed of and when they were formed. I think this approach would also save significant space.

There is a distinct lack of information about sedimentary facies in this paper. The interpretation could be greatly aided by these results. Was there bedding or was each unit massive? What general attributes did these beds/laminations have? Was there fine rootlets embed in the sequence to suggest that the landscape was covered by vegetation? Was there any soil development? If so, what horizons / weathering is present?

The type of material that was 14C dated should be described in the text. 'Plant remains' should be specified to taxa.

It should also be specified how many aquatic plants were dated from this section, but not reported in this study. The methods give the impression that some 14C dates were excluded, but which ones, where were they sampled, and what were the ages?

Discussion in General:

The authors are assuming that the three sub-units in Unit II represent different marine isotope stages (MIS 4-2). Radiocarbon dating does not back up this assertion. It only seems to be based on the fact that there are three units and three isotope stages occurring at roughly this time. Much of the discussion on the possible links between the MI stages and subunits in Unit II could be removed.

The apparent erosional unconformities can be included in the Results if there are available sedimentary features that indicate where they are. As of now, most of the interpretation is based on large differences or reversals of 14C ages. This may not be warranted if the 14C dates are reworked, which the authors describe as a possibility.

The plant macrofossil identifications should be in the results.

I am skeptical that the current resolution of dating allows the statement that the Batagay sequence is in good agreement with global climate events over the last 125 ka. Mostly this section shows the landscape response to the last interglacial warm times, but there are a number of other climate events since the mid-Pleistocene that may or may not be represented here. It is difficult to say with the unconformities and current dating resolution.

In my opinion, it makes much more sense to describe the depositional setting of the section prior to the paleoclimate interpretation.

The authors ignore the possible interpretation that much of the sediment was reworked from the nearby floodplain by aeolian processes and deposited into the uplands. Because many of these rivers have nival flow regimes, large areas of exposed sediment would have been available for aeolian transport. This has long been described as the mechanism to get loess into the uplands in many periglacial zones. I think it should be considered here as well.

The authors assert that proluvial and nival processes were at least partly responsible for the deposition of fine-grained material in the section. A description of the sedimentary facies could help back up this claim, but this is lacking. ***I suggest that the authors outline the methods and references used for identifying different depositional environments. These should be based on modern-day analogues with pictures of the modern-day depositional processes and their facies compared with these corresponding facies in section. In general the final section of the Discussion makes several assertions about the origin of the sediment without such comparisons.***

There are a number of other sections from around Siberia mentioned in the Discussion. It would be helpful to include the general locations of these places in the Map figures.

Figures in General:

The dating results should be much clearer then they are.