

## ***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 #3**

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Manuscript

By: Thomas Opel and co-authors

Journal: Climate of the Past.

The aim of the paper submitted by T. Opel and co-authors to CP is to provide new reconstructions of winter temperatures from Pleistocene relict ice-wedges exposed in the

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huge Batagay mega-slump structure in Central Yakutia, completed by a close Holocene outcrop from the Adycha River. This contribution provides new data for the reconstruction and discussion of the palaeoclimate of the Yana highlands on a long time span (< 140 ka or older). The study is based on a short field investigation focusing on the sampling of ice from ice-wedges for isotopic analyse and organic remains from encasing sediments for <sup>14</sup>C dating (the oldest part of the sequence being already dated by OSL). The reconstruction of winter palaeotemperatures is achieved using co-isotopic composition ( $\delta D$ - $\delta^{18}O$ ) of ice from the ice-wedges preserved in the various Batagay sections and from intra-sedimentary ice.

It is a very interesting contribution based on a methodology that has already been developed and published by the first author in PPP (Opel et al., PPP, 2018). The manuscript is well organised and the topic is fully in agreement with the publication goals of Climate of the Past.

Observations and questions: 1) My main concern is that the author never consider that the climate of the Last glacial was subject to extremely rapid climatic changes between stadial and interstadial conditions that would probably have affected winter temperatures. This point should be discussed. In addition, in the discussion the various episodes of thawing of the ice complex and associated erosional surfaces are only allocated to interglacial conditions (P. 8/L.16) whereas it is likely that they could also occur during MIS 3 during the warming phases characterising the beginning of the stronger interstadials as GI 14 or 12 for example. . . This has been apparently observed by the authors (P.10/L15): “The upward transition of wedges from the upper ice complex to upper sand unit, however, was interrupted episodically by thaw, producing a number of thaw unconformities at different depths”.

2) Please explain the difference between Ice complexes and Sand units including intra-sedimentary ice / % of ice?

3) General concern: even if it is not the main topic of the paper, palaeoenvironmental

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and palaeoclimatic information derived from vegetal macro-remains should be exposed with more details (only short mentions as in P.9/L.1).

4) According to the various radiocarbon dating results, exposed in part 4.1, no ages corresponding to MIS 2 are found in the upper Ice and upper Sand Complexes presented as dating from MIS3-2 (P.8/L.13) ?. In addition, most of the <sup>14</sup>C ages are even older (37 - 38 000 BP or infinite MIS 4?). + P8/L.28-30: "radiocarbon dating of a wood layer (up to 1.5 thick) ABOVE the Lower sand unit and the upper Ice Complex reveal an infinite radiocarbon age . . ." According to this statement the upper Ice Complex should not be allocated to MIS3 and even more to MIS 2.

5) As pointed by R2 I also think that the change in isotopic composition of ocean water between glacial periods and the Holocene should be taken in account . . .

6) Finally: in the whole paper the water supply, necessary to the development of individual ice-veins forming ice wedges, is supposed to result only from precipitations (snow). What about the proportion of water originating from the melting of markedly older ice-rich permafrost sediments or ice wedges that could be trapped in permafrost cracks and mixed in various proportion with snow melt water during ice wedge forming process?

Figures I have presently great difficulties to connect the very large-scale Figure 3, based on a panoramic photograph, and the schematic but detailed cryo-stratigraphic illustrations of figures 4 & 5. In addition in Figure 3 the author should at least include a vertical scale! and the location of the various studied profiles is not very clear. To resolve this problem we need an additional medium scale Figure (a kind of stratigraphic log) based on a summary of the various formations, including their stratigraphic relations, respective thickness and the position of the various studied sections.

Conclusion This is a new and interesting contribution, well suited for Climate of the Past, and I think that it can be published after a moderate revision taking in account the various questions and observations exposed above.

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Please also note the supplement to this comment:

<https://www.clim-past-discuss.net/cp-2018-142/cp-2018-142-RC3-supplement.pdf>

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-142>, 2018.

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