

## ***Interactive comment on* “Spatial pattern of accumulation at Taylor Dome during the last glacial inception: stratigraphic constraints from Taylor Glacier” by James A. Menking et al.**

### **Anonymous Referee #1**

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The article “Spatial patterns of accumulation at Taylor Dome during the last glacial inception: stratigraphic constraints from Taylor Glacier” presents new data from Taylor Glacier obtained from blue ice covering the last glacial inception (74–65 ka). This period was primarily thought not to be recorded within the Taylor Glacier area. The comparison of data obtained on site and later in the lab permits to give an idea of the analytical uncertainty and robustness of the Taylor Glacier records of CH<sub>4</sub> concentration and particle count. Using these records, they construct chronologies for the Taylor Glacier ice core through alignment with the EDML CH<sub>4</sub> and NGRIP d18O<sub>atm</sub> records for the gas phase, and the EDC dust record for the ice phase, all on the AICC2012 chronology. They moreover revise the chronology of Taylor Dome ice core using the

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same method. Based on their new chronologies they calculate the  $\Delta$ age, the age difference between ice and gas at the same depth, for both Taylor Glacier and Taylor Dome ice cores. While the  $\Delta$ age remain nearly constant at Taylor Dome during the last glacial inception, at Taylor Glacier the  $\Delta$ age progressively increased during MIS 4. The authors interpret the increasing  $\Delta$ age gradient through MIS4 as variations in the snow accumulation rates between Taylor Dome and the supposed accumulation area of Taylor Glacier.

This paper present interesting new data obtained from blue ice of Taylor Glacier. I appreciate the efforts made to present the chronology construction, however the chosen figures do not permit to assess the robustness of the method. The choice of tie-point is subjective and when looking at your figures one could argue your choices, which weaken your article. Even if the authors tried to quantify some uncertainties, they did not finalise the uncertainty propagation for the final chronology, limiting the reader in the evaluation of the validity of their work. However, giving estimation of the minimum and maximum values of  $\Delta$ age variations for both Taylor glacier and Taylor Dome ice cores is a good idea. I am not convinced about the authors interpretation of evolving gradient in  $\Delta$ age between the two sites solely in terms of accumulation changes between Taylor Dome and the supposed accumulation area of Taylor Glacier. The authors need to give more proof for their preferred interpretation and need to justify why they completely disregard variations of thinning between the sites. This article is well within the scope of Climate of the Past and will be of value for the paleoclimate community. I suggest that this article should be accepted for publication after major revisions. You will find my general and specific/technical comments in the supplement.

Please also note the supplement to this comment:

<https://www.clim-past-discuss.net/cp-2018-53/cp-2018-53-RC1-supplement.pdf>

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-53>, 2018.