

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 #3

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General comments

The manuscript presents the initial multi-tracer dating of recent large size ice cores from Taylor Glacier (TG), covering a period of about 25 ka around the MIS 4/5 transition, as well as new data aiming at improving the gas chronology of the Taylor Dome (TD) ice core during the same period. Such characterization of a blue ice field providing large amounts of ancient ice is certainly of interest for the paleoclimate community and well within the scope of Climate of the Past.

The results are discussed in terms of age difference between the gas and ice phases (Δ age) and related varying accumulation rates. This interpretation involves some as-

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sumptions and simplifications that are not enough described in my view. For example, a number of age synchronization tie points appear ambiguous to me and the remaining discrepancies between records are not sufficiently commented. The inferred very low accumulations are likely to imply erosion periods, and the impacts of the ice-flow (thinning, hiatuses, possible folding etc.) should be better considered. Even if firm modelling with somewhat empirical models well outside the calibration range of their parameters is not compulsory, the physical processes controlling Δ age and $\delta^{15}\text{N}$ fractionation should be better described.

Overall I think that major revisions are needed in order to better discuss the approximations made (e.g. ignored firm and ice physics), describe the consequences of alternative assumptions on ambiguous chronological tie points for multi-species consistency, age scales and Δ age. I think that the paper should be more focused on an in depth discussion of the ice cores dating and dating issues, and less focused on somewhat spectacular but uncertain conclusions on Δ age and accumulation. A number of suggestions are provided below.

Specific comments

p2 l34-35 and p3 l26-28: Missing MIS 4 and MIS 4/5 transition in previous TG records. The authors should provide references and introduce more the possibility of having different hiatuses in different TG ice cores. The ice flow in the area should be better illustrated, for example Figure 1 (a) could be further zoomed on the drill sites and some flow line directions could be provided.

p2 l37: a reference should be provided for the previous TD chronology

p3 l14-16: a reference should be provided for these site characteristics

p3 l25-28: a reference should be provided for the ice flow structure of the “main transect”

p3 l30-31: the exact location of the “-380m” drill site (coordinates) should be provided.

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More site information could be provided (e.g. altitude, mean annual and summer temperatures etc.)

p5 l8-11 and p8 l1-4: the depth offsets, uncertainties and unification method between the different “TG 5/4” cores should be better described.

p5 l17: “The interpretations that follow do not depend on data taken from 0-4 m”, and similar statement p7 l22. In Figure 2, the 3 TG CH₄ data series are not consistent above 5m depth, and in Figure 3 the CO₂ consistency with the composite in the upper part of the TG record mostly rely on the 2 upper points. What would be the consequence of matching the TG CO₂ record below 4 or 5 m depth to the composite CO₂ record instead of using the CH₄ record which is nearly flat between ~4.5 and 7 m depth for multi-species consistency and Δ age? In Table 2, two CH₄ tie points and half of the ice phase tie points are located well above 4 m depth.

p5 l27-30: In Figure 2, the TG CH₄ records look a lot smoother than the EDML record. The dissimilarity of the two signals limits the possibilities of unambiguously synchronizing them. This could be due to different processes such as analytical smoothing (Stowasser et al., 2012), longer gas trapping duration in firn at very low accumulation rates (Spahni et al., 2003; Köhler et al., 2011; Fourteau et al., 2017), gas diffusion through ice (Bereiter et al., 2014 and references therein). This should be discussed, possibly smoothing the EDML record to try to simulate the TG record, comparing with the lower accumulation EDC record etc.

p5 l34-35: I did not understand why the $\delta^{18}\text{O}_{atm}$ record is tied to NGRIP only: a North Hemisphere discontinuous record covering only parts of the studied period. Could other data also be used? (e.g. Petit et al., 1999; Kawamura et al., 2007; Buiron et al., 2011)

p5 l38: some tie points look ambiguous to me and the tie points assignment should be further discussed. For example, the EDC and TG $\delta^{18}\text{O}_{ice}$ records look quite different in Figure 2, thus the $\delta^{18}\text{O}_{ice}$ tie point does not look robust to me. On the dust plot in

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Figure 2, I do not understand why the small EDC peak at 75.75 ka was tied to the TG particles peak at ~12m rather than the one at ~9m depth.

p6 l9-27: Due to the dissimilarities between the records in Figure 2, I believe that it is impossible to unambiguously assign the tie points. Thus I doubt that the choices were made without taking into account the constraints discussed in this section. An overall discussion of the constraints, what led to the current best guess dating and how other assumptions could be (or not) discarded would be most useful.

p6 l31-32 and Figure 3: I do not understand how the CH₄ record from the “-380m” core could be unambiguously tied to AICC2012. On the other hand the CO₂ records seem easier to match and matched. The overall dating constraints should be better described.

p6 l31 - p7 l14: I did not understand this discussion of the differences between the TG records. The dating of the “-380m” core is presented in one line and the CO₂ mismatch with “TG 5/4” not discussed, nor the $\delta^{18}\text{O}_{atm}$ mismatch with NGRIP at ~66 ka. The lack of information on flow line directions make the direct comparison between TG records difficult to understand, and few references are provided. I suggest to focus more this section on gas scales consistency between the “-380m” and “TG 5/4” cores, and how the CO₂ mismatch between the two TG cores in the 60-64ka age range could be explained. Is the ice phase of the “-380m” ice core also dated? Are large Δ age values also inferred?

p7 l8-9: As this paragraph comes just after the section comparing the “-380m” and aggregated “TG 5/4” cores, readers may wonder which one is the new ice core.

p7 l8-13 and p9 l25-31: providing and discussing plots of annual layer thicknesses (based on depth - ice age, depth - gas age relationships at TG and TD) would help understanding the interpretations related to accumulation and thinning variations.

p7 l24 - p8 l12: This discussion of uncertainties should appear earlier in the article and

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be more detailed (see also above comments on p5 l27-30, p5 l38, p6 l9-27).

p8 l1-4: this is not consistent with p5 l11. Due to the strongly varying depth-age gradients on Figure 3 (b), the overall largest age bias related to depth offset/uncertainty should be mentioned.

p8 l5-8: the smoothing due to gas trapping duration most likely dominates the diffusive smoothing in the open pores of the firn. It is accumulation rate dependent (e.g. Spahni et al., 2003; Köhler et al., 2011; Fourteau et al., 2017) and thus likely different at EDML and TG. In Figure 2, the TG CH₄ record looks much smoother than the EDML record. It would thus be interesting to discuss the gas trapping duration consistent with the firn sinking speed due to the estimated accumulation rates (time needed by the firn to sink by a few meters).

p8 l16-18 and l35-36: a much more in depth presentation of firn processes influencing Δ age, Δ depth and the physics of $\delta^{15}\text{N}$ should be provided. The consistency between a very large Δ age and a very shallow firn ($\delta^{15}\text{N}$ indication) should be commented.

p8 l24-30: the example of the successive datings of the Taylor Dome ice core, well discussed in Baggenstos et al. (2018) could be used as a base for a more realistic uncertainty discussion.

p8 l35 - p9 l4: the fact that the physics of $\delta^{15}\text{N}$ (thermal and convection effects) is much more complicated than a pure gravitational effect can't be ignored (e.g. Severinghaus et al., 2001; Severinghaus et al., 2010). The very low $\delta^{15}\text{N}$ values measured in TG ice suggest that either the firn is very thin (an estimate should be provided) or non-gravitational effects are important.

p9 l6-19: the new Taylor Dome age scales presentation repeats methodological information already provided for TG cores but does not discuss the remaining inconsistencies between records and ambiguous tie points. A more in depth discussion of the Taylor Dome age scales should be provided.

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p9 l21-31: this section is unclear to me. If TG and TD ice cores have strongly different Δ age in the study period (assuming that the tie points sufficiently constrain the age difference between the gases and ice in a single ice sample), TD can't be the origin site of TG ice even considering differential thinning.

p9 l2-4 and p9 l33 - p10 l26: no accumulation values were derived from the Taylor Glacier record and the discussion is focused on different time periods (present and LGM), thus it could be shortened.

Technical corrections

p8 l18-20: smaller Δ age values were obtained at very high accumulation rate sites such as DE08-2 (40 years, Etheridge et al., 1996)

p11 l24-25: twice "spanning the MIS 5/4 transition"

p13 l15: Baggenstos, 2015 (PhD) a web link could be provided.

p13 l23 and in article text: Update reference to Baggenstos et al. (2018), now available as a preprint.

p15 l49: suppress QUATERNARY

p16 l56: uppercase/lowercase issue

Figure 2, dust panel: some grey lines are not consistent with the tie points in Table 2 (chronology inversions in some grey lines)

Figure 3: the top part of the TG particles count record, including the tie point at 0.31 m depth, is not shown.

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