

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 #2

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1- SUMMARY AND GENERAL COMMENTS:

The study by J. Menking and collaborators presents three new ice cores from the Taylor Glacier Blue ice area that they combine to provide the first “composite” ice core record from this location that covers the transition between Marine isotopic Stage (MIS) 5 and MIS 4 (~74 to 65 ka). The chronology for the air trapped in the ice is defined based on the analysis of the global atmospheric tracers CH₄ and atmospheric $\delta^{18}\text{O}$ of O₂ ($\delta^{18}\text{O}_{\text{atm}}$) and their synchronisation with well-dated CH₄ and $\delta^{18}\text{O}_{\text{atm}}$ records from other Antarctic ice cores. The ice age scale is defined mostly based on the ice dust content synchronisation, again with other well-dated Antarctic dust profiles. From these

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two ice and gas age scales, they infer the evolution of the age difference between ice and gas at the same depth – the so-called Δage – through this MIS5-MIS4 climatic transition. Substantial Δage changes are observed through time over this time interval i.e. with values from ~2000-3000 years at ~74 ka and approaching ~10 000 years at ~60 ka. The authors also provide a new evaluation of the Δage evolution throughout the same period in the Taylor Dome ice core (located south of the glacier) which suggests no significant Δage changes for this site. The authors attribute these contrasting Δage evolutions between the two sites to a steep accumulation gradient across Taylor Dome that intensified across the transition from MIS 5 to MIS 4.

This paper presents a study that will be of great interest for the ice core community and to the extended paleoclimate community. It is thus well within the scope of Climate of the Past. Overall the manuscript is well-written and presents substantial new material and interesting interpretation of the results. However several aspects of the paper need improvements and clarifications and thus I believe that major revisions are needed before it can be considered for publication.

My first major comment is related to the fact that the authors interpret the differences in the Δage evolutions between the Taylor Glacier area and the Taylor Dome ice core site almost exclusively in terms of a change in the accumulation gradient between the two areas. While this could be an acceptable interpretation, they absolutely need to build a much stronger case regarding why this is their favoured one (e.g. versus ice thinning) and thus provide a much more elaborated discussion of their new results. But also, they should discuss the other possible controlling factors, in particular, those are commonly identified as impacting the firnification processes e.g. the role of surface temperature vs accumulation rate vs ice impurity content have already been discussed over the past few years (e.g. Bréant et al. 2017, Capron et al. 2013; Hörhold et al. 2012). I believe that a summary of the current knowledge (and knowledge gaps) regarding the climate and environmental factors that impact changes in Δage would be useful. In particular, it would be of added value to further mention firn densification models that provide an

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alternative method to estimate Δ age. At the moment the authors only acknowledge the Herron and Langway model (1980) although several other models building on this original work have been developed in the more recent years (e.g. Goujon et al. 2003 and more recent development in Bréant et al. 2017, dynamical version of Herron and Langway (1980) used in e.g. Buizert et al. 2015).

My second major comment is related to the form of the paper. First I believe that some reorganisations of some sections are necessary and I detail this in the next section. Second, I think that the Figures 2, 3 and 4 need to be revised so that the readers are able to better visualised the different records that are being presented but also so they better support the results and the proposed interpretation. More details are provided in the next section of the review.

Additional comments are also provided in the following and I would strongly advice the authors to consider them when preparing a revised version of their manuscript.

2- SPECIFIC COMMENTS:

- Section 2 (Field site and analytical methods) is not always easy to follow, in particular regarding which type of measurements has been performed on which core and where (on site or in labs back in the USA). I would suggest the authors to propose a summary table in the revised manuscript that detail clearly this information.

- The authors propose to treat the three ice cores covering the MIS5-4 transition as a single ice core record (unified depth and age scales). While I agree with them that it is justified, I believe that they should provide additional details on how they line up the different records together (and possibly provide a specific figure?) and discuss the attached uncertainties that arise from proceeding as such on the resulting “composite” record.

- Section 3.1 is hard to follow, the authors should consider restructuring it such as 1) they present how the ice age scale has been defined and then 2) as the gas age scale

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has been defined. Regarding the definition of the tie points based on the alignment of the dust record, I find that some of them are quite ambiguous considering the number of spikes present in the TG records. For instance why would they assign the tie point at 73.6 ka to the spike at 12 m rather than the spike at 9 m? I believe that the authors have a good reason for doing so, however, it should be spelt out more explicitly. It is necessary that the figure is much enlarged to allow a detailed inspection of the records.

- I do not think that the analytical uncertainties should be discussed after the determination of the age model. The authors should consider adding a brief description of each dataset after the analytical method descriptions and there, add details regarding their specificity and limitations.

- It is a little strange that the presentation of the new measurements on the Taylor Dome ice core and the definition of the new age scale and for Taylor Dome are currently presented as part of the discussion. Why not instead presenting the new age model of Taylor Dome as an additional sub-section in the age model section that is currently only dedicated to the dating of the Taylor Glacier ice? And similarly for the new measurements, they should be also included in the analytical description section and information should be also added in the table I propose to add in the revised manuscript. Also, I think it would be very useful that more background information is provided regarding the Taylor Dome site, in particular regarding the previous age scales available for this time interval.

3- FIGURES

- I appreciate the effort of the authors to show how they defined the different tie points to link between the Taylor Glacier records on a depth scale the dated reference records. However, it should be bigger to allow a closer inspection of the different records and where the tie points have been chosen.

- Figures 3 and 4 should appear much bigger. Also, to facilitate the comparison of Δ age evolutions between Taylor Glacier and Taylor Dome, I suggest to remove the panels b

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from each figure and combine these panels b into a single and additional figure. They can be presented in parallel, making sure that the scale used for the Δ age evolution is the same for both sites.

4- STYLISTIC, TYPOGRAPHICAL COMMENTS AND MINOR COMMENTS

P2, L16: You should also mention the work that has been done in the Patriot Hills blue ice area e.g. Fogwill et al. (Scientific Reports 2017).

P2, L34: I find the expression “MIS 4 paleoarchive” to be an awkward formulation; I would suggest to reformulate the sentence e.g. “(2) the description of a new climatic record from Taylor Glacier across MIS 4”.

P4, L1: “second exploratory core”: this is a bit confusion to say “secondary” since the PICO core was also referred to as a “secondary exploratory core”. It should be rephrased e.g. “During the same 2014-2015, another exploratory core was obtained directly ...”.

P4, L5: Again the numbering of the core is confusing (as in total, as far as I understand, four cores were drilled with only the last three having MIS5/4 transition ice). Hence it would be could to reformulate such as e.g. “In the 2015-2016, an additional core was drilled. . .”.

P5, L26: The authors should be more specific in the title of the section e.g. “Determination of the ice age and gas age scales”.

P6, L4: “minimal” please be more quantitative here and give a quantitative range at least.

P8, L11: Although you refer to the tables, the authors should also provide at least a quantitative range regarding the relative age uncertainties.

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son between simulations and ice core. *Climate of the Past*, 9, 983–999.

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Fogwill et al. 2017. Antarctic ice sheet discharge driven by atmosphere-ocean feedbacks at the Last Glacial Termination. *Scientific Reports*, DOI: 10.1038/srep39979.

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