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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 #4

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Review of the manuscript "SPATIAL PATTERN OF ACCUMULATION AT TAYLOR DOME DURING THE LAST GLACIAL INCEPTION: STRATIGRAPHIC CONSTRAINTS FROM TAYLOR GLACIER" by J. Menking et al.

General comments: The authors collected and analyzed a set of new ice cores from the Taylor Glacier blue ice area covering the MIS 5 to 4 transition and whole MIS4, and present a suite of data (d18Oice, dust, Ca ion, d18Oatm, CH4, CO2, d15N2). Through age synchronization of gas and ice with other dated ice cores, they find extremely large delta age (ice age - gas age difference), which suggests much reduced accumulation rate at the snow accumulation area for the analyzed core and, by comparing the results

with the Taylor Dome ice core data with their revised chronology, give climatic implications of the accumulation contrasts between the Taylor Glacier accumulation area and Taylor Dome.

Regional reconstructions of glaciological and climatological conditions in the glacial period in Antarctica are important for better understanding of the climate system in the Antarctic and its relation with wider areas, and thus the topic of this manuscript is well suited for the Climate of the Past, and the data presented in general seems to be of high quality. First I would like to respect and congratulate the authors for finding the ice from entire MIS4 after the years of fieldworks and high-quality investigations.

I review it mainly in terms of whether the ages, delta age and resulting accumulation rate reduction are reasonably estimated, because they are the basis for the climatic interpretation and conclusions, and also because they have the highest scientific value in this study in my opinion. In doing so I find that the manuscript needs a major revision to make much stronger cases for the extremely increased delta age and reduced accumulation rate (including its timing) at the Taylor Glacier accumulation area, which in turn are based on age synchronization and interpretation of the resulting delta age. In particular, I find it difficult to evaluate the robustness of their choice of the age tie points for some cases from the given text and figures/tables. There are also several tie points which I did not understand how they could match with the existing ice core records. The authors made poor use of the data (especially d15N) for the discussion of the accumulation rate. While I agree with the authors that the delta age increased and accumulation rate probably decreased in MIS 4, it should be based on much more rigorous considerations. Also, some parts of the manuscript needs to be reorganized to better present the field works, ice core samples, measurements and methods, results and discussion. I strongly encourage the authors to improve the article by deeper analyses, interpretation and better presentation of their excellent data.

Specific comments:

Abstract: Add description that there are different ice cores, and that how the delta age was estimated ("Dating the ice and air bubbles" is too short even for the abstract). Similarly, "A revised chronology for the Taylor Dome ice core" needs some more explanation. Also, give numbers and error ranges for delta age and accumulation rate ("very low accumulation" is too vague; later in the text it is stated as virtually zero accumulation rate).

Introduction: P2, L30: "glacial inception" is used differently (it is often used for MIS5e to 5d transition), so perhaps replace it with "major sea level fall" or "major ice sheet growth in the Northern Hemisphere".

P3, L1-2: "the differences in the ice age-gas age difference". Delete one of the "difference".

Field site and analytical methods: The title of the chapter should reflect the fact that it also describes ice cores drilled in different seasons.

P3, L33 and P4, L1: The phrase "a second exploratory core" appears twice for different cores (PICO and BID cores).

P3, L35-37: Please clarify if this measurement was done in the field (brown markers in Fig 2), and when and how did you conduct the whole CH4 measurements. It is unclear to me because you mention D/O 19 but not the larger increase at D/O 17 (D/O 17 is only mentioned earlier for the "-380 m" core). Did you obtain all data before you drilled the BID core? Didn't you take the D/O 17 CH4 transition in the 2014-15 PICO core into account for the preliminary age estimate in the field?

P4, L3-5: Please clarify which data you mean (Fig. 2, green markers). Also, ice sampling for d18Oatm and d15N is not mentioned here (2014-15 BID core) but there are data points in Fig. 2 that says the measurements were done in two years (2016 and 2017). Please give full explanation about the cores, sampling, measurements and periods for all data you present in a better way (not only about this core; using a table

may be a good way).

P5, L6-8: Didn't you take any samples to have overlaps with the previous cores? Did you make the sample cuttings for OSU and SIO in the field?

P5, L9-10: The description of the sampling of the 2015-16 core in the laboratory is better placed after describing the core transportation. Overall, the descriptions of field and lab samplings and analyses are scattered so they should be better organized.

P5, second paragraph: This part is about field measurement methods so it should come earlier before the first presentation of the relevant data. And, for which core this paragraph's description applies (2015-16 core only)?

P5, third paragraph: Please clarify which kinds of measurements were made for the different cores (maybe use a table). The measurement methods should come earlier than the first description of the data, or tell the readers that the methods are described later if you introduce the data first (like in the current manuscript).

P5, L16: The CH4 field data from 4 - 5 m in the 2014-15 core disagree with the CFA data of 2015-16 core by several tens of ppb, which is much more than your precision and should be discussed as well.

Results and discussion: P5, L36-37: The oldest tie point between the TG and EDC using d18Oice seems unacceptable given the different shapes of the isotopic curves of TG, EDC and EDML cores for this and other periods presented in Fig. 2.

P5, L38 - P6, L1: Some of the dust tie points seem unacceptable or maybe you didn't explain the details of the manual matching. You put one at 12 m but why did you choose that particular one and not other peaks? Another one at about 6.5 m is described as low point in dust, but the 2015-16 field data (purple in Fig. 2) actually show a peak there (I guessed that orange plot in Fig. 3 is the same as purple in Fig. 2, showing high values at the tie point). The grey lines for the dust (Fig. 2) are drawn between the EDC and TG purple data (orange in Fig.3), but is this particular one connects EDC and TG DRI data instead? Why did you choose the point where the two dust records from the same core disagree? Around the one at 70.11 ka, the TG dust peak is offset compared to EDC dust peak (isn't it better not to match the highest point in the peak which have certain width?). Similar examples are at ~65.6 and 63.9 ka. Overall, the lack of details on the matching force me to suspect that you chose the dust tie points while actually checking the resulting chronology by comparing Ca ion data from the two cores (I see that Ca ion data between TG and smoothed EDC compare much better than between the dust records), meaning that Ca is not just used for the validation of blind test (looking only dust) but effectively involved in the tuning. Otherwise, how could you choose the dust tie point at 12 m?

In fig. 3, dust data look like bar graph (vertical grey and orange bars) but they should actually be line plots. It is hard to evaluate the match in this figure so please improve the plots.

From the text (linear interpolation), I think the TG depth-age plot (Fig. 3b) should be straight lines between the tie points, but they don't look like so. A clear example is at an inflection point in the ice chronology at about 72.3 ka, for which there is no ice tie point. There might be my misunderstanding and if so please give full explanation for the interpolation. Please also plot markers at the tie points on the depth-age curves (Fig. 3b). The depth-age and delta-age lines in the figure are too low in resolution (the lines consist of tiny segments of horizontal and vertical lines, like aliasing in low resolution digital images).

You should reject two youngest CH4 tie points. Cracking and contamination should increase the measured CH4 concentration, so those two tie points are probably put on contamination peaks (note large disagreements between purple line, red line and brown markers). Perhaps you can use the peak at 3 m in brown data (if you take only low values in the two of the CFA data you see the same peak, which is uncertain but this could be a true atmospheric peak concentration).

The match of d18Oatm records look somewhat uncertain especially for the older one. Why did you connect the oldest d18Oatm data point to the beginning of the d18Oatm enrichment in NGRIP data (why not the second oldest data point in TG d18Oatm which is the highest)? I think the measurement precision is high for the TG dataset, but then I wonder what is the gap at 17m between the 2014-15 and 2015-16 cores. It might suggest depth offset between the two cores. Please discuss.

For matching d18Oatm records, why did you only use the NGRIP data as the reference? There are clear discrepancies in the values (probably regardless of the matching quality) for some periods (~73 and 64-49 ka). I think Siple Dome d18Oatm data (Severinghaus et al., 2009) is of higher precision (not only measurement precision but also smaller and smoother thermal fractionation) and resolution, and Siple Dome and TG were measured in the same lab. Siple Dome also has the data younger than ~63 ka where NGRIP data is lacking. So there seem good reasons that you should try using it as well (of course you have to match SD to AICC2012 using CH4). There may be a hope to match around 60-65 using small fluctuations in d18Oatm.

P6, L1-3: You should take into account the potential age error due to linear interpolation between tie points. The comparison between linear and cubic spline interpolation is insufficient as the demonstration of the age uncertainty between tie points. You should consider using available gas records as much as possible (CO2, d18Oatm; see comments above and below).

P6, L9-16: Agreement of TG CO2 with existing records is overall very good. However, I think the decision not to use CO2 for the synchronization between 60 and 65 ka is not satisfactory especially because there is no other tie points. You should at least try matching CO2 and look how the resulting chronology look reasonable or not.

P6, L20: See the comment above about the dust and Ca.

P7, L19-22: See comment above about CH4 for 0-4 m.

P7, L31-34: Explanation is insufficient. What do you mean by "surveying the value-matched or correlated data"? How exactly did you consider resolution and analytical errors.

P8, L1-4: The offset of 20 cm is quite large when comparing laboratory measurements by CFA and discrete samples. Please explain the possible causes for this. Why don't you correct the CFA depth assignment by matching the depths of the sharp CH4 features? Why is the 300 yr estimate a conservative one? You have other CH4 tie points where you have much steeper depth-age slope, so it does not sound conservative at all.

P8, L9-10: Absolute age uncertainty attached to AICC2012 for this age range is probably incorrectly cited. Please check. Also, it is useful to refer to Chinese speleothem ages (using CH4 and d18Oatm) for the possible range of absolute age error for the studied period.

P8, L14: It is common to use small 'a' for the term Delta-age (not Delta-Age).

P8, L16-18: A better explanation would be that delta-age depends on firn thickness (ice or water equivalent) and accumulation rate, and the firn thickness depends primarily on temperature and accumulation rate.

P8, L24: I think a weakness of the delta age estimation and whole discussion based on it is that there is no ice and gas age estimates for the same depth, so the uncertainty of delta-age depends on the uncertainties of ice and gas ages between tie points, which is not evaluated well. You should try to have more constraints on the gas age (with CO2 or d18Oatm) between 60 and 69 ka where you have the very large delta-age (which is the basis for your argument of "virtually zero accumulation rate").

P8, last paragraph: Discussion here is too qualitative (with the words like "near zero", "where ice accumulates very slowly"). Please be more quantitative by giving possible range for the surface mass balance in the TG accumulation zone in MIS 4 from your

data.

P8, L35-36: d15N is not only controlled by gravitational fractionation. You should introduce it appropriately.

P8, L37 - P9, L1: I agree that d15N likely reflect firn thinning during MIS4, but the change is not linear with respect to delta-age. About half of the d15N change actually occur at around 71 ka when delta-age is still stable at ∼4 kyr, and it is in fact before MIS4 (so this should also be discussed in your climatic discussion part). You should definitely discuss this large change while delta-age is small and stable, and in doing so, also run firn models for the high and low d15N around 70-73 ka. You might get some idea on how much non-gravitational signal could be contained in d15N data, or how much accumulation reduction is needed to explain d15N at that change (assuming that the change is purely gravitational). Another exercise is to use the presumed ratio (firn thickness in ice-equivalent) / (real firn thickness), which seems stable over wide range of temperature and accumulation rate (∼0.7 from Parrenin et al., 2012, CP) and use the d15N and delta age to estimate accumulation rate, for example at 72, 70 and 61 ka and some times in between. I think you obtain a mm or so for the 10000-yr delta-age with d15N around 61 ka.

P9, L18-20: You should actually put the constraint ice age > gas age.

P9, L22: "onset of the last glacial period" is confusing as it is often mean the MIS5e-5d transition.

P9, L30-31: Suggesting the accumulation control on the depth-age curve instead of thinning based on the delta-age and d15N while avoiding deeply discussing delta-age uncertainty and d15N is not acceptable (see above).

Conclusions P11, L26: The statement "virtually zero net accumulation" needs more solid basis and quantification as commented above.

The rest of the discussion (about atmospheric circulation and ice sheet) may change after the revision with deeper look into your data, so I would not review it (and it is not my speciality in any case).

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