## Response to Referee #4

**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 need 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.

We thank referee 4 for helpful comments. In general we will add stronger justification for the tie points that we chose, including adding text that explains our reasoning and improving the figures to facilitate the reader being able to see clearly why we chose the tie points the way we did. We will strengthen our discussion of the low accumulation rate interpretation and include a conservative estimate (~ 0.5 mm/yr – 2 cm/yr ice equivalent). We still think it is not possible to make robust, quantitative estimates of accumulation rate from firn models given that the models are not built or calibrated to describe firn columns where delta age is this high. Nevertheless for the purpose of strengthening our claims, we will (1) discuss in more detail the controls on delta age, (2) discuss the controls on d15N including more detailed discussion of why d15N is low at Taylor Glacier, (3) provide stronger justification for why firn models are not appropriate to estimate accumulation rates at these extremes, and (4) discuss the Megadunes, Antarctica site as a point of comparison to the Taylor Glacier accumulation zone. We will also reorganize the text following comments from referee 4 as well as referees 1-3. Please see specific answers to the referee's comments below for more details.

**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).

We will change "A new ice core" on p1 l24 to "New ice cores…" We will change "Dating the ice and air bubbles in the new ice core" on p1 l26 to read "We determine chronologies for the ice and air bubbles in the new ice cores by visually matching variations in gas and ice phase tracers to preexisting ice core records. The chronologies reveal…"

We stated in the text that we refrained from estimating accumulation rate quantitatively because we recognized that our large delta age value would require accumulation rates that are well below the empirical calibration range of the Herron-Langway firn densification model (the lowest accumulation site in the Herron & Langway paper is Vostok at 2.2 cm/ yr) (Herron and Langway, 1980). To our knowledge there is not a more appropriate model that accurately predicts firn densification under conditions of extremely low accumulation. We maintain the view that extrapolating beyond the empirical range of firn densification models may lead to errors that cast any determined accumulation rate into considerable doubt. Nevertheless we proceed with caution to determine a conservative maximum accumulation rate for the Taylor Glacier accumulation zone given delta age = 10 kyr as determined from our new ice core records. We used Herron and Langway to solve a matrix of density profiles for different temperatures (-20 to -70C) and accumulation rates (0.001 to 0.3 m/yr water equivalent). Assuming the density of snow is 0.36 g/mL and the close-off density is 0.83 g/mL we computed the age of the firn (the delta age) at the close-off depth using Herron and Langway's equation 11 (Herron and Langway, 1980). We then computed the d15N due to gravitational enrichment for a matrix of diffusive zone heights using the barometric equation (Craig et al., 1988). A contour plot of d15N and delta age on temperature and accumulation axes allowed us to examine the range of temperatures and accumulation rates expected given our independently determined delta age and d15N. For a site like Taylor Dome (likely between -40C to -50C at MIS 4), accumulation rates < 5 mm/yr water equivalent are needed to get delta age near 10 kyr and d15N = 0.08 ‰, like we see at Taylor Glacier. We understand there are uncertainties in this estimate: (1) we did not correct for the thermal fractionation of d15N, though we expect it to be small on the order of < 0.01‰, (2) we do not know the height of the convective zone, which could be quite deep given the very low values of d15N at Taylor Glacier. Even if we assume a deep convective zone of 25 m, similar to Megadunes site in Antarctica where the firn receives ~ 0 cm/yr accumulation and is highly cracked (Severinghaus et al., 2010), accumulation rates at the probable Taylor Glacier deposition site must be between 1.0-1.5 cm/yr to get d15N below 0.1‰. We reassert that the uncertainties in these estimates are quite high, but we will provide the quantitative estimate of accumulation rate as well text that cautions readers to not interpret the numbers strictly. Our best estimate is that the accumulation rate must be between ~ 0.5 mm-2 cm/yr, less than but possibly similar to the accumulation rate at Vostok, the lowest accumulation site used to calibrate the firn model.

**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".

We will change "glacial inception" to "ice sheet expansion."

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.

We will replace "ice age-gas age differences" with "delta age."

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

We will remove the redundancy by changing the second "a second exploratory core" to "a third core."

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?

We will clarify that the measurements described on P3, L35-37 were done in the field. We mention the DO 19 increase here because it is a marker in the gas phase for the MIS 5/4 transition. The larger rise at DO 16/17 is the marker for the MIS 4/3 transition. We did take into consideration that we found the MIS 4/3 transition in the early exploratory PICO core at -380 m on the Main Transect, which is why we moved 1 km down glacier to hopefully find slightly older ice that contained the MIS 5/4 transition.

We will clarify in the text how we used the early CH4 data to inform our site selection for the second PICO core and the BID cores.

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).

We will clarify which data we mean with a reference to the figure that says "(Figure 2, green markers)." We will give a more complete explanation of the sampling and measurements by reporting a table that lists each core, whether the core was drilled with the BID or the PICO, which measurements were made including where and when, and the analytical uncertainty of the measurement.

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?

We think here the referee means P4, not P5. No, unfortunately we did not take overlapping samples. The BID cores were cut into quarter cores for SIO and OSU in the field, but the samples were cut at the respective laboratories.

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.

We think here the referee means P4, not P5. Here we are describing the sampling that happened in the field, so it makes sense to us to describe it before the transportation of the cores. We will clarify that this sampling occurred in the field before storing and transporting the cores. The organization of the sampling and analyses will be much improved with the table we described.

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)?

We think the referee means P4, not P5. We will move this paragraph to come earlier, before the discussion of the lab measurements. Correct, the continuous melter analyses in the field occurred in the 2015-2016 season, whereas only discrete analyses occurred in the field in 2014-2015. We will clarify this.

We will clarify this point in the text.

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).

We will make sure that all descriptions of the measurements come before we present the data. We will include a table that details the measurement metadata.

P5, L16: The $CH_4$ 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.

It is not uncommon to have an outlier of several tens of ppb in discrete field measurements, as the precision is much worse than the laboratory analyses due to environmental conditions in the field laboratory and the use of a small, portable instrument. There is not a blank correction applied to these measurements, and there may also be depth offsets from the BID core. These factors likely cause the offsets seen in the top 4-5 m that referee 4 mentions here. We will include discussion of these factors so that readers are aware that the quality of the discrete field measurements is less than the lab data that we compare to in Figure 2.

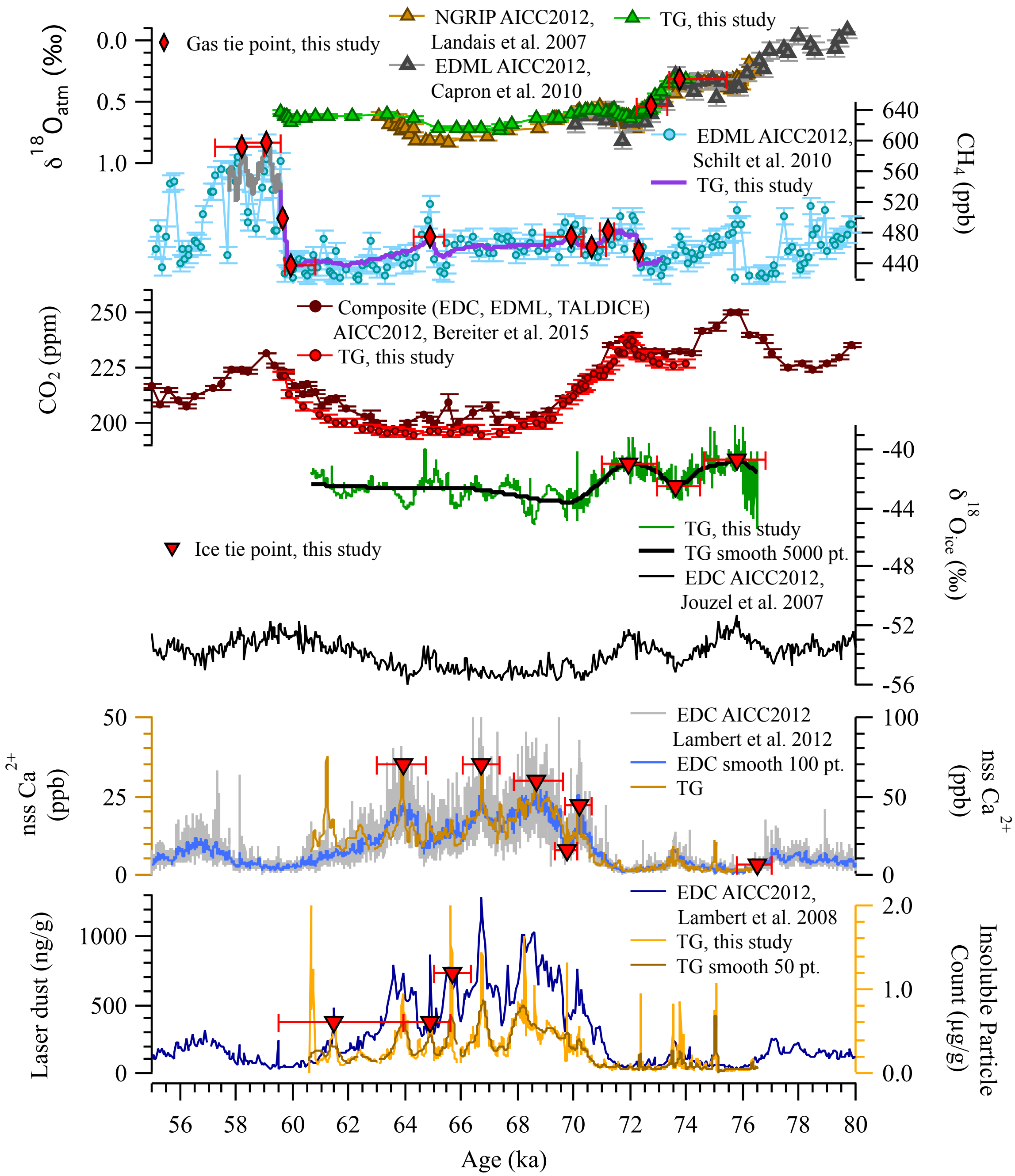
**Results and discussion:**

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.

The AIM events we recognize in the Taylor Glacier core are large features that exist in EDC and EDML. We state in the text that we recognize Taylor Glacier d18O is noisier than the records we match to. Nevertheless, the AIM features are unmistakable changes of up to ~ 3‰ that we think represent robust features for tie point selection. Our smoothing of the d18O noise helps identify the peaks and troughs of the features more clearly. These features are important for our record because they provide tie points that importantly resolve ambiguities in the nssCa (because nssCa varies little in the deeper part of the record).

We will expand the axes on Figure 3 so that the shapes of the isotope curves are more visible to readers. We will add text clarifying what we see as robust features in the d18O ice record. We also picked tie points directly from the d18Oice records so that readers clearly see the variability at the AIM events that we are matching in the d18Oice records.

Revised Figure 3:




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?

Referees 1-3 made similar comments concerning ambiguous tie points, including the tie point at

12 m. We assign the dust peak at 12 m (rather than at 9 m or at 15 m) to the dust peak in EDC at 73.6 ka because this way the AIM 19 and AIM 20 that we identify in the d18Oice line up with the AIM events in EDC. If the peaks at 9 m or 15 m are fit to the dust peak at 73.6 ka instead, the d18O no longer matches.

We will justify this in the text in the revised manuscript. We also picked tie points directly from d18Oice to avoid the ambiguity.

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?

The peak that appears as a thin purple line in Figure 2 that exceeds the axis limits is either a measurement artifact in the raw data or too small of an event to match to EDC. The raw data shown in Figure 2 are not filtered for outliers, and we prefer to show the full raw data set to demonstrate the data quality. In the 40 cm above this noise there is a real peak (smaller amplitude up to 0.4 ug/g, 6.1 m) that appears to lead the dust rise in EDC in Figure 3.

We understand the confusion because we did not describe our criteria for matching dust peaks. We only fit features that span a range of depths on the order of at least 10's of centimeters and show structure (more than one data point comprising the peak). The peak that exceeds the axis limits is an example of noise because the high dust concentrations span less than 2 mm of ice. The smaller peak centered at 6.1 m that spans ~ 30 cm of ice is a real dust event.

In any case, the new tie point for the period before the MIS 4 onset is 7.75 m, 71.95 ka, chosen from the d18Oice variability. This way the dust ambiguity is avoided altogether.

We will expand Figure 2 so it is easier for reviewers to see the variability we are matching in the dust records. We will also describe in the text what we consider a true feature in the dust versus noise. We are also plotting smoothed versions of the d18Oice and particle count records (the two noisiest records from Taylor Glacier) so that the large-scale variations are seen clearly.

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.

The small adjustments to center peaks perfectly are not important considering that those differences are well within the errors we place on the ice age scale. Originally we left some of these peaks off center because we did not want to "over fit" the data. However, we understand the value of matching the peaks more perfectly in terms of communicating what we did and convincing readers that our matches are good.

We adjusted the tie points so that the peaks are more centered.

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.

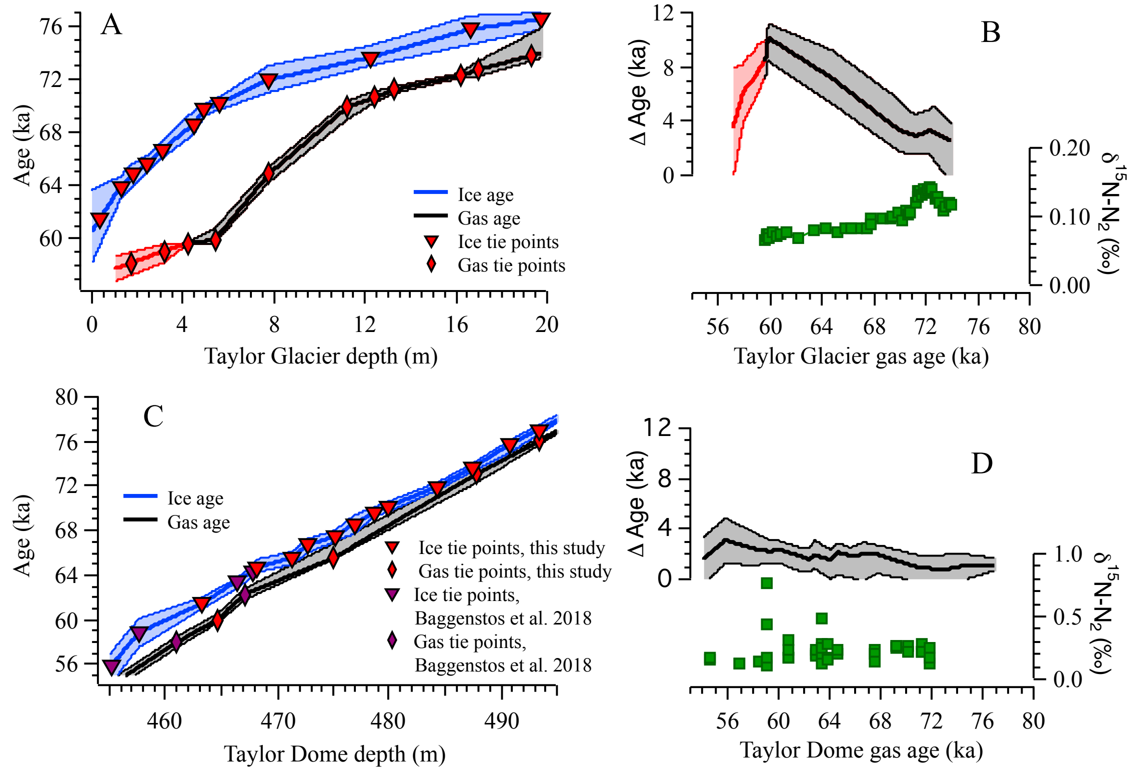
This is true that there was some iterative feedback from the other records. We thought the way that this was described in the original text was sufficient, but we see how the reader could be misled by the very good fits between, for example, nssCa but not particle count. We revised the tie point scheme so that more tie points are chosen for all of the records (d18Oice, nssCa, and particle counts) to be more transparent about our tie point choosing process. For the gas tie points we do not see good candidate tie points in the d18Oatm or CO2 that would improve upon the tie points already picked from CH4. Also see the response to other referees above about our hesitation to value-matching CO2.

We will revise the ice age scale tie points including more tie points for d18O ice and nssCa in addition to particle counts. We will present them clearly in the tables and justify our choices clearly in the text.

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).

Curvature in the age scales in Fig. 2 and Fig. 3 is an illusion because of tie points that are close to one another in depth or because of the low graphic resolution of the figures. We moved the "B" panels in Figure 3 and Figure 4 to their own figure that is easier to see (Figure 5).

Figure 5:




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 looks somewhat uncertain especially for the older one.

We do not want to match the brown data in Fig 2 because it was measured on a system in the field that is lower precision. That tool is used as a rough guide for determining ages in the field, and those data must always be verified in the lab. Other referees also had issues with tie points/ data in the top 4 meters of our cores.

We again defer to the editor's guidance on this issue as we see two options for moving forward – (1) throw out all data 0-4 m and only interpret the section of core where there is the most reliable delta age data, or (2) emphasize more strongly in the text that we are NOT rigorously interpreting 0-4 m, merely providing a plausible gas chronology for the 0-4 m section based on our view that the CH4 field data are showing the true atmospheric signal. Choice 2 is our preference.

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.

We will change the tie point to match the second (and most depleted d18Oatm) data point to the

NGRIP data. Yes, there is an offset between the 2015-2016 core and the 2014-2015 core d18Oatm (~0.05‰) at 17m. This could imply a depth offset - that the cores are in fact not supposed to overlap there because there is a depth logging error. The measurement precision, which is quite good, seems to suggest this is more likely the case. Unfortunately it is not possible to deduce what the depth error actually is here, but it is worth noting that shifting the red data 20 cm deeper (our estimated depth uncertainty, stated in the paper) would result in a plausible scenario where d18Oatm is decreasing monotonically with depth.

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.

We agree that Siple Dome would be ideal, but the recently published age scale (Seltzer et al., 2017) only extends to 50 ka. We would need to first sync the rest of the age scale to AICC 2012 to be consistent with our record, which we think is outside the scope of this work. We are also aware of other efforts to sync the Siple Dome age scale (Buizert, personal communication) and would thus prefer not to do it. If you look at other d18Oatm records for this time period (TALDICE, Vostok) you will see that they are very low resolution and offer no clear alternative for tying the d18Oatm more robustly than what we have done. You will also see that the offsets between TALDICE and Vostok are of the same magnitude as the offsets between TG and NGRIP.

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).
We interpret this comment to mean that we should value-match in between tie points where our Taylor Glacier data show differences from the reference records. There is only one section where our records depart significantly from the reference record (CO2 mismatch between 64-60 ka), and we deliberately chose not to value-match the CO2 data given that CO2 offsets between different ice cores are a known and as of yet unresolved issue (Luthi et al., 2008). It is unclear to us where else we might be introducing large errors due to linear interpolation. We think that the uncertainty we estimate with our method (interpolating between maximum and minimum ages at each tie point to generate an oldest and youngest age model) reasonably estimates the uncertainties between tie points (Figure 5 above).

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.

If we match the CO2 to the (Bereiter et al., 2015) composite where referee 4 describes (around 60.5 ka), the gas record shifts to older ages by ~ 650 years. The error we stated (in Table 1) is already larger than this, so the case where CO2 is matched is essentially already accounted for if you consider our error range. In this section we would have to value-match the CO2 because there are no robust or obvious inflection points. We prefer not to value-match using CO2 because of the unresolved issue of $CO_2$ offsets between different ice cores, described in the text.

We will draw the errors in the age scale on our plot of ice age and gas age versus depth so that the errors in the chronology are more visible to readers.

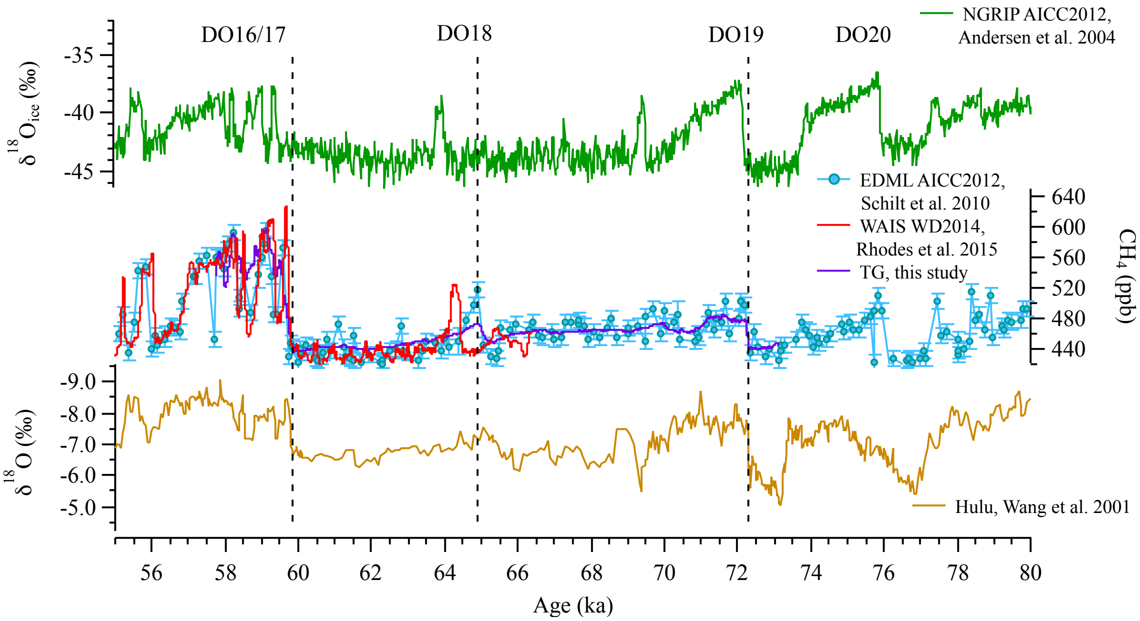
P6, L20: See the comment above about the dust and Ca.
P7, L19-22: See comment above about $CH_4$ for 0-4 m.

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.
In the original manuscript we assigned maximum/ minimum ages to each tie point that estimated the range of possible ages. Our choice of age range for each tie point was based on consideration of (1) the resolution of the data for a given feature that we matched (i.e. do we know the age of a true peak or trough in the data, or is it masked by low resolution?), (2) the analytical uncertainty of the data that we matched to, and (3) how robust (or possibly ambiguous) the matched feature was (i.e. could we be matching the wrong feature?). If any of the three criteria were poor or ambiguous then we enlarged the age uncertainty range to reflect a worse quality match. We then propagated the uncertainties by interpolating through the maximum and minimum age at each tie point, which resulted in an oldest and youngest possible chronology (and therefore also a maximum and minimum delta age via calculation). We considered calculating a fit index for each tie point and a probability distribution for each match, but this method is more suited for value-matching data whereas we are matching features where multiple parameters are changing at the same time (i.e. peaks and troughs in $d^{18}O_{atm}$ and $CH_4$, or in $nssCa^{2+}$ and particle count). We think that an algorithm will not necessarily do this better than we can do by eye, or at least the difference will be negligible for the delta age story we are telling in this manuscript.

We think the uncertainties estimated by the methods described above are justified because (1) even with assigning very generous uncertainty to each tie point, the uncertainty does not affect our interpretations about delta age (i.e., the delta age that we calculate after propagating the uncertainties to our chronologies is still large during MIS 4 and supports the notion of the development of a steep accumulation gradient between the Taylor Dome coring site and the Taylor Glacier accumulation zone), (2) the uncertainty we estimate for delta age is realistic and is similar in magnitude to the uncertainty in delta age from other Antarctic ice cores, including the delta age uncertainties in Baggenstos et al. 2018, and (3) the $CH_4$ record on our new gas age scale matches Hulu speleothem $\delta^{18}O$ very closely at the onset of DO 16/17 and DO 19 (Figure 6 below). The last point supports our choice of tie points for synchronizing to the AICC2012 gas age scale because the Hulu data are independently dated.

In the revised text we will explain more clearly how we assigned uncertainty to each tie point, and we will justify more clearly why we think the uncertainty is reasonable.

Figure 6 - Comparison of the timing of abrupt $CH_4$ changes in the new Taylor Glacier ice core with abrupt events in the Hulu speleothem record.




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.

The 20 cm depth offset does not have to do with CFA versus discrete samples. We explained the cause of the depth offset in the text – it is because of angle breaks in BID cores that were not aligned and accounted for during drilling. It is possible that there are smaller depth errors due to mistakes in depth logging, but these must be smaller than the offsets introduced due to angle breaks.

The place in the ice core where age is changing the most with depth is where the slope is the shallowest on Figure 3 (age axis is on the bottom). If you compute the age change for 20 cm along this slope, you get 416 years. So the referee is right that we did not estimate the value high enough. We will change the conservative estimate to 420 years and propagate it accordingly.

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.

We cited the age uncertainty from AICC2012 incorrectly. We will correct the absolute uncertainty that we cite to 1 $\sigma$ = 1500 years for the EDML gas age scale and 1 $\sigma$ = 2500 years for the EDC ice age scale. A comparison of Taylor Glacier CH4 to Hulu d18O shows very good agreement in terms of the onsets of DO 19 and DO 16/17 (Figure 6, above). Though we necessarily acquire the aforementioned uncertainties when using AICC2012 as our reference age scale, we think that the absolute age uncertainty in our gas age scale is probably less than this given the close match to Hulu. We also note that the relative errors in our ice cores will be less than the total propagated EDC and EDML 1 σ uncertainties because the uncertainties in gas age and ice age are correlated with depth.

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

We will change Delta-Age to 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.

We agree that our explanation is lacking. We will summarize the controls on delta age more thoroughly. We like referee 4’s explanation of delta age here and will include it in our revised delta age discussion.

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").

We are hesitant to value-match the CO2 data (there is not another way to tie the CO2 given the nature of the variability – inflection points are somewhat unclear/ poorly defined, a ramp-fit algorithm or some other statistical tool would be needed to define them. We also chose not to fit the d18Oatm in this interval given the offsets between TG and NGRIP d18Oatm between 62-68 ka. The most convincing variability in d18Oatm occurs during DO 18 where we already have a more robust CH4 tie point. If you look at other d18Oatm records (e.g. Vostok, TALDICE, or EDC) the resolution is too low to match in this range. You will also see that the offsets in d18Oatm between those cores are larger than the offsets between TG and NGRIP.

We agree with the referee that it would be better to have ice and gas age tie points for the same depths, but the reality is there are not robust features in the gas and ice phases at all depths.

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.

See other comments above regarding estimating the accumulation rate quantitatively. We think that a quantitative estimate is not meaningful given the uncertainties in firn modeling. If we use the d15N approach, the uncertainties in the convective zone are so large that the accumulation rate estimate spans at least 1 order of magnitude (more detailed discussion of this below).

We preferred not to provide quantitative estimates of accumulation rate given that they require extrapolating beyond the calibration range of firn models. An estimate is 0.5 mm/yr-2 cm/yr (described above and below), which we will include in the text with the caveat that it is a very cautious estimate.

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

We will summarize the d15N controls more completely including discussing the effects of temperature gradients in the diffusive zone as well as mixing in the convective zone.

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 occurs 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.

We agree with referee 4 that the changes in d15N and delta age are not linear with each other. We will discuss this point.

We performed this exercise following (Parrenin et al., 2012) and (Buizert et al., 2012) and found that d15N implied accumulation between < 1 mm/yr – 2 cm/yr depending on the height of the convective zone. The convective zone is the major uncertainty in this calculation. If the Taylor Glacier accumulation area resembles something like Megadunes, Antarctica (Severinghaus et al., 2010), then a convective zone of up to 25 m may be possible, which would drive d15N to much lower values for accumulation rates on the order of a couple cm/yr. On the other hand, if the convective zone is 5-10 m deep then accumulation rates must fall to something on the order of a couple mm/yr.

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

We will add this constraint by removing delta age data where it is < 0. This does bring up the issue of what the minimum delta age should be… delta age = 0 is equally impossible, for example. Rather than force delta age data to be an arbitrary minimum when < 0, we will simply remove data that is < 0 and explain in the text that we also think values near 0 are likely wrong but that we do not know the minimum delta age so chose not to impose that constraint.

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

We will change it to “onset of full MIS 4 glaciation.”

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).

We will develop our discussion of delta age more deeply by summarizing more completely the controls on delta age, by summarizing the meaning of d15N including reference to the barometric equation for computing the height of the diffusive zone, and including more references to previous work on delta age (Parrenin et al., 2012; Buizert et al., 2012; Martinerie et al., 1994; Martinerie et al., 1992) as well as published firn models (Goujon et al., 2003; Herron and Langway, 1980). We still think that accumulation and temperature have the greatest first-order control on delta age, and that an extremely high delta age as found here is very unlikely to occur without extremely low accumulation. When discussing controls on delta age we will distinguish which controls we think are secondary (e.g. impurities, wind stress, thinning of firn) versus primary (e.g. temperature and accumulation). Since HL is our firn model of choice we will add 1-2 sentences describing the model and the particular controls on densification (i.e. densification to 0.55 g/mL does not depend on accumulation, but second-stage densification to 0.84 g/mL depends highly on accumulation.)

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).

See above comments about accumulation rate estimates. We will include 0.5 mm/yr-2 cm/yr as our estimate of accumulation rate, and we will clearly state that it is a conservative estimate that is limited by the fact that we lack a firn model to accurately describe very low accumulation conditions.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