Response to Referee #3

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 (delta age) and related varying accumulation rates. This interpretation involves some assumptions 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 firn modeling with somewhat empirical models well outside the calibration range of their parameters is not compulsory, the physical processes controlling delta age and $d^{15}N$ fractionation should be better described.

Overall I think that major revisions are needed in order to better discuss the approximations made (e.g. ignored firn and ice physics), describe the consequences of alternative assumptions on ambiguous chronological tie points for multi-species consistency, age scales and delta 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 delta age and accumulation. A number of suggestions are provided below.

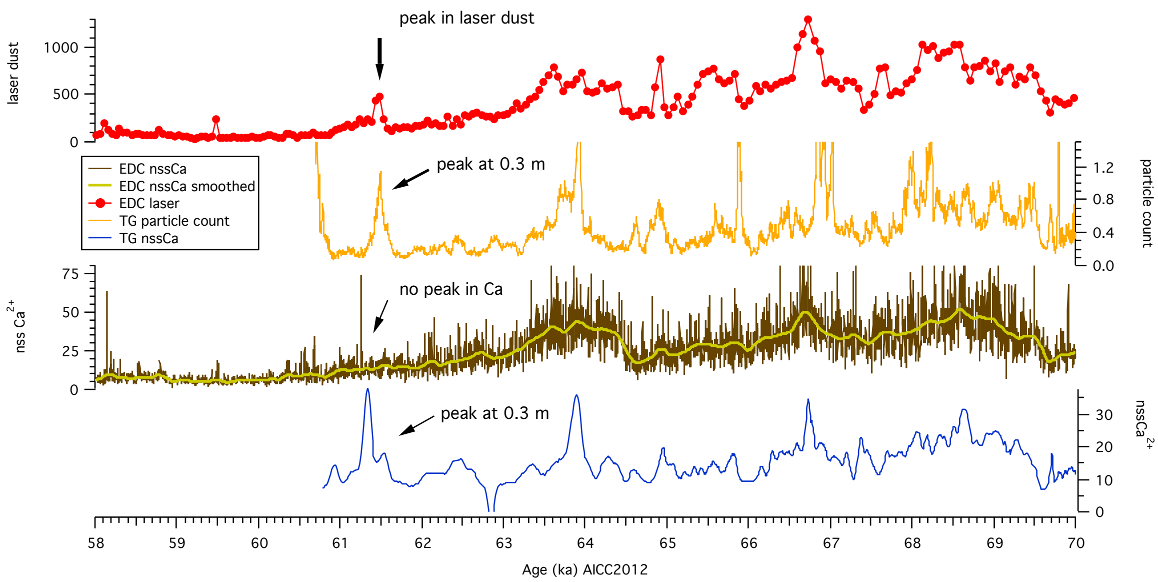
We thank Referee #3 for helpful comments.

As discussed in the response to other reviewers, we are addressing the perceived ambiguity of tie point selection by justifying them with more extensive discussion in the text. We will enlarge the figures (particularly Figure 2, but also Figure 3 and Figure 4) so that it is easier to see why we chose to match variations the way we did. We will also describe why alternative tie point selections produce poorer matches with EDC records. A specific example of a possibly ambiguous tie point choice was brought to our attention by reviewer 2 and reviewer 4 – why assign the peak in dust at 73.6 ka to 12 m instead of 9 m? The reason is that if we assign the 9 m peak to 73.6 ka then the d18Oice is shifted such that the minimum between AIM 19 and AIM 20 no longer aligns with the EDC d18O record. The correlation between d18Oice EDC and d18Oice TG gets worse due to stretching the TG AIM 19 peak by several thousand years. This way we also would not align the nssCa peak at 73.6 ka (there is no nssCa variability in our record at 9 m).

A second possible ambiguity is the dust peak at 15 m. In our set of tie points we do not align this peak, so we tried two alternatives to align it to variations in EDC nssCa. If we align it with the EDC nssCa peak at ~77 ka (1) this stretches the d18Oice record out such that the signal no longer matches EDC d18Oice at AIM 20, (2) the nssCa variability in TG doesn't really match the variability seen at 77 ka in EDC, and (3) the delta age gets unreasonably high (we expect accumulation to be higher in stage 5 versus stage 4 due to warmer average temperatures and thus delta age to be relatively lower than during stage 4). We also explored aligning the 15 m dust peak with the EDC variability at ~73.6 ka, but again this causes a mismatch the d18Oice in EDC at AIM 19/20.

A third possible ambiguity is in the dust peaks between 0-1 m. We could align the large dust peak at 0.3 m to the nssCa peak in EDC at ~ 64 ka. This shifts other aligned dust peaks back in time - i.e. the peak at 1.25 m aligns with a very small dust peak at 65 ka and seems out of place, and the three particle count peaks between 1-3 m depth do not have corresponding 3 laser dust peaks to align with. Instead one peak has to be skipped. We prefer to align the particle count peak at 0.3 m with the smaller EDC peak at 61.5 ka because we observe that background particle count appears to be decreasing toward shallower depths (see minima in the particle count record between peaks) similar to how EDC nssCa and laser dust decrease between from 64 ka to about 60 ka. However, we recognize this interpretation puts the 0.3 m nssCa peak at a place on the AICC 2012 age scale where EDC nss Ca has no corresponding peak (see figure below). We have contacted the original authors of the data in question, and no logs of contamination or processing errors exist for these depths in EDC. The existence of the laser dust peak without a corresponding nssCa peak is as of yet unexplained (Fischer, H. and Lambert, F. personal communication). It is possible the dust captured in EDC at 61.5 ka had very little Ca while the dust at TG did.

The nssCa mismatch we are describing above is shown in this figure (black arrows show particle count peak and corresponding laser dust peak (red) on AICC2012, the blue trace has a nssCa peak without a corresponding peak in EDC (brown)).



One plausible alternative for the 0.3 m tie point is to shift it to older ages, which causes a mismatch in the rest of the data and increases the delta age estimate by 2.5 ka.

Another plausible alternative is that the “stray” nssCa peak in the Taylor Glacier record is from local, wind-blown Ca^{2+} dust and is not representative of a larger-scale Antarctic dust event. The peak occurs in the top 30 cm of the ice core where dust data have been rejected previously ((Baggenstos et al., 2018) rejected top 40 cm) due to contamination of vertical cracks by local wind-blown dust.

We will discuss completely our justification for tying the 0.3 m dust peak, but we will also emphasize that we do not interpret the age scale in the top 40 cm rigorously, similar to (Baggenstos et al., 2018).

Because we are discussing the shallow part of the core here, we think it is appropriate to inform the editor about a mistake we made in the presentation of data in the original manuscript. We cut off the top meter of the TG records in the original Figure 3. This is why there are tie points for ice as young as 61.5 ka but no data that young in the original Figure 3. We did this for the gas data because there is clearly CH4 contamination up to 1200 ppb in the 0-1m section of our cores (which appeared in all measurements, no disagreement between DRI and field CH4). We suspect this is due to snow machine oil/ exhaust at the drill site.

We will include text that describes this issue in the revised manuscript. We do not, however, see any reason to reject the entire 0-1 m in the dust, particle count, or d18Oice records. Rather, wind blown dust contamination has only been observed in the top 40 cm at Taylor Glacier (Baggenstos et al., 2018).

We prefer to show all data in the revised manuscript and revised figures for completeness, but the ice records shallower than 40 cm and the gas records shallower than 4 m will not be interpreted rigorously. This will be described and justified clearly in the text.

Regarding interpretations of high values of delta age – we noted in our response to reviewer 2 that while differential ice thinning would affect the depth-age relationships, it would have no effect on delta age because thinning does not disrupt the stratigraphic relationship between ice and gas bubbles at depth. The reviewer also referred to hiatuses in accumulation. We think an accumulation hiatus is in line with (if an extreme example of) how we are currently interpreting the high delta age values – i.e. high delta ages correspond to low accumulation rates. We did not explicitly discuss what our records would look like if a complete cessation of accumulation occurred. We will include text that discusses how the records might look if accumulation hiatuses occurred. A hiatus, if it did occur, is most likely in the section 60-64 ka where CH4 is flat, d18Oatm variability is small, and full MIS 4 conditions are underway with extremely cold temperatures and low accumulation at the TG catchment. Because our record does contain the complete CO2 rise for the MIS 4/3 transition, we think there is good reason to believe there is no significant hiatus in bubble trapping. 60-64 ka on the gas age scale corresponds to 68-71 ka on the ice age scale, where there is still clear variability in our particle count and nssCa records. We think this is further proof that there is not a hiatus in accumulation. We will include these justifications in the text. Regarding folding, there is no evidence of folding in the records we developed, which would show up as reversals in gas and ice phase records as compared to known trends from other ice cores. We see no reversals in our gas records and ice phase records. For the sake of demonstrating our thinking - one might for example question whether the CO2 variability at AIM 19 is in fact two limbs of a fold with its center at the CO2 peak. Looking at CH4, d18Oatm, nssCa, and insoluble particle count tracers on depth axes rules out the possibility that the ice is folded because the records are not identical on both sides of the hypothesized fold axis. The same can be said even where the gas records are relatively flat – e.g. between 8.5-10.5 m depth when gas concentrations are relatively low, d18Oatm is relatively enriched, and there is little variability. Here there is also little variability in the nssCa and insoluble particle count records to resolve the problem. We note in this ambiguous section that the d18Oatm is steadily becoming more enriched and d15N is becoming steadily more depleted with no evidence that the trends reverse, as you would expect if the ice were folded there.

We will include similar discussion of hiatuses and folding in the revised text.

We will include more information in our discussion on the physical parameters controlling delta age (lines 15-22, page 8) and d15N (line 35-36, page 8). Specifically we will include discussion of secondary effects on firn evolution/ delta age, including impurity concentrations, wind stress (wind pumping), and surface temperature (here I am citing topics mentioned by other reviewers as well as referee 3). We will also state specifically that thinning can affect the depth-age relationship but not delta age because there seems to be some confusion about this.

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.

The missing MIS 4 is explicitly discussed in (Baggenstos et al., 2017), which we will add as a reference for lines 34-35 on page 2 and lines 26-28 on page 3, with explanation.

It is unlikely that there are different accumulation hiatuses in the different ice cores presented in this work, if that is what the referee means. The 5/4 BID cores as well as the PICO auger exploratory core were drilled within ~ 1 m of one another and so must have traveled down glacier as a unit from the exact same accumulation area. Even the -380 m core on the Main Transect, which is < 1 km from the location where the 5/4 BID cores and PICO core were drilled, came from the same accumulation zone as the 5/4 BID cores (and all other TG stratigraphic units) without experiencing a hiatus or any sort of prolonged difference in accumulation relative to the 5/4 cores. Our point here is that the whole accumulation zone sourcing the TG ice archive would have experienced accumulation hiatuses at the same time, broadly speaking. We recognize it is possible for a glacier accumulation zone to have small-scale heterogeneity in accumulation rate either due to differences in precipitation rate or due to different magnitudes of wind scouring. We think this kind of variability would not affect the 5/4 BID cores or the PICO exploratory core because they were obtained so close to one another, but it is conceivable that prolonged heterogeneity in the accumulation zone caused discrepancies between the -380 m Main Transect core and the 5/4 cores. However we observe that the -380 m core d15N values are quite comparable to those measured in the 2015-2016 5/4 BID core. We interpret this as evidence that the two cores came from firn columns with similar characteristics, implying that the accumulation zone was more or less the same for both the Main Transect and the 5/4 drill site. In other words, the stratigraphy is continuous between the two drill sites. In case this is unclear or seems weak due to the arguable dating of the -380 m core, we are basically saying that the d15N/CH4, d15N/CO2, and d15N/d18Oatm ratios are the same in the -380m core as in the 5/4 BID core, supporting the conclusion that the stratigraphy on Taylor Glacier is continuous and that different hiatuses in accumulation, or even different accumulation zone sources altogether, were unlikely.

We will add general direction of flow lines to Figure 1a.

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

The original chronology st9810 (Steig et al., 1998) was based on CH4 matching to GISP2 and inferring delta age to get the ice chronology. But the ice chronology was incorrect because it assumed accumulation could not be exceptionally low (and thus delta age could not be exceptionally high). The error was pointed out by aligning the TD Ca record to EDC (Mulvaney et al., 2000). The TD gas chronology was updated by synchronization to the Vostok GT4 timescale (Petit et al., 1999; Barnola et al., 1991) that extends back to ~ 68 ka (Indermuhle et al., 2000). A full chronology (gas and ice) was most recently updated by (Baggenstos et al., 2018) back to 60 ka.

We adopt tie points from (Baggenstos et al., 2018) where our age scales overlap. We will include the aforementioned references and a summary of the previous TD chronology in the revised manuscript.

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

We will add references for the sublimation and flow rates in lines 14-16 on page 3 – (Kavanaugh et al., 2009a; Kavanaugh et al., 2009b).

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

We will add references to lines 25-28 on page 3 for the vertical dip of layers on the Main Transect (Bauska et al., 2016; Schilt et al., 2014; Baggenstos et al., 2017; Petrenko et al., 2017; Petrenko et al., 2016).

p3 l30-31: the exact location of the "-380m" drill site (coordinates) should be provided. More site information could be provided (e.g. altitude, mean annual and summer temperatures etc.)

The mean altitude, mean annual, and mean summer temperatures are not different from any other site on Taylor Glacier discussed in the paper. It is a drill site on the Main Transect (Figure 1), 380 m from a flag that marks the center "0 m" on the Main Transect. We will add all of this specific information in the revision.

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.

Referee 2 had a similar question and we repeat our response here. There is not a unification method, per se. Each core was drilled adjacent (within 1 m) to the original borehole drilled with the PICO auger. Each core has a depth scale determined by summing the lengths of individual, meter-long BID cores. We assume, for example, that 15.0 m in one core = 15.0 m in another core. The cores are not "aligned" in the sense that we did not stretch or alter the depth scales to match the data precisely. When you view all measurements on depth there are very small offsets between the records, indicating slight depth offsets, likely due to short angle breaks at core ends that effect the depth summation along the core. We conservatively estimated the effect of these offsets on our age model and propagated them through the delta age calculations (discussion of this begins at p7 line 36).

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 CH_4 data series are not consistent above 5m depth, and in Figure 3 the CO_2 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 CO_2 record below 4 or 5 m depth to the composite CO_2 record instead of using the CH_4 record which is nearly flat between ~4.5 and 7 m depth for multi-species consistency and delta age? In Table 2, two CH_4 tie points and half of the ice phase tie points are located well above 4 m depth.

We see why Referee #3 would be suspicious about the data 5m and shallower – the CH4 records depart from one another substantially above 4m with smaller differences between 4-5m depth, and the CO2 appears to date too young relative to the composite data. But what Referee 3 says here is not entirely correct. The shallowest CO2 measurement is at 4m depth, where the CH4 differences are much smaller, and the CH4 rise evident in both datasets (associated with DO17) is one of the most robust features. The discrepancy in the CO2 depends highly on the tie point at 5.4 m – the "low point before DO 16/17" in table 1. We think this is the most robust gas tie point of the entire set. If we shifted this to younger ages, it would smear the CH4 rise out such that TG CH4 would lead EDML. The next tie point is at DO18. We did not choose other tie points from the CH4 record because the CH4 variability between DO18 and DO17 is minimal, thus any tie points chosen there would be ambiguous. We could choose tie points deliberately from the CO2 record such that the slopes of the CO2 increases are more similar, but we refrained from doing this given that CO2 offsets between different ice cores are a known but relatively poorly understood phenomenon (Luthi et al., 2008). We addressed this in lines 13-16 on page 6. In fact, as an example, there are CO2 offsets between the TG CO2 and the composite record from (Bereiter et al., 2015) of even larger magnitude than at the 4/3 transition during the middle of stage 4 (Figure 3).

The consequence of matching the CO2 would be that the records would be more consistent (value-matched), and delta age would be lower by ~1.5 ka at the most. The uncertainty we estimated for the delta age calculation is already larger than this.

We would like to stress that the parameters in the ice phase (i.e. d18Oice and dust) are only affected by the surface cracks in the top 40 cm, not the entire top 4 m. This is stated in the Table 1 caption, but we will state it more clearly in the main text too. So the ice phase tie points are not an issue except potentially in the top 40 cm.

Referee #3 is correct that two tie points for the gases are chosen above 4m, which is why we shaded those tie points gray in Table 1. Our intention was that those points be interpreted cautiously. The CH4 record shows variability that looks very much like the CH4 variability associated with DO 16/17, hence the temptation to choose tie points and extend the gas chronology to depths shallower than 4m. But the mismatch in CH4 between the DRI and field data sets leaves us unable to reject the possibility that both data sets are wrong < 4m. This wouldn't change the conclusions of the paper because delta age begins to rise at 11.5 m depth in our core, with maximum delta age occurring at ~ 5.5 m. We would like to reemphasize that we do not interpret the gas data shallower than 4 m rigorously and that those data do not inform our interpretations of the high delta age values.

p5 l27-30: In Figure 2, the TG CH_4 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.

We think analytical noise in the EDML record is the main reason for the dissimilarity between the EDML and TG CH4. We would prefer to plot the error bars on the EDML data, which visually help the reader see the smooth atmospheric signal, rather than smooth the data set directly. EDC CH4 looks quite similar in resolution and smoothness to EDML. The relative amplitudes of abrupt CH4 features can be an indication of relative smoothing. EDC, EDML, and TG all have the same magnitude CH4 feature at DO 19. At DO 18 EDML CH4 is higher, followed by EDC CH4, followed by TG CH4. The CH4 rise at DO 16/17 (near the MIS 4/3 transition) is largest in EDML, only slightly smaller in TG, and lowest in EDC. Using this as an indication of smoothing, then the effect in TG is largest at DO 18 and negligible at other times in the record.

We don't think the CFA system is smoothing beyond what the firn has already done to the gas record. The main justification for this is that the discrete CH4 measured in the lab (green dots at DO19 in Fig 2) and CFA CH4 (purple and red lines in Fig 2) agree well.

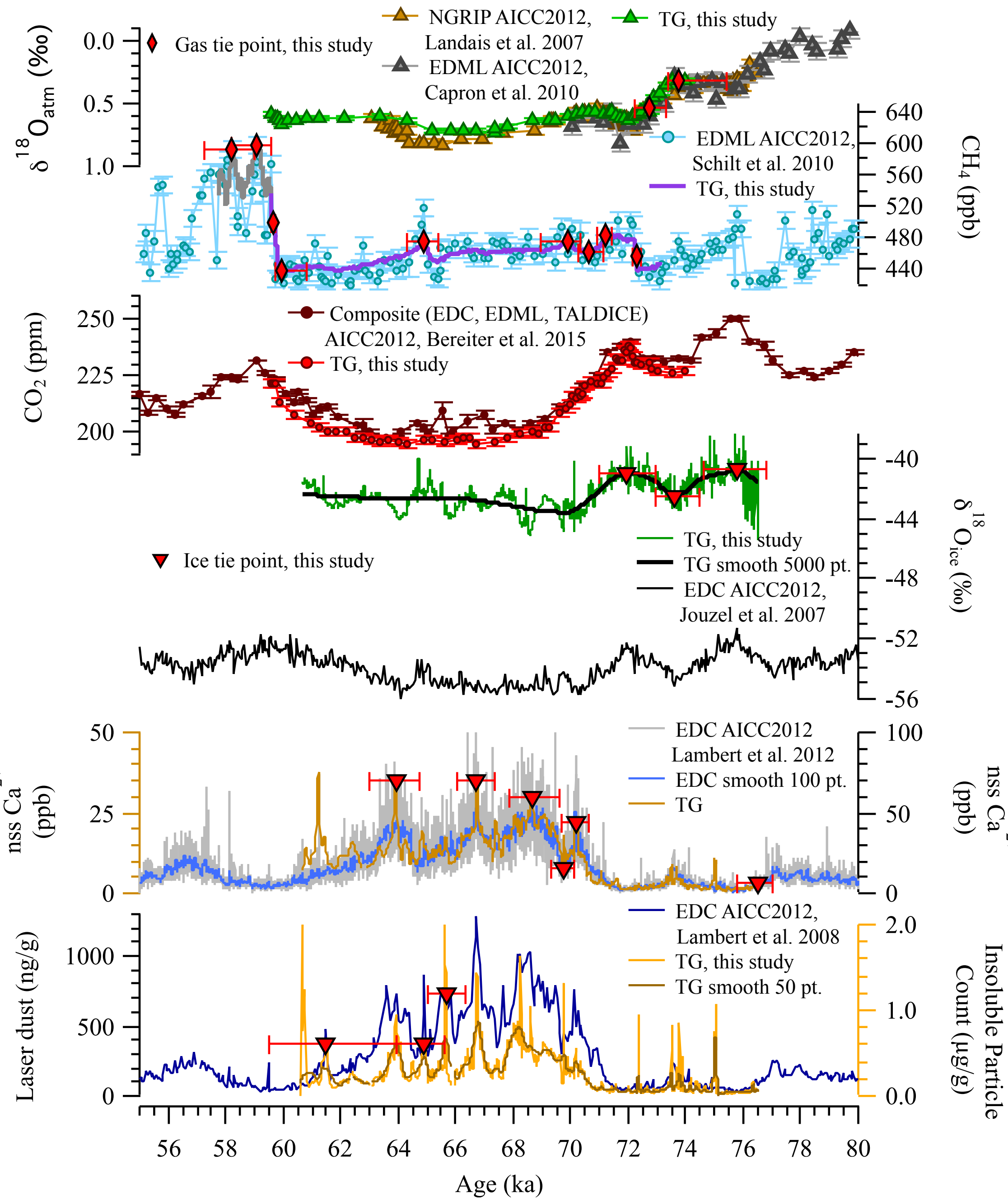
We will include discussion of smoothing in the text including justification of why we don't think smoothing effects are significantly impacting our tie point choices.

p5 l34-35: I did not understand why the _18Oatm 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)

The TALDICE and Dome Fuji datasets are unpublished and/or unavailable publicly) to our knowledge, though they appear in figures in the referee's citations. Both are low resolution through the time period of interest, and Vostok d18Oatm is also quite low resolution. To our knowledge the Dome Fuji DFO 2006 age scale is not synchronized to AICC 2012, though Vostok and TALDICE are. We do not think synchronizing to any of the three records helps eliminate ambiguity that CH4 doesn't already solve. Where d18Oatm is helpful is syncing TG to NGRIP in the older part of the gas record where CH4 variability is comparatively smaller but d18Oatm variability is large. Also worth noting here is that NGRIP d18Oatm is relatively high resolution across the 71-76 ka section. The match to NGRIP is further justified by the close agreement with EDML d18Oatm, which we plot in the revised Figure 3 (below).

We will justify the synchronization to NGRIP in the text.

Revised Figure 3:



p5 l38: some tie points look ambiguous to me and the tie points assignment should be further discussed. For example, the EDC and TG _18Oice records look quite different in Figure 2, thus the _18Oice tie point does not look robust to me. On the dust plot in 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.

We refer back to our response at the beginning of this document following the general comments.

The referee here likely made a typo because our 75.75 ka age is aligned with the d18Oice peak at 16.62 m. Thus we assume the referee means the 12.05 m dust tie point that we aligned with 73.58 ka (now updated to 12.20 m and 73.62 ka). We specifically addressed this tie point in the response above, as well as two other ambiguous tie points.

We will provide further justification of our tie point selections in the text as already described.

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.

We agree and will include more discussion of the rationale that led to our tie point choices. The way the manuscript is written now, it sounds like we picked CH4, d18Oatm, particle count, and d18Oice tie points and were happy to find that the CO2 and nssCa also looked good. In reality the referee is correct that there was some iterative feedback from the CO2 and nssCa records even though we didn't actually select any depth-age tie points from those records.

We will rewrite the text so that it more accurately reflects how we reasoned through the tie point choices, especially now that the tie point choices have been revised (described in the summary document).

p6 l31-32 and Figure 3: I do not understand how the CH_4 record from the "-380m" core could be unambiguously tied to AICC2012. On the other hand the CO_2 records seem easier to match and matched. The overall dating constraints should be better described.

We will rewrite the text to explain in more detail how we aligned the -380 m core to AICC 2012, including presenting the tie points in a table and discussing our tie point choices in the text. The main revision here is that we will deemphasize the -380 m core dating, presenting our tie points as a plausible chronology, and explaining more clearly why we think it is robust that the -380 m core is roughly late MIS 4 and MIS 4/3 age.

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 CO_2 mismatch with "TG 5/4" not discussed, nor the $d_{18}O_{atm}$ 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 CO_2 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 _age values also inferred?

We will address more completely the dating of the -380 m core in the text as well as include a table with tie points. We will also move the data to a separate figure where we compare the -380 m gas data with those from Taylor Glacier 5/4 BID cores as well as the reference records on AICC 2012. The CO2 mismatch with TG 5/4 was not discussed in the text, but we will address it explicitly. The d18Oatm mismatch with NGRIP was also not discussed.

We think the -380 m core implies that there is stratigraphic continuity between the Main Transect and the drill site of the new MIS 5/4 BID cores. We think the exact dating of the -380 m core is unimportant; rather the important part is that the gases appear to be late stage MIS 4, the d15N is similarly low, and the age-depth relationship is similarly steep. This supports the idea that different accumulation zones are not sourcing the Taylor Glacier blue ice area at different times. Instead, Taylor Glacier ice has likely come from the same deposition zone throughout the last ice age. We intend to deemphasize the exact dating of the -380 m gas age scale and instead will simply argue that the methane and CO2 rises and the d18Oatm depletion are roughly what we would expect if the gas age was ~ last MIS 4 and MIS 4/3, and that the d15N is similarly low in that core, implying that there is continuity between the Main Transect records and the new MIS 5/4 records.

Unfortunately there is no ice phase data for -380 m, so we cannot infer delta age.

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.

We will clarify in the text that the "new" ice core is the TG 5/4 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.

We calculated annual layer thickness, but we do not think it adds any information that is not already visible in the depth-age plots. The annual layer thickness is smallest where the age changes the most with depth.

p7 l24 - p8 l12: This discussion of uncertainties should appear earlier in the article and be more detailed (see also above comments on p5 l27-30, p5 l38, p6 l9-27).

Other referees also suggested this. We will move the uncertainty discussion to the Field Site and Analytical Methods section. We will more thoroughly discuss the uncertainties involved.

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.

Correct, we will update the text on p8 l1-4 to say 20 cm depth uncertainty instead of 10 cm.

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 CH_4 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).

We agree that the smoothing is probably somewhat different between EDML and TG, but not so different that it affects our tie point choices or age model significantly. See our comment above about smoothing in EDML, EDC, and TG, particularly the part about the amplitude of CH4 variability at DO 18. The smoothness of the TG CH4 record versus EDML is due less to the gas trapping process and more to (1) the different analytical methods employed – continuous measurements in TG (and thus some degree of smoothing in TG, though small compared to smoothing in firn), versus discrete measurements in EDML, and (2) higher analytical noise in EDML (the TG continuous CH4 is within the EDML CH4 error bars at all parts of the records).

We agree it would be interesting to estimate gas-trapping duration, but we do not have a robust estimate of accumulation rate given that the d15N and delta ages are well outside of the calibration range of firn models. We might estimate accumulation rate given the depth-age plots if we knew the thinning function, but we do not know the thinning function and think it is unwise to trust fundamental thinning approximations given the archive's unconventional path to the drill site.

We will more clearly demonstrate the analytical noise in EDML by plotting the error bars on Figures 2 and 3.

p8 l16-18 and l35-36: a much more in depth presentation of firn processes influencing delta age, _depth and the physics of _15N should be provided. The consistency between a very large _age and a very shallow firn (_15N indication) should be commented.

In response to this as well as other referees' comments, we will provide a more in-depth summary of the processes influencing delta age as well as the fractionation of 15N in the firn column. We will more clearly elucidate the correlation between large delta age and shallow firn. We will also discuss secondary effects of wind pumping in the convective zone and impurity content on grain metamorphosis.

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.
We are unsure what the reviewer is suggesting here. If the reviewer is referring to the evolution of the Taylor Dome ice core chronology, which was described at length in (Baggenstos et al., 2018), then it is unclear to us how this would be the basis for the discussion of the uncertainty on our delta age calculations discussed on p8 l24-20. Our uncertainty (for both Taylor Glacier and Taylor Dome cores) is based on independently estimating the uncertainty of individual tie points and interpolating the max/min possible chronologies. The evolution of the Taylor Dome timescale was the result of errors in determining the delta age in the Taylor Dome timescale during the LGM. The chronology was revised by dust synchronization (Mulvaney et al., 2000) to obtain a correct ice age scale, and later refined and extended further back in time by (Baggenstos et al., 2018). The successive datings of the Taylor Dome core are useful in understanding the history of the timescale, but they do not provide much useful information about how to estimate more realistic uncertainty for our time period (57-77 ka).

We will add discussion of the history/ evolution of the Taylor Dome chronology insofar as it puts our work in context, but we will refer to the discussion in (Baggenstos et al., 2018) in lieu of re-summarizing everything.

p8 l35 - p9 l4: the fact that the physics of _15N (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 _15N values measured in TG ice suggest that either the firn is very thin (an estimate should be provided) or nongravitational effects are important.

We will provide an estimate of firn thickness in the text. Based on gravitational effects alone the firn thickness is estimated ~ 15 m, though the height of the convective zone is a major uncertainty in this. A deep convective zone would drive d15N to lower values despite a thicker total firn.

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.

We will clean up the text with respect to repeated methodological information. We will discuss Taylor Dome dating in more detail including rationale for the tie points we chose. We will discuss in more detail the inconsistencies between the records.

p9 l21-31: this section is unclear to me. If TG and TD ice cores have strongly different _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.

The TG accumulation site is to the north of the TD ice core site, which we showed in Figure 1 and also stated in line 33 page 9. The point we are trying to argue in the paper is that over a small distance, accumulation varied significantly. At the LGM this trend is reversed. This is the interesting implication of the delta age histories.

We will describe this better in the text so that the point comes across more clearly.

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.

We will shorten this part of the text to explain what we mean more succinctly. We are also awaiting the editor's guidance concerning whether providing accumulation rate estimates (given the very large uncertainties) is necessary.

Technical corrections

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

We will include DE08-2 as an example of very small delta age.

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

Here we are referencing the gas age scale separately from the ice age scale. We will rewrite this to seem less redundant.

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

We will provide the link https://escholarship.org/content/qt9wn8789k/qt9wn8789k.pdf to the electronic version of the thesis.

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

We will update the reference to the accepted version of the manuscript.

p15 l49: suppress QUATERNARY

We will change QUATERNARY to 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.

We are unsure what the uppercase/lowercase issue is that referee 3 refers to here. The chronology does not actually invert in the dust panel, though we see where the referee is talking about – it appears to invert in the gray lines where the dust begins to rise. We will expand Figure 2 so generally it is easier to see/ read. This should help readers not only understand why we chose tie points, but also make it clear that the chronology is not inverted.

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