Response to Referee #1

General comments: I would strongly advise to reorganize the paper in separated sections for more clarity. The way it is now, you continuously go back and forth between the sites and methods. I would suggest the following organization: Introduction, Field sites and analytical methods (–> presentation of your sites and the measured data in the field and in the lab + analytical uncertainties), Age models (-> choice of tie-points and chronological uncertainty propagation for both TG and TD), Results -> Δage and Discussion. Your manuscript would gain in clarity and would guide the reader toward your results and interpretations. You should avoid the listing of sites and data in the text and instead propose tables summarizing the data/sites information you need. This is particularly true for the blue ice sites you cite in the text and the different measurements performed on your cores.

We reorganized the text of the paper following Referee 1's comment. The new manuscript is organized as follows:

Abstract

1 Introduction

2 Field site and methods
- 2.1 Field site
- 2.2 Core retrieval
- 2.3 Analytical methods
- 2.4 Analytical uncertainties

3 Age models
- 3.1 Taylor Glacier MIS 5/4 blue ice drill cores
- 3.2 Taylor Glacier -380 m Main Transect core
- 3.3 Taylor Dome
- 3.4 Age model uncertainties

4 Results
- 4.1 Δage
- 4.2 Δage uncertainties

5 Discussion

6 Conclusions

We added a table that summarizes all metadata concerning the measurements. It is now clearer which measurements were made on which cores, at which institutions, field versus laboratory, and continuous versus discrete.

Please find responses to specific comments below.

Specific comments & Technical corrections:

ABSTRACT:

- line 24: "Taylor Glacier (Antarctica)"

We added "(Antarctica)."

- line 27: "low SNOW accumulation WITHIN the Taylor…"

We added “snow” and change “at the Taylor Glacier accumulation zone” to “within the Taylor Glacier accumulation zone.”

- line 31: replace “Taylor Dome” (already used in the sentence) by “this area”
We changed “Taylor Dome” to “this area.”

INTRODUCTION:

Page 1:
-line 36: missing references for past atmospheric composition and a list of trace gases
We added references for “paleoarchive of the Earth’s past atmospheric composition” - (Bauska et al., 2017; Petrenko et al., 2017; Schilt et al., 2014).

-lines 40-41: This statement is not true, close to bedrock folding can happen, disrupting the order of ice/gas layers, as seen for the bottom part of NEEM ice core in Greenland for example.
We changed “age of ice and air bubbles always increases with depth” to “age of ice and air bubbles generally increases with depth.”

-line 41 “precise distance-age”-> from which reference is the distance measured?
The reference to “distance” here simply refers to any generic reference point from which distance is measured in a blue ice area. In the case of Taylor Glacier, distance is measured from a flag that marks the location of the Main Transect. The flag was originally placed at an arbitrary location along the transect, and it ensures continuity between different sampling efforts during different seasons. E.g. -58 m is always 58 m south of the flag.

These details are described by (Baggenstos et al., 2017), and we added this reference here.

Page 2:
-line 12: remove “with fast access to age information”
We removed “with fast access to age information.”

-line 13: as precise as what? The previous method? Replace “have” by “present”
We added “as precise as the aforementioned methods.” We changed “have” to “present.” We also deleted “providing” for better readability.

-paragraph 3: it would be easier for the reader if you summarize all in a table (site, location, period covered, references) and refer to it in the main text. Such a listing is difficult to follow with too many commas.
We do not think it is appropriate to add a table describing various blue ice areas because the paper is not a review of blue ice areas. There is already a published review of Antarctic blue ice areas that we cited in the original manuscript (Bintanja, 1999). We simply wished to point out that there are several blue ice areas that have been studied, however we will follow editorial guidance on this issue.

-line 29: replace “expands” by “extends” and replace “by developing ice and gas chronologies spanning” by “back to”
We replaced “expands” with “extends.” We prefer not to replace “by developing ice and gas chronologies spanning the MIS 5/4 transition” with “back to” because “back to” implies that the archive is continuous back to the 5/4 transition, which it is not.

-Line 31-32: remove “the across-flow transect”
The relevant sentence in the original manuscript is, “In 2015 a new ice core was retrieved approximately 1 km down-glacier from the ‘Main Transect,’ the across-flow transect containing ice from Termination 1 through MIS 3 (Baggenstos et al., 2017) (Figure 1).”

We prefer not to remove “the across-flow transect” from this sentence because we think it is important to define what the Main Transect is and to note its orientation with respect to the glacier flow explicitly.

-Line 34-36: “paleoarchive FROM TAYLOR GLACIER, where it was previously thought to be absent”. Remove “larger context of”. Replace “into” by “within”. Replace “at Taylor Dome” by “of this region”
We changed this to read “the description of a new MIS 4 paleoarchive from Taylor Glacier, where it was previously thought to be absent.” We removed “larger context of.” We replaced “into” with “within.” We replaced “Taylor Dome” with “of this region.”

FIELD SITE AND ANALYTICAL METHODS:

Page 3:
-line 5: if you are not citing an acronym, ice sheet is written without capital letters
We changed “Ice Sheet” to “ice sheet.”

-line 6: “northERN”
We changed “north” to “northern.”

-line 7: “ice EQUIVALENT accumulation”
We changed “ice accumulation” to “ice equivalent accumulation.”

-line 15: “80 km LONG ablation zone”, and you already said it in the previous paragraph
We removed “~ 80 km.”

-line 16: need rewording. I suggest the following: “Water stable isotopes obtained from an along-flow transect just below the equilibrium line”… “revealed uncontinuous ice covering the last glacial period” –
We changed the sentence to read “Water stable isotope data obtained from an along-flow transect from just below the equilibrium line to the terminus revealed ice from the last glacial period outcropping at sporadic places along the transect.”

line 20: “revealed continuous records of ice from the Holocene to the last ice age, with ice of the last interglacial and older found…” references for this statement?
We included references for this statement. They are (Schilt et al., 2014), (Bauska et al., 2016), (Baggenstos et al., 2017), and (Buizert et al., 2014).

-line 22: “the most COMMONLY USED archive” instead of utilized
We changed “utilized” to “commonly used.”

-line 27: Reference for the previous ice core study. Where was taken this new ice core compared to the previous study? Need more precision. What was the sampling problem with the previous record?
There is not a previous ice core study per se. We have worked on Taylor Glacier for over 5 years, and prior to the 2014-2015 field season the MIS 5/4 transition was thought to be missing from the glacier archive. Then in 2014-2015 we found the MIS 5/4 transition in a new location that was previously not sampled. The new location is 1 km down glacier from the Main Transect, which we note on line 34 and show in Figure 2a.

We referenced relevant discussion in (Baggenstos, 2015) to clarify.

-lines 30-33: need a reference
It is unclear what the referee wants referenced. We added the reference Rhodes et al. 2015 in case it is the CH4 variability at DO16/17. If referee 1 meant the results from the -380 m PICO core there is not a reference because those data are unpublished until this manuscript.

-line 36: replace “in” by “of”…”CH4 variations similar to those ASSOCIATED WITH DO19” or “corresponding to”
We replaced “in” by “of.” We replaced “similar to those at Dansgaard-Oeschger event 19” to “similar to those associated with Dansgaard-Oeschger event 19.”

-line 37: “CH4 CONCENTRATION increase”
We added “concentration.”

Page 4:
-line 3: replace “work” by “analysis”, replace “spanning” by “section”
We replaced “work” with “analysis.” We replaced “spanning” with “section.”

-line 4: need rewording, proposition: “…CH4 and CO2 concentrations, which confirmed the MIS 4/5 transition record in the gas phase”
We changed line 4 to read, “sampled for laboratory analyses of CH4 and CO2 concentrations, which confirmed the MIS 5/4 transition record in the gas phase.”

-line 6: spanning not properly used
We stated that the 0-9m and 17-19.8m sections were sampled, instead of using the word “spanning.”

-lines 7-11: it would help to make a table for all the analyses performed on the different cores, with specification of the proxy measures, where, the time coverage of samples (or portion of core) and the method used for measurements, analytical uncertainty…
Other referees requested a similar table. We included a table that summarizes the metadata for the analyses discussed in the manuscript including where the samples were taken, which coring device was used (BID or PICO), and in which laboratory and what type of measurements were made.

-line 24: “resulted in a good agreement of our measurements with other…”
We changed the sentence to read, “resulted in a good agreement between our measurements and other Antarctic CH4 records.”

Page 5:
-line 19: “… on archived Taylor Dome ICE CORE samples…”
We changed the sentence to read, “Discrete measurements of CH4 and CO2 were made at OSU on archived Taylor Dome ice core samples…”

-line 22: “(~10g OF ICE, …)”
We added “of ice” after “~ 10 g.”

Page 6:
-line 10: ‘CO2 CONCENTRATION decrease”, again later
We changed the sentences to read, “CO2 concentration decrease” and “CO2 concentration increase.”

-line 11: remove “and”
We removed “and.”

-line 13: value of the offset?
We stated the value of the offset. It is ~ 13 ppm at 61.5 ka.

-line 15: need rewording, a proposition: “…younger ages. Therefore, we refrain from further align the CO2 rises together for better consistency.”
We reworded the sentence to read, “…younger ages. However, CO_2 offsets between ice cores are observed (Luthi et al., 2008), and we cannot reject the possibility that the offsets are real. Therefore we refrain from value-matching the CO_2 rise.

-line 17: why not use the d18Oatm of Vostok or TALDICE instead of NGRIP? You would have a complete record over your period of interest on AICC2012, but potentially with a lower resolution.
Referee 4 had a similar comment. Deep TALDICE d18Oatm is unpublished, Vostok d18Oatm is low resolution, and the two records do not agree precisely in terms of when the light excursion begins at the MIS 4/3 transition. Siple Dome d18Oatm would be the best choice, but the new Seltzer timescale does not

extend beyond 50,000 years ago and the old timescale is not synced with AICC2012 (Seltzer et al., 2017). We think it is beyond the scope of this paper to sync Siple Dome to AICC2012, and we are aware of other work already in progress towards this goal. NGRIP is helpful because it provides variability to match where CH4 variations are small, and it is consistent with AICC2012 (which is the timescale that we use to tie to EDML CH4). We note that the EDML d18Oatm shows quite good agreement with NGRIP d18Oatm in terms of the onset of the MIS 5/4 excursion. Since EDML is lower resolution than NRIP, we still pick tie points using the NGRIP d18Oatm, however we show the EDML agreement in our revised Figure 3 below. (Capron et al., 2010; Landais et al., 2007)

-line 23: replace "has" by presents"
We changed "Taylor Glacier d18Oice has more variability…" to "Taylor Glacier d18Oice is more variable…"

-paragraph 3: I am not very much convinced by value matching for dating. We do not really understand the usefulness of the -380 core data until the idea of similar firn conditions. This and the following argument are important for your interpretation later. This paragraph needs rewording.
The reviewer points out that paragraph 3 on page 6 is poorly worded because the purpose of the -380 m core is not clear from the beginning. We think the -380 m core is useful because it shows similar trends in the gas data (CO2, d18Oatm, and CH4) as the MIS 5/4 cores from ~ 1 km down glacier. This suggests stratigraphic continuity between the Main Transect (where the -380 m core was drilled and where all previous work on Taylor Glacier has been conducted) and the new MIS 5/4 drill site. The d15N-N2 is similarly low in the -380 m core as in the 5/4 cores. The implication of this is that the archive of ice found at the Main Transect likely originated from the same accumulation zone as the 5/4 cores. In other words, the Taylor Glacier ablation zone is not a confounding mixture of ice that has flowed from different deposition areas at different times. Rather, the archive appears to be a stratigraphically continuous and intact record with a common source deposition zone.

We reworded the paragraph so that readers understand this point clearly and know the purpose of the -380 m core at the beginning of the paragraph.

The reviewer was also not convinced that our tie point choices for the -380 m core were robust. In the original manuscript we value-matched the -380 m CH_4 data because the data are sparse and we lack the context of a longer record to confidently match the beginning and ending of transitions and features like for the MIS 5/4 cores. However, we recognize that value-matching cannot provide unique ages for the -380 m core, especially before the MIS 4/3 transition where the variability in the gas records is small (i.e. one could assign different ages to a given depth). We intend to rewrite paragraph 3 on page 6 to de-emphasize the dating of the -380 m core, as it was not our intention to develop a robust chronology for that core. We think the important thing is that the -380 m core contains gas bubbles that span the MIS 4/3 transition and some of late MIS 4. We would like to emphasize that CO2, d18Oatm, and CH4 are all changing across the MIS 4/3 interval in the -380 m core, similar to in the new MIS 5/4 cores, and to find variability in all three of those parameters that is synchronous and of the right magnitude is unique. Thus we think assigning the age of the -380 m core broadly to the MIS 4/3 transition and late MIS 4 is robust, even if the exact chronology is uncertain.

In the revised text we de-emphasize the exact dating of the -380 m core, present the tie points we chose more clearly in a table, and display the -380 m data in a new figure so that it is not cluttered with the new MIS 5/4 BID data. We also emphasize the purpose of interpreting it – to show evidence for stratigraphic continuity between the MIS 5/4 drill site and the Main Transect, which implies that the source accumulation zone for the Taylor Glacier ice archive was the same through time. We think we are justified interpreting the -380 m core this way without necessarily improving the certainty of the -380 m chronology.

Page 7:
-paragraph 2: not useful, could be removed.
One puzzle that has emerged from our work at Taylor Glacier is why the MIS 4 dusty ice was so elusive to find, whereas the LGM dusty ice is clearly represented and is even visible at the surface. Paragraph 2

addresses this problem and offers an explanation - that the MIS 4 ice is quite thin. We prefer to keep this paragraph, but we moved it to the results section and emphasized the usefulness of the paragraph at the beginning.

3.2 ANALYTICAL AND AGE MODEL UNCERTAINTIES

Page 7:
-line 18: "is likely"
We changed this to read, "The mismatch between field and laboratory CH4 in the top 0-4 m of the core is likely due to…"

-lines 19-22: not clear. You say that you consider the 2015-2016 data as uncontaminated, but as the same record differ from the lab, in the end you do not interpret the data… but you did later in the text… Moreover, you did not discuss the reasons that could explain why the records are so different. I would possibly keep the tuning, but associate it with a much larger uncertainty than the other points due to the mismatch with the lab data. Then only use the CH4 data in grey area for dating purposes and no more.
Other referees had similar comments about the 0-4 m CH4 data and our choice to tie the field data to AICC 2012. We stated why we think the laboratory and field records are different – it is likely because resealed cracks in the glacier surface affected the CH4 in the lab samples but not the field samples. These kinds of cracks tend to penetrate the top 4 m of ice (line 19) at Taylor Glacier, and CH4 measurements in the 0-4 m surface ice have looked wrong in the past, so this is not a new observation (Baggenstos, 2015).

We assigned larger uncertainty in this section. We also emphasized how we do not interpret the top 4 m rigorously. We explained and rationalized in the text more clearly what we chose to do. We only presented the CH4 data from 0-4 m for completeness, and the delta age in the 0-4 m is not critical for our interpretations (the high delta age values occur at ~ 5.5 m). There are no CO2 or d18Oatm data from the 0-4 m section to interpret, and the delta age in 0-4 m section has very little bearing on the overall story we present. Thus we think it is justified to offer our best plausible gas age scale for 0-4 m, clearly show the discrepancy between the laboratory and the field data, and state that the 0-4 m section could be contaminated but that it will not be used in our interpretations that follow.

The discussion about the analytical uncertainty should be following the presentation of the analytical methods.

We reorganized the text so that the uncertainty discussion comes after the analytical methods, similar to comments from other referees.

–paragraph 4: I am not convinced about your argument for the confirmation of data. From the looks of the data presented on Figure 2, I would say that your choice of markers is not convincing, I would have chosen differently… From your Figure 3, I understand that your choices were made in order to align together the records you cite as confirming your alignment (e.g. nssCa). I would recommend to change the way you presented your figure 2 to make the reader see by himself why you choose these tie-points and not others. You should focus more on this aspect, which is the base of your discussion later, it would strengthen your work. Not necessarily in the main text, it could be an appendix.
Though referee 1 would have chosen tie points differently, he or she did not say exactly how. Thus it is difficult to defend our tie point choices specifically to this referee's criticism. Generally speaking, in the revision we provided stronger justification for the tie point choices we prefer. Other reviewers also asked for information like this.

Specifically we made figures clearer so that readers can see easily why we picked certain tie points. We added more tie point justification and moved it to the supplementary information. Here the original Figure 2 showing our tie point matches is expanded into Figures S1 and S2 so that readers can more easily see what features we matched.

We revised our final tie point choices. These are summarized in the preceding summary document, but the main changes from the original manuscript include: (1) 6 new nssCa tie points that match variability between TG nssCa and EDC nssCa, (2) only 3 particle count tie points matching TG particle counts with EDC laser dust (instead of the original 9), and (3) 2 additional d18Oice tie points that match variability in TG water isotopes with EDC water isotopes. We opted to include more nssCa tie points instead of particle count tie points because the nssCa data are more quantitative, we can compare to EDC nssCa (a like-like comparison) instead of comparing insoluble particle counts to EDC laser dust (different measurements), and the nssCa record is less noisy than the particle count record. We hope that the addition of two more d18Oice tie points helps readers see the similarity in the d18Oice variability at TG and EDC for AIM 19 (72.5 ka) and AIM 20 (76 ka).

The gas tie points between CH4 have not changed substantially from the original manuscript. The two tie points that match TG d18Oatm to NGRIP d18Oatm have changed slightly based on feedback from reviewers. The oldest one linking 19.8 m to 74.65 ka now ties 19.27 m to 73.74 ka in order to tie the lowest measured d18Oatm to the local minimum in the NGRIP record. The other d18Oatm tie point was shifted to tie to the midpoint of the MIS 5/4 transition in NGRIP.

The tie points and the final match are shown in our revised Figure 3 (above).

Page 8:
-lines 3-4: “20 cm = 300 years”, based on what? Which chronology?
The relevant part of the sentence in question is: “there is a 10 cm offset between the continuous field CH4 and the discrete laboratory CH4, and a 20 cm offset between the continuous laboratory CH4 and the discrete laboratory CH4. 20 cm depth uncertainty equates to, conservatively, 300 years on the gas age scale near the onset of Dansgaard-Oeschger event 19.” Here we were estimating the age error associated with depth offsets between the cores, the largest of which was 20 cm at DO 19. We believe it is clear when we say “on the gas age scale” that we are using our chronology.

Referee 4 pointed out that our conservative estimate was not conservative enough. We think referee 4 was actually misreading the axes of Fig 3B, but we did realize upon closer inspection that the slope of the gas age-depth curve in its steepest segment is 20.8 yr/ cm. So our conservative estimate of the effect of a 20 cm depth offset is ~ 420 years for the gas age scale. We changed “300” to “420” in the text and propagate the uncertainty to the chronologies and the delta age calculations.

Lines 5-8: This is not a proper argument. If you say that both CH4 data from TG and EDML are similarly smoothed in the firn column, you are implying that they have similar firn conditions (i.e. accumulation rates, firn depth…). Is it the case?

Our statement is based on the observation that the magnitude of the changes in CH4 and CO2 concentration are similar in TG and EDML. It appears that the CH4 signal in TG is more smoothed than EDML at DO 18 (65 ka), but this is the only place in the record where the magnitude of the changes is substantially different. It makes sense that the amount of smoothing in the firn would be the increasing between 60-70 ka where delta age is increasing and accumulation is presumably decreasing. There are no abrupt events in the gases during this interval besides DO 18, so we must use this as our metric for estimating the smoothing. EDML CH4 increases to ~ 515 ppb while TG only reaches ~ 475 ppb. Of course the peak CH4 at DO 18 is only defined by one data point at EDML, but if we assume it is correct then the maximum CH4 concentration recorded during DO18 at TG is 40 ppb lower than that at EDML. We also note the differences between d18Oatm at TG versus NGRIP during the same interval. Therefore we think the firn smoothing must be different in the two cores during this interval, and we think it is likely due to increasing the height of the lock-in zone consistent with delta age increasing to extremely high values as accumulation decreased.

Our initial statement was meant to reflect how the cores generally agree in terms of the magnitude of smoothing across the whole record, but we neglected to explore the larger discrepancies near DO18 that are likely due to firn smoothing.

We changed what we wrote in the paper to more accurately reflect our assessment of the degree of smoothing at DO 18. We don't think the smoothing is significantly contributing to the uncertainty in our tie point selection.

-lines8-9: Analytical noise… why is that? What is the measurement uncertainty of your method?
We deleted this sentence when rewriting the analytical uncertainty section. We provided a value in Table 1.

-lines 9-12: Please, when using a chronology as reference, make sure of the uncertainty values you cite… What you wrote is not correct. The AICC2012 chronology uncertainty over your period of interest (i.e. ~65-74 ka) at EDML is ranging between 1500 years and 1400 years (cf. supplementary material of Veres et al., 2013). The values you have indicated correspond to the uncertainty of the ice and gas chronology at the orbital scale, prior to the last interglacial. -Following all this discussion of uncertainties, what are the uncertainties associated with your ice and gas chronologies for TG? You never gave a value and I do not see them on your figures. The same for your revised TD chronology.
The reviewer notes an error in the original manuscript – we cited the wrong absolute age uncertainty associated with the AICC2012, which we use as a reference scale for dating the gas and ice records in the new Taylor Glacier cores. The reviewer pointed out the 1-sigma uncertainty in EDML is actually 1400-1500 years between 74-65 ka and can be found in the supplementary material of (Veres et al., 2013), but the supplementary material only gives the uncertainty in the ice age chronology for 74-65 ka. The main text gives the uncertainty in the gas age chronology for EDML (figure 2 in Veres et al. 2013), which is also ~ 1500 years. For the ice chronological uncertainty, the uncertainty in EDC should be considered instead of EDML because we tie our dust data exclusively to EDC to obtain the ice age scale. The 1-sigma uncertainty for the EDC ice age scale is ~1800-2500 years for the time period 74-65 ka.

We corrected the uncertainty we cited for the AICC2012 reference age scale to 1500 years for the gas phase and 2500 years (taking the maximum) for the ice phase so that it is consistent with the information in (Veres et al., 2013).

In general we presented the uncertainty estimation in the revised manuscript similarly to the original manuscript – i.e., each tie point we picked for Taylor Glacier and Taylor Dome is assigned a maximum and minimum age to estimate the uncertainty of the match, and these estimated uncertainties are propagated through the chronology by interpolating between the maximum and minimum ages at each tie point. The age range at each tie point is assigned by considering (1) the resolution of the data for a given feature that we matched, (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?).

In the revised text we explicitly display the errors along with the age models (shading in Figure 5A and Figure 5C below). The uncertainty range is also included in the delta age calculation (shading in Figure 5B and Figure 5D below).

3.3 ΔAGE AND COMPARISON TO TAYLOR DOME

Page 8:
-line 7: Temperature and accumulation are not the only factors influencing Δage. All factors acting on the firnification process do as they impact on the firn depth variability. What about insolation of wind stress affecting the snow metamorphism into ice?
We do not understand what referee 1 means by "insolation of wind stress." If he/she means wind stress, we did not include this because we think the effects on delta age are secondary. If he/she means insolation, then we also did not include this because insolation effects on delta age are also secondary. Temperature and accumulation are the primary controls on delta age. We are unaware of firn densification models that include wind stress or insolation with major influence on firn evolution. If insolation and wind stress affect delta age, we think they are of secondary importance to temperature and accumulation.

-line 19: remove "on the order of hundreds of years", it is given by the lower limits just before. Change "smaller" in "smallest"
We removed "on the order of hundreds of years," and we changed "smaller" to "smallest."

-line 21: replace "at" by "for"
We replaced "at" with "for."

-lines 26-27: ok for the two sources of uncertainties, but you forgot to take into account the absolute uncertainty of the ice and gas chronologies. You have ~1500 years uncertainty from the AICC2012 age scale, consequently the uncertainty of your new chronology should be around ~1600 years for the gas age, and ~1530 years for the ice age (I took one random range from your choice of tie-points). Then your maximum and minimum Δage should be obtained from the (ice age - 1sigma)-(gas age + 1sigma) and (ice age + 1sigma)-(gas age - 1sigma). You should give an approximate value of the Δage uncertainty for the reader to have an idea of the significance of your Δage values later.

We accounted for the uncertainty in delta age in the original manuscript by propagating our tie point uncertainty (described above) using the calculation that the reviewer describes here. We did not propagate the absolute uncertainty from the reference age scale, but we note that the actual uncertainty in delta age acquired from the reference age scale should be much less than the total propagated uncertainty from EDML (1500 years) and EDC (2500 years) because these uncertainties are correlated in depth. I.e., it is unlikely for one to be too old while the other is too young. Because the uncertainty estimates that we placed on our tie points are very generous, we think that we already estimate a reasonable uncertainty for delta age (between ~ 2000 years, Figure 5). This uncertainty range compares well with the uncertainty cited in (Baggenstos et al., 2018).

-line 30: "10 ka"+/- ??? uncertainty needed.
We stated our uncertainty more clearly in the text.

-line 33: then why is it so different? Replace "high" by "large"
We explain the difference in terms of accumulation gradients in the discussion section of the text. We replaced "high" with "large."

-line 34: now you talk of the influence of wind, but not before…
We talk about wind in terms of scouring, or removal of snow. Not in terms of influencing the snow grain metamorphism, which we think is of secondary importance in the firn densification. Wind scouring works to reduce net accumulation. Whenever we write "accumulation" in this manuscript, we are referencing the combined or net effect of addition of snow by precipitation AND removal of snow by wind scouring.

Page 9:
-paragraph 1: I do not think that the last sentence is necessary, you should delete it.
The sentence in question is, "Note that a **Δ** age of 0 or less is physically impossible, and minimum **Δ** age <= 0 in Figure 4 is merely an artifact of estimating the error for individual tie points generously and without the physical constraint that ice age > gas age." We deleted the sentence.

-paragraph 2: You should gather together in one section the chronology construction for your two sites, with the proper calculation of their respective uncertainties.
We reorganized the text so that there is an Age Models section describing the construction of the Taylor Glacier 5/4 BID cores and Taylor Dome chronologies (and the -380 m Main Transect core).

-line 13: "in the same manner AS described"
We added "as."

-lines 17-19: You should then directly give a 0 value. Note then the uncertainty associated to the Δage is then not guassian..
We simply give 0 values where the minimum error estimation causes the negative delta age artifact. No, the uncertainty is not Gaussian. It is not possible to assign a Gaussian error to our tie points given our methods.

-line 21: Δage of 2.5 ka, but p8 line 20 you cited an extrema value of 12 ka with reference to Baggentos et al., in review… why are the values so different?

This is explained in the Discussion section of the text. The accumulation gradient switches at the LGM relative to MIS4.

-lines 22-25: I disagree with this statement. It comes too soon. For TG, not located on a dome, ice thinning and ice flow are very important factors that could affect the depth-age relationship. For TG you cannot interpret directly your variations on Δage in terms of accumulation. To distinguish between the major influences of thinning and accumulation, you need an ice flow model. If your ice flow model indicate that there are no significant thinning variations, then and only then you can interpret it in terms of accumulation. Moreover, you give absolutely no justification for your favour toward accumulation changes, and you do not explain why you disregarded the thinning influence.

The statement in question is "The implication of the relatively 'normal' delta age is that accumulation at Taylor Dome did not dramatically change at the onset of the last glacial period or throughout MIS4 as Taylor Glacier did. Comparing the depth-age relationships in the new Taylor Glacier core versus the Taylor Dome ice core highlights the difference in accumulation between the two sites."

We are confused by referee 1's comment. If he/she means that thinning of the ice could affect delta age, then we disagree. Ice thinning can affect the slope of the depth-age relationship, but it cannot affect the stratigraphic order of bubbles and ice at depth, i.e. the Δage = ice age-gas age at any given depth will remain constant with any degree of thinning. In the revised manuscript we reference (Parrenin et al., 2012) and point out that delta depth (the difference in depth between ice and gas of the same age) can evolve with time due to thinning and glacier flow, but delta age is fixed when gas diffusion effectively ceases at the lock-in depth.

If referee 1 means thinning of *firn* at the original deposition site, then we agree that in an extreme case this could affect the delta age because the process occurs before bubble close off. However, in order to achieve a delta age of 10,000 years you would have to thin the firn such that 10,000 annual layers of snow were included in the firn pack before bubble close off. For example, if a typical delta age in east Antarctica is ~ 3000 years, then this means thinning firn to ~30% its thickness, which seems outside the realm of possibility even on the flank of a dome.

We do see how referee 1 takes issue with the second part of the relevant statement - interpreting the depth-age relationship strictly in terms of accumulation changes without considering thinning. We simply meant to state that the depth-age relationship supports our interpretation of the high delta age values. We changed the wording of this part of the paper and acknowledge that thinning due to glacier flow could be the cause of the observed depth-age relationship.

-last paragraph: you should give the modern values of accumulation measured at these two sites. It would give an idea of how much your prior assumption of all differences are due to accumulation changes is valid for modern times.
Modern accumulation rates at Taylor Dome were determined by (Morse et al., 1999), and a good illustration of a steep gradient in the modern accumulation across a 30 km north-south transect on Taylor dome is shown in (Morse et al., 2007). Accumulation changes along the gradient from 14 cm/yr to 2 cm/ yr.

(Kavanaugh and Cuffey, 2009; Kavanaugh et al., 2009a; Kavanaugh et al., 2009b) describe the modern accumulation rate in the Taylor Glacier catchment, which is informed by the accumulation gradient reported in (Morse et al., 2007). Taylor Glacier is estimated to receive 3-5 cm/yr. (Kavanaugh et al., 2009b) also reports the fact that Taylor Glacier is in a rain shadow and is much drier than the regional average, with references to (Morse et al., 2007; Morse et al., 1998).

We included this information and references in the discussion section where the accumulation gradient is discussed.

Page 10:

-lines 14-18: give values for the LGM reconstructed accumulation at both TD and the virtual sites. This gradient is reverse from yours. Why do you use it then? The useful result from this study to you is only "the opposite accumulation gradient (decreasing from south to north) for ice older than 60ka".
We include it because it is interesting to us that it shifted between the two time periods. It expands on a storyline in the literature that is related to the errors in the original TD age model. (Morse et al., 1998) first predicted the shift in storm gradients based on radar data, and we find it interesting that our delta age data support this.

-lines 18-26: bring nothing more, just show support for the LGM gradient that is different from yours. I would advise to remove these sentences.
This sentence becomes even more important given referee 1's prior comments about thinning. The authors of (Morse et al., 1998) rejected the notion of differential flow (i.e. thinning) because the layer thicknesses did not vary in the same way with depth.

-last paragraph: remove the first two sentences, you are only rewording your results.
We removed the first two sentences.

Page 11:
-line 4: need a reference for this statement.
We added a reference (Hall et al., 2015)

-paragraph 2: the MIS 4 gradient is similar to modern conditions. Are modern conditions in agreement with your proposed hypothesis?
Yes, though there are no data available to constrain the modern delta age at the probable deposition site for our samples.

FIGURES & TABLES:

Figure 1: I would advise to change the organization: a-Antarctica map, b-landsat imagery, simplified map of TG.
We changed the organization of figure 1 according to referee 1's suggestion.

Figure 2: The way the data are presented now, one can strongly argue your chosen tuning points. The scales are two small to see the consistency between the associated variability. I am not at all convinced about your tie-point between the d18Oice of EDC and TG, records present different variability. I would advise to remove from the legend the last two sentences. -Tables 1&2: You should add some indications on your figure 2, on the reference records, to directly make the link between the tables and your chosen points (e.g. DO19…). In Table 2 legend, remove the sentence "Ice phase…"
Unfortunately referee 1 did not state how he or she believes that one can strongly argue against the chosen tie points, which makes it difficult to rebut this point specifically. In the revision we further justify the d18Oice tie points along with other tie points used to construct the chronologies. We added labels for important features to ease comparison between the graphical display of tie points and the list of tie points in Table 1, e.g. "DO 19" or "DO 18". We also added figures graphically displaying the Taylor Dome tie points, analogous to the original Figure 2, which only showed Taylor Glacier tie points. Generally speaking, we justified our tie point choices more clearly in the supplementary information of the text.

Figure 3: I would say that there is absolutely no point in plotting together records that were tuned together, or if you really want to, it should be in an appendix. You already use some other untunned records to validate your chronologies. I would leave here only 1 gas, 1 ice records, and then the (b) part of the figure. You should extend the lines for the identification of MIS limits to the bottom of the figure for more clarity. In the legend your last sentence is not necessary, you could delete it.
We think displaying the tuned records helps the reader to see the variability we were matching in Figure 2 and shows how the data between the tie points agree. Showing the matched data in this way is common practice. The figure also puts the environmental records we are discussing in context.

Figure 4: Same comments as for Figure 3. Your should keep consistent the colours of curves from one figure to another. Why didn't you remove the three points in questions and simply state it in the measurement section?
Same responses for Figure 3. We kept the colors consistent between the two figures.

Why didn't you remove the three points in questions and simply state it in the measurement section?
We want to keep the three data points for completeness. It particularly aids readers who are using the same data set, or want to verify that the data set is similar to his/her own copy of the data.

Response to Referee #2

1- SUMMARY AND GENERAL COMMENTS:

The study by J. Menking and collaborators presents three new ice cores from the Taylor Glacier Blue ice area that they combine to provide the first "composite" ice core record from this location that covers the transition between Marine isotopic Stage (MIS) 5 an MIS 4 (~74 to 65 ka). The chronology for the air trapped in the ice is defined based on the analysis of the global atmospheric tracers CH4 and atmospheric d18O of O2 (d18Oatm) and their synchronisation with well-dated CH4 and d18Oatm records from other Antarctic ice cores. The ice age scale is defined mostly based on the ice dust content synchronisation, again with other well-dated Antarctic dust profiles. From these two ice and gas age scales, they infer the evolution of the age difference between ice and gas at the same depth – the so-called delta age – through this MIS5-MIS4 climatic transition. Substantial delta age changes are observed through time over this time interval i.e. with values from ~2000-3000 years at ~ 74 ka and approaching ~ 10 000 years at ~ 60 ka. The authors also provide a new evaluation of the delta age evolution throughout the same period in the Taylor Dome ice core (located south of the glacier), which suggests no significant delta age changes for this site. The authors attribute these contrasting delta age evolutions between the two sites to a steep accumulation gradient across Taylor Dome that intensified across the transition from MIS 5 to MIS 4.

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

My first major comment is related to the fact that the authors interpret the differences in the delta age evolutions between the Taylor Glacier area and the Taylor Dome ice core site almost exclusively in term of a change in the accumulation gradient between the two areas. While this could be an acceptable interpretation, they absolutely need to build a much stronger case regarding why this is their favoured one (e.g. versus ice thinning) and thus provide a much more elaborated discussion of their new results. But also, they should discuss the other possible controlling factors; in particular, those are commonly identified as impacting the firnification processes e.g. the role of surface temperature vs accumulation rate vs ice impurity content have already been discussed over the past few years (e.g. Bréant et al. 2017, Capron et al. 2013; Hörhold et al. 2012). I believe that a summary of the current knowledge (and knowledge gaps) regarding the climate and environmental factors that impact changes in delta age would be useful. In particular, it would be of added value to further mention firn densification models that provide an alternative method to estimate delta age. At the moment the authors only acknowledge the Herron and Langway model (1980) although several other models building on this original work have been developed in the more recent years (e.g. Goujon et al. 2003) and more recent development in Bréant et al. 2017, dynamical version of Herron and Langway (1980) used in e.g. Buizert et al. 2015).

The role of surface temperature was discounted in our initial interpretation because the differences in delta age between Taylor Glacier and Taylor Dome are so large, but the accumulation sites are quite close to each other and likely to not differ in surface temperature history very much. Accumulation seems much more likely to vary between the two sites, particularly given the previous work by (Morse et al., 1998), cited in our manuscript, showing different layer thicknesses across the dome. This interpretation is consistent also with the notion that accumulation has a greater control on delta age than temperature does. We think ice impurity content likely has a secondary effect compared to accumulation. We have a measure of impurity content in the particle count data and Ca concentrations. Particle count and Ca begin to rise at 7.5 m depth (moving up core), but delta age has already begun rising in non dusty ice at 11.5 m depth – so impurities do not seem to be driving delta age to first order.

We do agree that a summary of the factors controlling delta age would be appropriate, and in revision we added more text that references other factors controlling delta age.

The reviewer also mentions thinning. We believe the reviewer is suggesting that thinning due to flow from the dome to the sample site would somehow impact the age difference between gas and ice. Referee 1 made a similar point, to which we responded in detail. While thinning obviously could impact the depth difference between coeval points in the gas and ice phase, we do not see that it affects delta age because it does not disrupt the stratigraphic order of bubbles in relation to the ice matrix that encloses them. Our depth-age relationships are determined independently for the gas and ice phases, thus we make no assumption about accumulation to determine delta age. If we did assume accumulation rate to get delta age, and if we had assumed constant thinning for both Taylor Dome and Taylor Glacier, thinning could have been an issue.

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

See detailed comments below where these issues arise.

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

See detailed comments below.

2- SPECIFIC COMMENTS:

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

We added a table (Table 1) that details the metadata for all measurements made – i.e. which core, which measurement, at which institution, and in the field or lab.

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

The cores are not "aligned" in depth, per se. They were drilled adjacent to one another, so we assume that, e.g., 10.0 m depth in the 2014-2015 core is the same as 10.0 m depth in the 2015-2016 core. There was no shifting or stretching the depth scales to make the records match better between different cores. The only problem that leads to errors in the depth scales is irregular angle breaks at the ends of individual blue ice drill cores that were not properly aligned in the field immediately after recovery. This could theoretically lead to depth offsets of no more than 20 cm between cores as most angle breaks are < 10 cm. Our view is that the effect of depth offsets is visible in the comparison of the discrete CH_4 records from the 2014-2015 core versus the field CH_4, where you see up to a 10 cm depth offset between records at DO 19. 10cm conservatively equates to 210 years on our age model where age changes the most with depth. The continuous CH_4 measured at DRI versus in the field (same 2015-2016 core) actually exhibit larger offsets (up to 20 cm = 420 years on our age scale), likely from errors in the depth logging or again from angle breaks that cause depth offsets between sticks cut from the same core for field versus lab continuous flow analysis. Since this is the largest depth offset observed, we think this sufficiently estimates (and probably overestimates) the error due to depth offsets. Thus we propagated 20cm = 420 years error into our gas age calculations. A similar estimate was made for the ice age scale.

This is explained more clearly in the revised text. We added a paragraph in the age model uncertainties section that elaborates on the treatment of depth uncertainties. We also display the propagated error on the depth-age plots, not just the delta age (as in Figure 5 below).

- Section 3.1 is hard to follow, the authors should consider restructuring it such as 1) they present how the ice age scale has been defined and then 2) as the gas age scale has been defined. Regarding the definition of the tie points based on the alignment of the dust record, I find that some of them are quite ambiguous considering the number of spikes present in the TG records. For instance why would they assign the tie point at 73.6 ka to the spike at 12 m rather than the spike at 9 m? I believe that the authors have a good reason for doing so, however, it should be spelt out more explicitly. It is necessary that the figure be much enlarged to allow a detailed inspection of the records.

We appreciate Referee #2's suggestion to restructure section 3.1. We reorganized the text such that the 'Age Models' section comes before Results and Discussion. In this section ice age and the gas age models are explained in separate paragraphs. We also moved the explanation of the revised Taylor Dome age scales to the 'Age Models' section.

Regarding the tie points based on aligning the dust records – we feel these tie points are justified because they produce the best overall match between the Taylor Glacier dust and water isotope records with EDC. We explored a large number of alternate strategies, which did not perform as well. For example, the specific tie point questioned by the reviewer (12 m versus 9 m) is best justified with the d18Oice data. If the dust is matched at 9m, the correlation between Taylor Glacier and EDC d18Oice deteriorates substantially because of mismatches in the variability around AIM 19 and AIM 20. The uniqueness of the d18Oice and dust records together justifies the tie point.

We justify our tie point choices more clearly in the supplementary information of the main text. We also eliminated the tie point to dust completely and instead chose 2 new tie points from the d18Oice record so that readers clearly see which variability we are matching instead of potentially ambiguous variations in nssCa. We think matching directly to d18Oice instead of using d18Oice as justification for a possibly more ambiguous nssCa match makes a stronger case for the age model in this section of the core.

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

We restructured our discussion of analytical uncertainties such that they are discussed following the discussion of analytical methods. The total uncertainties in our age models are discussed after the age models are discussed.

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

We reorganized the text so that analytical methods and uncertainties come before the Age Models section. The age model section is divided into 3.1 Taylor Glacier MIS 5/4 cores, 3.2 Taylor Glacier -380 m Main Transect core, and 3.3 Taylor Dome so that each age model we developed is discussed thoroughly.

Metadata about the new Taylor Dome measurements were included in Table 1, and text was included in the analytical methods section about the methods used for Taylor Dome samples.

3- FIGURES

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

We split Figure 2 into Figures S1 and S2, which are now included in the supplementary information. The figures are larger so that the tie point picks are more clearly visible. We think this will make the picks more readily justified now that closer inspection is possible.

- Figures 3 and 4 should appear much bigger. Also, to facilitate the comparison of delta age evolutions between Taylor Glacier and Taylor Dome, I suggest to remove the panels b from each figure and combine these panels b into a single and additional figure. They can be presented in parallel, making sure that the scale used for the delta age evolution is the same for both sites.

We enlarged figures 3 and 4. We reorganized the panels so that the "b" panels are now plotted together in one separate figure for easier comparison (Figure 5 above).

4- STYLISTIC, TYPOGRAPHICAL COMMENTS AND MINOR COMMENTS

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

We included the Patriot Hills work in our list of blue ice areas.

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

We changed the sentence to read, "(2) the description of a new climatic record from Taylor Glacier across MIS 4."

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

We changed the sentence to read, "… another core was obtained directly…"

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

We listed the various cores and which measurements were made on which core (Table 1).

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

We reorganized the text so there is an Age Models section, which we renamed "Determination of Age Models."

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

We eliminated this text so the comment is no longer relevant.

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

We reworded how we assessed the uncertainty of the age models. We plotted the uncertainties along with the depth-age curves, and we provided the mean uncertainty along the cores.

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.

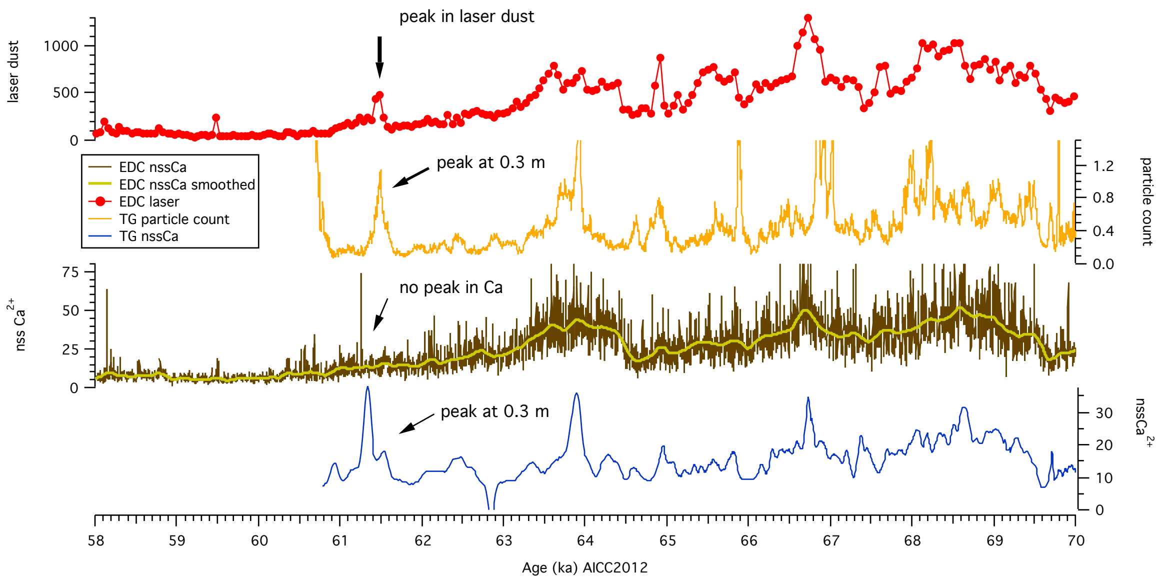
We thank Referee #3 for helpful comments.

As discussed in the response to other reviewers, we addressed the perceived ambiguity of tie point selection by justifying them with more extensive discussion in a supplement. We enlarged the figures so that it is easier to see why we chose to match variations the way we did. We made it more clear why alternative tie point selections produce poorer matches with EDC records by picking more tie points from other datasets (e.g., d18Oice). 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 discussed our justification for tying the 0.3 m dust peak, but we 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 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 are not be interpreted rigorously. This is 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 included 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 included more information in our discussion on the physical parameters controlling delta age and d15N.

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 added as a reference for relevant discussion.

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 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 added general direction of flow lines to the simplified map in Figure 1c.

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

The original chronology st9810 (Steig et al., 1998) was based on CH_4 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 (Barnola et al., 1991; Petit et al., 1999) 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 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 added 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 added references to lines 25-28 on page 3 for the vertical dip of layers on the Main Transect (Baggenstos et al., 2017; Bauska et al., 2016; Petrenko et al., 2017; Petrenko et al., 2016; Schilt et al., 2014).

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. This information is in the revised paper.

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 affect the depth summation along the core. We conservatively estimated the effect of these offsets on our age models and propagated them through the delta age calculations. This is discussed in the revised text in the age model uncertainties section.

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 CH_4 records depart from one another substantially above 4m with smaller differences between 4-5m depth, and the CO_2 appears to date too young relative to the composite data. But what Referee 3 says here is not entirely correct. The shallowest CO_2 measurement is at 4m depth, where the CH_4 differences are much smaller, and the CH_4 rise evident in both datasets (associated with DO17) is one of the most robust features. The discrepancy in the CO_2 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 CH_4 rise out such that TG CH_4 would lead EDML. The next tie point is at DO18. We did not choose other tie points from the CH_4 record because the CH_4 variability between DO18 and DO17 is minimal, thus any tie points chosen there would be ambiguous. We could choose tie points deliberately from the CO_2 record such that the slopes of the CO_2 increases are more similar, but we refrained from doing this given that CO_2 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 CO_2 offsets between the TG CO_2 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 CO_2 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 CH_4 record shows variability that looks very much like the CH_4 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 CH_4 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 and firn smoothing in the EDML record are the main reasons for the dissimilarity between the EDML and TG CH_4. We would prefer to plot the error bars on the EDML data, which visually help the readers see the smooth atmospheric signal, rather than smooth the data set directly. EDC CH_4 looks quite similar in resolution and smoothness to EDML. The relative amplitudes of abrupt CH_4 features can be an indication of relative smoothing. EDC, EDML, and TG all have the same magnitude CH_4 feature at DO 19. At DO 18 EDML CH_4 is higher, followed by EDC CH_4, followed by TG CH_4. The CH_4 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 CH_4 measured in the lab (green dots at DO19 in Fig 2) and CFA CH_4 (purple and red lines in Fig 2) agree well.

We included 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 justified the synchronization to NGRIP in the text.

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 provided 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.

We rewrote 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.

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

We edited 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 deemphasize the exact -380 m core dating, presenting our tie points as a plausible chronology. We explain 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 CO2 mismatch with "TG 5/4" not discussed, nor the d18Oatm mismatch with NGRIP at ~66 ka. The lack of information on flow line directions make the direct comparison between TG records difficult to understand, and few references are provided. I suggest to focus more this section on gas scales consistency between the "-380m" and "TG 5/4" cores, and how the CO2 mismatch between the two TG cores in the 60-64ka age range could be explained. Is the ice phase of the "-380m" ice core also dated? Are large _age values also inferred?

We addressed more completely the dating of the -380 m core in the text as well as include a table with tie points. We also moved the -380 m data to a separate figure where we compare the -380 m gas data with those from Taylor Glacier MIS 5/4 cores as well as the reference records on AICC 2012. The CO2 mismatch with TG MIS 5/4 was not discussed in the text, but we now address it explicitly in the age model section. The d18Oatm mismatch with NGRIP was also not discussed, but now it is with reference to offsets at DO 18.

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 deemphasize the exact dating of the -380 m gas age scale in the revised paper and instead 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, 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 clarify in the text that the “new” ice core is the TG MIS 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 moved the analytical uncertainty discussion to the Field Site and Analytical Methods section. We more thoroughly discussed the relevant uncertainties.

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 updated the text 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 CH4 record looks much smoother than the EDML record. It would thus be interesting to discuss the gas trapping duration consistent with the firn sinking speed due to the estimated accumulation rates (time needed by the firn to sink by a few meters).

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 more clearly demonstrate the analytical noise in EDML by plotting the error bars on Figures 2 and 4 (and S1 and S3).

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 provided more references to delta age studies, and we acknowledge the other processes influencing delta age as well as the fractionation of 15N in the firn column. We more clearly elucidated the correlation between large delta age and shallow firn with quantitative estimates of accumulation rate, close-off depth, and diffusive column height.

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 added 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 provided 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.

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 cleaned up the text with respect to repeated methodological information. We discussed Taylor Dome dating in more detail including rationale for the tie points we chose. We discussed 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 described 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 rewrote this section.

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)

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 rewrote this to seem less redundant.

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

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

We updated the reference to the published version of the manuscript.

p15 l49: suppress QUATERNARY

We changed 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 expanded Figure 2 by splitting it into Figures S1 and S2 so generally the tie points are 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.

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 added stronger justification for the tie points that we chose, including adding supplementary 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 strengthened our discussion of the low accumulation rate interpretation and included quantitative estimates. 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/ developing the discussion further, we (1) referenced other controls on delta age, (2) discussed more thoroughly/ quantitatively the controls on d15N including more detailed discussion of why d15N is low at Taylor Glacier, and (3) referenced the Megadunes, Antarctica site as a point of comparison to the Taylor Glacier accumulation zone, especially with respect to the influence of deep air convection on d15N. We reorganized 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 changed “A new ice core” to “New ice cores…” We changed “Dating the ice and air bubbles in the new ice core” 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 are cautious about 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 2.4 cm/ yr ice equiv.) (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 proceeded with caution to determine a conservative maximum accumulation rate for the Taylor Glacier accumulation zone given delta age = 10 ka as determined from our new ice core records. We used Herron and Langway to compute a matrix of density profiles for different temperatures. 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), which we use as an estimate of delta age. A contour plot of 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. We then computed the d15N due to gravitational enrichment for a matrix of diffusive zone heights using the barometric equation (Craig et al., 1988). Knowing the estimated close-off depth from the firn model allows us to estimate the height of the convective zone that must bring d15N into agreement with measured values. See supplementary for more details.

We opted not to include the accumulation estimates in the abstract because we don’t want to emphasize them as a robust result.

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 changed “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 replaced “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 removed the redundancy.

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 clarified where measurements were done with a table (Table 1). We rewrote this section so that the comment is not relevant anymore.

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 give a more complete explanation of the sampling and measurements by reporting a table (Table 1) 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 reorganized the paper so there is a core retrieval section that comes before analytical methods, which comes before the results. We think the paper is better organized now.

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)?

See previous comment about paper reorganization. Also see previous comments about Table 1. It is now clear which measurements were made on which cores.

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 now describe all measurements before we present the data. We included 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 MIS 5/4 BID cores. These factors likely cause the offsets seen in the top 4-5 m that referee 4 mentions here.

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 expanded the axes so that the shapes of the isotope curves are more visible to readers. We added supplementary 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.

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 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 expanded the original Figure 2 into Figures S1 and S2 so it is easier for reviewers to see the variability we are matching in the dust records. We also describe in the text what we consider a true feature in the dust versus noise. We also plotted 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 the 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.

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

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 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.

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 changed 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. We note deep TALDICE d18Oatm is unpublished.

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 CO2 offsets between different ice cores, described in the text.

We now 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 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.
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 also

a maximum and minimum delta age by 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 interpretations we are making 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 explain more clearly how we assigned uncertainty to each tie point, and we justify more clearly why we think the uncertainty is reasonable.

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 corrected the absolute uncertainty that we cite to 1 σ = 1500 years for the EDML gas age scale and 1 σ = 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 changed 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 think the reviewers will find our revised treatment of delta age more thorough.

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) now plotted in Figure S1 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/ EDML.

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 provided a quantitative estimate in the revised text.

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

We summarized the d15N controls more completely.

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 discuss the change in d15N but we did not try to pick apart the gravitational versus non-gravitational effect because we think the uncertainties in accumulation/ firn thickness/ convective zone height, etc. are too large to draw meaningful conclusions.

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

We added the 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 simply removed data that is < 0.

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

We changed the sentence to read "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 developed 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. 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 distinguished which controls we think are secondary (e.g. impurities, wind stress, thinning of firn) versus primary (e.g. temperature and 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 included our estimate of accumulation rate, and we clearly stated 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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SPATIAL PATTERN OF ACCUMULATION AT TAYLOR DOME DURING MARINE ISOTOPE STAGE 4: STRATIGRAPHIC CONSTRAINTS FROM TAYLOR GLACIER

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Abstract. New ice cores retrieved from the Taylor Glacier (Antarctica) blue ice area contain ice and air spanning the Marine Isotope Stage (MIS) 5/4 transition, a period of global cooling and ice sheet expansion. 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 an ice age-gas age difference (Δage) approaching 10 ka during MIS 4, implying very low snow accumulation in the Taylor Glacier accumulation zone. A revised chronology for the analogous section of the Taylor Dome ice core (84 to 55 ka), located to the south of the Taylor Glacier accumulation zone, shows that Δage did not exceed 3 ka. The difference in Δage between the two records during MIS 4 is similar magnitude but opposite direction of what is observed at the last glacial maximum. This relationship implies that a spatial gradient in snow accumulation existed across the Taylor Dome region during MIS 4 that was oriented in the opposite direction of the accumulation gradient during the last glacial maximum.

1 Introduction

Trapped air in ice cores provides a direct record of the Earth's past atmospheric composition (e.g., Bauska et al., 2016; Petrenko et al., 2017; Schilt et al., 2014). Measurements of trace gas species, and particularly their isotopic composition create a demand for large-volume glacial ice core samples. Blue ice areas, where

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a combination of glacier flow and high ablation rates bring old ice layers to the surface, offer relatively easy access to large samples and can supplement traditional ice cores (Bintanja, 1999; Sinisalo and Moore, 2010). Blue ice areas often have complex depth-age and distance-age relationships disrupted by folding and thinning of stratigraphic layers (e.g., Petrenko et al., 2006; Baggenstos et al., 2017). Taking full advantage of blue ice areas requires precise age control and critical examination of the glaciological context in which they form.

Effective techniques for dating ablation zone ice include matching of globally well-mixed atmospheric trace gas records (e.g., CH_4, CO_2, $\delta^{18}O_{atm}$, N_2O) and correlation of glaciochemical records (e.g., $\delta^{18}O_{ice}$, Ca^{2+}, insoluble particles) to existing ice core records with precise chronologies (Bauska et al., 2016; Schilt et al., 2014; Petrenko et al., 2008; Schaefer et al., 2009; Baggenstos et al., 2017; Petrenko et al., 2016; Aarons et al., 2017). Other useful techniques include $^{40}Ar_{atm}$ dating (Bender et al., 2008; Higgins et al., 2015), and radiometric ^{81}Kr dating (Buizert et al., 2014). Correlation of gas and glaciochemical records can provide high precision, requires relatively small samples, and some measurements can be made in field settings. $^{40}Ar_{atm}$ and ^{81}Kr require complex laboratory work and do not provide age precision available from correlation methods. These techniques do provide independent age information that can extend beyond the age range of existing records.

A number of blue ice areas have provided useful paleoclimate archives including Pakitsoq, Greenland for the Younger Dryas-Preboreal transition (Petrenko et al., 2006; Petrenko et al., 2009; Schaefer et al., 2009; Schaefer et al., 2006), Allan Hills, Victoria Land, Antarctica for ice 90-250 ka and > 1 Ma (Spaulding et al., 2013; Higgins et al., 2015), Mt. Moulton, Antarctica for the last interglacial (Korotkikh et al., 2011), the Patriot Hills, Horseshoe Valley, Antarctica, for ice from the last glacial termination (Fogwill et al., 2017), and Taylor Glacier, McMurdo Dry Valleys, Antarctica, for ice spanning the last glacial termination and MIS 3 (Bauska et al., 2016; Schilt et al., 2014; Baggenstos et al., 2017; Petrenko et al., 2017). Taylor Glacier is particularly well suited for paleoclimate reconstructions because of excellent preservation of near surface ice, large age span, and continuity of the record (Buizert et al., 2014; Baggenstos, 2015; Baggenstos et al., 2017). The proximity of the Taylor Dome ice core site to the probable deposition site for Taylor Glacier ice provides a useful point of comparison for the downstream blue ice area records (Figure 1).

This study extends the Taylor Glacier blue ice area archive by developing ice and gas chronologies spanning the MIS 5/4 transition (74-65 ka), a period of global cooling and ice sheet expansion. In 2014-2016 several ice cores were retrieved approximately 1 km down-glacier from the "Main Transect," the across-flow transect containing ice from Termination 1 through MIS 3 (Baggenstos et al., 2017) (Figure 1). This paper describes (1) dating the new ice cores via correlation of variations in CH_4, $\delta^{18}O_{atm}$, dust, and $\delta^{18}O_{ice}$ to preexisting records, and (2) the description of a new climate record from Taylor Glacier across MIS 4, which was previously thought to be absent from the glacier (Baggenstos et al., 2017). New

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measurements of CH_4 and CO_2 from the Taylor Dome ice core are used to revise the Taylor Dome chronology across the MIS 5/4 transition, and MIS 4 to allow better comparison of the glaciological conditions at Taylor Dome with those at the accumulation region for Taylor Glacier. This comparison allows inferences about the climate history of the Taylor Dome region implied from the differences in the delta age (Δage = ice age – gas age) between the two sites.

2 Field site and methods

2.1 Field site

Taylor Glacier is an outlet glacier of the East Antarctic Ice Sheet that flows from Taylor Dome and terminates in the McMurdo Dry Valleys (Figure 1). The Taylor Glacier deposition zone is on the northern flank of Taylor Dome, a peripheral ice dome of the East Antarctic Ice Sheet centered at 77.75 S, 159.00 E on the eastern margin of the Ross Sea (Figure 1). The Taylor Glacier deposition zone receives 3-5 cm ice equivalent accumulation annually in present-day climate conditions (Kavanaugh et al., 2009a; Morse et al., 1999). The glacier flows through Taylor Valley at a rate of ~ 10 m a^{-1} and terminates near Lake Bonney, approximately 30 km from the Ross Sea (Kavanaugh et al., 2009b; Aciego et al., 2007). The ablation zone extends approximately 80 km from the terminus (Kavanaugh et al., 2009b). The close proximity to McMurdo Station provides excellent logistical access to the site (e.g., Fountain et al., 2014; Petrenko et al., 2017; Baggenstos et al., 2017; Marchant et al., 1994; Aarons et al., 2017).

A combination of relatively high sublimation rates (~ 10 cm a^{-1}) and relatively slow flow creates an ablation zone where ancient ice with a large range of ages is exposed at the surface of Taylor Glacier (Kavanaugh et al., 2009a; Kavanaugh et al., 2009b). An along-flow transect of water stable isotopes from just below the equilibrium line to the terminus revealed ice from the last glacial period outcropping at sporadic places along the transect (Aciego et al., 2007). The sporadic nature of the outcrops was later shown to be an artifact of sampling nearly parallel to isochrones such that they were occasionally crossed (Baggenstos et al., 2017). More recent across-flow profiles dated with stratigraphic matching of well-mixed atmospheric gases revealed ice that varies continuously in age from the Holocene to ~ 50 ka (Schilt et al., 2014; Bauska et al., 2016; Baggenstos et al., 2017), with ice of last interglacial or older age found near the terminus of the glacier (Baggenstos et al., 2017; Buizert et al., 2014). The most heavily sampled archive is a 500 m section called the 'Main Transect,' oriented perpendicularly to isochrones (Figure 1) across a syncline-anticline pair containing ice spanning ~ 50 ka before present (BP) to the mid Holocene (7 ka) (Baggenstos et al., 2017). Ice stratigraphy in the Main Transect dips approximately vertically so that it is possible to obtain large quantities of ice of the same age by drilling vertical or near-vertical ice cores (e.g., Baggenstos et al., 2017; Petrenko et al., 2017; Petrenko et al., 2016; Schilt et al., 2014; Bauska et al., 2016; Bauska et al., 2018). Ice containing the full MIS 5/4 transition was formerly considered to be missing from the glacier (Baggenstos, 2015; Baggenstos et al., 2017), but we show here that a new ice core near the Main Transect contains an intact record with ice dating from 76.5-60.6 ka and air dating from 74.0-57.7 ka.

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2.2 Core retrieval

In the 2013-2014 season an exploratory core was drilled vertically using a "PICO" hand auger 380 m south ("-380 m" by convention) of a benchmark position (77.75891° S, 161.7178° E in Jan. 2014) along the Main Transect (Figure 1). In the 2014-2015 field season another exploratory core was drilled vertically using the "PICO" hand auger approximately 1 km down glacier from the Main Transect (77.7591° S, 161.7380° E in Dec. 2014) where older ice near the surface was suspected. This site is hereafter referred to as the MIS 5/4 site (Figure 1). An ice core was drilled directly adjacent to the PICO borehole at the MIS 5/4 site using the Blue Ice Drill (BID), a 24 cm diameter shallow coring device designed for retrieving large volume ice samples suitable for trace gas and isotope analysis (Kuhl et al., 2014). The section 9-17 m was sampled in the field for laboratory trace gas analyses at Oregon State University (OSU) and at the Scripps Institution of Oceanography (SIO).

In the 2015-2016 season a second large-volume core was drilled directly adjacent to the previous MIS 5/4 boreholes using the BID, and the sections 0-9 m and 17-19.8 m were sampled for trace gas analyses at OSU and at SIO. The entire 0-19.8 m of this core was sampled for continuous flow analysis (CFA) in the field and at the Desert Research Institute (DRI). Samples for all analyses were cut with a band saw on the glacier, stored in chest freezers at < -20° C in camp, and flown to McMurdo Station within 2 weeks of retrieval, where they were stored at < -20° C. Storage temperature remained at < -20° C for the remainder of their shipment to the USA and subsequent storage in laboratories.

2.3 Analytical methods

A temporary laboratory at the Taylor Glacier field camp permitted continuous measurements of CH_4 and particle count on ice core samples within days of drilling and recovery (Table 1). CH_4 concentration was measured using a Picarro laser spectrometer coupled to a continuous gas extraction line with a de-bubbler similar to that described in Rhodes et al. (2013). The continuous CH_4 data were calibrated by measuring standard air of known CH_4 concentration introduced into a stream of gas-free water to simulate a bubble/liquid mixture similar to the melt stream from ice core samples. The tests indicated 3.5-5.5% loss of CH_4 due to dissolution in the melt stream. We adjusted the continuous CH_4 data upwards by 5% to account for the solubility effect, which resulted in a good agreement between our measurements and other Antarctic CH_4 records (e.g., Schilt et al., 2010). Insoluble particle abundance was also measured continuously in the field using an Abakus particle counter coupled to the continuous melt-water stream. In order to obtain exploratory gas age information and verify the continuous CH_4 data, discrete ice core samples were also measured for CH_4 concentration in the field using a Shimadzu gas chromatograph coupled to a custom melt-refreeze extraction line, a manually operated version similar to the automated system used at OSU (Mitchell et al., 2011; Mitchell et al., 2013).

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Laboratory analyses on recovered samples and archived Taylor Dome samples included discrete CH_4 and CO_2 concentrations, $\delta^{15}N$ of atmospheric N_2, and $\delta^{18}O$ of atmospheric oxygen ($\delta^{18}O_{atm}$), continuous CH_4 concentration, $\delta^{18}O_{ice}$, major ion and elemental chemistry, and insoluble particle counts (Table 1). Continuous chemistry, dust, $\delta^{18}O_{ice}$, and CH_4 measurements were made at DRI by melting 3.5 cm x 3.5 cm x ~ 1 m longitudinal samples of ice and routing the melt stream to in-line instruments (McConnell, 2002; Maselli et al., 2013). Insoluble particles were measured using an Abakus particle counter, water isotopes using a Picarro laser spectrometer (Maselli et al., 2013), and CH_4 using a Picarro laser spectrometer and air extraction system similar to that used in the field (Rhodes et al., 2013). Continuous CH_4 data measured at DRI were calibrated with air standards as described above. The upward adjustment to account for dissolution in the melt stream was 8% in this case. Discrete CH_4 and CO_2 measurements were made at OSU. CH_4 was measured using an Agilent gas chromatograph equipped with a flame ionization detector coupled to a custom melt-refreeze extraction system (Mitchell et al., 2011). CO_2 was measured (1) on an Agilent gas chromatograph equipped with a Ni catalyst and a flame ionization detector coupled to a custom dry extraction "cheese grater" system for carbon isotopic analyses (Bauska et al., 2014), and (2) on a similar Agilent gas chromatograph coupled to a dry extraction needle crusher system (Ahn et al., 2009). $\delta^{15}N$-N_2 and $\delta^{18}O_{atm}$ were measured at SIO using a Thermo Delta V mass spectrometer coupled to a custom gas extraction system (Severinghaus et al., 1998; Petrenko et al., 2006).

Discrete measurements of CH_4 and CO_2 were made at OSU on archived Taylor Dome ice core samples following the same procedures described above (Table 1).

2.4 Data uncertainties

The analytical uncertainties associated with new data presented in this manuscript are reported in Table 1. In addition to the uncertainties in concentration and isotopic measurements, we address uncertainties related to: (1) smoothing of gas records due to dispersion and mixing in the CFA system (Rhodes et al., 2013; Stowasser et al., 2012), (2) depth uncertainty in gas and ice samples, and (3) artifacts due to contamination of gas and dust in near-surface ice. The effect of analytical smoothing is negligible, demonstrated by close agreement of continuous CH_4 with high-resolution discrete CH_4 data from 9-17 m in the 2014-2015 MIS 5/4 core (Figure S1). Depth uncertainties of up to 20 cm resulted from unaligned, angled core breaks of up to 10 cm in length as well as small depth logging errors. Contamination is only a concern in near-surface ice where thermal expansion and contraction causes abundant cracks on the surface of Taylor Glacier. The cracks rarely penetrate below 4 m and have never been observed deeper than 7 m (Baggenstos et al., 2017). Gas measurements may be sensitive to contamination from resealed cracks between 0-4 m depth, and dust measurements may be affected by local dust deposition between 0-40 cm depth (Baggenstos et al., 2017; Baggenstos et al., 2018). To minimize this problem we avoided analyses of ice with visible fractures.

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3 Age models for Taylor Glacier and Taylor Dome

3.1 Taylor Glacier MIS 5/4 cores

For the new MIS 5/4 cores the sections retrieved during the 2014-2015 season (9-17 m) and 2015-2016 season (0-9 m and 17-20 m) are hereafter treated as one ice core record (unified depth and age scales), which is justified given the close proximity of the boreholes (< 2 m spacing at surface) and the minimal depth uncertainty between the cores (≤ ~ 20 cm). The depth uncertainty is the maximum offset due to angle breaks at the ends of cores, which never exceeded 10 cm. Observable depth offsets between replicate measurements also do not exceed 20 cm (discussed in more detail below and in supplementary information). No depth adjustments were made to the raw data from any of the ice cores.

A gas age model for the Taylor Glacier MIS 5/4 cores was constructed by matching variations in CH_4 and $\delta^{18}O_{atm}$ to preexisting ice core records synchronized to the Antarctic Ice Core Chronology (AICC) 2012 (Veres et al., 2013; Bazin et al., 2013) (Figure S1). This approach is valid for the gas age scale because CH_4 and $^{18}O_{atm}$ are globally well mixed (Blunier et al., 2007; Blunier and Brook, 2001). Variations in CH_4 were tied to the EPICA Dronning Maud Land (EDML) record (Schilt et al., 2010), and $\delta^{18}O_{atm}$ was tied to the North Greenland Ice Coring Project (NGRIP) record (Landais et al., 2007). These datasets were chosen because they contain the highest-resolution CH_4 and $\delta^{18}O_{atm}$ data available on the AICC 2012 timescale for this time period. Tie points linking ages to depths were manually chosen (Figure S1 and Table 2). Ages between the tie points were interpolated linearly.

CO_2 data were not used to tie Taylor Glacier to AICC 2012. An offset between the Taylor Glacier data and the Antarctic composite record of Bereiter et al. (2015) during the MIS 4/3 CO_2 increase between 64 and 60 ka (Taylor Glacier lower by ~ 13 ppm at 61.5 ka, Figure 2) could bias our age model toward older ages. This offset may be real (e.g., Luthi et al., 2008), and we note that CO_2 offsets of even larger magnitude exist between Taylor Glacier and the composite record in the interval 68-64 ka (Figure 2).

Nonetheless, the general agreement with trends in preexisting CO_2 measurements supports the chosen tie points for the new gas age scale (Figure 2). The resemblance of the Taylor Glacier $\delta^{18}O_{atm}$ record to NGRIP $\delta^{18}O_{atm}$ between 72-63 ka also supports the gas age scale since tie points younger than 72 ka were picked only from CH_4 data. This is particularly important because CH_4 variability is small between 70-60 ka, limiting potential tie point selections. Good agreement between CH_4 variability in the new MIS 5/4 cores and the independently dated $\delta^{18}O$-$CaCO_3$ from Hulu Cave speleothems also suggests the gas age scale is accurate (Figure S5). Agreement between atmospheric CH_4 concentration (a global signal) and Hulu Cave speleothem $\delta^{18}O$-$CaCO_3$ is expected because both parameters are sensitive to shifts in the latitudinal position of the Intertropical Convergence Zone and the delivery of moisture via the tropical rain belts (Rhodes et al., 2015; Buizert et al., 2015).

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An ice age scale was constructed for the new Taylor Glacier MIS 5/4 cores by matching variations in Ca^{2+}, insoluble particle count, and $\delta^{18}O_{ice}$ to preexisting EPICA Dome C (EDC) dust (Lambert et al., 2008; Lambert et al., 2012) and $\delta^{18}O_{ice}$ records (Jouzel et al., 2007) synchronized to the AICC 2012 (Figure S2). This approach has been used successfully at Taylor Glacier before (e.g., Baggenstos et al., 2018), and it is possible because to first order the temporal patterns of dust content and $\delta^{18}O_{ice}$ in Antarctic ice are highly correlated at different ice core locations across the continent (Mulvaney et al., 2000; Schupbach et al., 2013). Tie points were chosen manually (Figure S2 and Table 3), and ages were interpolated linearly between them. The synchronized records are displayed in Figure 2. A more detailed discussion and justification of tie point choices for the Taylor Glacier MIS 5/4 chronologies is provided in the supplementary information.

3.2 Taylor Glacier -380 m Main Transect core

To investigate continuity between the Taylor Glacier Main Transect and the new MIS 5/4 site, we constructed a gas age scale for the ice core at -380 m on the Main Transect collected during the 2013-2014 season (Figure 3). Gas ages were determined by matching CH_4 data to EDML on AICC 2012 (Table 4). The chronology of the -380 m core is more uncertain than for the MIS 5/4 cores because there are fewer features to match in the gas records, but the synchronous variability in CH_4, CO_2, and $\delta^{18}O_{atm}$ is unique to the late MIS 4 and MIS 4/3 transition. The observation of late MIS 4 air (but not the full MIS 5/4 transition) was the basis for moving our 2014-2015 ice reconnaissance efforts down-glacier from the Main Transect where older ice is closer to the surface.

3.3 Taylor Dome core

The early Taylor Dome chronologies (e.g., Steig et al., 1998; Steig et al., 2000) were recently revised by Baggenstos et al. (2018) from 0-60 ka in light of evidence that the original timescales were incorrect (e.g., Mulvaney et al., 2000; Morse et al., 2007). To investigate the new Taylor Glacier MIS 5/4 climate archive in the context of the glaciological history of the Taylor Dome region, we revised the Taylor Dome gas and ice age scales for the period 84-55 ka (504-455 m). We adopted the recently published age ties (Baggenstos et al., 2018) for the interval that overlaps with our new records (60-55 ka). We then extended the timescale to 84 ka using new and preexisting data. Gas tie points were chosen by manually matching variations in Taylor Dome CH_4 data to EDML CH_4 on AICC 2012. One of the new tie points matches variability observed in a preexisting CH_4 record from Taylor Dome (Brook et al., 2000) to the EDML CH_4 record (supplementary information), and three tie points adopted from Baggenstos et al. (2018) match variations observed in preexisting Taylor Dome CO_2 data (Indermuhle et al., 2000) to WD2014 (Buizert et al., 2015) (Figure S3 and Table 5). Ice tie points were chosen by manually matching variations in the Taylor Dome Ca^{2+} record (i.e., Mayewski et al., 1996) to EDC dust (Lambert et al., 2012; Lambert et al., 2008) on AICC 2012 (Figure S4 and Table 6).

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The general agreement between the Taylor Dome CO_2 record and preexisting data from other ice cores supports our revised gas age scale (Figure 4), but we did not use the CO_2 data in constructing the age scale apart from the points mentioned above. The general resemblance between Taylor Dome $\delta^{18}O_{atm}$ and NGRIP $\delta^{18}O_{atm}$ also supports the gas age scale, although the Taylor Dome $\delta^{18}O_{atm}$ are somewhat scattered due to lower measurement precision (Sucher, 1997). Taylor Dome CH_4 data on the new timescale also agree well with $\delta^{18}O$-$CaCO_3$ variability in Hulu Cave speleothems (Figure S5). The supplementary information provides further justification of tie point choices for our revised Taylor Dome chronology.

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3.4 Age model uncertainties

There are two types of uncertainty associated with the new gas and ice age models: (1) absolute age uncertainty propagated from the reference age scale (AICC 2012), and (2) relative age uncertainty arising from depth offsets and the manual selection of tie points. The latter is a function of (a) choosing the correct features to tie, (b) the resolution of the data that define the tie point features, and (c) the measurement error. To estimate relative age uncertainty we assigned a maximum and minimum age to each chosen tie point (Figure 2, Figure 4, Tables 2-3, and Tables 5-6). The age ranges were determined by closely examining the matched features and estimating the maximum and minimum possible ages based on our judgment of factors (a)-(c) above. The resulting error ranges for our tie points are conservative. Maximum and minimum age scales were determined for the MIS 5/4 cores and the Taylor Dome ice core by interpolating linearly between the maximum and minimum age assigned to each tie point (Figures 5a and 5c).

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Depth errors contribute additional uncertainty to the total relative uncertainty described above. Depth errors between the Taylor Glacier MIS 5/4 cores were estimated by observing the depth offsets in features resolved by the continuous versus discrete CH_4 measurements (Figure S1). The largest depth offset was at the CH_4 rise at ~ 16.0 m: there is a 10 cm offset between the continuous field CH_4 and the discrete laboratory CH_4, and a 20 cm offset between the continuous and discrete laboratory CH_4. Approximately 20 cm offsets are also apparent in the ice phase by comparing insoluble particle count data measured in the field versus in the laboratory (Figure S2). 20 cm equates to 420 years on the new gas age scale where gas age changes most rapidly with depth (65-60 ka, Figure 5a) and 360 years on the ice age scale where ice age changes most rapidly with depth (70-61 ka, Figure 5a). We adopted 420 years and 360 years as conservative estimates of the relative gas age error and ice age error, respectively, due to depth uncertainty. These errors were propagated into the calculations of maximum and minimum Taylor Glacier age scales. We are unaware of depth uncertainties in the archived Taylor Dome samples used in this study so no additional depth uncertainty was added to the age error estimates for Taylor Dome.

The mean of the estimated age errors along the cores provides a reasonable cumulative estimate of the relative uncertainty in the new Taylor Glacier MIS 5/4 and revised Taylor Dome chronologies. For Taylor Glacier the mean relative uncertainty is ± 0.9 ka for the gas age and + 1.3 ka/ - 1.2 ka for the ice age. For

Taylor Dome the mean relative uncertainty is + 0.7 ka/ - 0.5 ka for the gas age and ± 0.6 ka for the ice age. The relative uncertainty is larger in Taylor Glacier due to the depth errors described above.

We did not explicitly account for errors associated with interpolation. Given our conservative estimates of tie point error we believe any additional uncertainty is minor relative to our conclusions. Tie points were not assigned to the end points of our records unless there was clearly a feature to match (with the exception of the last Taylor Glacier ice age tie point described in the supplementary information). Age models are extrapolated from the closest pair of tie points for the intervals 0-0.31 m for the ice age scale, and 0-1.74 m and 19.27-19.8 m for the gas age scale.

We suspect there are differences between Taylor Glacier and EDML due to gas transport in the firn layer, because the features resolved in the new Taylor Glacier CH_4 data are generally smoothed relative to the same features in EDML (Figures 2 and S1). However, we believe that the effect of firn smoothing on our tie point selections is within the estimated relative error for the chronology (Figure 5a). In contrast, CH_4 features in our Taylor Dome record appear less smoothed (Figures 4 and S3).

The absolute age uncertainty in the reference timescale (AICC 2012) is 2.5 ka for ice age and 1.5 ka for gas age (Veres et al., 2013). By nature, these errors are inherited by the Taylor Glacier 5/4 chronology and the revised Taylor Dome chronology, though the total error in our chronologies should 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. The close match of our gas age scales to the radiometrically dated Hulu Cave record indicates that the absolute age uncertainties in our gas age scales are equal to or lower than the AICC 2012 error estimates imply (Figure S5). We estimate an upper absolute age uncertainty of 1.5 ka for our Taylor Glacier and Taylor Dome gas age scales based on the phasing of features in the $\delta^{18}O$-$CaCO_3$ record from Hulu Cave and our CH_4 records.

4 Results

4.1 Data quality and initial observations

Preliminary observations of CH_4 variability in the MIS 5/4 PICO core revealed that the air likely contained the full MIS 5/4 transition and the MIS 4/3 transition (Figure S1). The new Taylor Glacier MIS 5/4 ice cores record the atmospheric history spanning 74-57.7 ka including the ~ 40 ppm CO_2 concentration decrease at the MIS 5/4 transition and the ~ 30 ppm CO_2 concentration increase near the MIS 4/3 transition (Figure 2). The new ice cores also record millennial scale variability in CH_4, CO_2, $\delta^{18}O_{atm}$, as well as $\delta^{18}O_{ice}$ and dust. Taylor Glacier $\delta^{18}O_{ice}$ is more variable than other Antarctic records, most likely recording local-scale changes in temperature and precipitation (Baggenstos, 2015; Baggenstos et al., 2018). We note that large features seen in other Antarctic stable isotope records are preserved (e.g. 2-3 ‰ changes at Antarctica Isotope Maximum (AIM) 19 and AIM 20).

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Field measurements (continuous CH_4 and insoluble particles) were replicated in the laboratory at DRI (Figures S1 and S2). Replication allowed assessment of data quality and supports the original data acquired in the 2014-2015 and 2015-2016 field seasons. Offsets between laboratory and field measurements are minor in the section 4-20 m and are due to the depth offsets described above (Figures S1 and S2). CH_4 offsets between field and DRI data in the section 0-4 m are much larger (Figure S1) and may be attributed to contamination of the gas signal due to resealed thermal cracks near the glacier surface (Baggenstos et al., 2017). We report these shallow CH_4 data for completeness. We assign two gas age tie points at 1.74 m (58.21 ka) and 3.15 m (59.10 ka) to offer a plausible gas age scale for the shallow ice, but the gas age scale for 0-4 m is not interpreted further and does not influence the conclusions of this study. CH_4 data from the section 0-1 m were excluded due to very high amounts of contamination in both laboratory and field samples (CH_4 > 1000 ppb). Continuous laboratory CH_4 data were also excluded between 14.57-15.0 m and 17.55-17.95 m due to technical problems with instrumentation. Variations in Ca^{2+} and insoluble particle counts generally agree with each other, suggesting both parameters are recorders of dust variability. Particle count data measured at DRI were averaged every 1 cm, explaining why the record appears less noisy than insoluble particles counts measured in the field (Figure S2).

CH_4 variations in Taylor Glacier are smoother than in EDML. The largest difference appears at DO 18 (64.9 ka) where Taylor Glacier CH_4 is ~ 40 ppb lower than EDML (and Taylor Glacier $\delta^{18}O_{atm}$ is ~ 0.1 ‰ more enriched than NGRIP) (Figure 2). The CH_4 rise associated with DO 19 is less attenuated, ~ 20 ppb lower in Taylor Glacier relative to EDML (72.3 ka, Figure 2). Some of these differences may be due to higher analytical noise in the EDML record (mean of EDML CH_4 1 σ = 10.25 ppb between 74-60 ka). New Taylor Dome CH_4 data from OSU show little or no attenuation relative to the EDML record. Taylor Dome CH_4 at the onset of DO 19 (72.3 ka) is 14 ppb higher than in EDML and 10 ppb lower at the onset of DO 20 (75.9 ka) (Figure 4). These offsets are within the combined 1 σ of the measurements. The smoothing in the three ice cores reflects the firn conditions in which bubble trapping occurred, with smoother variations resulting from a wider lock-in zone that traps bubbles with a larger age distribution. The new CH_4 data suggest Taylor Dome and EDML records are similarly smoothed by the firn while Taylor Glacier bubbles have a larger gas age distribution.

One clear observation from the new ice core is that the ice from MIS 4 is very thin at Taylor Glacier; indeed the entire MIS 4 period (70-60 ka) appears to be contained in ~ 6 m of ice (Figure 5a). This partially explains why the MIS 4 interval has been relatively difficult to locate. Thin ice could occur due to either low snow accumulation or mechanical thinning of ice layers due to glacier flow. The implications of thin layers for the accumulation history are discussed in more detail below. Taylor Dome, in contrast, does not show such a steep age-depth relationship (Figure 5c).

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Our new data also show that the ice at the MIS 5/4 site is stratigraphically linked to the Main Transect. The evidence for this is that the -380 m core contains air from late MIS 4 and the MIS 4/3 transition (Figure 3). The existence of MIS 4 ice on the Main Transect suggests continuity between the two archives, i.e. that both archives originated from the same accumulation zone. This is important because it means that it is possible to compare climate information from the new MIS 5/4 site to climate information from different intervals (e.g. the LGM) in ice from the Main Transect. More broadly speaking, it is important to note that geologic evidence from Taylor Valley suggests that Taylor Glacier has not changed dramatically in terms of its extent or its thickness in the last ~ 2.2 Ma, and that Taylor Dome has remained a peripheral dome of the East Antarctic Ice Sheet through the last ice age (Marchant et al., 1994; Brook et al., 1993). It is therefore unlikely that the location of the Taylor Glacier accumulation zone drastically changed during the intervals preserved in the Main Transect and the MIS 5/4 site (~ 77-7 ka).

A final observation is that the MIS 5/4 ice cores from Taylor Glacier have very low $\delta^{15}N$-N_2 (Figure 5b). The $\delta^{15}N$-N_2 enclosed in ice core air bubbles is controlled primarily by gravitational fractionation in the firn column (Sowers et al., 1992) (supplementary information). To first order the $\delta^{15}N$-N_2 records the height of the diffusive air column (Sowers et al., 1992), an estimate for total firn thickness. $\delta^{15}N$-N_2 is also influenced by convective mixing near the top of the firn (Kawamura et al., 2006; Severinghaus et al., 2010) and vertical gradients in firn temperature induced by rapid shifts in ambient temperature (Severinghaus et al., 1998). Low $\delta^{15}N$-N_2 (< 0.1 ‰) has been observed at Taylor Glacier (e.g., Main Transect position -125 m) and Taylor Dome (e.g., 380-390 m) previously and could result from thin firn and/or deep air convection (Baggenstos et al., 2018; Severinghaus et al., 2010; Sucher, 1997). The observation that $\delta^{15}N$-N_2 in the -380 m core is similarly low as $\delta^{15}N$-N_2 in the MIS 5/4 core supports our interpretation that the archives originated from the same deposition site (Figure 3).

4.2 Gas Age-Ice Age Difference (Δage)

Gas is trapped in air bubbles in firn at polar sites typically 50-120 m below the surface, thus ice core air is younger than the ice matrix that encloses it (Schwander and Stauffer, 1984). The magnitude of the difference between ice age and gas age (Δage) depends primarily on temperature and accumulation rate with accumulation having a stronger control (Herron and Langway, 1980; Parrenin et al., 2012; Capron et al., 2013). Δage ranges from 100-3000 years in polar ice cores under modern conditions (Schwander and Stauffer, 1984) with high accumulation sites having the smallest Δage (e.g., Buizert et al., 2015; Etheridge et al., 1996) due to fast advection of firn to the lock-in depth where gases no longer mix with the overlying pore space. Extrema in Δage up to 6500 years (Vostok) and 12,000 years (Taylor Dome) have been documented for cold, low accumulation sites at the last glacial maximum (e.g., Veres et al., 2013; Bender et al., 2006; Baggenstos et al., 2018) where slow grain metamorphism and slow advection of firn increase the lock-in time. Other important factors may include ice impurity content (Horhold et al., 2012; Freitag et al., 2013; Breant et al., 2017), surface wind stress, local summer insolation (Kawamura et al., 2007), and firn

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thinning. These factors are of secondary importance for polar ice cores compared to the effects of temperature and accumulation rate.

Δage was calculated for the new Taylor Glacier ice core by subtracting the gas age at a given depth from the independently determined ice age at the same depth (Δage = ice age – gas age). The Δage in the Taylor Glacier MIS 5/4 core approaches ~10 ka during late MIS 4 (Figure 5b), which exceeds Δage for typical modern polar ice core sites even where ice accumulates very slowly. This finding is unprecedented in ice from Taylor Glacier as Δage in ice from the Main Transect does not exceed ~ 3 ka between 10-50 ka (Baggenstos et al., 2018). Our large Δage values imply that accumulation in the Taylor Glacier accumulation zone decreased significantly through MIS 4, which could have been caused by low precipitation and/ or high wind scouring. This interpretation is supported by the following lines of evidence: (1) The depth-age relationship suggests the ice during MIS 4 is very thin (Figure 5a). This is in contrast to ice from the last glacial maximum, which is found at the surface of Taylor Glacier in two thicker (layer thickness = ~ 50 m) outcrops that dip approximately vertically and strike along the glacier longitudinally (Baggenstos et al., 2017; Aciego et al., 2007). Thin MIS 4 layers could be due to mechanical thinning of the ice rather than low accumulation rates. However, we note that ice thinning does not alter Δage because Δage is fixed at the bottom of the firn when the ice matrix encloses bubbles (Parrenin et al., 2012). This is unlike Δdepth, the depth difference between ice and gas of the same age, which evolves with thinning. So even if increased thinning caused the steep depth-age curve observed during MIS 4, one would still need to invoke an explanation for high Δage. (2) There is some degree of smoothing in the Taylor Glacier CH_4 data relative to EDML, which can result from longer gas trapping duration in firn where accumulation rates are relatively low (Kohler et al., 2011; Fourteau et al., 2017; Spahni et al., 2003). (3) As Δage increased at the onset of MIS 4, the $\delta^{15}N$-N_2 progressively decreased (Figure 5b), which is consistent with thinning of the firn column in response to decreased net accumulation. Inspection of Figure 5b reveals the change in $\delta^{15}N$-N_2 is not linear with Δage, potentially due to non-gravitational effects like thermal fractionation (Severinghaus et al., 1998) or kinetic fractionation related to convective mixing near the top of the firn (Kawamura et al., 2006).

In contrast to Taylor Glacier, Δage at Taylor Dome reaches a maximum of 3 ka at ~ 56 ka and does not rise above 2.5 ka throughout MIS 4 (Figure 5d). The implication of the relatively "normal" Δage is that net accumulation at Taylor Dome did not dramatically change throughout MIS 4 while Δage in the Taylor Glacier accumulation region did.

Δage uncertainty was determined by propagating the error reported for the age models described above (Figures 5a and 5c). The maximum and minimum Δage curves were calculated by subtracting the oldest gas age scale from the youngest ice age scale and vice versa. The mean Δage uncertainty is ± 2.2 ka for the Taylor Glacier MIS 5/4 cores and + 1.0 ka/ - 1.3 ka for the Taylor Dome core. The larger uncertainty for

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Taylor Glacier is due to the larger age uncertainties arising from the depth error. The uncertainties we estimate for Δage are of similar magnitude to the Δage uncertainty in other Taylor Glacier chronologies (Baggenstos et al., 2018).

4.3 Accumulation rate estimates

Given mean annual temperature and Δage, it is possible to use models of firn densification to estimate the accumulation rate at the Taylor Glacier accumulation zone. We used an empirical firn densification model (Herron and Langway, 1980) to compute firn density profiles for a range of temperatures and mean accumulation rates (supplementary information). Δage in the model is estimated by calculating the age of the firn when it has reached the close-off depth (when the density = 0.83 g cm^{-3}). The estimated accumulation rate comes from a simple lookup function that scans the full range of temperature and Δage and picks the corresponding accumulation rate (similar to work by Parrenin et al. (2012)). For Δage = 10 ka and temperature = -46 ºC the estimated accumulation rate for the Taylor Glacier MIS 5/4 cores is 1.9 mm yr^{-1} ice equivalent. The temperature -46 ºC is derived from the average $\delta^{18}O_{ice}$ for the period of firn densification (70-60 ka) using the relationship $\delta^{18}O_{ice}$ = 0.5 °C^{-1} calibrated using modern $\delta^{18}O_{ice}$ = -41 ‰ and modern temperature = -43 °C (Waddington and Morse, 1994; Steig et al., 2000), similar to Baggenstos et al. (2018). We used the average $\delta^{18}O_{ice}$ from the Taylor Dome record because it is less noisy and avoids the question of whether Taylor Glacier $\delta^{18}O_{ice}$ accurately records temperature (Baggenstos et al., 2018). Since the close-off depth is estimated from the firn density profile (30 m), it is possible to estimate the expected $\delta^{15}N\text{-}N_2$ assuming that the close-off depth is an approximation of the height of the diffusive air column (supplementary information). Assuming a 3 m lock-in zone height and a 0 m convective zone height, the predicted $\delta^{15}N\text{-}N_2$ (0.14 ‰) is enriched by a factor of 2 relative to measured values (~ 0.07 ‰ at 60 ka, Figure 5b). The difference in expected versus measured $\delta^{15}N\text{-}N_2$ may imply the influence of deep air convection in the Taylor Glacier firn column (Kawamura et al., 2006; Severinghaus et al., 2010). To bring the predicted $\delta^{15}N\text{-}N_2$ into closer agreement we introduced a convective zone height of 13.5 m (Figure S7). The apparent influence of air convection could be due to cracks that penetrate the surface of the firn (e.g., Severinghaus et al., 2010), which only occur in firn with a low mean accumulation rate.

A similar estimate was performed for the Taylor Dome core. Running the models with Δage = 2.3 ka (the Taylor Dome Δage at ~ 60 ka when Taylor Glacier Δage is maximum, Figure 5) and temperature = -46 ºC yields an estimated mean accumulation rate of 1.6 cm yr^{-1} ice equivalent, almost a factor of 10 larger than Taylor Glacier. The estimated diffusive column height (53 m) with a 3 m lock-in zone height and 0 m convective zone height predicts $\delta^{15}N\text{-}N_2$ of 0.26 ‰ (Figure S8), in somewhat better agreement with measured $\delta^{15}N\text{-}N_2$ (Figure 5d) implying less influence of deep air convection. The $\delta^{15}N\text{-}N_2$ data from Taylor Dome are lower resolution and less precise than the new Taylor Glacier data; in fact there is not actually a $\delta^{15}N\text{-}N_2$ measurement at 60 ka (Figure 5d). Still, we think the closer agreement between modeled

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$\delta^{15}N\text{-}N_2$ and the nearest measured $\delta^{15}N\text{-}N_2$ suggests a shallower convective zone, consistent with higher mean accumulation rate.

These accumulation rate and firn thickness calculations estimate how low the accumulation at Taylor Glacier may have been relative to Taylor Dome in late MIS 4. We caution that these estimates are uncertain given that we extrapolated below the empirical calibration range of the firn densification model (lowest accumulation = 2.4 cm yr^{-1} ice equivalent at Vostok) (Herron and Langway, 1980). We are unaware of firn densification models that are specifically tailored to very low accumulation sites. Another potential uncertainty in our estimates is that we did not account for geothermal heat transfer through the firn, which is relatively close to bedrock at Taylor Dome (depth to bedrock = ~ 550 m). The effect of excess geothermal heat would drive firn temperatures higher, decreasing Δage (Goujon et al., 2003). Higher firn temperatures could also cause lower $\delta^{15}N\text{-}N_2$, perhaps partially explaining low values of $\delta^{15}N\text{-}N_2$ observed at Taylor Glacier and Taylor Dome.

5 Discussion

Despite the model uncertainties we believe the simplest explanation of the Δage patterns described in the previous section is different accumulation rates in the Taylor Dome versus Taylor Glacier accumulation zones during MIS 4. Today the Taylor Glacier accumulation zone is on the northern flank of Taylor Dome, whereas the Taylor Dome ice core site is on the south flank (Figure 1). The difference between the estimated accumulation rate at Taylor Glacier versus Taylor Dome implies a gradient in precipitation and/or wind scouring between the two locations. This implication is perhaps not surprising given that a modern accumulation gradient is observed in the same direction, with accumulation decreasing from 14 cm yr^{-1} to 2 cm yr^{-1} going from south to north (Morse et al., 1999; Morse et al., 2007; Kavanaugh et al., 2009b). Moisture delivery to Taylor Dome primarily occurs during storms that penetrate the Transantarctic Mountains south of the Royal Society Range and reach Taylor Dome from the south (Morse et al., 1998), therefore the modern-day accumulation rate decreases orographically from south to north. The Taylor Glacier accumulation zone is effectively situated on the lee side of Taylor Dome with respect to the prevailing storm tracks (Morse et al., 1999) (Figure 1). The difference between Δage at Taylor Glacier versus Taylor Dome is too large to be explained by temperature contrasts between the two sites, which are on the order of 1-3 °C in present day (Waddington and Morse, 1994).

It is thought that the accumulation gradient across Taylor Dome (and hence between Taylor Dome and the Taylor Glacier accumulation zone) may have varied in the past. Morse et al. (1998) calculated the accumulation rate history for the Taylor Dome ice core site using modern accumulation data, a calculated ice flow field, and an age scale determined by correlation of isotope and chemical data to Vostok ice core records (Figure 6). By mapping the Taylor Dome age scale to ice layers resolved in radar stratigraphy, Morse et al. (1998) also inferred the accumulation rate history for a virtual ice core situated in the lee of the

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modern prevailing storm trajectory, ~7 km to the north of the Taylor Dome drill site and likely near the hypothesized Taylor Glacier accumulation zone (Figure 1). The accumulation histories inferred from the layer thicknesses revealed differences for the two sites, but not in the direction expected from the modern south-to-north storm trajectory. The last glacial maximum accumulation histories were characterized by extremely low accumulation at the Taylor Dome ice core site relative to higher accumulation at the northern virtual ice core site. The possibility that different layer thicknesses (and inferred accumulation histories) were a result of differential ice flow was rejected because deeper layers did not show the same effect (Morse et al., 1998). The reversed accumulation gradient inferred from ice layer thicknesses was qualitatively confirmed by independent Δage determinations on Taylor Glacier and Taylor Dome ice made by Baggenstos et al. (2018), which revealed that Taylor Glacier Δage = ~ 3000 years and Taylor Dome Δage = ~12,000 years at the last glacial maximum. Accumulation rate estimates from a firn densification model (Figure 6) confirmed that the orientation of the accumulation gradient was north-to-south, in the opposite direction of the gradient observed today (Figure 1).

Our new Δage data and accumulation rate estimates indicate an accumulation gradient in the same direction as the modern, but opposite the last glacial maximum. The accumulation rate estimates by Morse et al. (1998) qualitatively agree with this pattern > 60 ka (Figure 6). It is hypothesized that the reversed accumulation gradient at the last glacial maximum resulted from a shift in the trajectory of storm systems that delivered moisture to Taylor Dome, possibly in response to the extension of grounded ice far into the Ross Sea (Morse et al., 1998). If indeed the Antarctic ice sheet extended far enough into the Ross Sea to alter the atmospheric circulation during the last glacial maximum, the implication of our new data is that a similar situation did not exist during MIS 4. This hypothesis seems at odds with evidence that the Southern Hemisphere experienced full glacial conditions during MIS 4 (Schaefer et al., 2015; Barker and Diz, 2014). A possible explanation is that the sea level minimum at MIS 4 was 25 m higher than during the last glacial maximum due to the lack of extensive Northern Hemisphere ice sheets (Shakun et al., 2015; Siddall et al., 2003; Cutler et al., 2003), which limited how far grounded ice from the West Antarctic Ice Sheet could extend into the Ross Embayment. This suggestion is consistent with (1) data suggesting the maximum Ross Ice Shelf extent occurred during the last glacial termination (Hall et al., 2015; Denton and Hughes, 2000) rather than MIS 4 and (2) the notion that grounding line position in the Ross Sea is set by the balance between marine forcing (basal melting) and accumulation on the Antarctic ice sheets (Hall et al., 2015).

A second hypothesis arises from the notion that broad differences in regional atmospheric dynamics between MIS 4 and the last glacial maximum might occur without invoking changes in the extent of the Ross Ice Shelf as a mechanism for disrupting the atmospheric circulation. The Amundsen Sea Low, a low-pressure center that influences the Ross Sea and Amundsen Sea sectors of Antarctica, responds strongly to changes in tropical climate (Raphael et al., 2016; Turner et al., 2013) and exhibits cyclonic behavior that likely controls the path of storms that enter the Ross Embayment and reach Taylor Dome, as implied by

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Morse et al. (1998) and explored by Bertler et al., (2006). An intensified or shifted Amundsen Sea Low during MIS 4 relative to the last glacial maximum might result in strong meridional flow across Taylor Dome that maintained a south-to-north orographic precipitation gradient. Interestingly, variability in the Amundsen Sea Low has been linked to the extent of Northern Hemisphere ice sheets (Jones et al., 2018), which were smaller in extent at MIS 4 relative to the last glacial maximum.

6 Conclusions

We obtained the first ice core from the Taylor Glacier blue ice area that contains air with ages unambiguously spanning the MIS 5/4 transition and the MIS 4/3 transition (74.0-57.7 ka). The ice core also contains ice spanning the MIS 5/4 transition and MIS 4 (76.5-60.6 ka). The gas age-ice age difference (Δage) in the cores approaches 10,000 years during MIS 4 implying extremely arid conditions with very low net accumulation at the site of snow precipitation. To the south of the Taylor Glacier accumulation zone, the Taylor Dome ice core exhibits lower Δage (1000-2500 years) during the same time interval. This implies a steep accumulation rate gradient across the Taylor Dome region with precipitation decreasing toward the north and/or extreme wind scouring affecting the northern flank. The direction of the gradient suggests that the trajectory of storms was south-to-north during MIS 4 and that storm paths were not disrupted by Antarctic ice protruding into the Ross Sea or by changes in the strength and/or position of the Amundsen Sea Low, as occurred at the last glacial maximum.

Data will be made available through the US Antarctic Program Data Center and the National Center for Environmental Information.

Author contributions

JAM made measurements at OSU on Taylor Glacier samples, developed chronologies, and prepared the manuscript; JAM, EJB, and JRM made measurements in the field; TKB made measurements at OSU on the -380 m Taylor Glacier core; SB and SM made measurements at OSU on Taylor Dome samples; SAS made measurements on all new Taylor Glacier samples at SIO except the -380 m core, which were made by DB; JRM made measurements on Taylor Glacier samples at DRI; all authors provided valuable feedback and made helpful contributions to writing the manuscript.

The authors declare no conflicts of interest.

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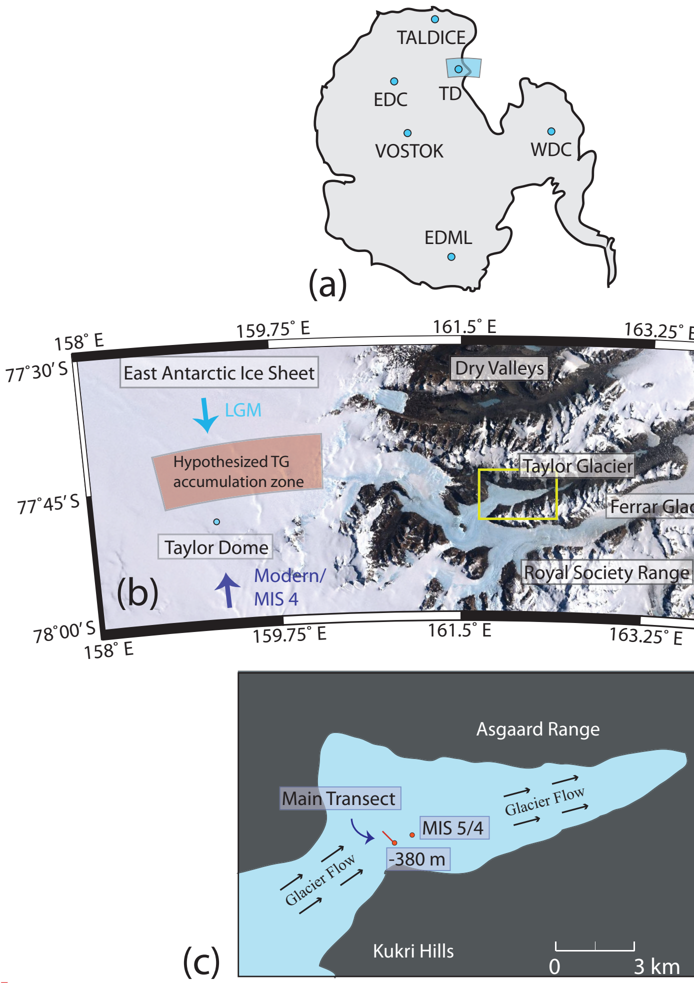
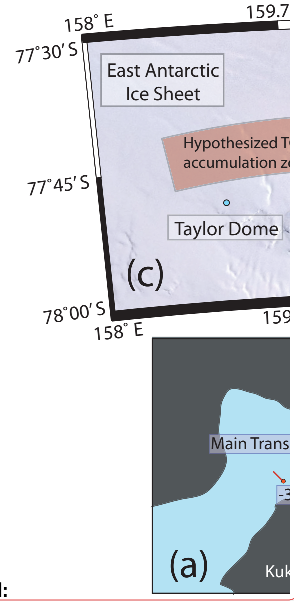


Figure 1 – (a) The locations of ice core sites discussed in this text are indicated with blue dots on the continent outline (EDC = EPICA Dome C, EDML = EPICA Dronning Maud Land, TALDICE = Talos Dome ice core, TD = Taylor Dome, WDC = West Antarctic Ice Sheet Divide core). (b) Landsat imagery of Taylor Valley (Bindschadler et al., 2008). Blue arrows conceptually show the modern storm trajectory as well as the hypothesized storm trajectories for the last glacial maximum (LGM) and the Marine Isotope Stage (MIS) 4 discussed later in the text. (c) Simplified map of Taylor Glacier showing main transect (red line) containing ice spanning the Holocene-MIS 3 time period and drill sites discussed in the text (red dots).



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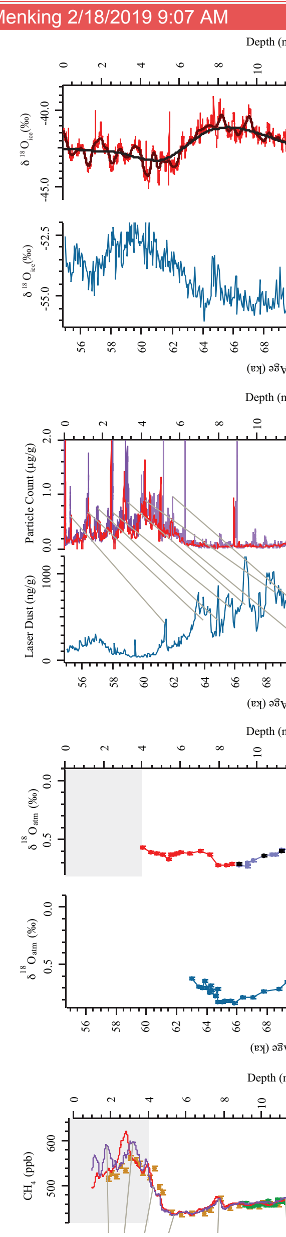

Table 1 – Summary of new datasets. Gas chromatograph (GC) and mass spectrometer (MS) measurements were made on discrete samples. Picarro, Abakus, and ICP-MS measurements were made by continuous-flow analysis. Analytical precision is from method reference or pooled standard deviation of replicate samples. OSU = Oregon State University, SIO = Scripps Institution of Oceanography, DRI = Desert Research Institute.

Dataset	Drill Site	Ice Drill	Season Extracted	Approx. Depth Range	Location Measured	Instrume-ntation*	Analytical Precision (1 σ)
CH_4	Taylor Dome	GISP2	1993-1994	455-505 m	OSU	GC[1]	3.5 ppb
CO_2	Taylor Dome	GISP2	1993-1994	455-505 m	OSU	GC[2]	1.5 ppm
CH_4	-380 m MT	PICO	2013-2014	4-15 m	OSU	GC[1]	3.5 ppb
CO_2	-380 m MT	PICO	2013-2014	4-15 m	OSU	GC[2]	1.5 ppm
$\delta^{18}O_{atm}$	-380 m MT	PICO	2013-2014	4-15 m	SIO	MS[3]	0.011 ‰
$\delta^{15}N$	-380 m MT	PICO	2013-2014	4-15 m	SIO	MS[3]	0.0028 ‰
CH_4	MIS 5/4	PICO	2014-2015	2-17 m	Field	GC[1]	10 ppb
CH_4	MIS 5/4	BID	2014-2015	9-17 m	OSU	GC[1]	3.5 ppb
CO_2	MIS 5/4	BID	2014-2015	9-17 m	OSU	GC[2]	1.5 ppm
CO_2	MIS 5/4	BID	2014-2015	9-17 m	OSU	MS[4]	1.5 ppm
$\delta^{18}O_{atm}$	MIS 5/4	BID	2014-2015	9-17 m	SIO	MS[3]	0.011 ‰
$\delta^{15}N$	MIS 5/4	BID	2014-2015	9-17 m	SIO	MS[3]	0.0028 ‰
CH_4	MIS 5/4	BID	2015-2016	0-20 m	Field	Picarro[5]	2.8 ppb
Insoluble Particles	MIS 5/4	BID	2015-2016	0-20 m	Field	Abakus[7]	
CO_2	MIS 5/4	BID	2015-2016	4-9 m, 17-20 m	OSU	MS[4]	1.5 ppm
$\delta^{18}O_{atm}$	MIS 5/4	BID	2015-2016	4-9 m, 17-20 m	SIO	MS[3]	0.011 ‰
$\delta^{15}N$	MIS 5/4	BID	2015-2016	4-9 m, 17-20 m	SIO	MS[3]	0.0028 ‰
CH_4	MIS 5/4	BID	2015-2016	0-20 m	DRI	Picarro[5]	2.8 ppb
$\delta^{18}O_{ice}$	MIS 5/4	BID	2015-2016	0-20 m	DRI	Picarro[6]	
Insoluble Particles	MIS 5/4	BID	2015-2016	0-20 m	DRI	Abakus[7]	
Ca^{2+}	MIS 5/4	BID	2015-2016	0-20 m	DRI	ICP-MS[7]	± 3 %

*Superscripts denote references for analytical procedures: [1] (Mitchell et al., 2013; Mitchell et al., 2011); [2] (Ahn et al., 2009); [3] (Severinghaus et al., 1998; Petrenko et al., 2006); [4] (Bauska et al., 2014); [5] (Rhodes et al., 2013); [6] (Maselli et al., 2013); [7] (McConnell, 2002).

Table 2 – Tie points relating Taylor Glacier depth to gas age on the AICC 2012 timescale. Gray shading indicates tie points < 4 m depth where abundant cracks in shallow ice may cause contamination of gas records (see text). "DO" refers to Dansgaard-Oeschger event.

Depth (m)	Gas Age (ka)		Age Range (ka)	Data	Data Source	Feature Description	Reference Record	Tie Point Source
1.74	58.21		57.30 - 59.00	CH_4	This study	Peak during DO 16/17	EDML CH_4	This study
3.15	59.10		58.21 - 59.60	CH_4	This study	Peak during DO 16/17	EDML CH_4	This study
4.19	59.66		59.60 - 59.70	CH_4	This study	Midpoint transition DO 16/17	EDML CH_4	This study
5.40	59.94		59.71 - 60.78	CH_4	This study	Low before DO 16/17	EDML CH_4	This study
7.79	64.90		64.30 - 65.40	CH_4	This study	Peak during DO 18	EDML CH_4	This study
11.24	69.92		69.00 - 70.36	CH_4	This study	Small peak between DO 19 and DO 18	EDML CH_4	This study
12.43	70.62		70.25 - 71.10	CH_4	This study	Low after DO 19	EDML CH_4	This study
13.25	71.21		70.94 - 71.42	CH_4	This study	High before transition late DO 19	EDML CH_4	This study
16.20	72.27		72.10 - 72.45	CH_4	This study	Midpoint transition DO 19	EDML CH_4	This study
17.40	72.70		72.20 - 73.30	$\delta^{18}O_{atm}$	This study	Midpoint transition	NGRIP $\delta^{18}O_{atm}$	This study
19.27	73.74		73.35 - 74.50	$\delta^{18}O_{atm}$	This study	Low before transition	NGRIP $\delta^{18}O_{atm}$	This study

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Table 3 - Tie points relating Taylor Glacier depth to ice age on the AICC 2012 timescale. Ice phase parameters (dust and $\delta^{18}O_{ice}$) are unaffected by surface cracks below 0.4 m depth. "AIM" refers to Antarctic Isotope Maximum event, and "MIS" refers to Marine Isotope Stage.

Depth (m)	Ice Age (ka)	Age Range (ka)	Data	Data Source	Feature Description	Reference Record	Tie Point Source
0.34	61.47	59.50 - 63.93	Insoluble particles	This study	Peak near end of MIS 4	EDC laser dust	This study
1.25	63.93	63.00 - 64.70	$nssCa^{2+}$	This study	Peak late MIS 4	EDC $nssCa^{2+}$	This study
1.80	64.91	64.00 - 65.65	Insoluble particles	This study	Peak late MIS 4	EDC laser dust	This study
2.45	65.65	65.00 - 66.30	Insoluble particles	This study	Peak mid MIS 4	EDC laser dust	This study
3.10	66.73	66.10 - 67.40	$nssCa^{2+}$	This study	Peak mid MIS 4	EDC $nssCa^{2+}$	This study
4.47	68.63	67.86 - 69.60	$nssCa^{2+}$	This study	Peak mid MIS 4	EDC $nssCa^{2+}$	This study
4.94	69.72	69.30 - 70.10	$nssCa^{2+}$	This study	Low early MIS 4	EDC $nssCa^{2+}$	This study
5.60	70.20	69.70 - 70.65	$nssCa^{2+}$	This study	Peak early MIS 4	EDC $nssCa^{2+}$	This study
7.75	71.95	71.00 - 73.00	$\delta^{18}O_{ice}$	This study	Peak AIM 19	EDC $\delta^{18}O_{ice}$	This study
12.20	73.62	73.00 - 74.50	$\delta^{18}O_{ice}$	This study	Low between AIM 19 and AIM 20	EDC $\delta^{18}O_{ice}$	This study
16.62	75.75	74.60 - 76.75	$\delta^{18}O_{ice}$	This study	Peak AIM 20	EDC $\delta^{18}O_{ice}$	This study
19.76	76.50	75.75 - 77.00	$nssCa^{2+}$	This study	End of record, loosely constrained	EDC $nssCa^{2+}$	This study

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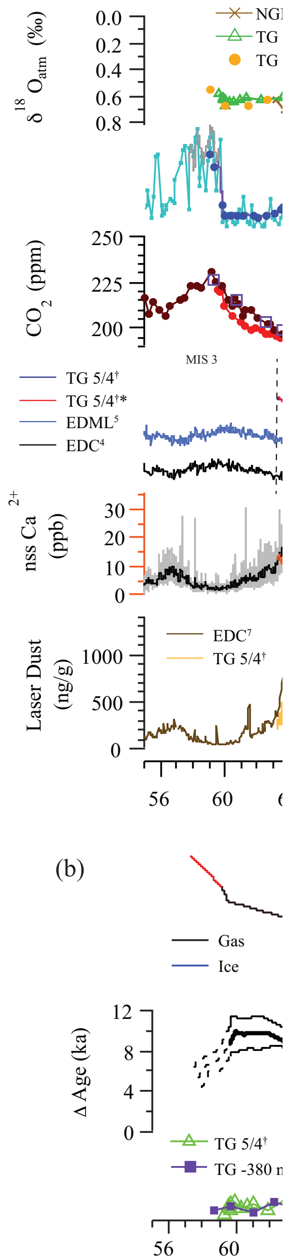

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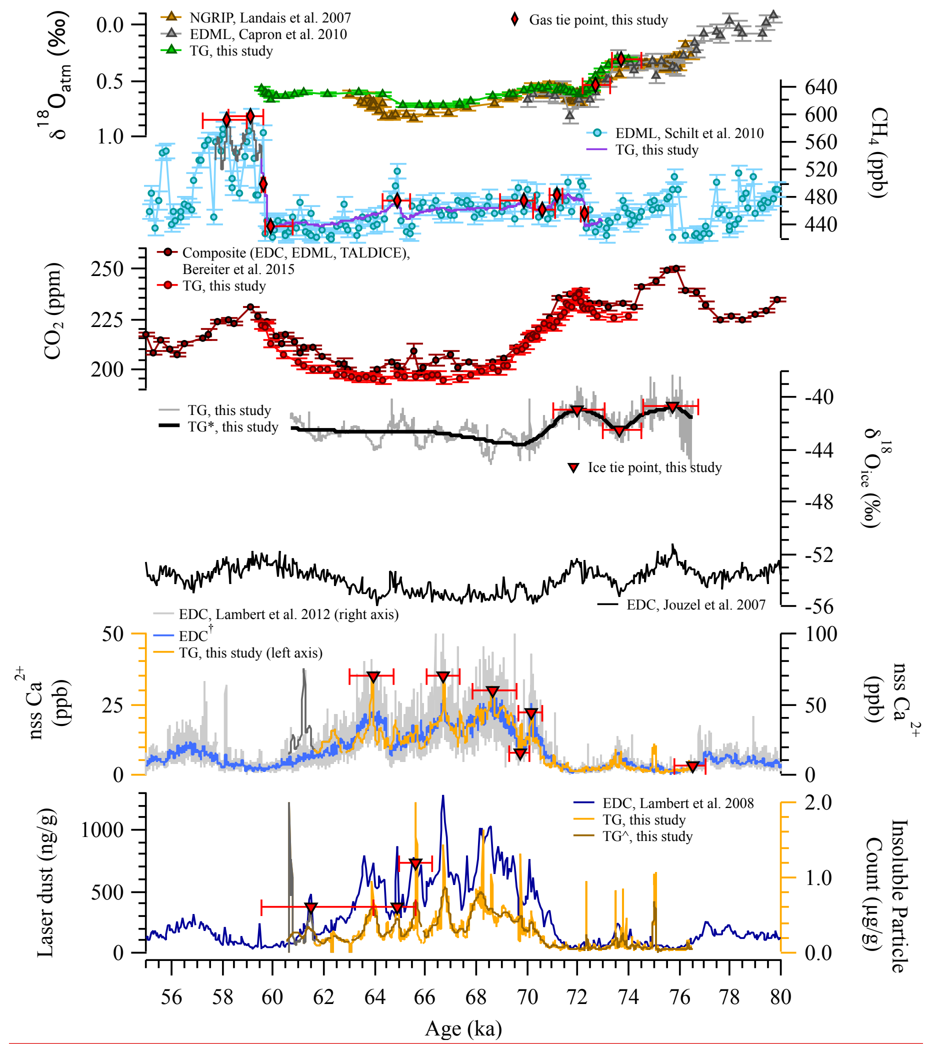

Figure 2 – Measurements of trace gases (CH$_4$ and CO$_2$), stable isotopes (ice and O$_2$), insoluble particles, and nss-Ca^{2+} from the Taylor Glacier ice core on new gas and ice age scales. All ice core data are synchronized to AICC 2012. CH$_4$ data from < 4 m depth and dust data from < 40 cm depth are colored dark gray to denote potential contamination by surface cracks. NGRIP = North Greenland Ice Coring Project, TG = Taylor Glacier MIS 5/4 BID cores, EDML = EPICA Dronning Maud Land, EDC = EPICA Dome C, TALDICE = Talos Dome. *, †, and ^ denote smoothing with 5000 point, 100 point, and 50 point LOESS algorithms, respectively.

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Table 4 – Tie points relating 380 m Main Transect core depth to gas age on the AICC 2012 timescale.

Depth (m)	Gas Age (ka)	Data	Data Source	Feature Description	Reference Record	Tie Point Source
3.751	59.53	CH_4	This study	High value at start of DO 16/17	EDML CH_4	This study
5.301	59.83	CH_4	This study	Low before DO 16/17	EDML CH_4	This study
9.929	64.40	CH_4	This study	Low after DO 18	EDML CH_4	This study
14.849	66.00	CH_4	This study	Low before DO 18	EDML CH_4	This study

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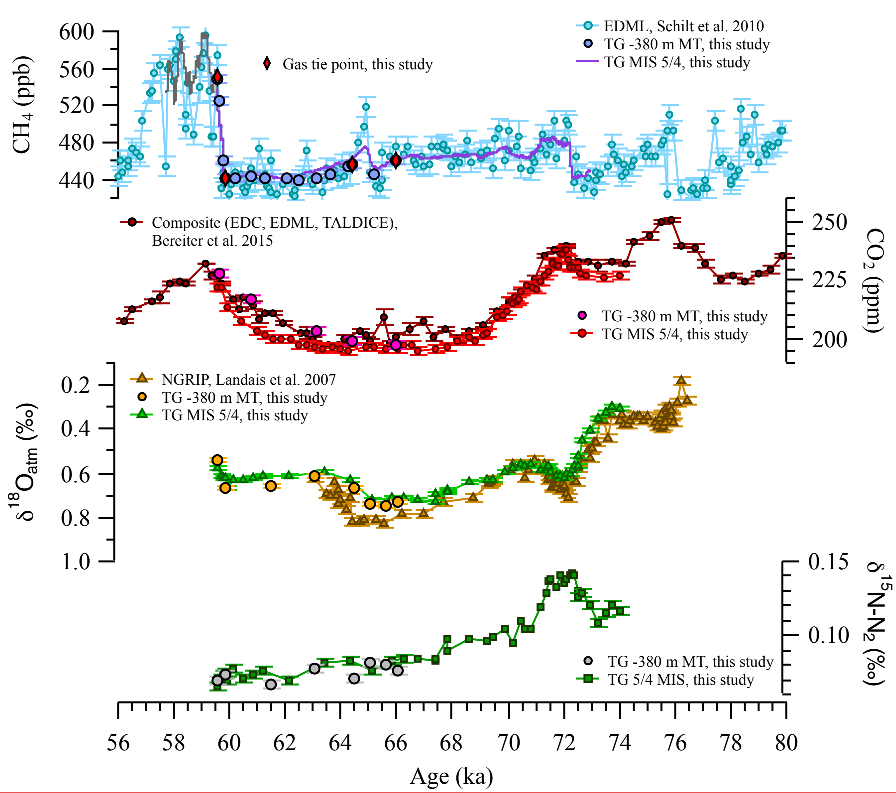

Figure 3 - Measurements of trace gases (CH_4 and CO_2), and stable isotopes (O_2 and N_2) from the -380 m Main Transect Taylor Glacier ice core and MIS 5/4 ice cores on new gas age scales. All ice core data are synchronized to AICC 2012. CH_4 data from < 4 m depth are colored gray to denote potential contamination by surface cracks. NGRIP = North Greenland Ice Coring Project, TG = Taylor Glacier, EDML = EPICA Dronning Maud Land, EDC = EPICA Dome C, TALDICE = Talos Dome.

Table 5 – Tie points relating Taylor Dome depth to gas age on the AICC 2012 timescale.

Depth (m)	Gas Age (ka)		Age Range (ka)	Data	Data Source	Feature Description	Reference Record	Tie Point Source
455.95	54.667		54.167 - 55.167	CO_2	Indermühle et al. 2000	Midpoint transition A3	WAIS CO_2	Baggenstos et al. 2018
460.90	57.913		57.413 - 58.413	CO_2	Indermühle et al. 2000	Midpoint transition A4	WAIS CO_2	Baggenstos et al. 2018
464.62	59.99		59.70-60.50	CH_4	Brook et al. 2000	Low before DO 16/17	EDML CH_4	This study
467.10	62.303		61.803 - 62.803	CO_2	Indermühle et al. 2000	Midpoint transition A4	WAIS CO_2	Baggenstos et al. 2018
474.95	65.50		65.00-66.80	CH_4	This study	Low before DO 18	EDML CH_4	This study
483.10	70.40		69.70-71.20	CH_4	This study	Low CH_4 after DO 19	EDML CH_4	This study
486.95	72.27		72.00-72.70	CH_4	This study	Midpoint transition DO 19	EDML CH_4	This study
493.50	76.05		75.75-76.30	CH_4	This study	Midpoint transition DO 20	EDML CH_4	This study
503.90	83.90		83.65-84.10	CH_4	This study	High at DO 21 onset	EDML CH_4	This study

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Table 6 - Tie points relating Taylor Dome depth to ice age on the AICC 2012 timescale.

Depth (m)	Ice Age (ka)	Age Range (ka)	Data	Data Source	Feature Description	Reference Record	Tie Point Source
455.10	55.80	54.25-57.00	Ca^{2+}	Mayewski et al. 1996	See original work	WAIS Ca^{2+}	Baggenstos et al. 2018
457.60	58.85	57.50-60.10	Ca^{2+}	Mayewski et al. 1996	See original work	WAIS Ca^{2+}	Baggenstos et al. 2018
463.30	61.47	61.00-62.00	Ca^{2+}	Mayewski et al. 1996	Peak late MIS 4	EDC laser dust	This study
466.40	63.50	62.80-63.75	Ca^{2+}	Mayewski et al. 1996	See original work	WAIS Ca^{2+}	Baggenstos et al. 2018
467.80	64.30	63.90-64.80	Ca^{2+}	Mayewski et al. 1996	See original work	WAIS Ca^{2+}	Baggenstos et al. 2018
468.10	64.66	64.20-65.40	Ca^{2+}	Mayewski et al. 1996	Low late MIS 4	EDC $nssCa^{2+}$	This study
471.37	65.57	65.00-66.10	Ca^{2+}	Mayewski et al. 1996	Peak mid MIS 4	EDC laser dust	This study
472.70	66.71	66.00-67.25	Ca^{2+}	Mayewski et al. 1996	Peak mid MIS 4	EDC $nssCa^{2+}$	This study
475.12	67.47	67.00-68.00	Ca^{2+}	Mayewski et al. 1996	Low mid MIS 4	EDC $nssCa^{2+}$	This study
476.90	68.63	67.75-69.40	Ca^{2+}	Mayewski et al. 1996	Peak early MIS 4	EDC $nssCa^{2+}$	This study
478.70	69.70	69.25-70.10	Ca^{2+}	Mayewski et al. 1996	Low early MIS 4	EDC $nssCa^{2+}$	This study
479.90	70.15	69.70-70.60	Ca^{2+}	Mayewski et al. 1996	Peak early MIS 4	EDC $nssCa^{2+}$	This study
484.30	71.95	71.60-72.30	$\delta^{18}O_{ice}$	Steig et al. 1998	Peak AIM 19	EDC $\delta^{18}O_{ice}$	This study
487.40	73.62	73.30-74.00	$\delta^{18}O_{ice}$	Steig et al. 1998	Low between AIM 19 and AIM 20	EDC $\delta^{18}O_{ice}$	This study
490.80	75.75	75.00-76.10	$\delta^{18}O_{ice}$	Steig et al. 1998	Peak AIM 20	EDC $\delta^{18}O_{ice}$	This study
493.40	77.08	76.65-77.50	$\delta^{18}O_{ice}$	Steig et al. 1998	Low before AIM 20	EDC $\delta^{18}O_{ice}$	This study
502.75	83.9	83.00-84.90	$\delta^{18}O_{ice}$	Steig et al. 1998	Peak AIM 21	EDC $\delta^{18}O_{ice}$	This study

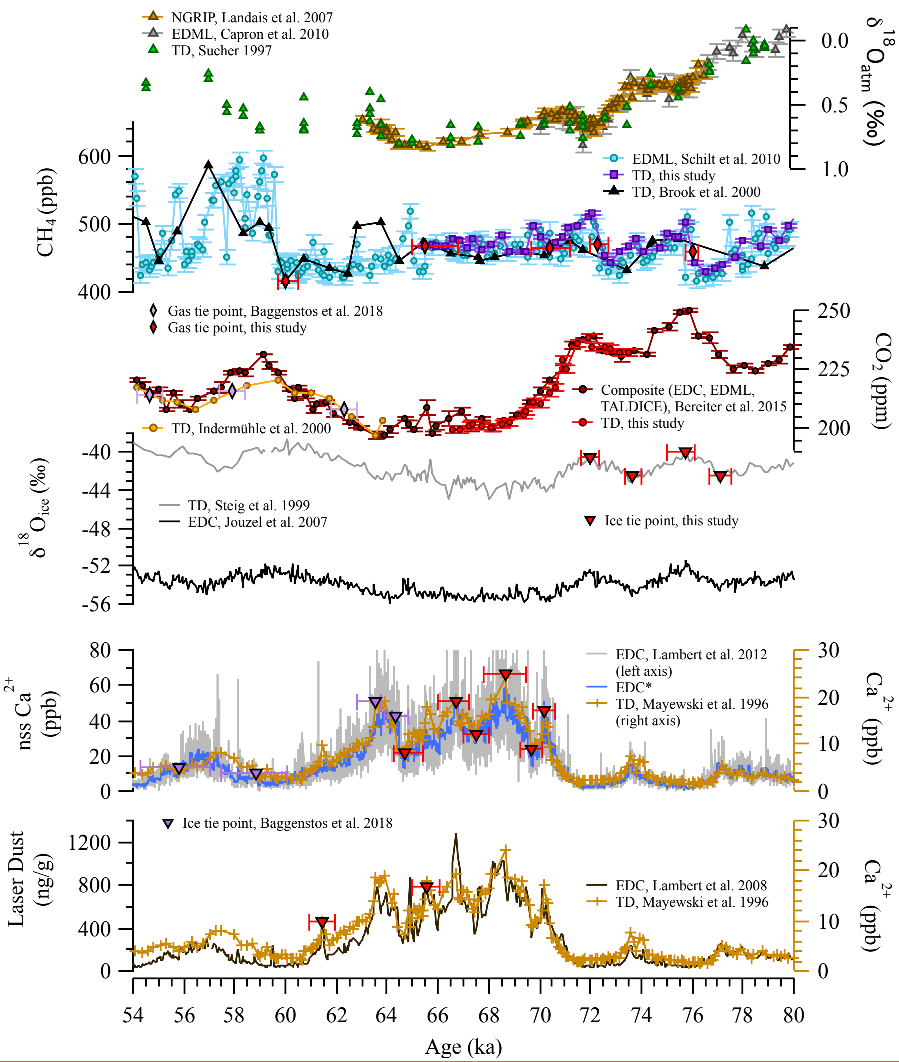

Figure 4 - Measurements of trace gases (CH_4 and CO_2), stable isotopes (ice and O_2), and Ca^{2+} from the Taylor Dome ice core on new gas and ice age scales. All ice core data are synchronized to AICC 2012. NGRIP = North Greenland Ice Coring Project, TD = Taylor Dome, EDML = EPICA Dronning Maud Land, EDC = EPICA Dome C, TALDICE = Talos Dome. * denotes smoothing with 100 point LOESS algorithm.

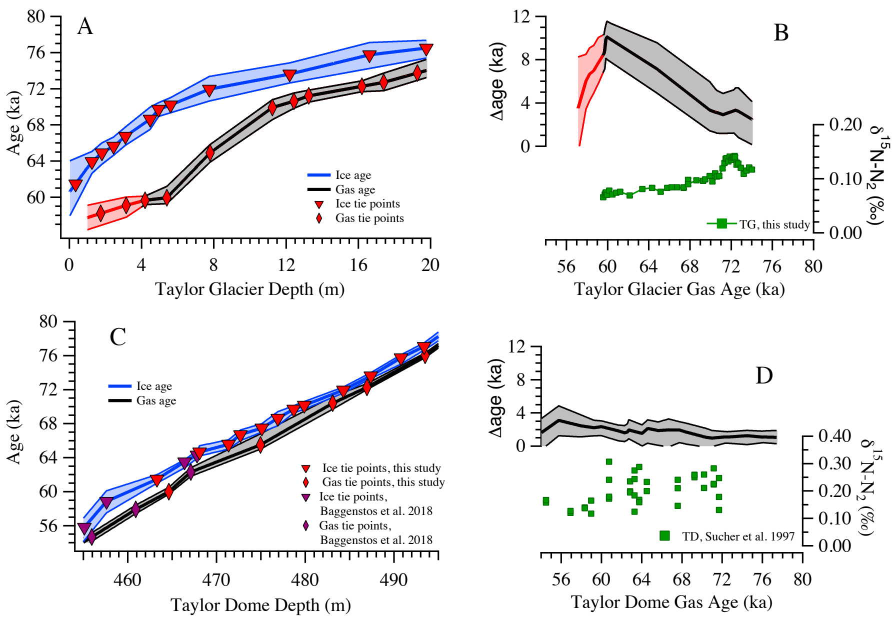

Figure 5 – (A) New Taylor Glacier MIS 5/4 gas and ice age models, and (B) Taylor Glacier Δage and $\delta^{15}N$-N_2. Where age data and Δage are plotted in red denote that gas data are from the top 4 m where contamination from surface cracks is possible. (C) Revised Taylor Dome gas and ice age models, and (D) Taylor Dome Δage and $\delta^{15}N$-N_2.

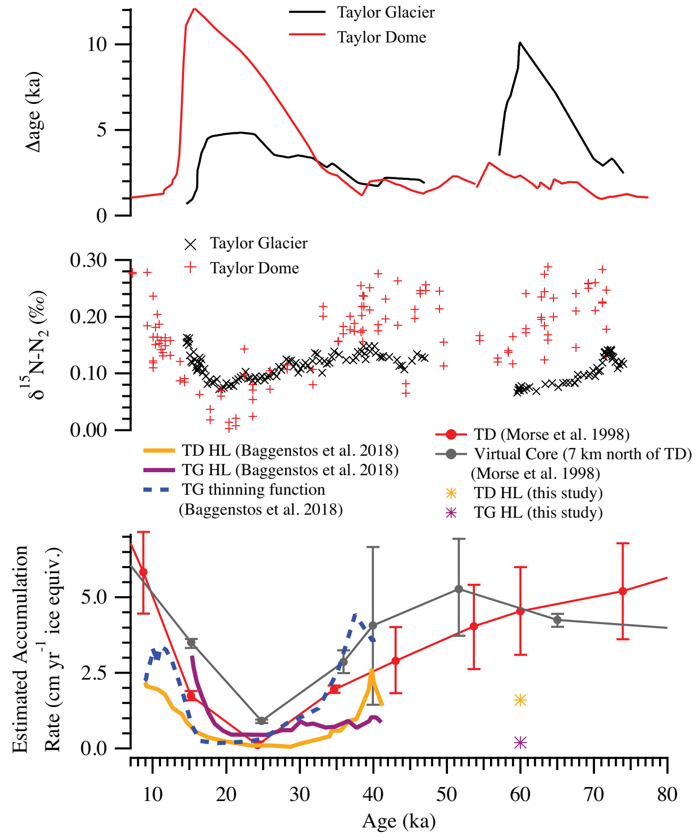

Figure 6 – Δage, $\delta^{15}N$-N_2, and estimated accumulation rate for Taylor Glacier and Taylor Dome from 75-7 ka. Δage and $\delta^{15}N$-N_2 data between 55-7 ka are from Baggenstos et al. (2018) and 80-55 ka are from this study, except all Taylor Dome $\delta^{15}N$-N_2 are from Sucher (1997). TD = Taylor Dome, TG = Taylor Glacier, HL = Herron and Langway (1980).