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 thank referee #1 for helpful comments. We will reorganize the text of the paper so that there is an "Age Models" section preceding the "Results" section, and therein the choice of tie points and chronological uncertainty will be discussed with greater justification for our tie point selections. We will present all metadata concerning the measurements in a table so it is clear which measurements were made on which cores, at which institutions, field versus lab, etc. Please find responses to specific comments below.

Specific comments & Technical corrections:

ABSTRACT:

- line 24: "Taylor Glacier (Antarctica)"

We will add "(Antarctica)" to line 24.

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

We will add "snow" to line 27 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 will change "Taylor Dome" in line 31 to "this area."

INTRODUCTION:

Page 1:

-line 36: missing references for past atmospheric composition and a list of trace gases

We will add references for "paleoarchive of the Earth's past atmospheric composition" on line 36 including (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 will change "age of ice and air bubbles always increases with depth" on line 40 to "age of ice and air bubbles always increases with depth except at or near the ice/bedrock interface."

-line 41 "precise distance-age"-> from which reference is the distance measured?

The reference to "distance" on line 41 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 will indicate on page 1 line 41 that this paper provides these details.

Page 2:
-line 12: remove "with fast access to age information"
We will remove "with fast access to age information" on line 12.

-line 13: as precise as what? The previous method? Replace "have" by "present"
We will add "as precise as the aforementioned methods" on line 13. We will change "have" to "present" on line 13.

-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 will replace "expands" with "extends" on line 29. We would 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 would 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 will change line 34-35 to read "the description of a new MIS 4 paleoarchive from Taylor Glacier, where it was previously thought to be absent." We will remove "larger context of" in line 36. We will replace "into" with "within" in line 35. We will replace "Taylor Dome" with "of this region" in line 36.

FIELD SITE AND ANALYTICAL METHODS:

Page 3:
-line 5: if you are not citing an acronym, ice sheet is written without capital letters
We will change "Ice Sheet" to "ice sheet" in line 5.

-line 6: "northERN"
We will change "north" to "northern" on line 6.

-line 7: "ice EQUIVALENT accumulation"
We will change "ice accumulation" to "ice equivalent accumulation" on line 6.

-line 15: "80 km LONG ablation zone", and you already said it in the previous paragraph
We will remove "~ 80 km" from line 15.

-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 will change line 16 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). They are found in lines 21-22.

-line 22: "the most COMMONLY USED archive" instead of utilized
We will change "utilized" to "commonly used" on line 22.

-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 will clarify in the text that we are not referring to a specific study, and we will reference relevant discussion in (Baggenstos, 2015).

-lines 30-33: need a reference
It is unclear what the referee wants referenced. If it is the CH_4 variability at DO16/17 then we will add an appropriate reference (Rhodes et al., 2015; Schilt et al., 2010). If it is 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"…"CH_4 variations similar to those ASSOCIATED WITH DO19" or "corresponding to"
We will replace "in" by "of" on line 36. We will replace "similar to those at Dansgaard-Oeschger event 19" to "similar to those associated with Dansgaard-Oeschger event 19" on line 36-37.

-line 37: "CH_4 CONCENTRATION increase"
We will add "concentration" on line 37.

Page 4:
-line 3: replace "work" by "analysis", replace "spanning" by "section"
We will replace "work" with "analysis" on line 3. However we don't understand the meaning of the sentence if we replace "spanning" with "section" on line 3, so we prefer not to make that change.

-line 4: need rewording, proposition: "…CH_4 and CO_2 concentrations, which confirmed the MIS 4/5 transition record in the gas phase"
We will change line 4 to read, "sampled for laboratory analyses of CH_4 and CO_2 concentrations, which confirmed the MIS 5/4 transition record in the gas phase."

-line 6: spanning not properly used
We will state 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 will make a table that summarizes the metadata for all analyses discussed in the manuscript including where the samples were taken, which coring device was used (BID or PICO), in which laboratory and what type of measurements were made, when the measurements were made, and the analytical uncertainty of each measurement.

-line 24: "resulted in a good agreement of our measurements with other…"
We will change line 24 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 will change line 19 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 will add "of ice" after "~ 10 g" to line 22.

Page 6:
-line 10: 'CO2 CONCENTRATION decrease", again later
We will change line 10 to read, "CO2 concentration decrease" and "CO2 concentration increase"

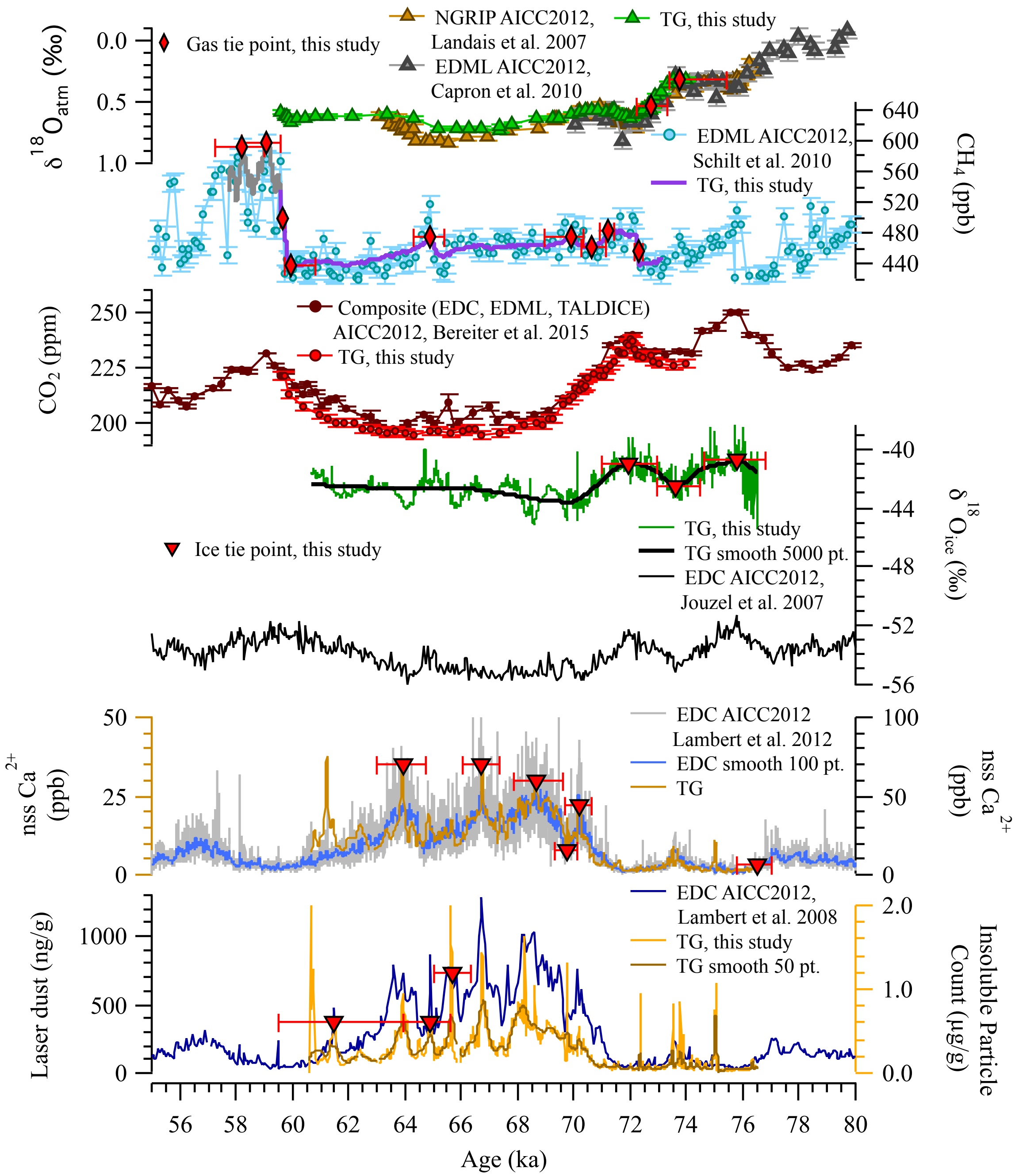
-line 11: remove "and"
We will remove "and."

-line 13: value of the offset?
We will state the value of the offset on line 13. 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 will reword line 15 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. TALDICE d18Oatm is unpublished, as far as we know, 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 (black x' in figure below). (Capron et al., 2010; Landais et al., 2007)

Revised Figure 3:



-line 23: replace "has" by presents"

We will change "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 will reword 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 will de-emphasize the 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 5/4 BID data. We will 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 will emphasize the usefulness of the paragraph at the beginning.

3.2 ANALYTICAL AND AGE MODEL UNCERTAINTIES

Page 7:
-line 18: “is likely”
We will change line 18 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 prefer not to assign larger uncertainty in this section because we think we matched the correct peaks in CH4. Instead we would prefer to emphasize how we do not interpret the top 4 m rigorously. We will explain and rationalize in the text more clearly what we chose to do. We only present 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. We will follow the editor's guidance on this issue if needed.

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

We will reorganize 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 will provide stronger justification for the tie point choices we prefer. Other reviewers also asked for information like this.

Specifically we will make Figure 2, Figure 3, and Figure 4 clearer so that readers can see easily why we picked certain tie points, and we will rationalize our choices thoroughly in the text.

We have 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 CH_4, and a 20 cm offset between the continuous laboratory CH_4 and the discrete laboratory CH_4. 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. We will change "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 CH_4 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 CH_4 and CO_2 concentration are similar in TG and EDML. It appears that the CH_4 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 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 CH_4 increases to ~ 515 ppb while TG only reaches ~ 475 ppb. Of course the peak CH_4 at DO 18 is only defined by one data point at EDML, but if we assume it is correct then the maximum CH_4 concentration recorded during DO18 at TG is 40 ppb lower than that at EDML. We also note the differences between $d18O_{atm}$ 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 will change 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?
Because there is very little analytical noise, < 0.5 ppb. We will elaborate in the text and provide a value.

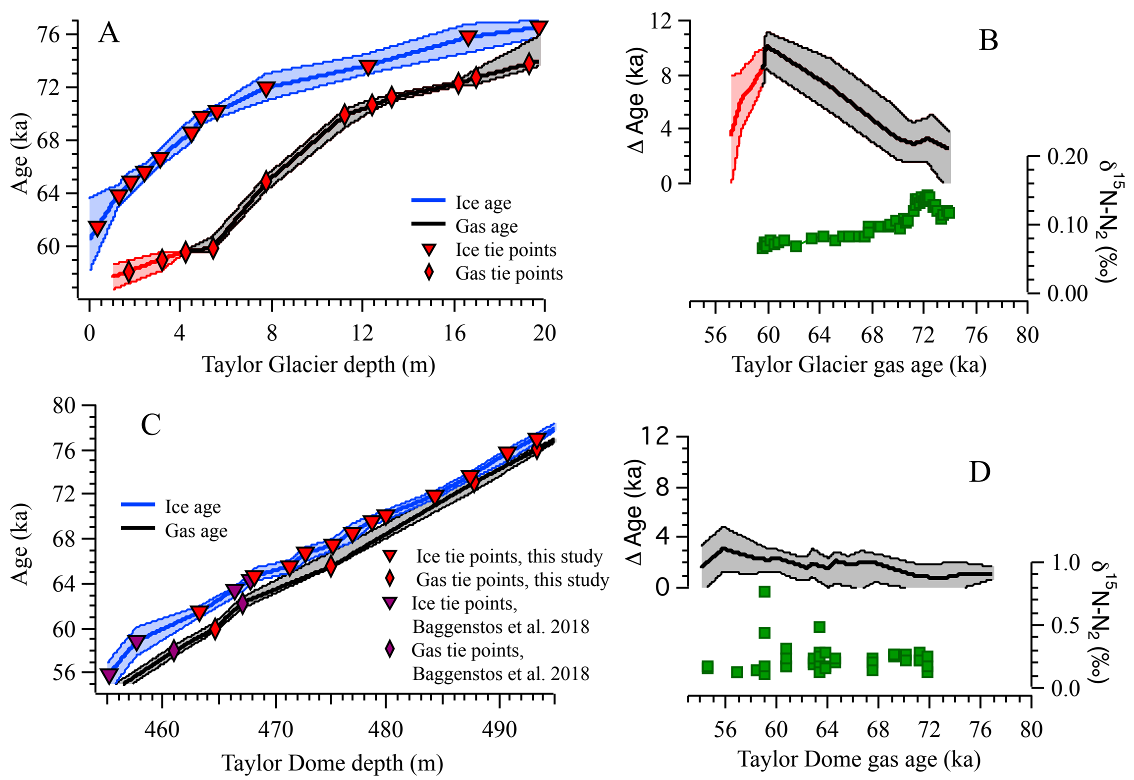
-lines 9-12: Please, when using a chronology as reference, make sure of the uncertainty values you cite… What you wrote is not correct. The AICC2012 chronology uncertainty over your period of interest (i.e. ~65-74 ka) at EDML is ranging between 1500 years and 1400 years (cf. supplementary material of 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 will correct the uncertainty we cite 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 will present 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 are explicitly displaying 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).

Figure 5 – Age models for new Taylor Glacier 5/4 BID cores (A), Taylor Glacier delta age and $\delta^{15}N$-N_2 (B), and Taylor Dome revised age models (C), and Taylor Dome delta age and $\delta^{15}N$-N_2 (D). Red shading on Taylor Glacier gas age chronology and delta age indicates ice shallower than 4 m where surface cracks may affect the CH_4 age matching.



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 will remove “on the order of hundreds of years” from line 19, and we will change “smaller” to “smallest”.

-line 21: replace “at” by “for”
We will replace “at” with “for” on line 21.

-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 ~ 2000-4000 years, Figure 5). This uncertainty range compares well with the uncertainty cited in (Baggenstos et al., 2018).

-line 30: “10 ka”+/- ??? uncertainty needed.
We will add the uncertainty to the text in line 30.

-line 33: then why is it so different? Replace “high” by “large”
We explain the difference in terms of accumulation gradients beginning on the last paragraph of page 9. We will replace “high” with “large” on line 33.

-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.
We will delete the last sentence in paragraph 1.

-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 will discuss the construction of the Taylor Dome chronology in a preceding section, analogous to the Taylor Glacier chronology.

-line 13: “in the same manner AS described”

We will add "as" to line 13.

-lines 17-19: You should then directly give a 0 value. Note then the uncertainty associated to the Δage is then not guassian..
We will 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 Results and 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 will 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 will change the wording to read, "Comparing the depth-age relationships in the new Taylor Glacier core versus the Taylor Dome ice core supports the notion of large accumulation differences between the two sites. Though differences in thinning between the two accumulation zones likely exist, we think that this effect is secondary to accumulation in terms of setting layer thickness." We will also reference (Morse et al., 1999; Morse et al., 2007; Morse et al., 1998) and other relevant work, in which large differences in thinning across Taylor Dome were rejected based on multiple lines of evidence, one being that layers do not show the same thinning trends with depth.

-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 will include this information in section 2 – Field site and analytical methods.

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 will remove the first two sentences of the last paragraph on page 10.

Page 11:
-line 4: need a reference for this statement.
We will add a reference for this statement.

-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 will change the organization of figure 1 according to referee 1's suggestion.

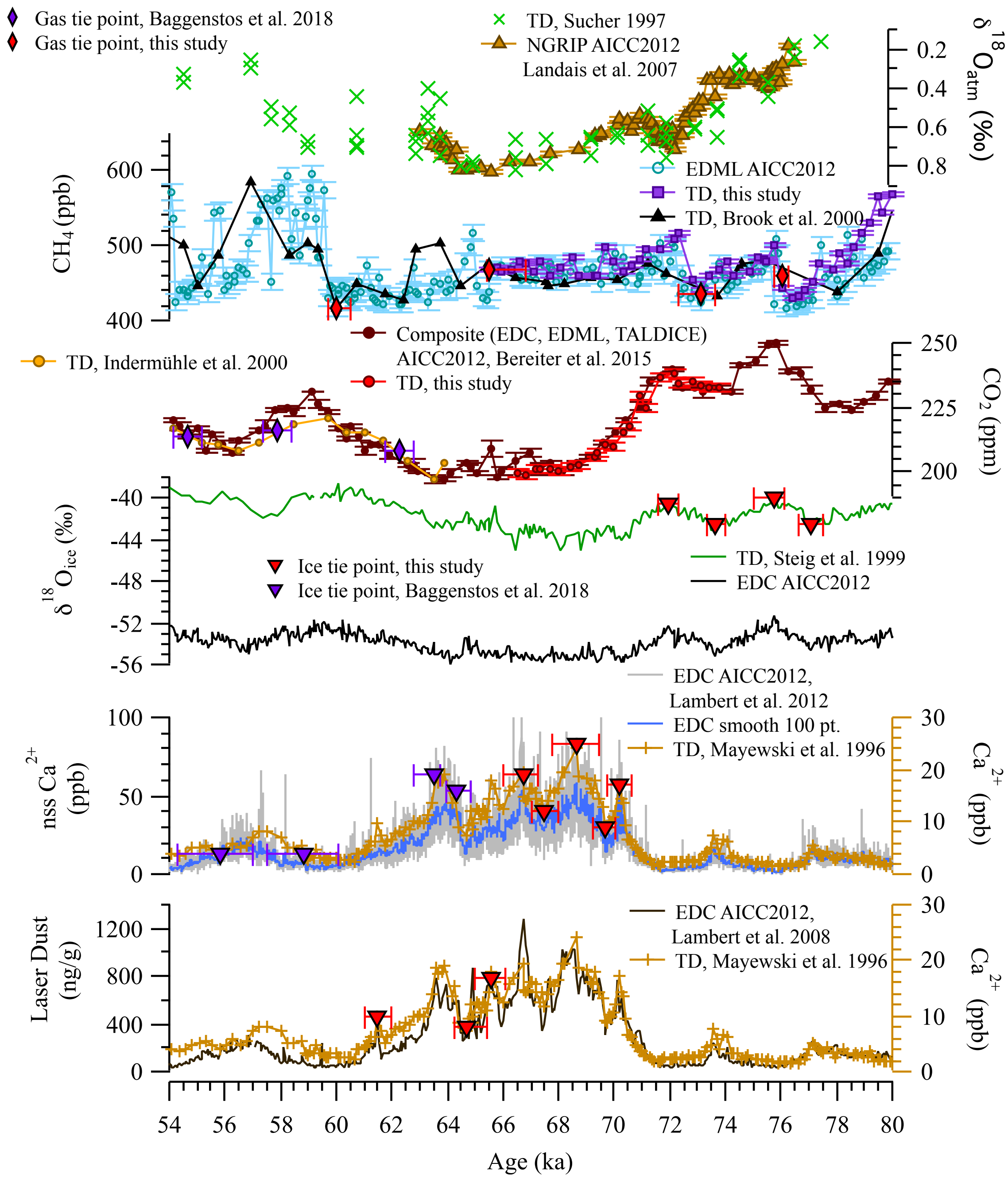
Figure 2: The way the data are presented now, one can strongly argue your chosen tuning points. The scales are two small to see the consistency between the associated variability. I am not at all convinced about your tie-point between the d18Oice of EDC and TG, records present different variability. 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 will further justify the d18Oice tie point along with other tie points used to construct the chronologies. We will add labels for important features to ease comparison between the graphical display of tie points in Figure 2 and the list of tie points in Table 1, e.g. "DO 19" or "DO 18". We will also add a figure graphically displaying the Taylor

Dome tie points, analogous to Figure 2, which only shows Taylor Glacier tie points. Generally speaking, we will justify our tie point choices more clearly in 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. We will put the MIS and other identifiers at the bottom for clarity. We will delete the last sentence in the caption.

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 will keep the colors consistent between the two figures.

Revised Figure 4:



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.

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