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 will add text that includes the points made above.

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.

We will cite other firn model studies, but all models support the general statements in the paper about the relationships between temperature, accumulation, and delta age.

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 will add a table that details the metadata for all measurements made – i.e. which core, which measurement, at which institution, and in the field or lab.

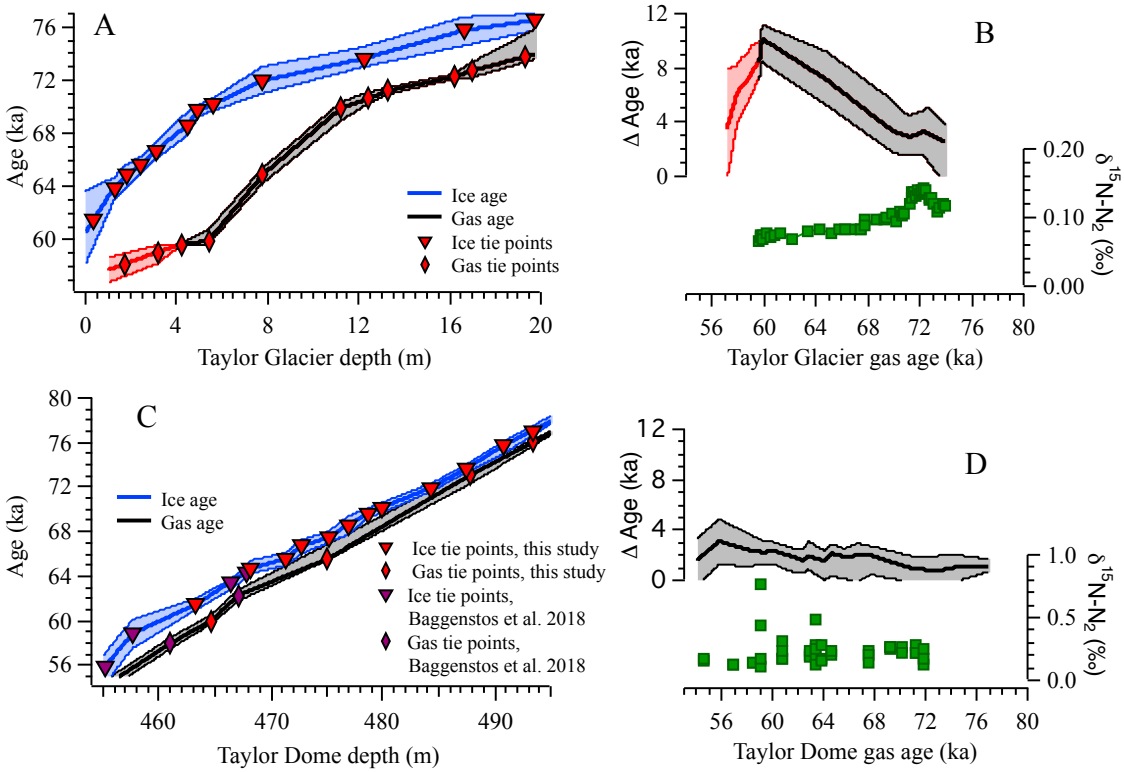
- The authors propose to treat the three ice cores covering the MIS5-4 transition as a single ice core record (unified depth and age scales). While I agree with them that it is justified, I believe that they should provide additional details on how they line up the different records together (and possibly 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 CH4 records from the 2014-2015 core versus the field CH4, where you see up to a 10 cm depth offset between records at DO 19. 10cm conservatively equates to 210 years on our depth scale where age changes the most with depth. The continuous CH4 measured at DRI versus in the field (same 2015-2016 core) actually exhibit larger offsets (up to 20 cm = 300 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 delta age calculations.

We will explain all of this more clearly in the text by adding a paragraph in the analytical methods section that elaborates on the depth uncertainties. We will also display the propagated error on the depth-age plots, not just the delta age (as in Figure 5 below).

Figure 5 – Age models for new Taylor Glacier 5/4 BID cores (A), Taylor Glacier delta age and $\delta^{15}N\text{-}N_2$ (B), and Taylor Dome revised age models (C), and Taylor Dome delta age and $\delta^{15}N\text{-}N_2$ (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.



- 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 will reorganize 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 sub-sections or paragraphs. We will also move the explanation of the revised Taylor Dome age scales, though this will require some additional text to explain why Taylor Dome is of interest here.

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 will justify our tie point choices more clearly in the Age Models section 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 will move the discussion of analytical uncertainties to section 2 where the analytical methods are first introduced.

- 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 understand this point, but ordered the text the way we did because there has been a lot of previous work on Taylor Dome. However, we are willing to reorganize the text as suggested. We will split the Age Models section into 3.1 Taylor Glacier Chronology, 3.2 Taylor Dome Revised Chronology. We will need to add some text in 3.2 explaining why Taylor Dome is of interest. At this point in the paper we can also add more background information about the Taylor Dome site with special attention to the previous age scale.

Included in this change will be the addition of metadata about the new Taylor Dome measurements in the new metadata table, and discussion of methods for Taylor Dome samples in the methods section.

3- FIGURES

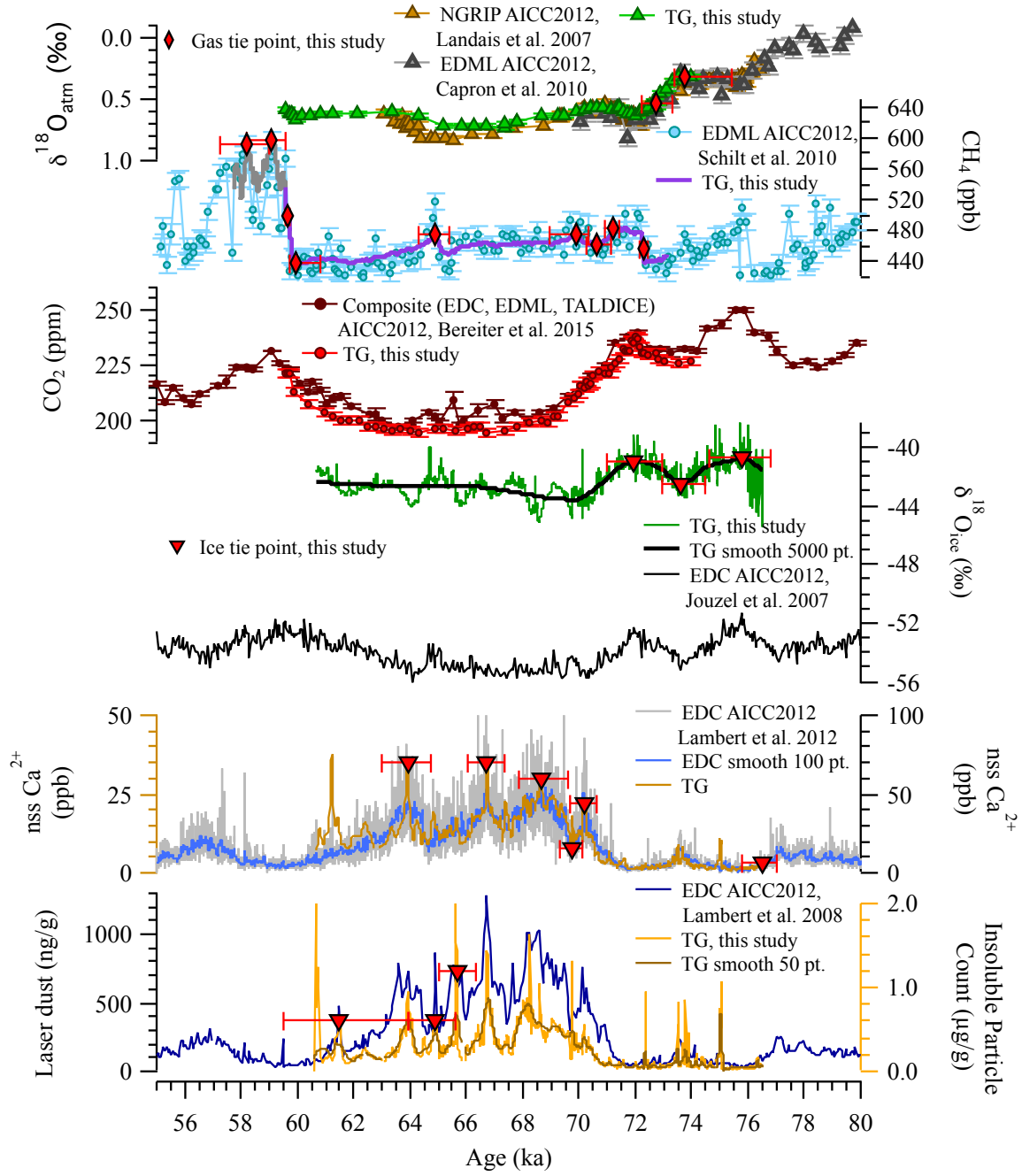
- I appreciate the effort of the authors to show how they defined the different tie points to link between the Taylor Glacier records on a depth scale the dated reference records. However, it should be bigger to allow a closer inspection of the different records and where the tie points have been chosen.

We will make Figure 2 larger so that the tie point picks are more clearly visible. We think this will make the picks more justifiable with closer inspection.

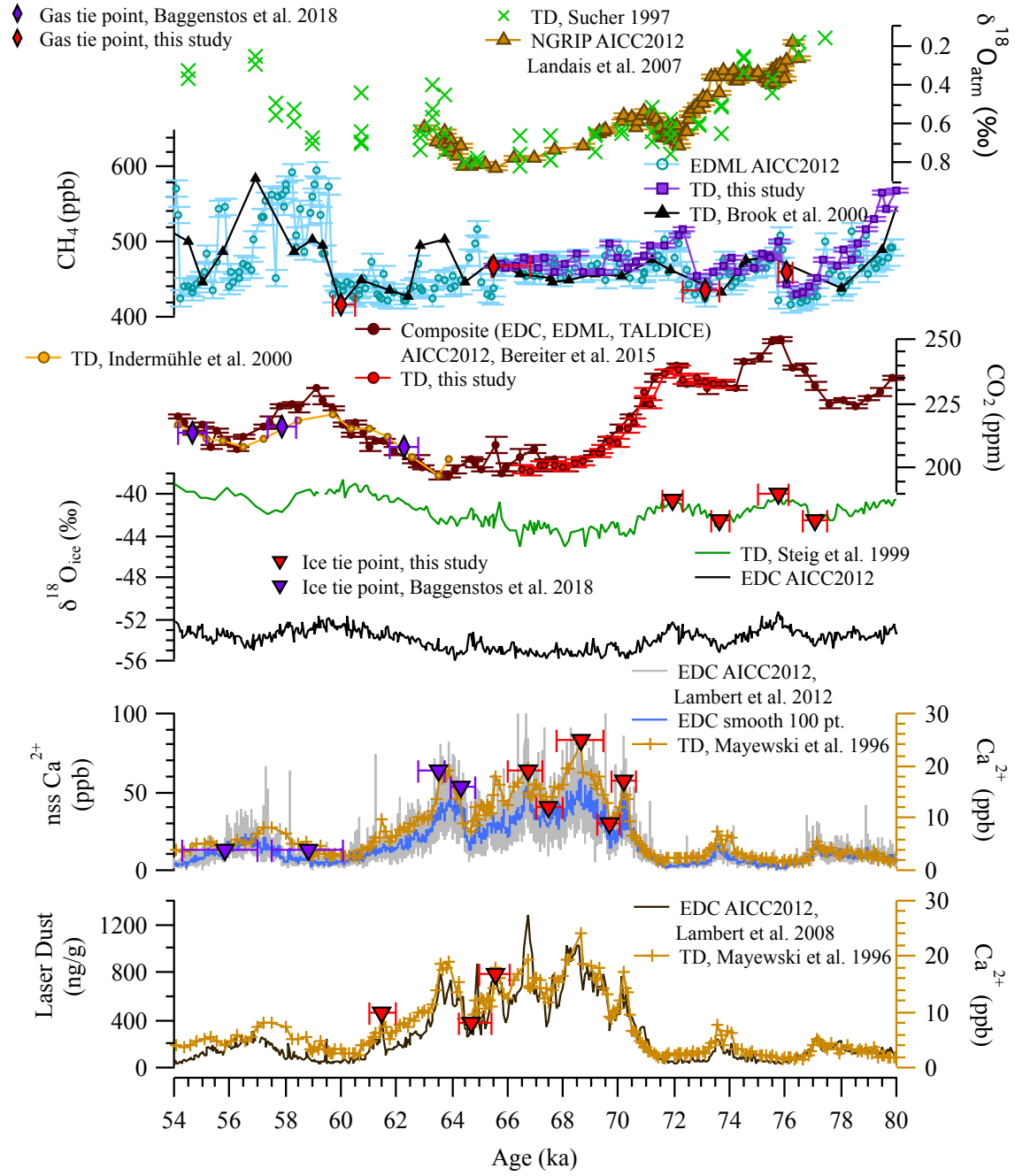
- Figures 3 and 4 should appear much bigger. Also, to facilitate the comparison of delta age evolutions between Taylor Glacier and Taylor Dome, I suggest to remove the panels b from each figure and combine these panels b into a single and additional figure. They can be presented in parallel, making sure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

We will enlarge figures 3 and 4. We will reorganize so that the "b" panels are together in one separate figure for easier comparison (Figure 5 above).

Revised Figure 3:



Revised Figure 4:



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 will include 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 will rewrite the sentence to read, "(2) the description of a new climatic record from Taylor Glacier across MIS 4" on line 34.

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 will rewrite the sentence to read, "… another exploratory core was obtained directly…" on line 1.

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 will make the listing of various cores more clear with a table.

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 will rename the title of the section to "Determination of the ice age and gas age scales" on line 26.

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

We will provide a quantitative range on line 4.

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

We provided a quantitative range for each tie point in the original manuscript, though we did not state the uncertainties clearly in the main text. While we propagated our estimated uncertainty to the delta age calculations to provide maximum and minimum delta age estimations, we did not clearly show the error propagated into the age models themselves. In the revised manuscript we will shade the uncertainty around the depth-age plots in Figure 5 (above). We will also state an age uncertainty clearly in the text. We also enlarged the estimated uncertainty range for tie points that are possibly more ambiguous, particularly the dust tie point at ~ 61.5 ka.

REFERENCES:

Capron et al. 2013. Glacial–interglacial dynamics of Antarctic firn columns: compari- son between simulations and ice core. Climate of the Past, 9, 983–999.

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constraints on sensitivity to temperature and impurities. Climate of the Past, 833–853, 2017.

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Goujon C. et al. 2003. Modeling the densification of polar firn including heat diffusion: Application to close-off characteristics and gas isotopic fractionation for Antarctica and Greenland sites, J. Geophys. Res., 108, 4792, doi:10.1029/2002JD003319, 2003.

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Morse, D.L., Waddington, E.D., Steig, E.J., 1998. Ice age storm trajectories inferred from radar stratigraphy at Taylor Dome, Antarctica. Geophysical Research Letters 25, 3383-3386.