We thank the three Referees for their constructive comments. We appreciate the generous offer by Dr. Parrenin to provide uncertainty estimates for Δage from IceChrono. We have found the IceChrono code on GitHub and will incorporate the AICC2012 Δage uncertainty in the revised manuscript.

Since all three Referees have commented on our treatment of ice age uncertainty, and Referee #2 and #3 in addition have explicitly raised concerns about the interpretation of the accumulation rate spike around 128 ka, we will organize our response in the following way. We will begin with specifically addressing the uncertainty associated with ice age. Next, we will discuss the possibility of alternative interpretations of the increased accumulation rates. Finally, we will respond to the remainder of comments by individual Referees that do not fit into the first two categories.

1. Treatment of the ice age uncertainty

We acknowledge that the ice age uncertainty needs to be considered because the MIS 5e isotope peak in EDC and S27 may not be perfectly synchronous. This offset may become significant when the relative gas age uncertainty becomes sufficiently small, such as during the Δage minimum around 128 ka. We thus proceed to consider the following two sources of ice age uncertainties.

First, we review the synchroneity of temperature variations across Antarctica. Of special importance is the comparison between the $\delta^{18}O_{ice}$ record of Taylor Dome (closest deep core to S27) and that of EDC (the matching target of S27 δD_{ice}). The assumed synchroneity between S27 and Taylor Dome is supported by their physical proximity (115 km)in the discussion below. Modeling results show that in the event of a collapsed WAIS, both the EDC and Taylor Dome sites are going to experience the same trend in temperature changes (Steig *et al.*, 2015). In addition, the overall deglacial warming is almost synchronous during Termination I in the Taylor Dome and EDC stable water isotope records (Stenni *et al.*, 2011). Both records have an apparent mismatch in peak $\delta^{18}O_{ice}$ around 14 ka, right before the Antarctic Cold Reversal. This offset is about 200 years, translating to the uncertainty of ±100 years associated with the aligning EDC and Taylor Dome ice cores, and by inference, between EDC and S27. Beyond 15 ka, the resolution of Taylor Dome isotope record becomes too low to permit an effective comparison.

Second, we ask how precisely peaks in two time series can be identified and tied. Because we are explicitly targeting the maximum or minimum isotope peaks, the linkage of the observed peaks should be very clear and unambiguous. However, we realize that the peaks in the record were based on discrete sample analysis. In other words, the real peak in the record might not be sampled and captured in the observed peak. Intuitively, the higher the sampling resolution, the smaller the chance of missing the real peak. In the worst-case scenario, the real peak could be located infinitely close to the two samples next to the observed peak. If the sampling resolution is 100 years, for example, then the maximum error associated with identifying the peak in this record is 200 years. In the case of EDC and S27, the average sampling resolution of stable water isotopes during MIS 5e is ~40 and ~20 years, respectively. In attempting to tie the peaks, their respective errors should be added up. In the case of EDC and S27, therefore, an uncertainty of ±60 years related to the identification and matching peaks in different isotope records will be introduced in the revised manuscript.

Taking the two forms of errors in ice timescale into account, we will include an ice age uncertainty of ±160 years in the revised manuscript and update the Δage uncertainty accordingly.

Finally, we wish to take this opportunity to acknowledge that a more refined ice chronology, perhaps made available by absolutely dated tephra layers and synchronizing ion content such as sulfate, as Referee #2 has pointed out, will further improve the manuscript. We hope that this manuscript will stimulate future work on this problem.

2. Interpretation of the MIS 5e accumulation rate spike

Referee #2 suggests that the abrupt change in accumulation pattern could be a local effect rather than a broader climate signal, possibly linked to the migration of ice domes and the subsequent changes in accumulation gradient, as some pioneering studies in this region have revealed (Morse *et al.*, 1998; Morse *et al.*, 1999). We are also aware of a recent study by Menking *et al.* (2019) on a horizontal blue ice record drilled from Taylor Glacier (TG). Menking *et al.* calculate the accumulation rate of the TG blue ice record and compare that to the Taylor Dome accumulation rate. They confirm "a spatial gradient in snow accumulation" across the Taylor Dome region. More importantly, their data reveal a reversal in that gradient in LGM compared to MIS 4 [Figure 6 in Menking *et al.* (2019)]. We will acknowledge such possibilities in the revised manuscript.

However, we note that Steig *et al.* (2000) also finds a spike in accumulation rate in Taylor Dome during MIS 5e (Figure 1). Although the timing is not well-constrained, the peak accumulation rate at Taylor Dome is close to 0.08 $m \cdot yr^{-1}$, in good agreement with our estimates of peak accumulation rate at S27. Since the no accumulation estimates is available for TG blue ice record extending back to MIS 5e, we can only compare Taylor Dome and S27 here. We thus consider the increase in accumulation rate during MIS 5e to reflect a regional climatic shift.

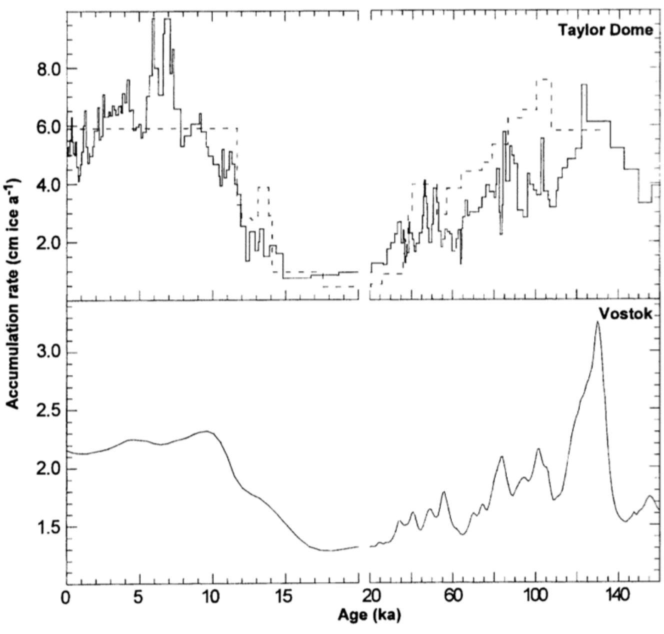

Figure 1. Taylor Dome and Vostok accumulation rate reconstruction [Figure 7 in Steig *et al.* (2000)]

Referee #3 further suggests that additional data such as deuterium excess (*d*-excess) could be utilized to test our hypotheses, which we agree and will acknowledge it in our revised manuscript. Indeed, water-tagging experiment in an isotope-enabled model shows that the *d*-excess of the precipitation over the Allan Hills region is most dominated by the moisture source on interannual timescales (Figure 2; Jun Hu, personal communications). Moisture originating from higher latitude has lower *d*-excess values, meaning that all other things being equal, the open-water conditions at the peak of MIS 5e would lead to lower *d*-excess in the S27 record. However, the stable water isotope composition (δD_{ice}) of the S27 ice core was measured using a mass spectrometer after Cr-pyrolysis at 1050 °C, so no ice core $\delta^{18}O_{ice}$ data is available. We will add these considerations and limitations to our revised manuscript and hope future work can be done to examine the hypothesis put forward in our current study.

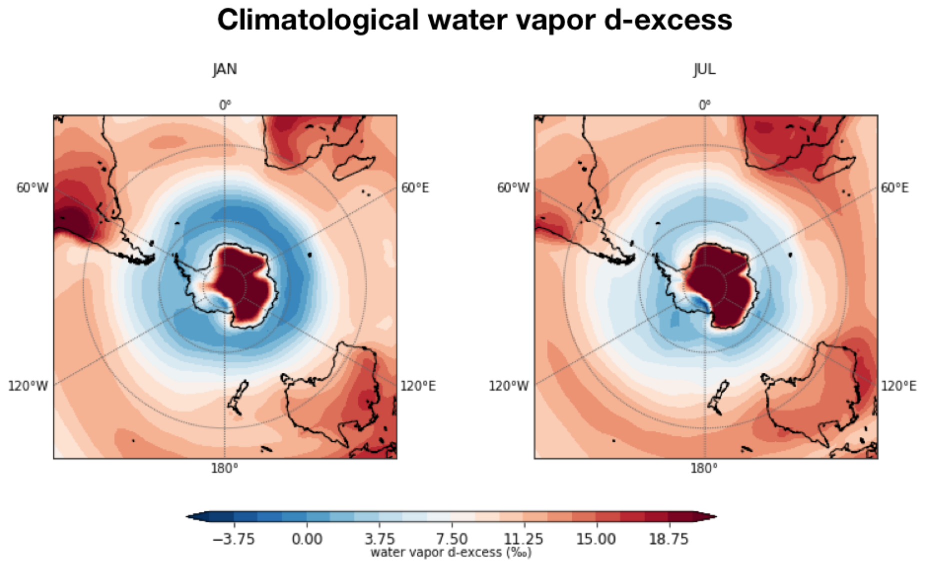

Figure 2. Climatological d-excess of water vapor simulated in iCESM (Jun Hu, personal communications).

Now we proceed to address the individual points raised by each Referee that are not related to ice age uncertainty or alternative explanations for the accumulation rate increase.

3. Response to Referee #1

We have addressed Dr. Parrenin's main comment about the error estimates above. Below are our responses to the minor comments.

- l. 25 : "the peak in S27..."

Thanks for catching that.

- l. 428-429 : Are you sure 3 ka is enough to re-form the WAIS and/or Ross ice shelf? This could be discussed.

This is a very good point. It is unlikely that the WAIS could have re-advanced within 3,000 years. Based on the modern observation that the Ross Ice Shelf is fed by both West Antarctic and East Antarctic ice streams (Rignot *et al.*, 2011), it is plausible that the ice shelf recovery originated from East Antarctica. We will add a sentence in the final paragraph of the revised manuscript discussing the recovery of RIS.

4. Response to Referee #2

We appreciate the time and efforts by Referee #2 to delve into our data and to raise three very important points. First, a near-zero Δage is present around 145 ka and would imply very large accumulation rate in the glacial period. If this feature is robust, the attribution of elevated accumulation rate during Termination II to the RIS retreat would be weakened. Second, the ice age scale has no error associated with it or independently established age controls (e.g. tephra and sulfate). Third, the accumulation rate change may be a local phenomenon, perhaps related to the migration of accumulation areas. Among them, point #2 and #3 have been addressed in our response above. We therefore discuss the feature of a very small Δage at ~145 ka here.

We underscore the fact that the very small Δage around 145 ka is defined by two $\delta^{18}O_{atm}$-derived, GHG-corrected gas age point at the depth of 136.20 m (140.916 ka) and 139.66 m (143.477 ka). There are only four gas age points between the interval of 140 and 145 ka. In addition, the ice age scale in this interval is constrained by only two tie points, one at 128.32 m (135.808 ka) and the other at 158.69 m (157.096 ka). This is in direct contrast to the small Δage around 128 ka, where 15 $\delta^{18}O_{atm}$ samples are covering the 5,000-year interval from 128 to 133 ka and four δD_{ice} tie points lie within the interval between 125 and 130 ka.

To sum up, given the lower temporal resolution of $\delta^{18}O_{atm}$ samples and the fewer ice age tie points around 145 ka, we cannot confidently conclude this small Δage around 145 ka is a robust feature. A similar case can be made for the dip in Δage around 168 ka, where only three $\delta^{18}O_{atm}$ data points provide constraints. In the revised manuscript, we will incorporate these considerations to the text that is currently located between Line 281 and 283. We will also add a new panel to Figure 2 to demonstrate the tie points for ice age scales and

5. Response to Referee #3

We thank Referee #3 for the very detailed comments. Before addressing those individual points, we would like to first respond to Referee #3's comments on the impact of gas loss on ice with and without fractures.

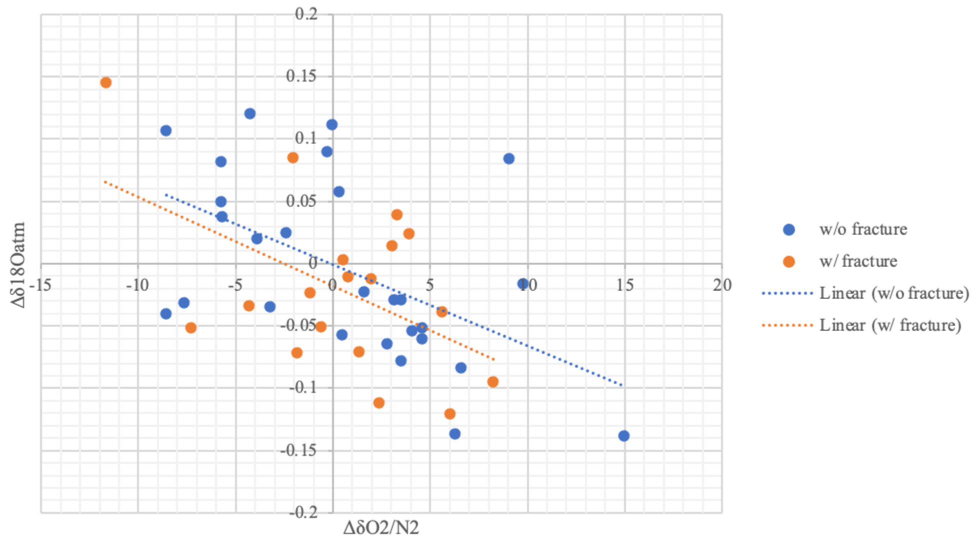

Figure 3. Gas loss as observed in ice with and without fractures. Dashed lines are regression lines.

In Figure 3 above, we divide samples into ice with fractures (w/ fracture) and ice without fractures (w/o fracture) and redo the calculation in Figure S4. This yields a slightly steeper slope for ice with fractures (-0.00715±0.00318; 1σ) than for ice without fractures (-0.00654±0.00216; 1σ). The lack of large difference justifies a unified gas loss correction equation. We will add in the revised manuscript that the presence or absence of fractures does not seem to have an impact on the extent of gas loss.

It is curious as to why fractured ice does not appear to experience gas loss differently. One possible explanation is that the gas loss correction here applies to sample measured in 2018. An important assumption is that all data in 2013 were measured on "gas loss-free" ice. This assumption clearly may not hold true for samples below 150 m, as the presence of fractures likely have already impacted the quality of $\delta^{18}O_{atm}$ data back then. In other words, the 5-year gas loss experienced by both fractured and non-fractured ice appears to be the same, but their original status pertaining to gas loss in 2013 was different.

The more detailed points raised by Referee #3 are marked in bold and addressed below. All the text-related comments are fully acknowledged and will be revised accordingly. Here, we focus on the points that substantively impact the interpretation of the S27 record or the presentation of our conclusion.

Line 43: Specify that it is for both past and future simulations.

Thanks for pointing this out. We are aware of a recent equilibrium-state simulation of the future warming that shows a widespread retreat of RIS due to the partial collapse of WAIS (Garbe *et al.*, 2020). We will cite this new development in the revised manuscript.

Line 72: Rephrase the sentence. I guess the missing peak in $\delta^{18}O_{atm}$ is only because no measurements have been done at these depths.

Line 73: It is not clear what the $\delta^{18}O_{atm}$ sampling strategy was. Improve the resolution? Complete missing intervals?

These two comments are related so we will address them together. Part of the initial motivation of this work is indeed finding the missing peak and understanding the stratigraphic integrity of the record. Realizing what could be achieved with a new gas chronology, we eventually decided to measure additional samples from 27 depths above 150 m to further improve the sampling resolution. We will outline the motivation in the revise manuscript with greater clarity.

Line 145: This sentence suggests that there are also fractures in the ice above 151 m. Are they numerous? Is there an influence on the $\delta^{18}O_{atm}$?

Yes, fractures are sporadically present between 90 m and 130 m and all ice become fractured once the depth falls below 150 m. We observed an increasing occurrence of fractures with depth between 130 and 150 m. Why the transition of ice quality happens in this depth interval remains not clear. In any case, a $\delta^{18}O_{atm}$ sample requires 20 to 30 g of ice, corresponding to 4 cm in ice length. This is small enough that we may be able to single out the section with no fractures for $\delta^{18}O_{atm}$ analyses even in the transitional zone (130-150 m). A single CH_4 sample on the other hand demands a larger sample size (60-70 g) and therefore means longer ice length (10 cm) sample. It is therefore much harder to get a fracture-free ice for CH_4.

Figure 4: Change "per mil" into "‰". You also compare in the main text the $\delta^{18}O_{atm}$ variations to orbital variations. Maybe add the insolation curve on the figure.

This is a great suggestion. We will add an insolation curve on a second y-axis.

Figure 5: Add the tie-points and anchor points used for the chronology on the figure. In the caption, precise that CH_4 data are from EDC, the CO_2 is a composite record and the timescale is AICC2012.

We will mark tie-points and anchor points in the revised manuscript.

Line 271: To conclude this part on the gas chronology I missed a sentence on the total uncertainty associated with this new chronology. How much is it?

We will add more description about the uncertainty associated with the new gas chronology.

Lines 321-324: I don't know if we can say that the accumulation rate at S27 is comparable to Vostok and EDC. The trend is similar yes but the absolute value not. And how is the 0.02 m.yr-1 value defined?

We will state that the accumulation rate at S27 is lower than that at Vostok and EDC during glacial periods. The 0.02 $m \cdot yr^{-1}$ is the arithmetic mean value of the Vostok and EDC accumulation rate between 115 and 140 ka, excluding 125 to 132 ka. We realize this is misleading because line 324 states it is "glacial periods", but the interval between 115 and 125 does not technically belong to a glacial period. To avoid confusion, we will not mention this 0.02 $m \cdot yr^{-1}$ in the revised manuscript and instead focus on the relative relationship between S27 and EDC (as well as Vostok) accumulation rates.

Lines 338-339: Could you support this hypothesis using model comparison?

Yes, we will add the modeling work by Krinner *et al.* (2007) to support the claim of increased precipitation due to enhanced moisture transport towards the interior of the continent. We note that this work compares the end of the twentieth to the end of twenty-first centuries, but expect the underlying physical mechanism also applies to past climate. In addition, the pattern of precipitation change revealed by the model is spatially heterogenous: while much of Antarctica experiences a higher precipitation, sections of East Antarctic coast (Northern Victoria Land) and West Antarctica receives less precipitation in a warmer climate. That said, for Southern Victoria Land an increased precipitation is observed in the model.

Lines 345-350: TALDICE's accumulation rate starts to increase earlier than S27 (and is more similar to Vostok and EDC). As for the magnitude, it is much larger for S27 than for TALDICE. The S27 site is already pretty coastal so I would rather say that the peak in accumulation rate at 128 ka reflects more open-ocean conditions than a transition into a coastal site.

We agree that the timing of the accumulation rate increase Talos Dome precedes the increase in S27. The reason we suggest S27's transitioning into a coastal site is the comparable magnitude of the peak accumulation rate around 128 ka. We will focus on more open-ocean conditions near S27 instead of vaguely calling it a "coastal site" in the revised draft.

Figure 9: I would have removed the Greenland temperature record and drawn instead the variation in mean ocean temperature from Shackleton et al. (2020). It could also be good to add an insolation curve to have an orbital context to refer to in the discussion. Change "(g) Relative sea-level vs present day".

We will replace the Greenland temperature curve with the mean ocean temperature series in Shackleton *et al.* (2020) and add a 65 N summer insolation curve. We still believe that the delayed warming of Greenland is important in understanding the sequence of events during Termination II, so the discussion from Line 386 to 388 will be retained.

In the supplementary:

Figure S4: It is not clear if the data presented here are only for non-fractured ice or for both non-fractured and fractured ice. Please indicate if this is non-fractured ice or differentiate the data with two regression lines for the two zones.

This is from both fractured and non-fractured ice. We have shown in Figure 3 above that the presence or absence of fractures does not appear to impact the gas loss correction.

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