Response to Anonymous Referee #2

We thank the reviewer for his/her valuable and helpful comments on the manuscript. We propose to implement the following changes in a revised version.

Black = reviewer comment / blue = author's response / "*italic*" = *revised text.*

Review of Bouchet et al., 2023 – AICC2023

Bouchet et al., 2023 present an update to the AICC ten years after the first AICC. The update is focused on the EDC ice core and the older portion of the timescale that is based on orbital tuning. The increased density of measurements of d18Oatm, TAC, dO2/N2 and d15N are welcome and represent a significant improvement.

This manuscript describes a useful update the AICC. I remain confused by the exclusion of all(?) US, British, New Zealand, and Australian ice cores from the AICC. This is of relatively minor importance to this manuscript given that the different age ranges and the focus here on ages older than 100 ka and almost exclusively on EDC. I will urge that this chronology is names the EAICC – the East Antarctic Ice Core Chronology – given that there are more West Antarctic cores excluded from this chronology than East Antarctic cores that are included.

Author's response: Many thanks for this comment. We acknowledge that the aim of this study was perhaps not made very clear. The objective was indeed to focus on the long timescale (before 60 ka BP) to present the numerous new data available on the EDC ice core and using them to update AICC2012 with a special focus on deep time scales.

Adding the ice cores not yet included in the Paleochrono tool and mainly covering the last 60 kyr would be a different study which requests a lot of resources for the implementation of the ice cores in the Paleochrono tool. It was thus not possible to include everything in this study and we decided to focus more on the deep timescales with a particular focus on EDC. This will be better explained in the new manuscript.

As for the name of the chronology, after discussion with co-authors, we acknowledge that the name EAICC could have been a more suitable choice in the first place.

However, we would prefer to keep the name AICC for several reasons. It is less confusing and allows to show that AICC2023 is an update with respect to AICC2012 and that it should replace it. AICC provides an age model mainly for multiple glacial cycles where only the East Antarctic cores provide information. AICC uses age constraints from a set of cores (including NGRIP, so not just East Antarctic) and can be used as a template for West Antarctic cores as well (as for the Skytrain Ice Rise, Mulvaney et al. 2023). Finally, as mentioned above, one important future development would indeed be to include the high-resolution information from WAIS Divide and other cores.

The authors describe a large range of atmospheric gas measurements. The improvement in resolution of the many records is impressive. The orbital tuning of these records remains quite challenging and thus requires a myriad of subjective choices to develop both the timescale and the uncertainty. The orbital tuning, and the tuning to speleothem calcite, suffer from a lack of understanding in either cause of the variations in the measured parameter, the orbital parameter to tune to, or both. In particular, both O2/N2 and TAC have no process-driven explanation for why they vary based on the orbit characteristics and the variations are not produced by firn models. While this highlights the need for better understanding, particularly as great effort is going to extracting multiple >1Ma ice cores that reach the 40 ka world, it should not prevent doing the best that can be done with current understanding. And Bouchet et al. do this. They have produced a thoughtful chronology and while the manuscript is dense, it is also clearly written.

There are a couple of areas that stand out as areas of concern:

1) The firn modeling

This sentence is particularly confusing: “To obtain a coherent scenario, the firn modeling estimates have been adjusted, by standard normalization, to the scale of LID values derived from δ15N data (later referred to as experimental LID).”

This seems to hiding a major limitation in the methodology. If I understand correctly, the authors cannot get the firn model to match the d15N-inferred firn thicknesses, so they just give up on the actual values and instead seek to match the variations. Whether this is due to an inappropriate firn model (Breant) or outdated forcing (the forcing isn’t shown but I suspect the authors are using the classical isotope-temperature scaling that Buizert et al. 2021 showed to be too cold at the LGM). The firn modeling should really be done with multiple models – which is actually relatively easy to do thanks to the Community Firn Model – and with a range of climate forcings. I think the authors efforts would be better served employing other firn models and forcings rather than the impurity scenarios which the author reject.

Author’s response: Thank you for raising this contradiction. The idea behind this proposition of fitting the modeled LID (orange curve on Fig. 1) to experimental LID values was to avoid any discontinuity when switching from experimental to modeled values when no data are available (grey rectangles on Fig. 1).

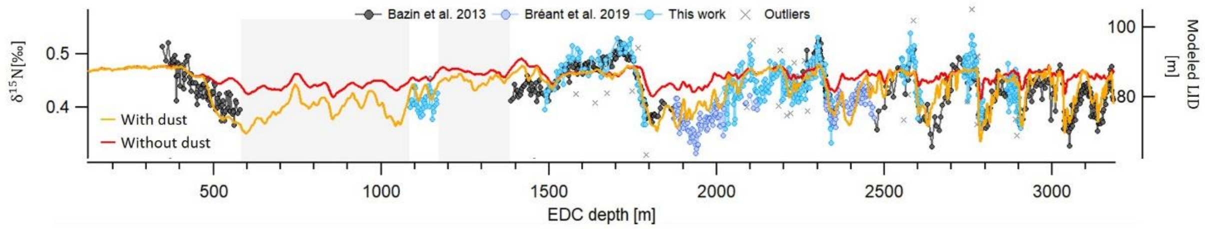

Figure 1. Modeled LID and δ^{15}N data over the 0-3200 m depth interval. Grey rectangles indicate depth intervals where δ^{15}N data are not available (either between 578 and 1086 m or between 1169 and 1386 m).

However, adjusting the modeled LID to experimental LID values induces a modification of 4.7 m at most which remains within the background relative uncertainty (20%) so that the adjustment is small and probably not really needed. This was already shown by the good comparison between modeled and δ^{15}N-inferred LID (L.91-140). To check this, we performed several Paleochrono runs to assess the credibility of the two modeled LID scenarios (with and without adjustment).

On the depth interval from 578 to 1086 m, the raw background modeled scenario (orange curve, Fig. 2) is almost as credible as the one that was adjusted (blue curve, Fig. 2) (i.e., close $\Delta_{\mathrm{no\ data}}$ values). On the second depth interval of interest, from 1169 to 1386 m, both scenarios show equal $\Delta_{\mathrm{no\ data}}$ values, hence equally credible.

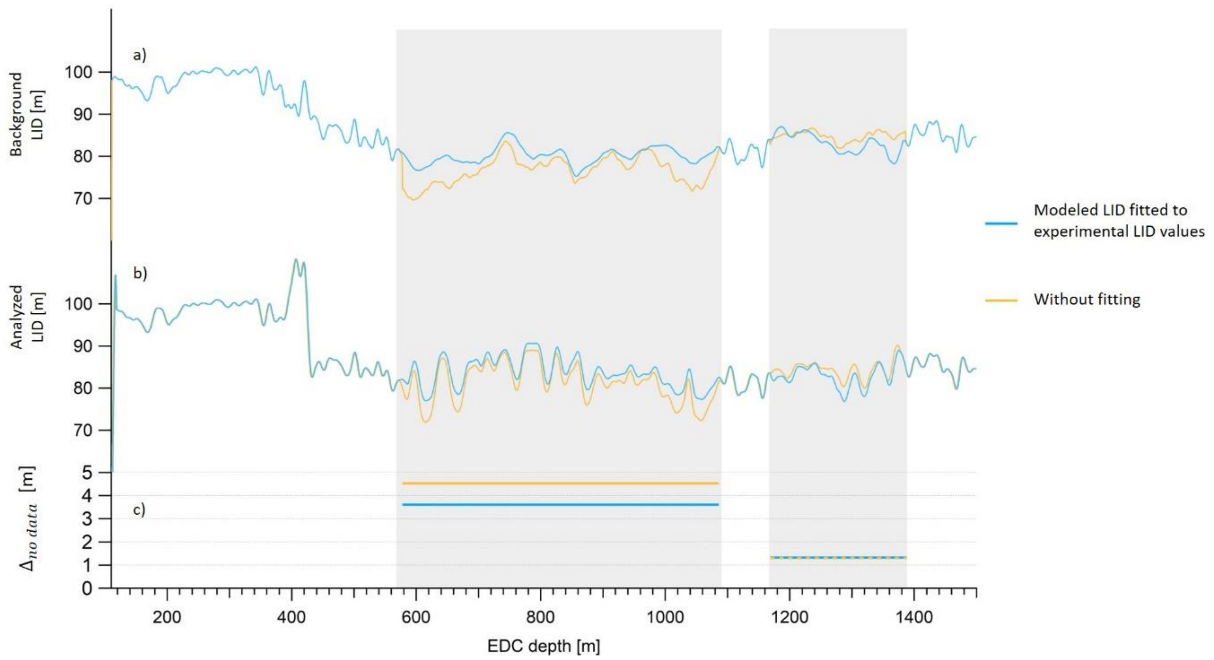

Figure 2. Background and analyzed LID scenarios at EDC. a) Background LID as per AICC2023 (blue) and without fitting of the modeled LID to experimental LID values (orange). b) Analyzed LID. c) The averaged value of the misfit, $\Delta_{\text{no data}}$, is calculated for the two LID over the two depth intervals where δ^{15}N data are not available (either between 578 and 1086 m or between 1169 and 1386 m, see intervals shown by grey rectangles).

For more coherence, we believe that we should use the raw firn thickness predicted by the firn model, rather than fitting it to experimental LID values. This modification will be considered in the revised manuscript.

Finally, we would like to emphasize the facts that such modifications of the background LID scenario (less than 20% of the LID value) do not significantly affect the final age model and that the major improvement of the LID background scenario with respect to AICC2012 is the use of new highly-resolved δ^{15}N data over the 100-800 ka BP period.

We also noted the comment on the use of other firn models. Actually, we tested other firn models in a first instance (in particular the simple Herron and Langway model used also by Buizert et al., 2021) but we chose to keep the firn model outputs giving the best agreement with the δ^{15}N data over the last 800 kyr at EDC to fill the few gaps existing in the data series. The reason why we did not use the Buizert et al. (2021) approach is that it would require (i) a new EDC temperature scenario over the last 800 kyr while Buizert et al. (2021) only provided the temperature scenario over the last Termination as well as (ii) a new adapted temperature scenario for Vostok (which would be confusing for the readers since our goal is not to revise the Antarctic temperature reconstructions over the last climatic cycle). Indeed, to use the Buizert approach, we would have needed to adjust the temperature scenario so that the Herron and Langway model reproduces best the δ^{15}N data. We thus do think that testing the Breant model with different parameterizations (all of them published) and keeping the outputs resembling the most the δ^{15}N-inferred LID was the simplest approach (and less confusing) to fill the few gaps in our δ^{15}N-inferred LID.

2) I would like to see an analyses of the thinning function. The EDC AICC2012 thinning function does not decrease monotonically as expected from ice flow modeling (i.e. the input background scenario). If AICC2023 results in a smoother thinning function, this would provide significant support for the methodology.

Author's response: Although the new AICC2023 chronology reduces the absolute uncertainty of the thinning function compared to AICC2012, it does not provide a smoother and strictly monotonous scenario (see Figure).

However, we believe that this is not a problem for the following reasons:

(i) In a tube flow model, like Vostok's (Parrenin et al., 2004), the thinning function is not monotonous since ice thickness variations are reflected in the thinning function. If the location of the dome at Dome C shifted over the past 800 kyr, the same effect could have affected the thinning function.

(ii) There may also be non-laminar flow effects such as deformation due to more or less hard ice layers.

For instance, Dreyfus et al. (2007) described such a particularly complex thinning scenario at Dome C over the MIS 15 (~580-560 ka BP).

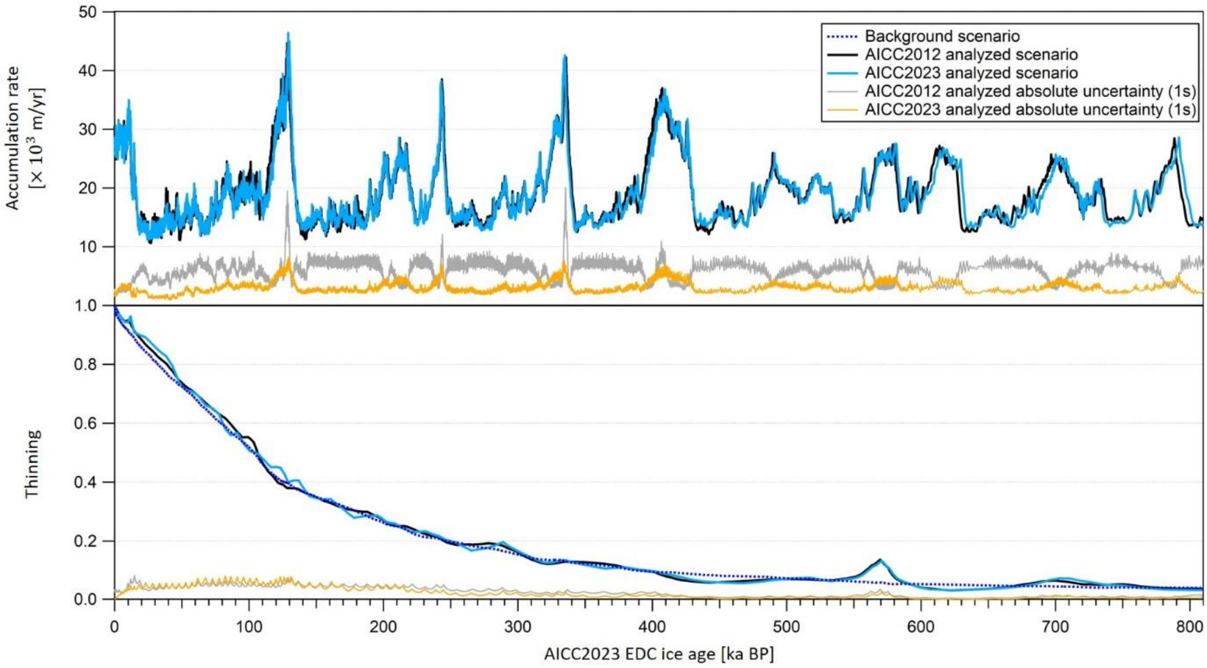

Figure. Analyzed accumulation and thinning functions of EDC provided by AICC2012 and AICC2023 (black and blue plain lines respectively) along with their absolute uncertainties (gray and yellow respectively). The background thinning function is the same for AICC2012 and AICC2023 (dark blue dotted line).

General comments on Figure

For all figures, the timescale that each parameters is plotted on should be stated explicitly. It gets really confusing when match points are connected with lines which are not vertical but the two parameters are plotted on the same age x-axis.

Author's response: The changes will be made.

Vertical lines corresponding the major axes ticks would be really helpful in assessing the alignment of features

Author's response: The changes will be made.

It would be really helpful to see the uncertainty assigned to each tie point. Presumably this could be done with a horizontal bar on the match (on the EDC record)

Author's response: The changes will be made.

Specific comments

L36 – The introduction could really use subheadings.

Author's response: We agree and suggest the following subheadings:

1.1 Building age scales for deep polar ice cores
1.1.1 Motivation
1.1.2 Glaciological modeling
1.1.3 Chronological constraints derived from measurements
1.1.4 Bayesian dating tools
1.2 The AICC2012 chronology
1.3 The new AICC2023 chronology

L43 – "zipped" I don't think this is the right translation to English. I'm not sure what you are going for. I think you are trying to say that a large amount of time is stored in a thin amount of ice.

Author's response: "zipped" will been changed to "stored".

L44 – need to make community possessive > community's

L44 – "core" not "cores"

L46 – add "the" before surface

Author's response: The changes will be made.

L53 – what about Nye?

Author's response: We suggest to change to: *"chronologies of ice cores at low-accumulation sites are commonly established using ice flow and accumulation models (Nye, 1959; Schwander et al., 2001), later on tied up with chronological and glaciological constraints (Bazin and Veres et al., 2013; Parrenin et al., 2017)."*

L95 – I think it's worth emphasizing that Bender found no causal link between dO2/N2 and insolation and was quite forthright about that.

Author's response: We believe that the quote from Bender (2002): *"We assert that insolation influences snow metamorphism and grain properties in shallow firn. The insolation signature in these properties is retained throughout the firn, and influences O_2/N_2 fractionation during bubble closeoff"* is coherent with what we wrote in the introduction: *"observations led Bender (2002) to assert that local summer solstice insolation affects near-surface snow metamorphism and that this imprint is preserved as snow densifies in the firn and, later on, affects the ratio $\delta O_2/N_2$ measured in air bubbles formed at the lock-in-zone."*

We agree to come back to this point if you believe it still needs modifications.

I don't expect that the authors will agree to incorporate WAIS Divide, but the introduction should have a paragraph that acknowledges the exclusion and points readers to the timescales for these cores that are tied to WAIS Divide as the best ones to use for past ~60 ka.

Author's response: We agree to designate the WD2014 chronology as the best candidate for the past 60 kyr. For greater coherence within the manuscript, we suggest to add a paragraph at the end of sect. 2.1 at L.206: *"We acknowledge the exclusion of the WAIS Divide ice core from the construction of the AICC2023 age scale. Over the last 60 kyr, though, we recommend the use of timescales tied to the WAIS Divide 2014 age model (WD2014, Buizert et al., 2015; Sigl et al., 2016). WD2014 hands over to AICC2023 for the period older than 60 ka BP (that is for the section below the depth of 950 m for the EDC ice core)."*

As mentioned above, the depth-depth correspondence between AICC2023 and WD2014 age models will be given in supplement.

L118 – "peculiar" I think you mean "particular"

Author's response: We suggest to change *"peculiar"* to *"singular"* as "particular" is not exactly what we meant.

L308 – shouldn't you reference Tison et al. 2015 here?

Author's response: The reference will be added.

More general comments

L349 – "superior" in English implies "better". I think "greater than" is better phrasing

Author's response: The change will be made.

L372 – the discarding of "tie points" worries me. Doesn't this imply that you don't understand the underlying mechanisms that link the measurements parameter on the target tuning parameter? If you are discarding tie points all together, should the uncertainty for the tie points you keep be increased to respect that the relationship the ties are based on are not stationary?

Author's response: I think the word "discarding" was poorly chosen. Over the period of MIS 11 (gray frame in the Figure), it is impossible to match $\delta O_2/N_2$ and insolation variations as they do not resemble each other. For instance, two peaks in the insolation curve (dashed black line) only correspond to one peak in the $\delta O_2/N_2$ data (blue circles). Hence, there is no tie point in the first place to be discarded. We suggest the following modifications at lines 370-372: *"In such cases, the uncertainty associated with each tie point is ranging from 6 to 10 kyr (precession half period) and some extrema in the target are not used to tune the record (5 extrema over MIS 11 out of 63 over the last 800 kyr)."*

We argue that the uncertainty of 3 kyr for the δO_2/N2 tie points is enough. It was evaluated by Bazin et al. (2016) who examined three $\delta O_2/N_2$ records from Vostok, Dome Fuji and EDC ice cores over MIS 5 and detected some site-specific $\delta O_2/N_2$ variations. This observation, along with the presence of a 100 kyr periodicity in the $\delta O_2/N_2$ record and the difficulty of identifying $\delta O_2/N_2$ mid-slopes and maxima, led them to recommend the use of a 3 kyr uncertainty.

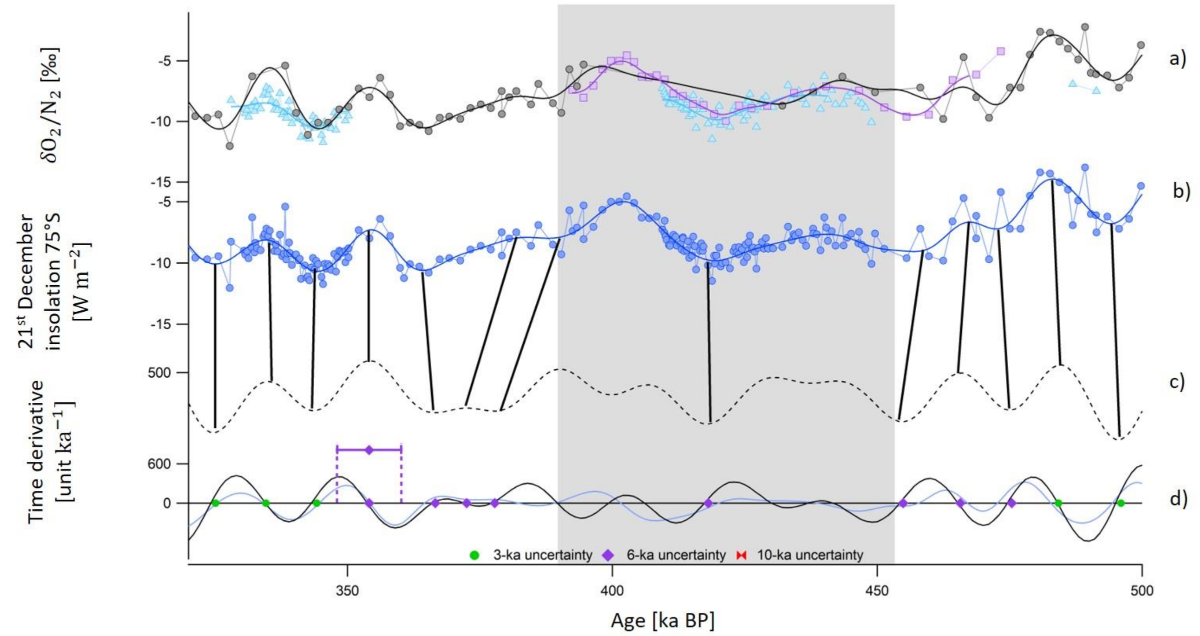

Figure. Alignment of $\delta O_2/N_2$ and insolation between 500 and 300 ka BP. (a) EDC raw $\delta O_2/N_2$ old data (black circles for data of Extier et al. (2018) and purple squares for data of Landais et al. (2012)), outliers (grey crosses) and filtered signal (black and purple lines). EDC raw $\delta O_2/N_2$ new data (blue triangles, this study) and filtered signals (blue line). The $\delta O_2/N_2$ data are plotted on AICC2012 ice timescale. (b) Extrema in the compiled filtered $\delta O_2/N_2$ dataset (blue plain line) are identified and matched to extrema in the (c) 21st December insolation at 75° South plotted on a reversed y-axis and on the age scale given by Laskar et al. (2004) (dash line). The matching peaks are linked by black vertical bars. (d) The 0 value in the time derivative of insolation (black line) and of the filtered $\delta O_2/N_2$ dataset (blue line) corresponds to extreme values in the signals. The determined tie points between $\delta O_2/N_2$ and insolation are depicted by markers on the horizontal line. Green circles are attached to a 3 kyr uncertainty and purples squares are associated with a 6 kyr uncertainty (purple horizontal error-bar represented at 354.1 ka BP). Between 390 and 475 ka BP, all extrema are not tuned to the target due to the poor resemblance between the signal and insolation (see gray frame).

L396 – The authors should not use “continuous” to describe the discrete gas measurements. These samples are still quite sparse. Instead, the authors should emphasize increase in sample resolution and the reduction in the largest gaps.

Author’s response: We agree and several changes will be made to remove the adjectives “continuous” or “discontinuous” when designating the gas records.

Figure 5 – I find the match points between d18O-O2 and speleothem d18O to be unconvincing. What features are being matched and what features aren’t seems arbitrary. Maybe this would be improved by showing the uncertainty

Author’s response: This point was also raised by the Referee 1 and we agree to modify the Fig. 5 so that the uncertainty is shown.

L500 – I’m concerned the 6ka uncertainty is way to small. 6ka seems reasonable for the actual matches, but shifting the tie points based by 5ka based on whether there is a Heinrich-like event is not well founded. This really needs process modeling for support. Since that is outside the scope of the study, I recommend increasing the uncertainty at least 10 ka (5ka since you don’t know what to tune to and 5ka for the murky matches themselves).

Author’s response: Please don’t forget that 6 ka is a 1 sigma uncertainty, so the 2 sigmas uncertainty is 12 kyr, which seems enough for us.

Jouzel et al. (2002) presented the drawbacks of assuming a constant phase between $\delta^{18}O_{atm}$ and insolation which is a key assumption of the orbital tuning approach. To evaluate the uncertainty of the phasing between $\delta^{18}O_{atm}$ and insolation, Parrenin et al. (2001) assumed that the number of precessional cycles can be counted in the $\delta^{18}O_{atm}$ record. For them, this assumption *"is straightforward considering how clearly this cycle is imprinted in the $\delta^{18}O_{atm}$ series"* and implies that *"ice and gas chronologies are assigned to pass through a succession of large doors with a width of 6 kyr (1/4 of a precession cycle)"*. The authors estimated this width by combining glaciological modeling and orbital tuning.
We chose to stick with the recommendation of Jouzel et al (2002) and to use a 6-kyr uncertainty, which also allows to remain coherent with other orbital dating studies already conducted (Bazin et al., 2013). Yet, it is possible to run another dating experiment with the uncertainty increased to 10 kyr if needed.

L576 – As mentioned above, I don't really understand what you are doing to get a coherent scenario. Are there other firn models which get better agreement? And what are the climate forcings?

Author's response: We agree that the standard normalization is not necessary and are willing to let the rough modeled LID values.

L588 – Why are you not using the tie points to WAIS Divide directly? These ties are well established in Buizert et al. 2018. The WAIS Divide timescale is more accurate than GICC05 as demonstrated by Svensson et al. 2020 who had to shift the dates of GICC05 more than WDC14 for the bipolar matches.

Author's response: Although we agree, we would prefer to remain coherent with the AICC2012 study, that is to say to update the timescale AICC between 60 and 800 ka BP while keeping GICC05 between 60 and 0 ka BP. However, we understand fully this comment and will implement a correspondence between AICC2023 and WD2014 age models in the dataset submitted to PANGAEA. In the new version of the manuscript we will insist that for now, we focus mostly on the 60-800 ka BP age interval and stipulate that the WD2014 age model is more accurate over the last 60 kyr.

Ideally, one possible future development would indeed be to include the high-resolution information from WAIS Divide (and other cores). To do so, the WAIS Divide ice core should be added to the Paleochrono experiment along with the ties established by Buizert et al. 2018 and background glaciological scenarios that need to be determined. This development is beyond the scope of this study.