

## Response to Editorial comments:

We want to thank Hubertus Fischer for his editorial support and comments, which are here reproduced in red font. Our response is given in black.

Referee #2 points to the exceptional thinning function, which has so far not been discussed in the paper. I very much agree with the referee that this is an important issue for further discussion and that you should elaborate, why the thinning function looks unexpected, and/or include an alternative thinning scenario and discuss the implication of such an alternative.

As explained in our response to referee #2, the thinning function we use is not exceptional. It is smooth, decreases monotonically with depth, and is based on 1-D ice flow modeling. Not all the relevant data were available to the referee, which led him to conclude the thinning function is unrealistic.

In the revised version of the manuscript (MS) we have added the thinning function to figure 1, and discuss it in the text (see response to Referee #2).

Referee #1 stresses the insufficient documentation of the new Hulu chronology itself and its link to WD2014. Please, expand the discussion on this point as suggested by ref #1.

Publication of the refined Hulu record has unfortunately been delayed. We have now added three references to support the use of the refined Hulu record and its chronology. The IntCal13 paper by Reimer et al. [Reimer et al., 2013] and the paper by Southon et al. [Southon et al., 2012] present the updated, U/Th chronology for the H82 speleothem that is used in the new Hulu record. We also include a reference to a future publication presenting the updated record of Hulu calcite- $\delta^{18}\text{O}$  (Edwards et al., in prep).

A detailed figure showing the full updated Hulu record with our selected transition midpoints has been sent to the editor, which can be shared with the reviewers.

Finally the paper would benefit of a wider discussion in the end that would go beyond its current form, as stated by referee #1

We have now updated our discussion on the phasing of  $\text{CO}_2$  and Antarctic climate during the last deglaciation, following the publication of Marcott et al. [Marcott et al., 2014].

The precise inter-polar phasing of the bipolar seesaw is the topic of a separate manuscript authored by the WAIS-Divide community members, which is currently under review. We have added a reference to this work in the revised MS.

Nevertheless, here already a few minor editorial comments that I would ask you to consider in any future versions of the paper. This does not need any action from your side at this point of time, but refers to future changes after the review process:

1. Please add some information on the uncertainties in the measurement techniques. This appears most important for the Ca measurement, which has an influence on the impurity effect on densification. Note that in Freitag et al., 2013, the critical Ca value is operationally defined by the limit of detection (LOD) of the analyses he refers to. In case the LOD is very much different in your analysis, your Ca(crit) may be different too.

In the revised MS we now state the analytical precision for all data used. The analytical system we use has a different detection limit than that used in [Freitag et al., 2013]. However, firn densification rates depend on the actual concentration of Ca in the firn (in absolute terms), and not on the setup one happens to use in analyzing these Ca concentrations. Therefore, to be consistent with the Ca sensitivities derived by Freitag et al. [2013] we need to use the  $Ca_{crit}$  used in that work, regardless of the detection limit of our analytical setup.

Both CFA systems have been calibrated with prepared Ca standards, and so we can reasonably assume that both setups would measure the same Ca loading in the ice, regardless of their Ca detection limit.

2. The argument on page 7 on the glacial layering is weak, as the evidence of bubble reformation in glacial ice with respect to a layering at the firn/ice transition appears circumstantial.

We are unsure what “evidence of bubble reformation” the editor is referring to, as the Bendel et al. paper [Bendel et al., 2013] does not discuss bubble reformation. Our statement of increased layering during the LGM is a direct paraphrase of the conclusion by Bendel et al. [2013], who conclude that “the high contrast in bubble number density in glacial ice, induced by the impurities, indicates a much more pronounced layering in glacial firn than in modern firn.”

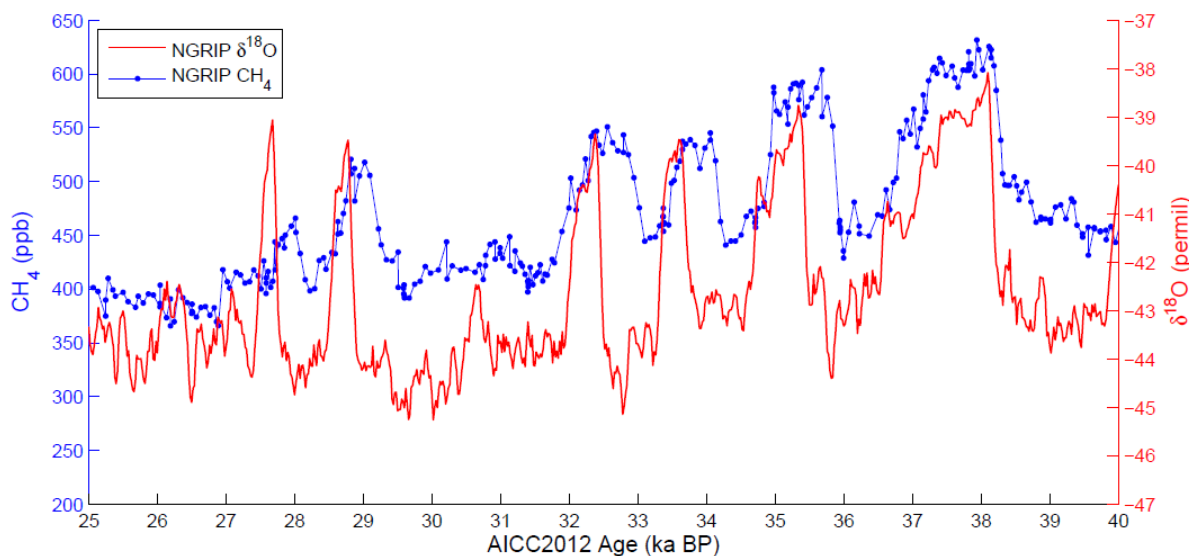
Bubble reformation is associated with hydrate formation. From our reading of Bendel et al. [2013], the issues of increased layering in the LGM, and that of the bubble-hydrate transition (and subsequent bubble reformation?) are two separate issues. The images used to map the bubble distribution were taken within a few days of drilling, exactly to prevent relaxation phenomena. Johannes Freitag, who acted as a reviewer on our manuscript, is an author on the Bendel et al. study. He did not criticize our interpretation of the Bendel et al. paper.

For the time being we left our statement about increased layering unchanged, as we are unsure how to interpret this comment. We would be happy to revise our statement at a future time if the editor deems this appropriate.

3. On page 10 you state that there is no gas age scale available for NGRIP and that is why, among others, you directly synchronized to the d18O. While I have no problem with your approach of directly matching CH4(WAIS) to d18O(NGRIP), please note that in the official AICC2012 age scale there is a gas age scale provided for NGRIP. As the ice age scale in AICC2012 is essentially GICC05 for MIS3, this implies that the gas age scale given in AICC2012 for NGRIP is in line with GICC05.

The editor is correct in pointing out there is indeed a GICC05/AICC2012 gas age chronology available from [Veres *et al.*, 2013]. This is an oversight on our part. We plotted up the NGRIP CH<sub>4</sub> and δ<sup>18</sup>O data on the AICC2012 NGRIP chronology; see the figure below (showing DO 3-8). We find unfortunately that the AICC2012 Δage is not particularly well calibrated through certain sections of the ice core, resulting in a 300-700 year lead of CH<sub>4</sub> over δ<sup>18</sup>O for DO 3-7. This is certainly incorrect given what we know about the CH<sub>4</sub>-climate phasing from δ<sup>15</sup>N [Baumgartner *et al.*, 2014; Huber *et al.*, 2006; Rosen *et al.*, 2014]. We suspect this error is due to the fact that NGRIP δ<sup>15</sup>N data for DO3-7 were unavailable in 2012 when the AICC2012 chronology was constructed. As such, the AICC2012 NGRIP gas chronology is not suitable for our purposes.

Rather than explaining the complications related to each of the individual Greenland gas chronologies, we have simply removed our erroneous statement that no GICC05-based gas chronology is available for NGRIP.



## Response to reviewer #1

We want to thank anonymous reviewer #1 for his or her positive evaluation of our work, and for the constructive comments. Below we reproduce the reviewer comments in red, with our response in black.

1- The most important one is the link to the Hulu chronology. The new chronology for Hulu cave is not presented in this paper except for the short period between 58 and 60 ka BP. It does not seem to have been published elsewhere. As a consequence, it is not really possible to support the chronology of WAIS based on Hulu chronology if the latter is not shown / published.

Publication of the updated Hulu record has unfortunately been delayed. In the updated MS we now provide three references for the refined Hulu record. Two of them present the updated chronology, namely the IntCal13 manuscript [Reimer *et al.*, 2013] and Southon *et al.* [Southon *et al.*, 2012], where the same well-dated Hulu speleothems were used to generate atmospheric  $\Delta^{14}\text{C}$  calibration curves. The third reference is Edwards *et al.* (in prep), which will present the  $\delta^{18}\text{O}$  of calcite record that we used in determining the matchpoints:

Edwards, R. L., Cheng, H., Wang, Y. J., Yuan, D. X., Kelly, M. J., Severinghaus, J. P., Burnett, A., Wang, X. F., Smith, E., and Kong, X. G.: A Refined Hulu and Dongge Cave Climate Record and the Timing of Climate Change during the Last Glacial Cycle, *Earth Planet. Sci. Lett.*, in preparation.

We sent a copy of the refined Hulu record to the editor that can be shared with the reviewers, which will allow them to verify the quality of the new record, and validate our selection of tie-points.

2- It is very difficult to understand how the link was done to the Hulu chronology. In the text, the authors explain that they use either warming or warming + cooling. When looking at Tables 1 and 2, it is clear that the link to Hulu has been made only through warming but cooling are linked to NorthGRIP chronology only. If the authors claim that there is a direct relationship between Hulu  $\delta^{18}\text{O}$  and WAIS CH<sub>4</sub> and/or NorthGRIP  $\delta^{18}\text{O}$  for the warming, why should it not be valid for cooling ? Actually, when looking at figure 5, the shapes of events recorded in Hulu  $\delta^{18}\text{O}$  does not always reflect shapes of CH<sub>4</sub> and NorthGRIP  $\delta^{18}\text{O}$  of the same events (e.g. shoulder at 59.5 ka BP in the Hulu record). This raises question on the correspondence between Hulu variations and CH<sub>4</sub> and/or Greenland water  $\delta^{18}\text{O}$  records. This correspondence should be much more discussed in this paper before giving this ice core chronology based on speleothem dating.

In all records of abrupt DO variability, the DO interstadial onset (associated with Greenland warming) is much a more pronounced and abrupt than the interstadial termination (associated with Greenland cooling). This is also true for the Hulu record. The age of the DO warming

transitions can be pinpointed much more reliably than the age of the DO cooling transitions. As we note in the text, Hulu provides strong constraints on the absolute age of the events, but not on their duration. Our strategy of uniformly stretching the GICC05 chronology by 0.63% ensures that we match the Hulu absolute age constraints in an average sense, while still retaining the timing structure of stadial and interstadial periods as given by GICC05.

In summary, evaluating the timing of the Hulu DO interstadial terminations would provide us with absolute age constraints that are less reliable than those already obtained for the DO interstadial onsets, and with (inter-)stadial durations that are less reliable than those already obtained from GICC05.

In response to this comment we have clarified the manuscript in 3 places.

At the end of the first paragraph of section 4.4 we now note:

“In the Hulu data, as in other records of DO variability, the interstadial onsets are more pronounced and abrupt than their terminations. We therefore only use the timing of the former as age constraints, as they can be established more reliably.”

And further down in section 4.4:

“Note that the GICC05 x 1.0063 target chronology only respects the Hulu age constraints in an average sense; the age of individual events differs between Hulu and our target chronology by up to 180 years.”

In section 4.5 we clarified:

“Because the duration of (inter-)stadial periods is well constrained in the layer-counted GICC05 chronology, using both the NH warming and NH cooling tie-points results in a more robust chronology. The duration of (inter-)stadial periods is 0.63% longer in WD2014 than in GICC05, which is well within the stated GICC05 counting error of 5.4% (31.2–60 ka interval).”

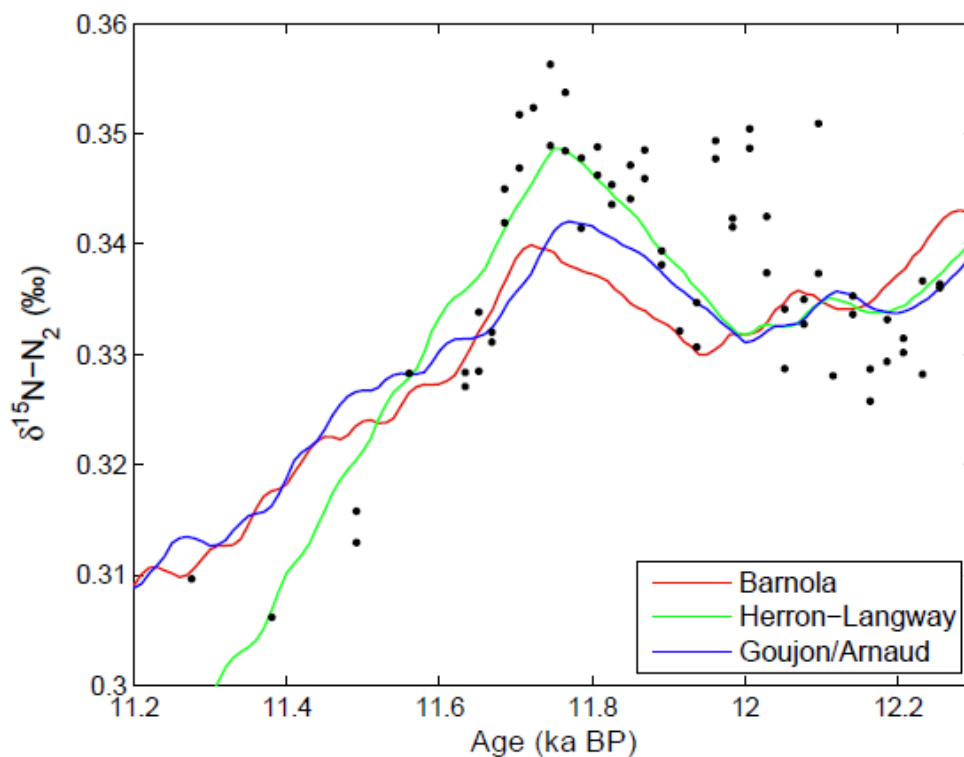
3- A wealth of firnification models have been developed over the last 30 years. Why then have the authors chosen to use the Herron and Langway model which is one of the oldest model with only empirical parameterization? The author states that they have compared this model with other firnification models but no comparison is shown which could have been useful to quantify the uncertainty in Dage calculation due to the use of a particular model.

In response to this reviewer comment, we have performed our inverse firn modeling approach using the firn densification physics from Arnaud et al. [Arnaud et al., 2000], which is also the physics implemented in the commonly used densification model of Goujon et al. [Goujon et al., 2003]. This model is based on a description of the physical processes of firn densification,

rather than on an empirical parameterization, in line with the reviewer request. The alternative  $\Delta$ age solution is shown in Fig 3 on top of the  $\Delta$ age solutions found in the (Herron and Langway-based) sensitivity study, and a brief description of the model is included in Appendix A.

On average, the  $\Delta$ age found with Arnaud et al. firn physics is 19 years smaller than that found using Herron-Langway firn physics (about 7 % of the modeled  $\Delta$ age). The Root Mean Square (RMS) difference between the two solutions is 35 years, which corresponds to 0.63 times the estimated  $2\sigma$  uncertainty. We state this in the revised Manuscript.

The Herron-Langway model is preferred because the internally consistent solution for temperature, accumulation and ice flow associated with the H-L model provides a better fit to borehole temperature data than solutions associated with the Arnaud model. Furthermore, the Herron-Langway model is more successful in simulating the magnitude of the  $\delta^{15}\text{N}$  signal that accompanies the 12ka accumulation anomaly at WD; this proved more problematic with both the Arnaud and Barnola firn densification models (see figure below this paragraph), suggesting the latter models are not sensitive enough to accumulation variability.



The high-resolution record of WD  $\delta^{15}\text{N}$  is still a work in progress, and as such our conclusion regarding the superior performance of the H-L model is still tentative. We prefer not to include this preliminary figure in the peer-reviewed literature at this early stage.

4- The calculation of  $\xi(t)$  at the bottom of p. 3545 and the top of p. 3546 and in figure 2 is unclear. Please rewrite more clearly how the accumulation rate scenarios are determined. I think that it may be useful to display the two  $A_{init}$  scenarios on Figure 2 in addition to the final  $A(t)$  scenarios / or show the  $\xi(t)$  functions.

Following the reviewer's suggestion we now also show the two  $A_{init}$  scenarios in figure 2, by adding an additional panel.

5- The discussion l. 7 – l. 23 on p. 3554 is difficult to follow without the Hulu data.

See our comments above regarding the publication status of the refined Hulu record. The manner in which the tie-points were derived is explained in detail in section 4.3. A plot of the midpoint evaluation process is shown in Figure 5, which directly shows a part of the Hulu data. All the tie-points used in the NGRIP-Hulu comparison are provided in Table 1. We believe that all the “ingredients” of the discussion on lines 7-23 are thus well explained.

In response to this reviewer comment we have further clarified the discussion on lines 7-23 in two places to better guide the reader:

“A plot of the Hulu-NGRIP age difference is shown in Fig. 6, where the error bars denote.....”  
was changed to:

“In both the NGRIP and Hulu  $d^{18}O$  records we have determined the ages of the midpoints of the DO transitions (Fig. 5; Table 1); a plot of their difference (Hulu age minus NGRIP age) is shown in Fig. 6, where the error bars denote .....”

At the end of the discussion we added:

“Note that the GICC05 x 1.0063 target chronology only respects the Hulu age constraints in an average sense; the age of individual events differs between Hulu and our target chronology by up to 180 years.”

6- p. 3555 : there are some inconsistencies in the text when you discuss the phasing between CH<sub>4</sub> and Greenland temperature (in phase or not ? l. 10 and l. 17). Baumgartner et al. Have clearly identified lags of methane over Greenland temperature over DO 5, 9, 10, 11, 13, 15, 19 and 20.

When we wrote that Greenland  $\delta^{18}O$  and CH<sub>4</sub> change “in phase”, we meant that they are in phase on the millennial timescales of the DO oscillations – when one investigates this claim on the decadal time scales this is obviously untrue, as the reviewer notes. We agree that our use of the term “in phase” was sloppy. We removed “in phase” from line 10, which now reads:

“Moreover, CH<sub>4</sub> emission changes are near-synchronous with Greenland δ<sup>18</sup>O variations, which they lag by only a few decades on average [*Baumgartner et al.*, 2014; *Huber et al.*, 2006; *Rosen et al.*, 2014]. Since CH<sub>4</sub> emissions are closely linked to tropical hydrology, this corroborates the notion that any time lags between NGRIP and Hulu are on decadal time scales.”

The updated MS thus consistently notes the decadal lag of CH<sub>4</sub> behind Greenland δ<sup>18</sup>O.

7- The discussion is very disappointed. Indeed the authors suggest many applications but do not show any. At least one figure showing the seesaw relationship of WAIS vs NorthGRIP should be added since the new chronology is partly linked to the GICC05 chronology.

In the revised MS we elaborated on the discussion of the phasing of CO<sub>2</sub> and Antarctic climate. We now provide values for the deglacial onset of the CO<sub>2</sub> and CH<sub>4</sub> rise in the WD2014 chronology, based on the records published by Marcott et al. [*Marcott et al.*, 2014].

The seesaw relationship with NGRIP is an important result of the WAIS-Divide ice core. That result, and its climatic implications, is the subject of a separate manuscript aimed at a broad audience, authored by the WAIS-Divide community. We have chosen to present the technical aspects of the chronology and Δage reconstruction in this paper, which is aimed at specialists in ice core science. We have included a reference to the upcoming WAIS-Divide community paper discussing the bipolar seesaw timing, which is currently in revision.



## Response to reviewer #2 (Dr. Johannes Freitag)

We want to thank Johannes Freitag for his kind evaluation of our work, and for his constructive comments. Below we reproduce his comments in red, with our responses in black.

By reading the paper one gets the impression that dating of the deep part of WAIS-D is solved and quite robust even for the estimates of temperature and accumulation rate. Most convincing is in this context Figure 1 where the overlap between the estimates of two different methods for the accumulation rate,  $d_{15N}$  and Dage is plotted. However, one parameter in the whole dating procedure is not shown: the thinning on which their approach based on (and the comparing model outputs of the Parrenin Ddepth method as well).

The thinning function we use is based on a simple 1-D ice flow model. We have now added a plot of the thinning function in Fig 1, and added the following detailed description to the text:

“The 1-D ice flow model calculates the vertical ice motion, taking into account the surface snow accumulation, the variation of density with depth, and a prescribed history of ice thickness. Vertical motion is calculated by integrating a depth-profile of strain rate and adding a rate of basal melt. As in the model of Dansgaard and Johnsen (1969), the strain rate maintains a uniform value between the surface and a depth equal to 80% of the ice thickness, and then varies linearly to some value at the base of the ice. This basal value is defined by the "basal stretching parameter"  $f_b$ , the ratio of strain rate at the base to strain rate in the upper 80% of the ice column. The basal ice is melting, so part of the ice motion likely occurs as sliding. The along-flow gradient in such sliding is unknown and thus so too is the parameter  $f_b$ . We overcome this problem by making both the current ice thickness and the basal melt rate free parameters when optimizing models with respect to measured borehole temperatures. Because the basal melt rate and the  $f_b$  parameter affect the vertical velocities in similar fashion, the optimization constrains a combination of melt rate and  $f_b$  that is tightly constrained by the measured temperatures. Thus we find that varying  $f_b$  through a large range, from 0.1 to 1.5, changes the reconstructed temperature at LGM by less than 0.2°C. (Temperatures prior to the LGM are determined relative to those at LGM by isotopic variations, so this number applies further back in time as well.) Effects of the prescribed ice-thickness history are likewise minor; assuming a 150 m thickness increase from LGM to 15 ka changes the reconstructed temperature at LGM by less than 0.2°C compared to a constant thickness. Note that the 1-D flow model used here is simpler than the one used by Cuffey and Clow (1997) in that it does not attempt to calculate changes in the shape of the strain rate profile; the unknown basal sliding motion at the WD site negates the usefulness of such an exercise.

One output of the 1-D flow model is the strain history of ice layers as a function of depth and time. The cumulative strain is represented by the thinning function  $f_\lambda(z)$  \citep{Cuffey2010}, the

ratio of annual layer thickness at depth in the ice sheet to its original ice-equivalent thickness at the surface when deposited. The modeled thinning function is shown in Fig. 1e (solid line). In the deep part of the ice sheet  $f_\lambda(z)$  becomes increasingly uncertain as the unknown basal melt rate and  $f_b$  become the dominant controls. Here we optimize the model by comparing accumulation rates derived from  $f_\lambda(z)$  with those implied by a firn densification model and the measured  $\delta^{15}\text{N}$  of  $\text{N}_2$ . While this has little effect on the temperature history reconstruction, it provides an important constraint on calculated basal melt rate, an interesting quantity for ice dynamics studies. Our analysis of basal melt rates and further details of the temperature optimization process and 1-D flow modeling will be provided elsewhere (Cuffey et al., in preparation)."

The amount of added details justified a restructuring of the manuscript; we now discuss the ice flow model and temperature reconstruction in their own subsection to improve the overall flow of the manuscript.

In the supplement of the cited publication of WAIS-Divide Project Members (Nature, 2013) I found some data to infer the thinning function at least for half of the time interval in the overlap period (14-23ka BP). Attached to that review you will find a graph displaying the thinning function versus normalized depth (depth divided by total core length). The thinning factor during the glacial period is surprisingly very high in comparison to the earlier Holocene (almost 0.2 difference!, purple curve) or in comparison to the ideal case of constant thinning rate (blue dotted line) or even in comparison to the EDC thinning (red curve) of the same depth. I am not an expert but it seems that it is important to discuss why the thinning (higher thinning factor) of older/deeper ice is much less than younger/shallower ice. I would rather expect the opposite trend that the thinning is higher (lower thinning factor) in deeper ice and especially in glacial ice than in Holocene ice due to the proposed softness of impurity loaded ice.

The thinning function reconstructed by the reviewer is very different from the one we use (revised manuscript Fig. 1e). The reviewer (Johannes Freitag) was kind enough to identify himself, and so we contacted him to find the source of this discrepancy. He discovered an error in his calculations that led to the unusual structure he found in the thinning function. After correcting the error, the thinning function he reconstructs no longer contains the spurious structure. This issue is therefore based on a misunderstanding, which has now been resolved. The editor (Hubertus Fischer) was cc-ed on our email exchange with the reviewer, and is aware of this resolution.

On the other hand the results of the sensitivity study of the authors about the impurity effect on densification respectively accumulation rate (Figure 4, blue curve) show that in the Glacial period the accumulation rate would be enhanced by a factor of roughly 1.7 to fulfill the

constrains for  $\delta^{15}\text{N}$  and temperature. If we assume that there is an impurity effect during the Glacial at WAIS-D (what is negated by the authors so far) the thinning function would be changed to a much more continuously decreasing function (in the attached figure shown as green line) with depth what in my opinion is much more expected and similar to derived thinning functions of other deep ice cores and even to the ideal case. By including the impurity effect in the densification model one would change the glacial accumulation rate by the factor of about 1.7 (if one relies on the temperature reconstruction) and would only slightly change age by about 200 years (see Figure 4, upper and lower panel). These changes would have not much influence on the chronology at all. I am sure that the authors could give more arguments for the flow model that they use for calculating the thinning function.

As mentioned above, we now provide a more detailed description of how the thinning function was calculated using a 1-D flow model.

I would suggest that they could add a short discussion about the reliability of the flow model for that deep part of the ice sheet.

The thinning function in the deeper part of the ice sheet obviously has a larger uncertainty. We have calibrated the ice flow model by optimizing the fit between the accumulation rates implied by the  $\delta^{15}\text{N}$ /densification model, and those implied by the age constraints.

We have added the following text to the manuscript:

“In the deep part of the ice sheet  $f_\lambda(z)$  becomes increasingly uncertain as the unknown basal melt rate and  $f_b$  become the dominant controls. Here we optimize the model by comparing accumulation rates derived from  $f_\lambda(z)$  with those implied by a firn densification model and the measured  $\delta^{15}\text{N}$  of  $\text{N}_2$ . While this has little effect on the temperature history reconstruction, it provides an important constraint on calculated basal melt rate, an interesting quantity for ice dynamics studies.”

Or do we see here the impurity effect in the WAIS-D deep ice core?

Impurities do probably affect the rheology/viscosity of the ice, but it is uncertain how this would manifest in the thinning function. We performed experiments in which we linked the ice viscosity to the impurity loading, but it did not change the thinning function significantly; certainly not by the amount suggested by the reviewer (factor of 1.7). We did not succeed in finding a flow model/thinning function that was consistent with both the borehole temperature profile and high dust sensitivity in the firn densification model.

Technical comment:

Figure1: Unit of the axis label should be Acc rate ( $\text{m ice a}^{-1}$ ) instead of Acc rate ( $\text{m a}^{-1}$ ).

We have corrected this.

## References:

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- Veres, D., et al. (2013), The Antarctic ice core chronology (AICC2012): an optimized multi-parameter and multi-site dating approach for the last 120 thousand years, *Clim. Past*, 9(4), 1733-1748.