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-$\delta^{18}O$ (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 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."

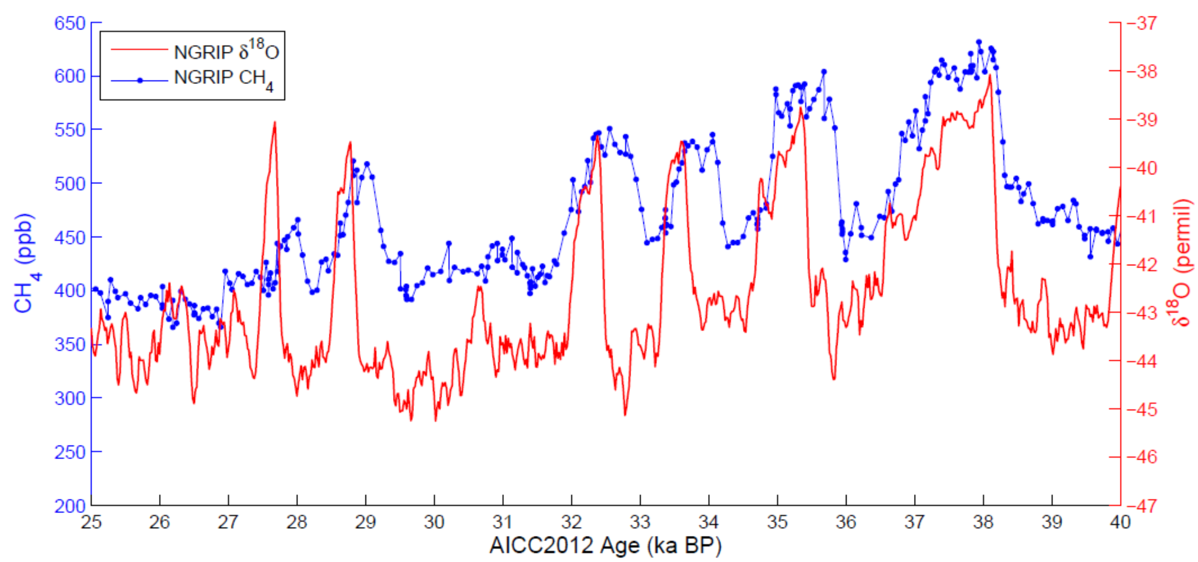
Bubble reformation is associated with hydrate formation. From our reading of Bendel et al. [2013], the issues of increased layering in the LGM, and that of the bubble-hydrate transition (and subsequent bubble reformation?) are two separate issues. The images used to map the bubble distribution were taken 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_4 and $\delta^{18}O$ 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_4 over $\delta^{18}O$ for DO 3-7. This is certainly incorrect given what we know about the CH_4-climate phasing from $\delta^{15}N$ [*Baumgartner et al.*, 2014; *Huber et al.*, 2006; *Rosen et al.*, 2014]. We suspect this error is due to the fact that NGRIP $\delta^{15}N$ 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.



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