Dear Hubertus Fischer,

Thanks for your detailed, constructive suggestions for improving the manuscript further. We really appreciate the time and effort you have invested in the review process. We have implemented the suggested changes, including the addition of a figure showing the full revised Hulu speleothem record in the main manuscript. A detailed response to your comments is given below.

Kind regards,

Christo Buizert, on behalf of the authors

Dear authors

Thanks for your careful revisions. I am overall satisfied with your changes, but have a few more comments, which require some more work before acceptance of the paper in CP.

The most important point is the criticism of reviewer #1 and my own assessment that it represents a problem that the Hulu cave record you are using, is not publicly available yet. Moreover you cite also a series of other papers "in preparation" or "in review", which are not available. This issue has also been discussed by the chief editors of CP and we come to the following conclusion:

In case of papers "in preparation" or "in review", which are not at the heart of your manuscript, papers "in review" can be cited like that, papers "in preparation" should be better cited as "personal communication". As for the Hulu record, this represents an absolutely crucial part in your paper and it is not acceptable that the readers of CP cannot replicate and assess your synchronization. The references you are citing with respect to Hulu are unfortunately not sufficient as the Southon paper only covers the last 27 kyr. Accordingly, it is essential that you include a plot with this record that illustrates the synchronization, favourably in the main text of your manuscript or as a supplement. Without this, acceptance in CP would have to be delayed until the new Hulu record is published elsewhere.

We have now included a figure showing the full revised Hulu d^{18}O record in the manuscript (Fig 5). This will allow readers to evaluate the tie-points we selected. We furthermore removed references that are "in preparation"

My further comments are minor and are only meant to further improve an excellent paper:

Line 90: you could add here the EPICA, 2006 paper

We have added the suggested reference.

Line 157: I assume you mean „replicate samples", if so please specify here
Yes, that is what we meant.. This has been clarified.

Concerning your comment on measured Ca concentrations in reply to my previous editoral comments, the problem is a potential high blank value, which would not be in the ice, but would be added externally during the measurement. This is why Freitag et al use the LOD as lower limit, below which the Ca concentrations are not „in the ice“. As you use a similar CFA method (although based on ICPMS) I am quite confident that your limit of detection (LOD) or blank contribution is not substantially higher. So I do not expect there is a problem but you should specify the LOD in your manuscript.
Unfortunately this information cannot be found in McConnell 2002 and 2007 for Ca derived using ICPMS.
One commonly used way to define the detection limit is 3 times the standard deviation of the blank. Typical standard deviations in Ca of the blanks during the continuous analyses of WD were ~0.03 to ~0.05 ng/g so 3 times that is 0.12 to 0.15 ng/g. To be conservative we report the higher value (0.15 ng/g) in the updated MS.

As such our detection limit is indeed lower, yet comparable to that from the work in Freitag et al., and there should indeed be no issues in using the values recommended by Freitag et al.

Note that in the formulation by Freitag et al. one requires to set a lower limit, below which densification rates become insensitive to Ca concentrations. Without this, a theoretical Ca-free ice column would no longer densify at all, as the activation energy goes to infinity.

Line 265: para-metrizes
The hyphenation was included automatically by the latex routine, and should be fixed in typesetting. To be sure we removed the term “parameter” in that sentence, as it was not needed there.

Line 271: The sentence in parentheses wasn’t clear to me. I would just delete it

We meant to state that pre-LGM temperatures are not constrained by the borehole, except in an average sense. The relative temperatures are thus controlled by the isotopes only. We have removed the sentence as suggested.

Line 275&276: the LGM

Has been fixed.

Line 299: Cuffey et al, in prep (see above)

We have removed the Cuffey (in prep) reference as suggested. Citing a personal communication seems inappropriate given that Kurt Cuffey is already an author on the manuscript (and we don’t mean to suggest that he communicates personally with himself)

Line 408: sensiti-vity

As in the earlier instance the hyphen is not in our original script, and the hyphenation is included automatically by the LaTeX routine. This can only be fixed in typesetting; we will check the proofs to make sure the text is hyphenated correctly.

Caption figure 3 last sentence: means

Has been fixed.

Concerning your reply to my comment on the paper by Bendel et al.: I have to apologize for being sloppy. What I meant is that the bubble distribution evidence for a stronger layering in glacial ice by Bendel et al. is most interesting but appears yet to be circumstantial. I could imagine that fast reformation/relaxation could be an issue in this ice, where bubble nucleation may be strongest in dusty layer, which then would not necessarily imply different density/porosity layering during bubble close-off. There is no action needed from your side. The wording should only make clear that a stronger layering in the glacial is not a proven fact yet.

Thanks for the clarification, now we understand your argument. We agree that more observations and different lines of evidence are needed to establish the interpretation by Bendel et al. more firmly.

We have weakened the statement in the text by replacing "was more pronounced" to "may have been more pronounced".

Line 580: core, where … not available, we

Has been fixed.

Line 617: time series

Has been fixed (here and in two other places).

Line 721: I would suggest a sentence like the following one: "Note that this approach represents only a first order correction of a growing offset between GICC05 and Hulu, while non linear temporal changes in the counted dating error may exist from one tie point to the next (Fleitmann, D., et al. (2009), Timing and climatic impact of Greenland interstadials recorded in stalagmites from northern Turkey, Geophys Res Let, 36(L19707), doi:10.1029/2009GL040050)

We have added this sentence and the reference to Fleitmann et al; we added it one paragraph earlier than suggested. (line 691 rather than line 721).

Line 783: the stretched GICC05

Has been modified

Line 785: wrong Figure number

Has been fixed

Another comment from my side concerning the 2nd paragraph on page 13: The deviation is largest below 60 kyr, i.e., below the counted part of the GICC05 age scale and below the last Hulu tie point. Accordingly, the age of the WD-NGRIP tie point at 65 kyr may be not so well constrained as the other tie points, having a direct influence on the accumulation between 60-65 kyr. May be, you want to mention that.

We now note this in the text. Curiously, on the AICC2012 chronology DO 18 is even older than on GICC05, which would imply even thinner layers at WAIS Divide.

Line 844: I would suggest to add a black dashed line in Fig. 7c that does not include the uncertainty in absolute ages.

We have added a black dashed line as suggested, to show the uncertainty in the relative ages.

Line 1000: Is there a reason why you use a different equation here than Freitag et al., 2013? It would be nice , if you could show how one can come from equation (4b) in Herron-Langway to (4c). In equation (4c) it actually says dp/dt not drho/dt in the Herron-Langway paper, I assume it is a typo in the Herron Langway paper.

There is indeed a typo in the original 1980 HL manuscript, Eq. 4c (the p should be a rho). We use HL Eq. 4c rather than 4b, because it can be used to run the HL model dynamically (this is not possible with Eq. 4b as it depends on the accumulation rate A, which is assumed constant in the HL equations). In the approach by Johannes Freitag, Ca loading influences the HL activation energy, which is the same for HL Eqs. 4b and 4c.

Ed Waddington has repeated the derivation of Eq 4c from 4b, and has assures us it is valid.

Line 1039. Please explain what you did to get the relationship between alpha and beta.

We have now provided a more detailed explanation for our choice of alpha and beta. In revising the text, we now also use a slightly different equation, alpha = 1.007/(1-beta*log[0.8/0.5]), which makes more sense from a mathematical point of view but gives identical results (for beta << 1, as is the case here).