Climate of the Past Discussions

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Comment on cp-2021-116

Anonymous Referee #3

Referee comment on "Continuous synchronization of the Greenland ice-core and U-Th timescales using probabilistic inversion" by Francesco Muschitiello, Clim. Past Discuss., https://doi.org/10.5194/cp-2021-116-RC3, 2021

The work presents a new alignment of Greenland ice-core and Hulu Cave records, aiming to quantify the continuous chronological offsets between the U/Th-dated Hulu Cave record and the annual-layer-counted ice core records. The work is highly relevant to a wide audience and presents some interesting new thoughts of how to align the records, but also suffers from three main weaknesses (mentioned in detail below). The results confirm previous low-resolution studies of the chronological offsets between the U/Th-dated Hulu Cave record and the ice-core GICC05 time scale but also suggests that on shorter time scales, the GICC05 ice-core time scale suffers from very large and fast-varying biases that do not correlate with climate and would – unless explained – mean that GICC05 essentially should not be trusted. I do not think that the manuscript sufficiently backs these controversial results up.

Main weaknesses:

1) Greenland and Hulu Cave data covariation.

The study is based on correlation of NGRIP Deuterium excess data with Hulu d18O. The reason is explained in line 157-160 and may be true for the response to the H events (as it is for D-O events), but it is a critical and very, very daring assumption that Hulu d18O and NGRIP D excess trace the same smaller-scale climatic changes. I do not think that this has been demonstrated previously, and the manuscript does not provide compelling evidence that the smaller-scale features correlate significantly. As I understand, Figure 1 does not separate the hosed D-O-scale/H-scale variability from smaller-scale variability. I think the manuscript will either have to demonstrate with statistical back up that the Hulu d18O signal correlates significantly with at least one of the Greenland records (e.g. d18O, Calcium, or D excess) also over smaller-scale (and preferably non-forced) changes, or refrain from presenting a match of the records across these rather long periods which do not have D-O- or H-scale variability. That would challenge the concept of a "continuous" transfer function.

2) Speleo dating uncertainty

Although the Cheng 2016 data set definitely represents an advance relative to earlier work (especially in terms of resolution), the lack of access to the underlying data and age-model details is a problem. The uncertainties of individual U/Th age determinations are small, but as demonstrated e.g. by the speleothem age-modelling work of Corrick et al., 2020, different but realistic assumptions about growth rates, interpolation methods, purity of samples etc. can lead to differences in ages at a certain speleothem depth that are larger than the raw U/Th age uncertainties (sometimes several times those). Especially at climatic transitions which are not located close to a U/Th-dated sample, this can lead to systematic dating offsets of D-O event onsets. If taken at face value, this forces the duration of the stadials and interstadials to change very significantly and way beyond what it compatible with the constraints from ice-core annual-layer counting (which is exactly what is seen here, described as an ice-core annual-layer-counting bias, line 139-141). This is why Buizert et al., 2014, stretched GICC05 by 1.0063 to fit the Hulu constraints ON AVERAGE and not on a transition-to-transition basis. This can likely be done better with Cheng 2016 data, but both the true age uncertainties from all sources (and not only the raw U/Th age uncertainties) and uncertainty due to that the D-O onset are not always similar between records must be included (and it is not clear if/how this is presently done). I recognize that the lack of access to the full raw U/Th data set by Edwards and Cheng makes it difficult for the author to properly account for the full uncertainties, but I believe that the current manuscript overemphasizes how tightly this one particular record (the Cheng/Edwards data) with its implicit assumptions about growth model, sample purity etc. can properly constrain individual D-O onset ages with realistic uncertainties, and that this introduces unrealistic stretching/compression of the ice-core time scale. Another way to address this problem would be to use data from other speleothems (e.g. the data from Corrick et al., 2020), and investigate if the results are reproducible under other assumptions.

3) Continuity

The method rests on an assumption that the records can be matched continuously, i.e. that there is robustly correlatable information everywhere in the record. There is always a best match between records being correlated, but the method seems not to address whether “best” is “good enough”. It thus becomes impossible for the reader to figure out in which sections the correlation is statistically significant and where there is nothing but noise (or local climate variability etc.) resulting in a transfer function that essentially just bridges between sections with statistically significant correlation.

Specific comments:

Line 84-86:

Svensson et al., 2020, should be properly reflected by the discussion. It shows that the bipolar lags are indeed smaller than previously suggested, but not that "ice-core data reveal bipolar synchrony during abrupt cooling and warming in Greenland". Firstly, Svensson et al. documents a lag of ~100 years between the climate impact in North and South, and secondly, even when considering the faster mechanisms e.g. documented by Markle et al., it is in my opinion not correct to talk about "synchrony" when the changes are so differently expressed in the different regions.

Thus, there is some basis for concluding that there are "fast global atmospheric reorganizations" (e.g. in the Markle paper), but it is a stretch to say that these "propagate within a decade or less" as stated, when talking about a global context, even though the reorganizations may have happened faster regionally .

Line 86-88:

The Pedro paper has many relevant references to work on ITCZ and monsoon responses, but it deals with bipolar seesaw dynamics and is does not seem like a good reference for this statement.

Line 89-95:

Corrick et al. 2020 is cited above, but not here, where it is most relevant.

Line 119-123: This statement needs a much more thorough discussion.

Line 132-141: Se the main points discussed above. I agree that there is a need to improve the transfer functions, but I am not convinced that the presented methods is doing this in a robust way.

Line 182-184: The Svensson tie points do not constrain the section approx 16.5 - 24 ka. At the very least, the linear interpolation must increase uncertainties in the transfer function that can be derived, but it must also be dealt with in the text (instead of describing them as "densely-spaced volcanic tie points") if the objective still is to derive a continuous transfer function across GS-2 and GS-3.

Line 189-194: It would be good to give precise details of how the annual layer thicknesses are computed for each core, what the related uncertainties are, and whether these reconstructions realistically capture likely past changes in precipitation. This could be in a supplement/appendix.

Line 218: Mention whether the DCF value is likely constant and climate-independent. If not, how does this influence the total uncertainty?

Line 388: Why 1.75? This limit seems pretty arbitrary.

Line 287-293 and section 2.3.4: It seems like the method allows that sections of GICC05 are stretched/compressed to fit the assumed perfect Hulu time scale. If needed, sections close to each other can be modified in opposite ways (even though the stretching is smoothed as described in 2.3.3). This seems physically implausible given the nature of the GICC05 counting process: There are likely biases in GICC05, but these are not likely to change abruptly on short time scales (except between interstadials and stadials) because neither the data basis nor the counting method changed quickly. This is mentioned in line 391-395, but I think the results are not at all "approximating the layer-counting structure of the GICC05 timescale" but in stark contrast to the layer counting procedure.

This is also what is seen on Figure 7c: The results indicate that at 26-28 ka, the GICC05 counting bias changes from 20-30% in one direction to 20-30% in the opposite direction. A similar slightly smaller feature is seen around 38 ka. These biases do not correlate with climate: e.g., the phases of largest overcounting happen during a stadial dust peak (26 ka) and during a low-dust interstadial (38 ka). If these features are real and unexplained, there is really no reason to trust GICC05 anywhere in the glacial. Extraordinary claims require extraordinary evidence, and I do not think that the manuscript provides sufficient evidence that these features are real, i.e. cannot at least to a large extent be attributed to weaknesses in the assumptions of Greenland - Hulu Cave climate correlation and underestimation of the total uncertainty of the Hulu Cave record, or alternatively, that the synchronization method does not produce statistically significant results in these sections.