(R2 comments in normal typeface; **responses in bold**)

I would like to thank the reviewer for the detailed commentary and especially for the time he took to analyse the Hulu 14C data. This is very much appreciated. In the following I will address the main points raised by the reviewer.

The present study exclusively relies on the Hulu Cave dataset. While I agree that this is the most suitable dataset for this study in terms of resolution and measurement uncertainty, I am still doubtful about the signals the author uses for synchronization which may very well be simply noise.

Hulu Cave 14C

The Δ14C-variability in the 150-500 yr frequency band is up to an order magnitude larger than what we observe during the Holocene. This is important! The whole paradigm behind the synchronization of radionuclides is, that we are synchronizing production rate changes driven by solar activity or the geomagnetic field. On these frequencies, we should be mainly looking at solar activity changes. So, at face value, this would imply that the Sun was a lot more variable? 10Be does not seem to support this. This requires a detailed evaluation whether this could be i) due to the carbon cycle, ii) lower geomagnetic field intensity, or iii) simply noise.

The points raised by the reviewer are fair and having examined Fig. R1, I acknowledge that beyond deglaciation there is probably no robust phase information to constrain the 14C-10Be synchronization at sub-millennial timescales. The reviewer requested a detailed evaluation of the carbon cycle effects on 14C in the <5ka frequency band, the impact of geomagnetic field intensity, the effect of climate-related variability in the Hulu 14C record, the potential influence of the DCF on the phase of signals, etc. These are valid suggestions. However, considering the significant additional time required to meet these requests and the fact that the results are unlikely to confirm that production rates dominate the multi-centennial frequency band, I believe these additional analyses would better fit the scope of a dedicated paper using ad-hoc carbon cycle sensitivity experiments. Thankfully the main conclusions of the present study do not depend on COSMO, which mainly served as supporting evidence. In light of this consideration and the reviewer's comments I think a more feasible approach is to shift the focus of the paper to the CLIM results and address issues and concerns surrounding the climate synchronization instead.

If the editor considered this to be important, I would still be willing to present the COSMO results for selected intervals such as in Adolphi et al. (i.e. deglaciation, 21-24ka). However, preliminary results suggest that they would not significantly improve the estimates presented by Adolphi and colleagues.

10Be

Regarding the inclusion of the WAIS record into the stack between 16.5 and 24.5 ka, I wonder how the author treated the fact, that Svensson et al. (2020) provide no tie-points between GICC05 and WDC2014 between 16.5 and 24.5.

This is not an issue as the WDC data only stretch back to ~15ka (see Fig. 3a).

So essentially, the only record left to address 10Be high-frequency variability between 16.5 and ~40k is GRIP. I think it would be good to hence, only use this record for the high-frequency part, and use a non-weighted stack of all records for the low-frequency component.

Thank you for the suggestion. I will keep this in mind for future reference.

Furthermore, the author suggests that large 10-20% stretches of GICC05 are necessary to fit Hulu 14C. If this is true, than this impacts snow accumulation rates and thus, 10Be fluxes. Hence, before estimating the cost-function, the flux would need to be updated accordingly, and the record filtered again.

This is a valid point also raised by R1. I will take this into account in future cosmogenic synchronizations.

The synchronisation method uses a cost function based on explained variance and root mean square error. Both of these measures imply a linear relation between the two compared variables. For 14C and 10Be this assumption can be assumed to be sufficiently correct if all production rate models and carbon cycle changes are accounted for. But can we assume that deuterium excess or (the logarithm of) Ca are linearly related to Hulu d18O? Even if on the very large scale, they may respond to the same re-organization of the climate system, they respond to very different physical processes, and record different reservoirs and different processes. If this was a good assumption, why do dxs and Ca look differently? And event after synchronization there are many large differences between Hulu d18O and the ice core records especially <25ka BP. Also: it is unclear how the author combined dxs and Ca into one record. […] For CLIM, the premise of the method (linear relation) should be critically evaluated and whether this is a good assumption even outside DO-type variability.

The CLIM synchronization has been performed again taking into account a number of considerations raised by all the reviewers (see also replies to R1 and R3). First, I understand that combining d-excess and Ca2+ may be confusing, so I have decided to employ only Ca2+ –the proxy that has been used for the climate synchronization in Adolphi et al. To provide a physical basis for the assumption of a roughly linear –and synchronous– behaviour between hydroclimate shifts at Hulu Cave and Greenland Summit, I have analysed climate model output of the LGM (the greatest concern for R2) from a transient simulation of the last glacial period (Armstrong et al., 2019), a transient simulation of the last deglaciation (Liu et al., 2009), and all the available LGM experiments from the PMIP4-CMIP6 framework. The new analysis confirms the presence of a persistent hydroclimatic covariability at multidecadal and centennial timescales between SE Asia and Greenland during pleniglacial (similarly to the results presented in Fig. 1b & d), which supports the approach used for the climate synchronization at timescales shorter than DO variability. These new findings will be included in the revised version of the manuscript and the linear relationship assumption will be discussed in more detail.

Generally I wonder, how the method evaluates what a "good fit" is. There will obviously always be a best fit, but it may still not be very good. In my opinion a metric for this should be added for both COSMO and CLIM because this may give an indication of the reliability of the synchronization through time. […] It would be good to have a metric of the quality of the fit through time.

A similar point was raised by R3 and it comes as a surprise. There seems to be a fundamental misunderstanding here as Bayesian results do not require validation via deterministic metrics –something inconsistent with the framework for fitting inversion problem models and bearing no purpose in the specialised literature. Within the probabilistic approach taken in this study, it is not informative to take on the null-hypothesis framework. One could tackle the synchronization problem with the frequentist approach but that would require determining the alignment *a priori* (e.g. using several tie points) rather than inferring it *a posteriori*, which is the main motif of this research. Estimating the significance of the correlation (or any other metric) to evaluate the 'goodness' of the Bayesian fit between the Hulu and ice core data is in general not a good idea and overall incoherent with the probabilistic approach employed here.

Last but not least: The author discusses the results of CLIM only in the light of GICC05 counting errors. However, while Corrick et al. (2020) show that the different monsoon regions respond synchronously on average, differences between individual records may be in the order of centuries, likely due to low signal to noise and dating uncertainties. Since the author only uses the Hulu Cave d18O record, it seems premature to only discuss this in the light of GICC05 counting errors, as this may in part originate in the Hulu cave record instead.

This is a fair comment that has also been raised by R3. I have taken these points on board and re-estimated the climate synchronization accordingly. To minimize the noise due to local environmental factors, account for dating errors, and altogether better represent the large-scale hydroclimatic imprint of the monsoon system, I have applied a Monte Carlo Empirical Orthogonal Function (MCEOF) (e.g. Anchukaitis and Tierney, 2013) approach that integrates all the available speleothem d18O records from SE Asia spanning the last 50ka. I utilized 17 d18O timeseries that were used to define the Asian Summer Monsoon region in Corrick et al. (2020). The compilation includes the U-Th age determinations underlying the speleothem chronologies.

The method uses iterative age modelling of the available U-Th ages and eigen-decomposition of the d18O records to isolate the common d18O patter and estimate uncertainties. By randomly resampling the age uncertainties of each speleothem record, I generated a 1,000 member ensemble of the first leading mode of the MCEOF analysis (EOF1), which is dominated by the characteristic regional monsoonal signal of the last glacial period (note that each MC iteration was set such that the sign of the EOF1 is consistent across the ensemble). The Monte Carlo approach results in some temporal smoothing of the EOF1 but does not affect the fidelity of millennial-scale trends or shorter features. The CLIM synchronization was then performed in a way such that for each MCMC iteration the algorithm employs as input NGRIP Ca2+ and as target a randomly resampled (with replacement) EOF1 from the 1,000 member ensemble of the MCEOF analysis described above. It should be noted that the speleothem compilation used here includes an updated version of the Hulu Cave data based on the newly published U-Th estimates presented in Cheng et al. (2021), which significantly improve the resolution and dating precision of the cave record across Heinrich Stadial 4 and bring the d18O data more in line with the trajectories observed in other regional records.

Altogether, the MCEOF approach and the updated Hulu Cave chronology improve the robustness of the CLIM synchronization and provide a more representative estimate of the alignment uncertainties.

References

Anchukaitis, Kevin J., and Jessica E. Tierney. "Identifying coherent spatiotemporal modes in time-uncertain proxy paleoclimate records." *Climate dynamics* 41, no. 5-6 (2013): 1291-1306.

Armstrong, Edward, Peter O. Hopcroft, and Paul J. Valdes. "A simulated Northern Hemisphere terrestrial climate dataset for the past 60,000 years." *Scientific data* 6, no. 1 (2019): 1-16.

Cheng, Hai, Yao Xu, Xiyu Dong, Jingyao Zhao, Hanying Li, Jonathan Baker, Ashish Sinha et al. "Onset and termination of Heinrich Stadial 4 and the underlying climate dynamics." *Communications Earth & Environment* 2, no. 1 (2021): 1-11.

Corrick, Ellen C., Russell N. Drysdale, John C. Hellstrom, Emilie Capron, Sune Olander Rasmussen, Xu Zhang, Dominik Fleitmann, Isabelle Couchoud, and Eric Wolff. "Synchronous timing of abrupt climate changes during the last glacial period." *Science* 369, no. 6506 (2020): 963-969.

Liu, Zhengyu, B. L. Otto-Bliesner, Feng He, E. C. Brady, Robert Tomas, P. U. Clark, A. E. Carlson et al. "Transient simulation of last deglaciation with a new mechanism for Bølling-Allerød warming." *science* 325, no. 5938 (2009): 310-314.