Review of Muschitiello 2021, Climate of the Past Discussions by Florian Adolphi

Summary:

Muschitiello presents an assessment of timescale differences between Greenland ice cores and the Hulu Cave speleothems. On one hand, he uses cosmogenic radionuclides (14C & 10Be) and thus, provides an update on Adolphi et al. (2018). On the other hand, he uses climate proxy data (deuterium excess and Ca in the ice cores and d18O in the speleothem) and assumes synchroneity of both signals – an approach that has been used many times in the literature, albeit typically only on specific tie-points, such as the rapid onset of DO-events. Contrary to previous studies, he uses a probabilistic method, which continuously models the ice core chronology, and evaluates the solution by using cost-functions based on the match of cosmogenic radionuclide records (COSMO) and climate records (CLIM).

COSMO essentially confirms the results by Adolphi et al. 2018, but also finds matches between 23-27 kaBP, which results in a shorter period having to be bridged by interpolation to Laschamps. This brings down the stated uncertainty substantially. CLIM agrees with COSMO, but is more continuous. However, CLIM provides more (fast) changes to the timescale difference between Hulu and GICC05, which the author interprets as ice-core layer-counting errors that exceed the stated uncertainties of GICC05.

I appreciate that the probabilistic approach to the synchronization of radionuclides is a step ahead compared to our earlier attempts. However, I do not think that the paper provides sufficient evidence to ensure the robustness of the presented results. Many methodological details and assumptions need explicit testing and explanation and a more critical approach to the data would be advisable.

Major Comments:

14C:

When we did our paper in 2018, we focussed on structures in Δ14C that are replicated in more than one archive, and discussed differences when apparent (such as around Laschamps), and still referred to the tie-point around 21ka as “tentative” because the signal to noise is very low. The present study, however, exclusively relies on the Hulu Cave dataset. While I agree that this is the most suitable dataset for this study in terms of resolution (albeit not quite, see below) and measurement uncertainty, I am still doubtful about the signals the author uses for synchronization which may very well be simply noise (see figure R1).

1. The sampling resolution and U/Th-error of the Hulu 14C record is too low to reliably reconstruct the 150-500 year frequency band. The Nyquist wavelength is always at least at the lower bound of this filter or even larger. The author should be aware, that while frequencies lower than the Nyquist frequency may be detected, higher resolution is required to reliably estimate phase and amplitude of the signal needed for a reliable synchronization.
2. The Δ14C measurement uncertainty is always substantially larger than the variability (signal) contained in the data in this frequency band (figure R1 middle panel).
3. The Δ14C-variability in this 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. Given that the increase in variability roughly follows the increase of the measurement uncertainty, I speculate it may be the latter.

4. All these factors combined lead to the fact, that there is no robust phase information in this frequency band. Performing a wavelet analysis of 1,000 random realizations of the Hulu Cave dataset, I obtained a 95% CI of the detected absolute phases that covers nearly 180° - i.e., the full circle. It seems that only at wavelengths >1,500 years, we are seeing phase-signals above noise level.

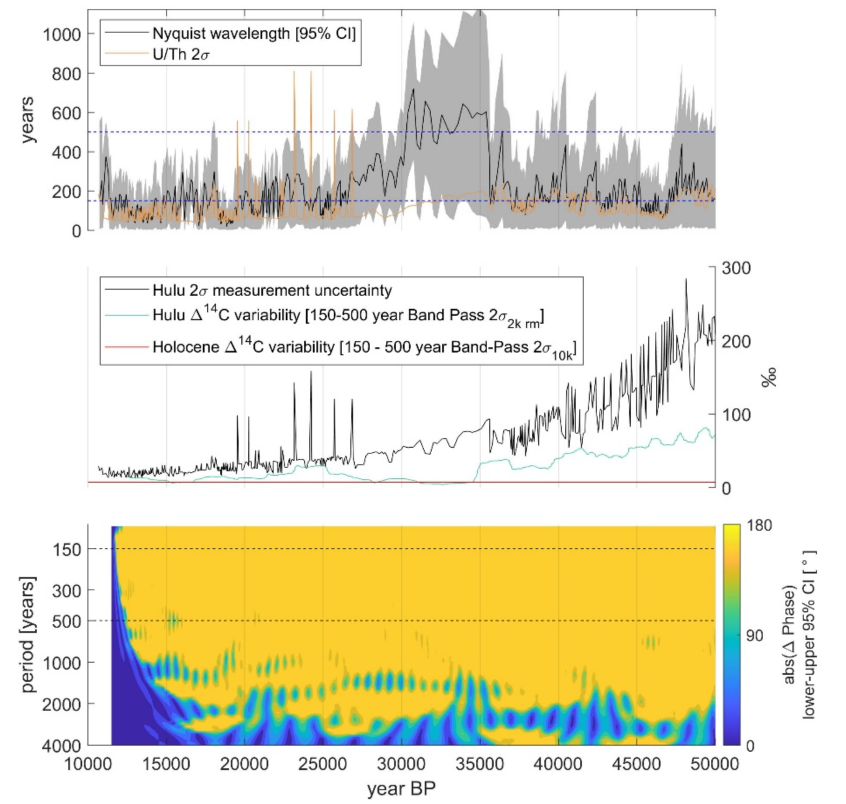

Figure R1. **Top panel:** Nyquist wavelength (black/grey) and U/Th 2σ dating error (orange). **Middle panel:** Hulu Cave Δ14C measurement uncertainty (black). Green: 2σ-variability within a 2000-year sliding window of the band-pass filtered (150-500 yr) Hulu Cave Δ14C-data. Red: 2σ variability of band-pass filtered Δ14C during the Holocene (IntCal20). **Lower Panel:** absolute phase difference between the lower and upper 95% CI of the phases detected by wavelet analysis of 1,000 random realizations of the Hulu Cave Δ14C record perturbed within its measurement uncertainties.

10Be

Similarly, for the 10Be records I think the information the data can provide should be critically evaluated. Except for the dataset by Raisbeck et al. (2017), only GRIP (whole study period) and WAIS (back to 18.5ka) can resolve the 150-500 year frequency band over extended periods of time. Hence, adding more low-resolution records to the stack will likely only induce biases to the stack. I also don't understand why the author weighs records by resolution? The low-resolution data is not less reliable and should obtain equal weight, since the goal of stacking is to remove depositional/transport-induced signals, where low resolution is quite possibly an advantage.

Regarding the inclusion of the WAIS record into the stack, 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 – a period where COSMO implies large counting errors in GICC05. If this was true, we would need to assume that WDC2014 has the same bias as GICC05, otherwise, one is stacking 2 records that quickly drift apart in their timescales.

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.

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. Generally, when filtering with such a narrow band, the filtering needs to be done on the stretched record, before calculating the cost function, since the frequencies of the record change with stretching.

Along the same lines: The author uses the Svensson et al. synchronization, but AICC12 accumulation-rates for the Antarctic records. But these two are inconsistent. The accumulation rates need to be updated accordingly.

As outlines above, I believe that the 150-500 year frequency band is likely an unreliable target. However, the <5k Band requires an updated analysis of the carbon cycle effects on 14C, since this is not negligible any longer as we are in the same timescale as DO-events and AIMs. In fact, (Cheng et al., 2018) have pointed out that they see climate related variability in the Hulu 14C record. This needs to be addressed.

What drives the synchronization between 22 and 26 kaBP? Neither of the frequency bands look like a good fit between 10Be and 14C?

Have you tested the influence of the dead carbon fraction of Hulu on the phase of signals? We have attempted this in our paper and found the effect non-negligible. Could this be included in the model?

CLIM:

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.

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.

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.

Suggestions:

I think the paper should be re-focussed on developing and testing the proposed method more explicitly and thoroughly. The method has the potential to improve the current estimates of the GICC05-Hulu timescale differences, but especially when relying only on one 14C-dataset, more work is needed to show the robustness of the results. In the high-frequency band, the influence of signal to noise needs to be addressed and most importantly: How do we explain the large amplitude of the Δ14C changes? It needs to be shown, that this is most likely production-related to fulfil the premise of the method. For the low-frequency band the effect of carbon cycle changes is likely important. It would be informative if the author showed the influence of the different targets (high-res, low-res, MCE) on the results separately and how this would change if one defined a less narrow frequency band. 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. For both approaches, it would be good to have a metric of the quality of the fit through time. I agree with reviewer one, that it would be more convenient to invert accumulation instead of age and use this information to update the fluxes before calculating the cost-function.

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