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Climate of the Past Discussions

Interactive comment on "Connecting the Greenland ice-core and U / Th timescales via cosmogenic radionuclides: Testing the synchronicity of Dansgaard-Oeschger events" *by* Florian Adolphi et al.

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Summary:

This paper provides a very thorough synchronization of the GICC05 time scale obtained from counting annual layers in ice cores, and (assumed to be) absolute U/Th dates from several (sub-)tropical speleothems via cosmogenic radionuclides with a focus on 14C, for the time period from 10ka to 45ka BP. Based on this synchronization, the timing of the DO events during this interval is compared among ice core and

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Discussion paper

speleothem records, and it is concluded that on average, no systematic lead or lag can be inferred, given the inherent uncertainties.

The paper is written very well, the subject is of great scientific importance, and the employed methodology seems accurate to me. I hence strongly support publication of this study in CP.

However, there are some instances where the presentation is not detailed enough at least for me to be able to precisely follow what is done exactly (see specific comments below). In addition, I have some slight conceptual concerns that I would suggest to be addressed prior to publication. Please note that I'm not a geochemist, so I apologize in advance for potentially trivial or irrelevant questions / concerns below.

Major comments:

1. Necessarily, some of the uncertainties are put in 'by hand', such as treating the MCE of the GICC05 time scale as 1 sigma, but also at several instances of the analysis of the cosmogenic radionuclides. This is not a critique per se, and I agree with the authors that their uncertainty estimates are probably very conservative. However, in the situations at hand, it cannot be quantified _how_ conservative, and this leads to a tricky situation: the more conservative the error estimates are chosen along the way, the harder it is to reject the null hypothesis of synchronous DO events in the different records. The final sigma reported for the average over all DOs and speleothems is 189yr, and a lot can happen with such uncertainties; the statement in the conclusion that on average the DO onsets occurred synchronously is thus misleading, I think. I'd suggest to rather emphasize here that no systematic leads or lags can be inferred given the (partly subjectively introduced) error estimates. In addition, the 189yr are not far from the delay between NGRIP and WAIS inferred to be significant by the WAIS members, would you mind to comment on this?

2. It is stated in the abstract , introduction, and in the discussion that the GICC05 uncertainties are reduced by 50-70%, but I don't understand where these numbers

come from, and as far as I can tell, they are not mentioned / explained somewhere else in the manuscript. If you compare the GICC05 MCE to the sigma of the transfer function ensemble, it might be problematic, since the MCE is not really related to a normal distribution, despite the pragmatic approach by Andersen et al.

3. It is not clear to me how exactly the interpolation in Sec. 4.4 is carried out. This is a key part of the study, and I would hence suggest to make this section considerably more detailed. It is written that the AR(1) realizations are used for interpolation, but how? You sample from the PDFs at the tie points, but how do you make sure that a given AR(1) realization, starting at one tie point, ends up close to the next tie point? Note that I might be completely off track here.

Specific comments:

p3, l83: How were the 50-70% uncertainty reduction inferred quantitatively?

p3, l91: the "Hence" suggests that the previous sentence implies the _inverse_ relationship, but I don't think it does, although I don't question the inverse relationship itself.

p6, l179: what do you mean by "more direct function of the timescale?"

p6, l190ff: Using both flux-corrected and non-corrected version of the ice core records is fine to infer systematic differences between the records via comparison to the expected error of the mean, but I find it a bit problematic to use such a stack for the synchronization; do you obtain different results when using only flux-corrected or only non-corrected versions of the records?

p7 l215: please define "cal"

p7 l221: Could you motivate the assumption of proportional 10Be and 14C production rate changes here?

Fig.2: the time scales are not equidistant, right? How do you perform the the FFT

filtering? Do you interpolate? If yes, using which method, and to which resolution?

p10, l315: It may be my fault, but where in the results section are the window length and frequencies given? Can you be more specific?

p10, l324: I don't understand this sentence: do you mean that the delay between associated peaks in different sinusoidal signals increases with wavelength? Why?

p11, l328: is the box-diffusion model by Siegenthaler et al referred to here?

p13, l284f: what do you mean by "deviations from the transition"? I find it a bit problematic to refer the reader to a paper in preparation here, since it's not clear given the presentation here, how the change-point detection is carried out. In particular, further below it becomes clear that for each potential change point, PDFs are obtained for its onset, mid point, and end point, but it's not clear how these PDFs are derived.

Fig.7: -there seems to remain quite a discrepancy between the variability of the bold grey line and the Towai treering data (green) even after synchronization, could you comment on this? - if I'm not mistaken, none of the time scales of the shown data are equidistant, how to you do the FFT filtering in this case? If you interpolate, how?

p15, l445: can you explain what you mean by "to remove offsets"? This also relates to l312 on p10; are't offsets at longer time scales potentially problematic? I guess _heuristically_ these longer-term offsets are attributed to carbon cycle changes, but a clarifying sentence would be good, I think.

Fig.8: You present the result of the synchronization, and show the PDFs for the different windows, but I think one or two extra sentences in Sec 3.4 on how exactly these PDFs are used to shift the record across the windows would be very beneficial.

Fig11: there's no inset and no blue dashed line! Also, shouldn't the four individual speleothem dates correspond to the measured (black) points of NRM/ARM?

p.19: As noted above, I don't understand how you interpolate between the tie points, the description is too brief in my opinion: by derivative of the MCE, do you mean the increments from one measured point to the next? Why do you multiply the AR(1) with these? Which "cumulative sum back in in time", i.e. from where to where? You say "strong autocorrelation", but what is the value of the parameter alpha?

p19, l 575: what do mean by "grow/shrink at a rate determined by the mce"? the latter is cumulative and hence always increasing back in time, but your AR(1) based uncertainties decrease when going back in time towards the next time point. I agree that it should decrease this way, but I don't understand the method sufficiently from the given description to understand how, specifically.

p22, l639: Here you say that you sample the PDFs for the DO onsets; am I correct in assuming that for each onset, you obtain a PDF of its dates from the change-point-detection?

p23, l679: I don't think that this study _shows_ that the counting error can be strongly correlated over extended period; please correct me if I'm wrong!

p25, l727-738: it would be good, I think, to add reference on the relation to the ITCZ position already here.

p25, l739ff: The fact that the precip increase in El Condor and Cueva del Diamante significantly predates the onset of H2 in Greenland suggests that the southward shift of the ITCZ, proposed to explain the precip increase, was not caused by H2, but rather by long-term solar insolation changes and in particular the NH minimum around this time, right? Also, Fig.16 suggests that the variability in AMOC strength (related to H2) does not substantially affect the position of the ITCZ, but merely the precipitation anomalies north and south of the ITCZ. If this is correct, please revise the paragraph accordingly.

p27, l783: see above regarding the 50-70%

p27, l784: note the above comment on the formulation that DO events occur on average synchronously, rather, the null hypothesis of synchronicity cannot be rejected given the uncertainties. Your statement in the abstract is more accurate, I think.

Sorry for the lengthy report, I hope it's helpful!

Best,

Niklas Boers

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