Climate of the Past Discussions

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Comment on cp-2021-116

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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-RC1, 2021

The study of Francesco Muschitiello analyses the difference between the Greenland layer counted GICC05 timescale and the U-Th based Hulu Cave chronology. The synchronisation between Greenland and Hulu Cave is done using two different type of records: climate records or cosmogenic radionuclide records. Technically, the synchronisation is performed using a new automated algorithm for Bayesian inversion. The calculated transfer functions are said to be more precise than the previous literature and give consistent results. It is found that the annual-layer count identifies on average 7.5-20% too few ice years within GS-1 (the Younger Dryas period), GS-2 (Heinrich event 1), GS-4, and GS-9 (Heinrich event 4), but in contrast, up to 15-25% too many ice years within GS-3 (the Last Glacial Maximum, LGM).

After reviewing this manuscript, I have a mixed feeling about its quality. The introduction is clear and well written. The discussion section is also interesting and well presented. But the method section, with its description of the synchronisation method is in my opinion unclear, and probably contains some mistakes. An effort is therefore needed in my opinion to better describe this method. If I understood correctly, the age transfer function is supposed to be continuous and linear by parts with only 4 segments, which is a very restrictive assumption that should be discussed in greater detail. Moreover, I am personally not convinced that current automated synchro methods can better synchronize than the human brain when the signal-to-noise ratio is low.

Specific comments:

- l. 300-305: When first reading this paragraph, I had difficulties to understand the difference between the “synchronisation parameters” and the “synchronisation vector”.

At the end, I eventually understood that the synchronisation vector has a particular shape that is given by a set of a few synchronisation parameters, as described in l. 340-355. I think the presentation could be improved on this aspect.

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Eq. (2): I have a lot of comments on this! First, on the general expression of this cost function. I am personally unfamiliar with this way of adding the R^2 and the RMSD. Where does this come from? Do you have a reference for this?

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Eq. (2): The R^2 contains a fraction of two sums, not a sum of fractions!

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Eq. (2): In the R^2, the differences are squared!

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Eq. (2): In the RMSD, the differences should be squared as well, as is indicated in the "S" of "RMSD"!

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l. 316: "The first argument in Eq.2 represents 1-R^2". This is not an argument (an argument is for a function), but a member. Moreover, this first member represents R^2, not 1-R^2.

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l. 344: To constrain the ages to be strictly increasing, it would be more convenient to invert positive sedimentation rates.

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Eq. (6): This equation seems unnecessary to me. It is clear from equation (5) that \Phi is continuous.

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Eq. (7): Why are there only 4 segments in the synchronisation, with the last two segments having a slope equal to the average slope (l. 352)? This seems to be a very restrictive way to define a synchronisation. I could not understand if this is a global formulation of the transfer function or only a local formulation. If this is global, it is a very restrictive assumption that should be discussed in greater detail. If this is local, I don't see how you can treat the problem locally while still preserving the continuity of the transfer function.

- l. 373: It seems there is a typesetting mistake here. The indices should be 2, 3, 4.

- l. 373: Why do you still use “n”, while you defined “n=5” previously?

- l. 374: So is X defined as \DeltaT_0, DeltaT_n, A'_i^{GICC05} (i=2,.., 4), s_2 and s_3? In this case it would clearer to explicitly state it.

- l. 385-390: You allow 10 times greater errors than the MCE of GICC05, this seems really too large. Then you restrict to â□□T < 1.75 â□□ MCE, so you actually modify Eq. (2) without giving the new formulation.

- Section 2.3.5: Nothing is said about the computation time to get the posterior distribution, it would be interesting to know that, since it is generally the Achilles’ heel of MCMC methods.