

Interactive comment on “A 60 000 year Greenland stratigraphic ice core chronology” by K. K. Andersen et al.

K. K. Andersen et al.

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Comments given by reviewer are shown in *italic*.

This is an important paper, which should be published in Climate of the Past, but only after some major revision. Given the extensive concerns that I and the other reviewers have, I think it would be appropriate to send the revised manuscript to re-review. I agree entirely with the comments from the other reviews and comments already published on line. I wish to emphasize especially that I agree with Dominique Genty’s comments on the ambiguity of linking cave records to Greenland. The authors seem to be assuming that Greenland $d18O$ changes and speleothem proxy changes are synchronous. This is a common assumption, but it is an assumption that becomes truly problematic when one is using this assumption to say something quantitative about timescales. More discussion about the risk of this assumption is needed. Care should be taken to be

very specific when discussing the "agreement" between records. For example, one might say "the timing of the event in Greenland agrees with the dating of that event in Hulu Cave within 800 years, ASSUMING THAT THE PEAK IN $d18O$ at Hulu Cave indeed corresponds with the peak in Greenland $d18O$."

We basically agree with those concerns, but as argued above in the answer to Genty, we find that the comparison between the Hulu Cave record and the Greenland record is justified because of the overall similarity of the records and because of the NGRIP dust and Asian monsoon linkages.

We have modified the text and state several times that the comparisons are made under an assumption of synchronous records.

This is an important point, but whether the cave records are synchronous with the Greenland profiles or not, the only means we have to compare them for the moment is to "wobble-match" them, i.e. to compare similar-looking events. At this point we are not able to discriminate whether the discrepancies are due to problems of the time scales or if they are caused by actual asynchronies of the profiles.

I have two additional major comments, and a few minor ones, below.

MAJOR COMMENTS

1) This is a dating paper, yet I find the terminology used for dates very confusing. As far as I know, the use of the term "100 kyr b2k" is not standard. I think that using "ka" would be appropriate. You should state at the beginning that you mean "before A.D. 2000.0". I don't know what the Climate of the Past Standards are, but they should be adhered to. I hope that Climate of the Past has not adopted "b2k"!

Throughout the manuscript, "kyr" has been replaced by "ka" and a few redundant "b2k"s have been removed. However, when referring to precise ages, we prefer to use the "b2k" notation in order to avoid confusion. In contrast to the "ka" notation that does not refer to a well defined datum (could be year A.D. 2008 or year A.D.1950,

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sometimes it has been year A.D. 1989), the "b2k" notation unambiguously implies the year A.D. 2000 as datum. The reason why we find it important to use a unit with a well defined datum is that we make comparisons to other time scales where the differences are of the order of hundreds of years. It, therefore, makes a difference if one time scale is shifted by 50 years with respect to the other. We do not assume that GICC05 is accurate within 50 years during MIS3, but in a relative comparison the 50 years do matter. The GICC05 chronology has consistently been published in units of "b2k" in JGR (Vinther et al., 2006 and Rasmussen et al., 2006) and in QSR (Andersen et al., 2006 and Svensson et al., 2006) and the notation is adopted by Lowe et al., 2008. See also discussion on this issue in QSR by Wolff, 2007.

2) It is stated on line 2, page 1234, that : The stadials preceding GI-1, 8, 12, and 17, which are concurrent with the Heinrich events H1, H4, H5, and H6, are constrained by absolutely dated Brazilian speleothems (Wang et al., 2004) that support the long-term GICC05 dating (Fig. 5).

This is a careless and ambiguous way to discuss Heinrich events. I am sure what is meant here is that the Heinrich events (which are not climate events, but are specific iceberg discharge events) occur during stadial periods, and it is those stadial periods that are dated by the Brazilian speleothems. The Heinrich events are very short lived and do not last as long as the stadials with which they are associated, so it makes no sense to say that the stadials are "concurrent with" the Heinrich events.

We agree that the wording is misleading and replace "concurrent" by "associated".

I have four suggestions. First, in Figure 5, if you wish to show the timing of Heinrich events, show the dates of the Heinrich events from the available dating from North Atlantic sediment records. Second, if you wish to also show the dates of the stadials, as dated in Brazil, show them, but label them correctly as stadials, not Heinrich events. Third, make the distinction clear in the text. Although it has become commonplace to say "Heinrich event" both for the iceberg discharges, and for the the cold stadial period

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during which a Heinrich event occurred, they are NOT the same thing. It would be very nice if this paper would clarify this, rather than add to the confused terminology in the literature. Fourth, make it clear what you think the Brazilian records show. As noted above and by Genty, there are assumptions here about the relationship between Greenland climate and Brazilian climate. What are those assumptions? What are the implications if those assumptions are wrong?

We acknowledge that the H-events and their relation to climate is an important issue that should be properly discussed. However, because the H-events cannot be directly identified in the Greenland ice cores, we think that a detailed discussion of this topic should be made in a context of marine records and belongs elsewhere. It was never the intention to get involved with such a discussion in the present manuscript and we, therefore, do not make any reference to the H-events except for their association to the stadial periods.

Concerning the Brazilian record we now state that the apparent agreement with Greenland assumes that the interpretation of the Brazil record as it is published is correct. The Brazilian record is not of key importance for our work and we find that a discussion of the assumptions made for that record will be out of the scope.

MINOR COMMENTS:

Page 1236, line 24: In order to interpret the climatic signal provided by the ice cores and to enable comparison with other paleoclimatic records [insert comma] accurate time scales are crucial.

Changed.

Page 1237, line 15: This period includes the Holocene, the last glacial period and the termination of the previous interglacial period [delete hyphen, insert comma] the Eemian.

Changed.

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Page 1237, line 17 "glacial ice to be thicker than in [delete "all"] other Greenland ice cores [insert "recovered so far"]. If you say "all" then you are precluding future ice cores that may have thicker glacier ice.

Changed.

Page 1239, line 12. It is stated that the Maximum Counting Error (MCE) and is regarded as a 2sigma error of the time scale but in the Abstract it says this is a 1 sigma error. Which is it?

We did not say in the abstract that MCE is a 1 sigma error. In the abstract, we said that the stated error estimates can be regarded as 1 sigma errors and later (p.1239 l. 13) we clearly define MCE as two sigma. However, to prevent confusion, we do not mention the sigma notation in the abstract any more.

Line 13 page 1240, you need to define "ECM". Not everyone reading this paper will be an ice core specialist!

We define that term the first time it appears on page 1237 line 27.

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