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Climate of the Past Discussions

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Interactive Comment

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 paper presents an extension of the GIC005 ice core chronology from 42 to 60 kyr, based on layer counting of the NGRIP ice core. Since this chronology has been adopted as a reference for many other studies, the extension is a welcome addition, for which the authors are to be complimented.

While the layer counting is labelled as "multi-parameter", it appears to rely most heavily on the visual stratigraphic record. As a non-specialist in this area, my reaction to the examples shown in Figs 1 and 2 is very similar to that given in the comments of E.Wolff and D.Genty, namely it is very difficult to understand the objective basis on which layers are considered "uncertain". Since, as emphasized by E. Wolff, it is on

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this basis that the uncertainties on the time scale are estimated, this is obviously a critical point. The authors give two justifications for their uncertainty estimates. One, the agreement between two independent observers for the entire section (although, as the authors note, locally this difference can exceed the estimated uncertainty). As the authors also note, such a procedure cannot exclude systematic bias. To their credit, the authors are very open about these difficulties, and it is clear that they are not trying hide anything. The question remains, nevertheless, as to whether such error estimates are quantitatively robust.

The layer counting is indeed a multi-parameter approach. In Andersen et al., 2006, Figure 4, we compare single-parameter counting of several parameters to the multi-parameter approach applied for the glacial part of GICC05. The single-parameter approaches lead to very different results strongly depending on the resolution of the applied record. Because the records have different resolution and because the annual layer thickness is strongly dependent on climate, it is not possible to rely on just one record. It is true that because the visual stratigraphy (VS) has the highest resolution we rely most heavily on that for the coldest periods (thinnest layers) but also the ECM and the conductivity records are applied. During milder periods, Sodium and conductivity have sufficient resolution to resolve the annual layers whereas both VS and ECM tend to show multiple peaks within an annual layer.

Concerning the magnitude of the error estimate we refer to the reply to the comment by Eric Wolff.

The second justification relies on the fact that the obtained chronology agrees with independently dated records within the estimated uncertainties. Since this is potentially a less subjective criteria, it is important to look closely at these comparisons, which are summarized in Table 2 and figs 4-7. When one does this, it can noted than in fact the majority of the reference dates used for comparison are on layers younger than the time range of the extension reported in the present paper. While these comparisons are useful for supporting the general approach and reliability of the previously published

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range of GIC005, they are unfortunately not very helpful for evaluating the accuracy of the extended range. I therefore concentrate here on this extended time range. In Table 2, only one of the seven cited reference levels (the NAAZ II volcanic layer) falls in the extended portion of the time scale. The Ar/Ar date cited for this ash layer is 54.5 +- 1.0 ky, which is well within the uncertainty of the GIC005 age. However, when I looked at the cited reference, I found that it is in fact a secondary reference in which this date is given as a "private communication". In fact, it is not even clear to me whether this is actually a "recent" determination, as described, or the same as that given in a 1998 abstract (AGU Spring Meeting Suppl. 79, S377), having the same value, and one of the same authors as the "private communication".

Yes, it is very unfortunate that there are no other independently dated reference horizons in the 42-60 kyr interval that can be used for comparison at the moment. It is also unfortunate that the NAAZ II reference is so badly documented. The "original" AGU abstract by Siggurdson et al., 1998, suggests a NAAZ II age of 76+-6 kyr! Therefore, we stick to the more recent age referred to by Southon. New independent dating of NAAZ II is under way, but not yet published.

The remaining comparisons are with radiometrically dated cave deposits. These involve not direct dating of events observed in the ice, but rather correlating climate records from the two reservoirs. The authors argue that, on the time scales considered, these climatic records can be assumed to be synchronous. I believe this is a reasonable assumption, although one can also find arguments why this might not be strictly the case. Also, because speleotherms are not dated continuously, and have inferior resolution, there is some subjectivity about placing the control points.

The most comprehensive comparison involves the Hulu Cave, where five well defined climatic features, fairly uniformly distributed over the extended time range, are compared. The agreement is within the estimated ice core uncertainties for all these features. Three climatic features from the Kleegruben Cave also appear to be in good agreement although, as the authors point out, agreement of features toward each end *of that record are less obvious. With regard to this comparison, I think it would be useful for the authors to include the assumed correlation lines in Fig 6, in the same way as fig 5.*

We changed the figure accordingly and corrected a minor error in Figure 4 at the same time.

Also shown is a comparison with a stalagmite profile from Moomi Cave, in which only one of three features falls within the estimated uncertainties. Although the climate records are not shown, the authors also claim satisfactory agreement with individual features from several other absolutely dated speleotherm records. Overall, if one accepts the critical assumption that the climate records from these two types of reservoirs are synchronous, the comparisons with the absolutely dated cave deposits are quite convincing.

Finally, there is one other important aspect of this chronology, and that is the accuracy of relatively short duration features. The details of such features is becoming more and more important, and one of the advantages of a continuous chronology, compared to one simply interpolated between reference levels, is that in principle it should give much better estimates of the duration of such features. This is nicely illustrated by the authors when they compare their chronology with that of Meese et al. As the authors note, although these two time scales agree within uncertainties at the beginning and end of the extended time region, they actually disagree by 20 % locally in between. In ice core studies themselves, paleo-accumulation rates deduced from continuous chronologies are often used to transform concentrations of trace species into fluxes. This local chronological information is in fact given implicitly by the annual layer thickness curve shown fig 3. If I understand correctly, the authors estimate that a maximum counting error (MCE) of 5 % (2 sigma) should be associated with this curve between 41.8-60 kyr. It should be noted that the method of comparison with independently dated control points, as shown in fig 4, does not really test this uncertainty. I believe it would be very useful to give a supplementary table with a numerical version of this curve.

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In the supplementary material we do provide a table containing the age, the NorthGRIP ice core depth, and the (MCE) in 20 yr resolution. From this table the annual layer thickness and the local counting error can easily be obtained. In order to provide past accumulation rates, we would need to apply a layer thinning model. Because such a model can be obtained in several ways, it needs a discussion of its own, which we prefer to leave for a modeling paper.

There is in fact an aspect of this curve which I find puzzling. As pointed out by the authors, a widely used model chronology for GRIP and NGRIP is ss09sea. This model is based on an estimate of initial accumulation, coupled with a thinning function. The accumulation is related to the 18O record via a semi-empirical correlation. For nearby features, where the thinning is relatively constant, one expects changes in the layer thickness to be dominated by changes in the estimated accumulation. This implies that the layer thicknesses will be proportional to 18O. If one compares GI 10 and 11, however, one finds that GI 10 has larger amplitude in layer thickness than GI 11, but smaller amplitude in 18O. A similar situation occurs for GI 6 and 7. This suggests to me that either there is a problem with the model predictions of the layer thicknesses in these particular regions, or a problem with the layer counting. I think it might be quite useful to include the layer thicknesses of ss09sea in fig 3, so that one could see where the major differences between the modelled and observed thicknesses occur. Such information might be helpful in identifying the cause of the differences, as well as giving added confidence in regions where the two agree.

Indeed, this is an interesting aspect that we want to elaborate on in a future publication. We do provide the comparison between annual layer thicknesses for ss09sea and GICC05 for the 11-42 kyr section in Svensson et al., 2006, Figure 3. Because the most significant differences between the two records appear in that interval (11-42 kyr), we decided not to include that comparison in the present manuscript, but, certainly, we intend to get back to this point later.

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