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Climate of the Past Discussions

Interactive comment on "Optimisation of glaciological parameters for ice core chronology by implementing counted layers between identified depth levels" *by* L. Bazin et al.

Anonymous Referee #2

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Review for L. Bazin et al.: Optimisation of glaciological parameters for ice core chronology by implementing counted layers between identified depth levels

Bazin et al. present a new way of incorporating depth-age constraints from an annual timescale into the Datice method. The previous AICC2012 chronology for 5 ice cores used the GICC05 timescale of the NorthGRIP ice core as the base chronology; however, using GICC05 as the reference chronology was not straightforward with the Datice methology and violated the assumption that the initial (background) chronology was independent from the age markers. This modified formulation allows the GICC05 age constraints to be included without violating the independence of the initial chronology. The result is timescales for the 5 cores that are similar to AICC2012 although the

authors note that these new chronologies should not be used in analyses of ice cores. Bazin et al., using the inferences of firn thickness from previous authors, infer lower glacial accumulation rates at North GRIP than were inferred from GICC05 and ice-flow modeling.

The approach of Bazin et al. is logical and fits in well with the overall Datice methodology. The authors demonstrate that the new age constraints are functional within the Datice framework. However, I have two major issues with the work, one general and one technical.

My general issue with the presented work is that I'm not convinced this improves the NGRIP timescale or the Antarctica timescales. The authors seem to agree that the new timescale for NGRIP is not an improvement as they note that it should not be used. That the final inferred timescale remains similar to GICC05 is likely because the age-interval markers used in the new formulation are still the primary age constraints - what data from the Antarctic cores they choose (EDC, EDML, Taldice, Vostok) are going to improve the dating of NorthGRIP? In the absence of an analysis of whether the NorthGRIP timescale was improved, I at the least expected a discussion of how these age markers would in the future lead to better timescales. The lack of discussion was especially disappointing as an annually resolved timescale for the past 30 ka was published for the WAIS Divide ice core a year ago. EDML also has an annual timescale for the past 10 ka (although the in preparation paper cited in the AICC2012 papers appears to still be in preparation). Is the methodology developed in this paper going to be able to make use of these annual timescale for Antarctica?

I would have preferred to see an analysis of whether this new implementation can improve the timescale. Why not use synthetic timescales and test the approach? The authors have already defined the uncertainties for the all the different ice cores, so creating synthetic timescales should be relatively straightforward. Then applying Datice to these timescale where the "true" timescale is already known could yield significant insight into not just the new formulation for Datice but for Datice itself.

My technical issue relates to the thinning function. The authors were kind enough to supply the Datice output. As the authors briefly allude to, the thinning function is no longer smooth. It also does not decrease monotonically. Ice of 30 ka age has thinned significantly less than ice of 20 ka age. This is a puzzling scenario and one that needs to be discussed by the authors. While Parrenin et al. (2004) showed that such variations could occur at Vostok, this was because of the large changes in the ice thickness and convergence or divergence along the flow line. Neither of these situations is applicable to NorthGRIP which is located on the divide and the slight flow down the ridge to the core site has no significant ice thickness variations (Dahl-Jensen et al., 2003). The authors hint that the cause might be impurity driven; however, softer (to shear) LGM ice will not simply cause the LGM layers to be thinned more. To do so would violate continuity. For the derived thinning function to be acceptable, the authors must show that it can be recreated with an ice flow model and conditions of the NorthGRIP site.

The unphysical thinning function appears to be the result of the inappropriate constraint on the thinning function. If I'm following the description in the appendix, the thinning function is only constrained by a user-tuned standard deviation and does not impose any physically based constraints. Hence, the thinning of a layer no longer has to satisfy being an integrative history of the ice sheet's deformation.

The authors are incorrect to conclude that "our study confirms the overestimation of GICC05 accumulation." They have only shown that if you remove constraints based on the physics of ice flow, you can infer an accumulation rate and timescale consistent with the firn-based accumulation reconstructions. This work sheds no light on why the ice-flow-based and firn-based accumulation rates reconstructions disagree.

Overall, I feel this work makes a limited improvement to, while also highlighting a fundamental flaw of, the Datice methodology. It fails to show, or really test, whether the new methodology improves the North GRIP timescale and provides no outlook for how this new methodology will improve future timescales. I think this work is an interesting

start that needs considerable work before publication.

Specific comments:

Introduction -The introduction is very focused on problems in implementing GICC05 in AICC2012. I think the focus is too narrow as this should be about improving ice core timescales, not Datice. It might also be a wise idea to discuss some of the other advances in ice core dating that are occurring and how this work will complement them. -The reference of Cutler et al. (1995) for a 30% uncertainty in accumulation and thinning function seems overly simplified. The uncertainty in the thinning function is depth dependent. The uncertainty at the surface is essentially 0, while uncertainty at the base of the ice sheet is essentially infinite. In addition, the uncertainty between accumulation rates for ice with similar ages is much reduced. - The accumulation inferences from d15N rely on accurate firn densification modeling. This uncertainty needs to be discussed as the PIRE firn project has shown that there is great uncertainty in the firn models even in steady-state modern conditions, let alone transient glacial conditions.

Methodology - I don't understand what constraints the Antarctic ice cores provide to the NorthGRIP chronology. It seems like there is so much uncertainty in the ice and gas timescales from these low accumulation rate East Antarctic ice cores that it would be better to just run NorthGRIP by itself.

- The references to Buiron et al. 2013, Veres et al., 2013, and Bazin et al., 2013 reminds me that the thinning functions for EDML and Talos Dome also produce reversals in the thinning function for ice in the upper half of the ice sheet. I suspect that thestruggles with the thinning function at NorthGRIP apply to the Antarctic cores as well.

-There is something unsatisfying about starting with background scenarios that produce timescales that we know are inaccurate. But I guess I have a larger question: are the background scenarios self-consistent? If I understand correctly, the background thinning function, accumulation rates, and lock-in-depths are all independently derived

and would not produce a realistic timescale.

Chronological and climatic implications - I am surprised that there is no discussion of the gas ages associated with GICC05. While I understand that GICC05 is technically just a timescale for the ice, it seems like the gas timescale will be more affected than the ice timescale.

Appendix L21- these numbers of cT2 don't have any meaning. What is this correction physically? L24- is this just saying that the uncertainty on the thinning function at the surface should be zero? If so, why write "the 0 variance hypothesis" ?

References -The references seem a little short on timescale work from non-European countries.

Table -Add the event name and approximate age for each event so readers can readily understand where the new delta-depth markers are being applied. -What is the justification of the uncertainties

Figures - Figure 4 has too any lines which are not visible. I'm not sure what the main point of the figure is, so I can't suggest a better presentation. - Figure 5 has too many lines as well. - Figure 7 is unintelligible.

References: Dahl-Jensen et al., 2003. Basal melt at NorthGRIP modeled from borehole, ice-core and radio-echo sounder observations. Ann. Glaciol., 37, 207-212. Parrenin et al., 2004. New modeling of the Vostok ice flow line and implication for the glacilogical chronology of the Vostok ice core. JGR, 109, D20102

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