

## ***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 #3**

Received and published: 17 October 2014

The manuscript describes a methodological extension of the Datice model used to create the AICC2012 ice-core time chronological framework. Although mainly of technical interest at this point, it is a methodological advance of potentially very significant importance that addresses one of the major shortcomings of the previous model: the lack of ability to include the results of annual layer counting in the model in a way that respects the nature of layer counting. Therefore, I believe the results are appropriate for publication in CP or possibly even more appropriate for publication in GMDD. However, as the AICC2012 papers were published in CP, it makes sense to publish the next step here too.

However, the proposed methodological extension seems to have some weaknesses  
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that seriously limits my confidence in the results. If these weaknesses can be addressed satisfactorily, the method will be a valuable addition to the Bayesian ice-core chronological modelling framework, and I therefore encourage that the authors try to address these issues.

In addition to the comments on the content below, the manuscript is in need of significant improvements in language and clarity. I have not attempted to comprehensively mark up language errors or minor issues in the presentation, but encourage the authors to make sure that the English is significantly improved before submitting a revised version.

CP review checklist 1) Does the paper address relevant scientific questions within the scope of CP? Yes

2) Does the paper present novel concepts, ideas, tools, or data? Yes

3) Are substantial conclusions reached? Yes, but as outlined in the review comments, I have some concerns about the robustness of the results.

4) Are the scientific methods and assumptions valid and clearly outlined? Some clarifications and language improvements are needed.

5) Are the results sufficient to support the interpretations and conclusions? See (3)

6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes, mostly. Some minor issues are mentioned above, but largely, the model documentation is sufficient when seen together with the previous Datice publications.

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes.

8) Does the title clearly reflect the contents of the paper? Yes.

9) Does the abstract provide a concise and complete summary? Yes.

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10) Is the overall presentation well structured and clear? Well structured: yes. Clear: some attention to language is needed.

11) Is the language fluent and precise? No.

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? I think so.

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? As discussed below, I think Fig. 7 and the corresponding text is outside the scope of the study.

14) Are the number and quality of references appropriate? Yes

15) Is the amount and quality of supplementary material appropriate? N/A

Detailed comments:

Page 3587

Line 7: "Tuning" is an understatement. AICC2012 was forced to fit GICC05 back to 60 ka.

Line 10: I do not think that "markers" is a good description of a duration estimate from layer counting. Similarly, "age-difference" is an unclear term to use for durations.

Line 19-20: ... which will not be an "exercise".

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Line 2: "1–8years for counting of 20 annual layers" Please refer to where this comes from or revise (8 out of 20 years is an order of magnitude more imprecise than the published estimates I am aware of).

Line 3-4: "Since the layer counting is not independent from one interval to another, the final uncertainty on the GICC05 chronology cumulates the counting error". Both parts are true, but one does not imply the other: uncertainties in counted chronolo-

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gies cumulate down core regardless of whether the counting is independent between intervals.

Line 6-7: GICC05 does not strictly speaking have a 1-sigma uncertainty. Setting  $MCE/2 = \sigma$  is an assumption.

Line 23: I believe that using naïve "first guesses" would be a bad idea, as the inversion cost function depends on the distance between the solution and the background scenarios. The model therefor "prefers" to give solutions as close to the background as possible. The background scenarios should be independent from the stratigraphic constraints in order to satisfy the underlying assumptions, but must represent fairly good guesses of the accumulation, thinning and LIDIE in order for the model to produce good results. Figure 4 clearly shows the non-trivial influence of feeding the model with different background scenarios (e.g. at depths around 2300 m).

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Line 8-9: "In this paper, we propose an improvement of Datice to better implement the chronological uncertainties". I believe the aim is to better represent the results of layer counting (and the associated uncertainties).

Page 3591

Line 6-7: ... provided that the uncertainty of all constraints and background scenarios are properly quantified (and that any correlations between the uncertainties are also properly described and modelled adequately). This may sound trivial, but I think it is far from that ... I agree that the model is useful because it provides the best compromise between a lot of different information under a certain set of assumptions, but the assumptions are not trivial, are most likely not fully met, and thus cannot be ignored.

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Equation 4 and text around it: Does this mean that all types constraints are added with equal weight (apart from the weighting that depends on the uncertainty/confidence), or

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is some normalization performed so that, for example, the cost function contribution of all “gs” and all “ad” type constraints are the same?

Page 3593

Line 15: The term “analysed chronology” confused me. I believe it is the model output, but it is not fully clear to me.

Page 3594

Line 4. “Largely” is used in a wrong meaning.

Line 4-6. This illustrates how difficult it is to argue that the background scenarios and their uncertainties are well-represented in the model input, and thus that the result is the “best solution”.

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Line 8 and 13: “time interval” sound like an absolute time period and “periodicity” sounds like something with an oscillation. I believe that “resolution”, “spacing”, or another similar word would more clearly convey the message.

Line 13-16: The assumption of absence of correlation between the uncertainties of the layer count in neighboring sections is most likely not justified. When counting layers, certain typical features that may or may not represent an annual layer occur repeatedly. If such recurrent features are misinterpreted in the layer counting procedure, they will give rise to correlated errors. And why 2xMCE? It seems like a rather random choice that must be justified.

Line 16: “We decide in this paper to treat the error on individual annual layer as normally distributed. On this assumption, one can apply the MCE to age-difference markers as a Gaussian error”. Sorry, but I do not follow the argument.

Line 18-20: “The periodicity of markers of age-difference in GICC05 is 20 years with a 1 to 8 years associated uncertainty (Rasmussen et al., 2006)”. Firstly: The GICC05

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data files are released in 20-year resolution but GICC05 is an annually resolved time scale. So the 20 years are not reflecting any intrinsic resolution. Secondly, the 1-8 years uncertainty for each 20-year interval seems very large. Are you sure? The GICC05 MCE is on average around 5% or less, very far from 8 out of 20.

Line 21: How do these inconsistencies occur? Or is it more a numerical limitation as the next sentence indicates?

Line 25 and Fig 2: One thing is that a short duration (ie. 20-60 years) of counted intervals leads to numerical problems (or inconsistencies ... see previous comment), but it is very worrying that the results seem to depend critically on the duration/resolution or the annual layer counting constraints used. Roughly speaking (by comparing the blue and cyan curves of figure 2), the deviation of the newly modelled time scale from GICC05 doubles when the number of constraints is reduced to half. As no a priori “correct” choice of marker/constraint resolution exists, and the model seems to produce a different result for different choice of marker/constraint resolution, the solution is not unique, but depends on a user-chosen arbitrary value for the marker/constraint resolution. The issue relates to the question to page 3592: How do the results depend on the number of markers used, and is some kind of normalization required (or desirable)? This issue needs to be further explored. It is possible that a similar issue could exist for other model constraints or for the resolution of the background scenarios. To summarize: At this point, I get the impression that the “best compromise” solution obtained depends critically on the “age-difference” marker resolution (and possibly on the resolution of the background scenarios), which has no intrinsic “best value”. The “best compromise” thus depends on a rather arbitrarily chosen parameter.

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Line 18: Now  $\sigma = \text{MCE}$ . It is still double the quoted uncertainty and still a rather arbitrary choice. Please argue for your choice.

Line 19: “... the error correlation value has been varied between its minimum and

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maximum possible values.” What are these values and how are the limits determined?

Line 21: “As for the chronology built without correlation, the maximum difference between the new chronologies and GICC05 is of 130 years (Fig. 3)”. There is a problem with the sentence structure.

Line 26-28: Why is this expected?

Line 10-28: It is great that authors try to test how different ways to add up counting uncertainties influence the results. This is a weak point of GICC05 and many other counted time scales. However, the way this is done needs more explanation (especially what the physical meaning of the error correlation value is). Also, sensitivity studies should be performed to show how the results depend on the choice of 100-year spacing of the markers in the case of correlated errors. As an example, consider a period of 1000 counted years with 50 years MCE evenly spread over the interval. In case the section is split up into 20-year sections each with MCE 1 year and the errors (for simplicity) are assumed to be fully correlated between sections, the total uncertainty would be 7 years ( $\sqrt{50}$ ). In case the splitting is made into 10 100-year long intervals each with MCE 5 years, the total uncertainty would be 16 years ( $\sqrt{10 \cdot 5^2}$ ), while the total uncertainty would be 35 years if the section is split up into 2 sections each 500 years long and with MCE 25 years. The difference is probably smaller in your case as you do not assume full correlation, but I would guess that the effect would be there (again emphasizing that the somewhat arbitrarily chosen 100-year marker spacing leads to non-uniqueness of the solution).

Page 3597:

Line 6: The statement “This result is not unexpected for two reasons” has some very complex logical implications :o) Please clarify. I also have a hard time understanding why the absence of a direct link between MCE/2 and a Gaussian sigma (partially) explains the result. The explanation could also simply be that MCE is a conservative uncertainty estimate.

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Line 13: It seems to be a stretch to claim that the method have been tested for correlation values larger than 0.6 when the full model does not run with these values.

Line 19-22: Can you back this statement up with data?

Line 23: Are the results with error correlation 0.5 with  $\sigma = \text{MCE}$  (as above) or  $2\text{MCE}$ ? If  $\sigma = \text{MCE}$ , I find it hard to compare the two sets of results.

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Line 26-28 and Fig. 4: Yes, except around 2300 m depth where the PK2014 background scenario has very low accumulation rates and drags the solution away from the other curves. It could indicate that the uncertainty of the PK2014 scenario is too low across this section.

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Line 10: “This conclusion is mainly valid for the last 60ka”. I would say “only”, not “mainly”

Line 31: “The  $\Delta_{\text{depth}}$  constraints from  $\delta^{15}\text{N}$  measurements cannot be questioned” is an overstatement. High confidence does not imply that a result cannot be questioned. For example the argument relies on a pretty large assumption, namely that  $\delta^{18}\text{O}$  directly reflects surface temperature. Rephrase and add modesty, please.

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Line 11-26: The procedure is worrying to me: The LIDIE results are too variable, so they are forced to disappear by reducing  $\sigma_{B,L}$ . Is there a physical meaning of this parameter adjustment? What are the possible reasons for these problematic results? Could it be an indication that some of the data constraints are internally inconsistent? If you decide to keep the current solution where you force LIDIE to have smaller variability, I think you at least should discuss more carefully whether the observed dips in LIDIE are indeed too large to sensibly be climate-related (they sit around 2050, 2200,

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and 2300 m, corresponding to GI-8, 12, and 14, which are the longest interstadials in that part of the record).

Page 3601

Line 16-18: These two lines and Fig. 7 seem detached from the rest of the manuscript. The chronological adjustments are not of large importance for a bipolar seesaw discussion, and in any case, the sentence and Fig. 7 must either be extended to an entire paragraph or removed. I suggest removal as the bipolar seesaw discussion seems outside the scope of the manuscript.

Line 22: Sentence unclear.

Page 3602

I agree that the GICC05free results should not be recommended for use over AICC2012 because of the small scale of the differences (i.e., the possible chronology improvement does not outweigh the hassle caused by having many parallel and almost identical time scales) and because the results seem to be dependent of model implementation choices that are somewhat arbitrary (see above).

Caption of Fig 5

“... over the glacial period” is not correct

“AICC2012 +  $\Delta$ depth” is shorthand and not easy to understand for non-specialists.

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Interactive comment on Clim. Past Discuss., 10, 3585, 2014.

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