

Interactive
Comment

Interactive comment on “IceChrono v1: a probabilistic model to compute a common and optimal chronology for several ice cores” by F. Parrenin

Anonymous Referee #4

Received and published: 7 December 2014

The manuscript describes a new implementation of the Datice model, which most notably was used to create the AICC2012 ice-core time chronological framework. The new implementation, IceChrono, is only marginally conceptually different from the Datice model, but is based on a Python platform that makes the model more accessible to the wider community. I believe IceChrono is appropriate for release/publication in GMDD. However, a number of open questions about the function of the model remain, and the presentation is not very clear. Without being an expert on the mathematical formulation of Bayesian models, I agree on the comments provided by Tim Heaton and the other anonymous reviewer:

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



- The model description needs to be more detailed, especially because none of the previous papers on Datice have clearly presented the modelling framework in an accessible format discussing in depth how to address problems with meaningful uncertainty assignment etc.. Some concrete examples are given below.
- The glaciological validity of the results needs to be explored further,
- The section on the Berkner Island dating needs massive improvements or could be removed to free space for a more thorough discussion of the model. I provide comments about the Berkner Island section separately below.
- The manuscript is in serious need of significant improvements in language and clarity. Just to add to the list provided by the other reviewers: use dash in “ice-core records” and similar expressions. The manuscript should pass a thorough grammar check before resubmission.

With regard to the first point above, I find the best approach to be to describe the workings of the model conceptually first and then, in a separate section that can be skipped by readers with limited technical interest and/or skill in Bayesian modelling, go into the level of detail asked for by reviewer Tim Heaton. In addition, I would like to raise three other central points, first one regarding the presentation of the IceChrono (and Datice) results, and two of more technical nature.

Firstly, I find it unjustified to claim that the Datice or IceChrono models produce “optimal” chronologies. The models use optimization techniques, and therefore, the use of the word “optimization” is acceptable. However, optimization only produces optimal results if the model underlying assumptions are justified, the simplifications insignificant, and the data basis is correct and with correct representation of uncertainty (which is particularly problematic in this context, see comment below on volcanic matching). I know that “optimal” is used in the title of Lemieux-Dudon et al 2010b,

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

but I still think that this use of “optimal” should be discontinued. I think that the word “consistent” in the title of the original Datice paper of Lemieux-Dudon et al. 2010a is the most appropriate description of the approach.

Secondly, a more technical point is the balance between data and model inputs. The Datice and IceChrono models essentially make trade-offs between background scenarios (that are known to be wrong as there would otherwise not be a reason to apply the model) and data-based constraints. The trade-off is made in the models’ costs functions, which gets contributions from the misfit between model and data constraints and the deviation of the model from the background scenarios. From eq. 9-14, it appears to me that all contributions are added weighted only by their uncertainties. There may be no “optional” way to do this, but at least the question of how to obtain a good balance between the data constraints and the backgrounds should be thoroughly discussed. In judging whether a fair balance has been obtained, I encourage the author to consider if a measure and/or figures showing the contribution to the cost function from the different terms in the cost function could be useful. Some specific questions to address:

- It seems clear that the resolution of the background scenarios influences the balance between data constraints and backgrounds. The experiment on page 6823 explores the effect of doubling the resolution of the correction functions of the background scenarios, which is a good test to make. Does the resolution of the background scenarios themselves (and not only their correction functions) influence the results? In other words: Is the relative contribution of the deviation of the model from the background scenario dependent on the grid resolution for the background, the grid resolution of the correction function, or both?
- Especially if the only weighting factor of the background scenarios is determined by the width of their confidence intervals, the assignment of confidence inter-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



- vals/uncertainty is of central importance. What measures have been taken to ensure consistent assignment of background scenario uncertainty between different cores and through time?
- Given that the models include background scenarios for cores that cover from 1 to 8 glacial cycles, and given that the flow regime and accumulation reconstructions are likely much better constrained for some cores and some time intervals than others, is it a reasonable assumption to use the same resolution of background scenarios for different cores and times? For example, by using a temporal resolution of 1 kyr for the accumulation background correction functions, I guess there will be 6–8 times more points that relate to the EDC accumulation reconstruction than the NGRIP reconstruction? If so, could/should the gridding/background scenario resolution be made variable to adapt to this?
 - A specific question along the same lines: Assume that two cores have been linked stratigraphically using 1000 volcanic fix points and 50 methane horizons with the same uncertainty. Does each horizon enter the cost function with the same weight? If so: Is this reasonable? If not: How is the weighting determined?

Thirdly, the author is encouraged to discuss how to better represent the uncertainty of volcanic matches in the model. I understand that no new such data are introduced here, but are adopted from the AICC2012 data basis. However, this is an obvious place to improve Daticc to increase the confidence of the results. The issue is that volcanic matches come in different categories. In rare cases, tephra have been found in several cores and geochemistry has confirmed that the tephra are indeed coming from the same volcano. However, more commonly, a series of acidity/sulphate peaks representing a characteristic pattern are matched. Assuming that the pattern match is correct, the uncertainties of the individual horizons are on the order of centimeters (i.e. years to decades) and largely uncorrelated between horizons. However, there is a small risk that the pattern match is incorrect, in which case the error can be

several meters or more (centuries or millenia), while being highly correlated between the horizons that belong to the same matched pattern. In contrast to this, as far as I know, the uncertainty of volcanic horizons is represented in Datice and IceChrono as Gaussian errors of typically 20-200 years. It would be great to hear if the author has ideas about how to implement a more realistic uncertainty estimate, and to respect the different types of volcanic ties.

Concrete comments to the manuscript:

6812

7: “use of dated depth intervals” is slightly misleading as the depth intervals are not dated, but represent a certain duration in the record. Please use another word, e.g. “use of intervals with known duration” or similar. This change should be applied consistently throughout the manuscript.

6813

3: ... field STRENGTH.

11: “pros and cons” - > strengths and weaknesses 17: “but is generally accurate for event duration”. Would this not only be true when the accumulation history is well known also far back in time?

18: It seems like a circular argument here: surface accumulation is modelled in line 13 and suddenly it is a result that can be used for interpretation in line 18.

24: low-accumulation sites.

27: Add that these methods rely on the existence of climate-independent horizons or the assumption that the synchronized records indeed show the same changes synchronously.

28: (4) The synchronization of ice-core records can be done.

6814:

C2611

GMDD

7, C2607–C2616, 2014

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



8: “Optimal”. See above.

8: “therefore”: There is really no argument presented to support this statement.

10: “calibrating 14C ages” is better than using the word “chronology”.

17: Please specify what these errors are.

23-24: Please replace 1 and 4 with one and four.

25: AICC Chronology not IES

6816:

5: Remove “just”.

5: “Un-thins” is modelling slang. Please revise.

7: “Second member” . . . do you mean “term”? Or right-hand side?

9-10: Very unclear sentence.

6818:

26: J_k^{ii} is linked to ICE, right? If not, J_k^{ii} and J_k^{ai} seems to essentially be the same.

6820:

10: Annex -> Appendix

6821:

24: The section starting here is unclear to me. In particular, is it possible to evaluate to which degree the residual vectors are indeed independent and whether their standard deviations are unity (which is what the word “unit” in line 27 means, right)?

6822:

15: The use of annual-layer-counted intervals in Datices is described in a manuscript in revision for Climate of the Past by Bazin et al., so this is only partially true.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



22: What is meant by “development”? The rest of the sentence is very convoluted.

6823:

15: Can you really conclude that the matrices do not describe the physical reality well because they are hard to invert?

6824:

2: It means that IceChrono is robust to a change of the resolution of the correction functions by a factor of 2. It may INDICATE robustness on a more general level.

13: A more thorough analysis of WHY IceChrono and Datice differs at the Laschamp event would be useful.

19: The consistency of the results confirms that the codings of Datice and IceChrono are performing similarly, which can be taken as an indication that they are correct. It shows nothing about the validity of the assumptions or the method itself. The formulation (and especially the similar statement in the conclusion) should clearly reflect this.

6825:

1 and 7: repetition

Comments about the Berkner Island dating section:

I'm not opposed to including the section on Berkner Island dating as an example of IceChrono, however if this is to be the first official timescale for the core, then more detail and figures are certainly required.

A discussion of possible reasons for the large accumulation correction around 80 kyr and (in particular) the physical realism of the reversal of the thinning function in fig. 7 below 830 m musty be included.

Moreover all assumptions and parameters that would be needed to replicate this dating

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



by another user of IceChrono should be provided/tabulated or included as supplementary information. Examples of information that should be provided in a revised version as follows:

- p. 6826, line 22-23: 'values of alpha, beta, gamma etc have been chosen to obtain a good fit with independent age markers along the core'. These values should be listed. Please detail which independent age markers have been used and if they are different from the constraints applied later and listed in the tables (and if not, if they are independent from these constraints).
- Figure 3: On the scale of this figure it is difficult to see the differences, if any, between the background scenario and the corrected age. Two things would help: Use colours with more contrast, and add a subplot showing (1) the age difference versus age and (2) the age difference versus depth between the background scenario and the corrected age. It is hardly relevant to state for each figure whether it has been produced by IceChrono. Mentioning it once the text is sufficient.
- Figure 4: As for Figure 3.
- Figure 5: As above use colours with greater contrast. Also please comment on the substantial differences between the background and corrected scenarios around 30-40 ka and 80-90 ka.
- Figure 6: As above for colours. Also please comment on the deviation from the background at 30-40ka is this due to a particular constraint?
- Figure 7, as above for colours.
- The isotope and gas records on the corrected timescale should also be shown in figures (see also below).

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



It is difficult to compare figure 7 (plotted versus depth) with figure 3-6 (plotted versus age). Please add a secondary age axis to the right axis of fig. 7.

Table 1 shows age ties based on ‘comparing the deuterium records’ of EDC and Berkner Island. The uncertainties attributed to these ties range between 150 and 300 years. Some more explanation and details should be provided here:

- Why is EDC used for the comparison, instead of EDML, which is closer to Berkner Island and better resolved?
- Also, how are these ties made? Is it by visual matching of Antarctic Isotope Maximum events, or perhaps by some statistical method? In any case, a figure illustrating this would be appropriate: i.e. a comparison of the deuterium records from EDC (or EDML etc) and Berkner Island and same for the CH₄ records.
- It seems optimistic to allocate centennial-scale time-scale uncertainties for deuterium ties. Noise and internal variability between Antarctic ice core sites is at least this large. The onset of deglacial warming in the deuterium record is a good example of this: at WAIS the onset of warming begins 2000 years earlier than at EDC. Yet the Table 1 is making the assumption that the onset of deglacial warming Berkner Island occur within 170 years of the onset of deglacial warming at EDC.
- Along the same lines: Please explain how the synchronization uncertainties in Table 2 are derived and why EDC was preferred over EDML or even a Greenland CH₄ record. Uncertainties in these methane ties as low as 50 years also seem highly optimistic. As above, figures illustrating these ties would be appropriate.
- It is not appropriate to give ages in Table 1 and 2 to two decimal points.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



If this is to be the first official/recommended Berkner Island timescale then the ice and gas phase age-depth profiles must be provided as supplementary data and/or deposited in a well-established data repository.

Interactive comment on Geosci. Model Dev. Discuss., 7, 6811, 2014.

GMDD

7, C2607–C2616, 2014

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C2616

