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Geoscientific Model Development Discussions

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

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First of all, I would like to express my appreciation to the referees who have posted comments. I found all the reports to be thoughtful, helpful and comprehensive, and am grateful to the referees for having taken the time out of their schedules to provide reviews.

Two of the referees are ice core glaciologists who know the field and literature well, and I think their opinions are very important regarding the utility and applicability of the model, the interpretation of results, questioning of various assumptions, and the equations behind IceChrono. One is a statistician who gives the caveat that he is

not an ice core specialist; nevertheless, he has made some very insightful comments regarding the mathematical framework of the approach.

An overarching issue in all of the reviews is one regarding the clarity of the paper. All three referees point out how things are not, or incompletely, explained in various places. This is an opinion I share. In my initial report I asked the author to give expressions for specific items that were introduced without, or with nonspecific, description. The result was the appendix, which follows the letter, but not the spirit, of what I was asking. It is very important that the author addresses these points of clarity. At the moment large parts of the paper read like documentation one might find on a website – unclear, confusing, and non-peer reviewed. The idea of this journal is to avoid just such things.

I myself find the manuscript to be particularly jargon-y, and many phrasings seem unclear to me although they may be the norm for the field.

For instance, I agree with Dr Heaton's confusion regarding a triangular (i.e. nonsymmetric) correlation matrix. Is a correlation matrix c_{i,j} not equal to the correlation of x_i and x_j? If so, how could it not be self-adjoint? Or possibly, by "correlation matrix", you mean something different from what Dr Heaton and myself expect?)

As such, I ask that you do not disregard Dr Heaton's specific comments and technical points as a non-ice core scientist, but rather that you make a point to respond to them all, either in a new manuscript or a letter. He has also given a very legible summary of the approach, as he understands it, and i would expect you to verify that he is correct in his understanding; furthermore, you might benefit from adopting his description and nomenclature into your new manuscript.

Dr Heaton questions the equations 1-3 as well – I don't think eq 1 bears a lengthy explanation in the text, but perhaps you could point out to non-experts what the thinning function really is (despite your very brief explanation, i had to infer from eqs 23 and 24 what its physical significance actually is), as distinct from density change. However, all referees had issue with equation 2, and I must admit I have no idea where it comes

from or how to derive it. I would like their questions addressed – and in this instance it may be helpful to provide a schematic figure which illustrates the equation graphically.

Referee 2 brings up a number of good points regarding methodology, which should either be addressed in the manuscript or in a response. I would echo his question as to why IceChrono has not been applied to synthetic noisy data. He also seems to think any berkner core discussion should be taken out. Prof Wolff has similar issues with the Berkner core, so given ref. 2's strong feelings on the subject i think it should be removed or its inclusion justified.

Beyond this both referee 2 and Prof Wolff have comments and questions regarding comparison to DatIce, and I think these should be addressed. Ref. 2 questions the veracity of DatIce itself, and this manuscript is probably not the place for such a discourse, but perhaps some of the oddities mentioned (reversal of \tau) could be mentioned. Prof Wolff gives some helpful comments on how to make the manuscript more readable.

In short, this manu should be accepted on the merits of its science and methodology, but I have serious concerns regarding its clarity, and the author would do well to heed the comments of the referees in this area, particularly those of Dr Heaton.

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