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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This paper describes and presents a new model for computing ice core age scales. As the author explains, it has a very similar philosophy and methodology to the already-published DATICE model. The paper claims that it has some (especially computing) advantages compared to DATICE. It presents two examples of the use of the model, one confirming that it obtains similar results to DATICE for the same experiment, and the other producing a first age model for the Berkner Island core.

In general this is a solid piece of work that serves the community by making the code freely available in a (relatively) user-friendly format. Age modelling for ice cores is really

important so there is no doubt the work is significant. However there are some issues that need to be dealt with:

a) Clarifying some of the equations and inputs

b) Making sure the code is clearly available on a formal and stable platform

c) The author's statements on the performance relative to DATICE should be discussed by DATICE people

d) There are some issues with the new Berkner age model that need to be explored.

Abstract, lines 2-10 consists of a single very long sentence, and one that seems more suited to the introduction than the abstract. I suggest the abstract needs more thought to ensure it truly explains what is new in this paper.

Abstract, Line 10 "here I propose" seems a slightly awkward wording. Maybe "Here I present".

Abstract, Line 15 "differences from" better than "differences on".

Page 6813, line 25-27. It should also be mentioned here that this method requires assumptions, not always fully acknowledged, about synchroneity between changes of similar appearance in different archives.

Page 6814, line 25. AICC actually is presented as "Chronology" not "Chronologies".

Section 2.1. There are several aspects I feel could be clearer here.

a) In line 7 "initial surface accumulation" – I don't really follow the purpose of the word "initial", surely each layer has only one accumulation rate, and the word "surface" already clarifies that you mean the one it had when it was laid down.

b) It might be worth being precise that in all the integrations, zero is at the surface (since many integrations in glaciology actually treat the bed as zero).

c) Line 16: "relative density": relative to what? If you use water equivalent accumulation

rates, then your densities are relative to that of water, and I think that, for eq 4 and 2 to be right this has to be so. However in the files online it looks as if acc rates may be in metres ice equivalent, and in that case for eq 1 to be right the densities would be relative to ice. In any case this needs to be made clear and consistent.

d) And indeed you should be clear what your acc rates are. Many readers would assume they would have units kg m^-2 time^-1. But actually they are in m (water or ice) equivalent depth time^-1.

As a general comment, at different times in the text you use "I" (eg p 6817, line 25), "we/us" (6818, line 2), or "one" (6817, line 16). You should standardise.

Section 2.8. Probably it was intended but just to avoid doubt, the model code should be a supplement to the paper. I appreciate that the code is available at github. However (a) it looked like a rather confusing set of files; (b) it is not clear which set of files corresponds to the model version presented and tested in the paper; (c) the software archive should be permanent and secure, and with great respect I don't think a personal area on even the most secure server meets that bill. I therefore suggest that the files referring to this version of the paper should be archived at GMD as a supplement.

Section 2.7 (comparison to Datice), also page 6826, line 25 claiming superiority of Ice Chrono. I am unable to judge these sections: one of the developers of DATICE should be strongly urged to comment.

Page 6824, line 14: can the author comment what is occurring in the DATICE code that causes it not to respect the confidence interval at the tie point? Knowing this would be very helpful in judging the way the models work.

Page 6825, Berkner age model. By synchronising on water isotopes the author is assuming synchronous climate changes between Berkner and East Antarctica. This assumption (which precludes testing phase leads and lags) should be made absolutely explicit.

Also re Berkner expt: I wonder why the author has done this in such a way that the Berkner age scale has only the synchro error. It would surely have been straightforward to in addition simply run AICC2012 again but including Berkner, in order to get a realistic uncertainty on the Berkner age model. As there would have been no new absolute age information, I assume that the ages would not have altered for the other cores, but an uncertainty for Berkner would have emerged.