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Interactive Comment

Interactive comment on "Ice flow modelling at EPICA Dome C and Dome Fuji, East Antarctica" *by* F. Parrenin et al.

F. Parrenin et al.

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Answers to referee's comments are in bold.

General comments. In the submitted manuscript authors summarize theoretical issues and provide with further development of their previous work [Parrenin et al., 2001, 2004] on the application of a 1-D glaciological model, first implemented to construct a chronology for the Vostok ice core. Now they apply their 1-D modeling method to another two ice cores obtained at Dome C and Dome Fuji.

In the case of Vostok, the model was a 2.5D one. But yes, it is the same kind of model, with prescribed surface elevation and prescribed velocity shape function.

Apparently, accurate and objective dating of the ice-core proxy records can be thought of to be a key problem in palaeo-reconstructions. That is why the submitted paper where authors present a method of solving this very complicated problem is a very important contribution into palaeo-climatology. New methods in ice-core dating are always welcome. Presented results have two aspects. The first one is methodological. Clear and systematical description of the modeling method and of the technology of its implementation, allows to apply it in future research. The second aspect is, so to say, practical, and provides with results of modeling: ice age vertical distribution, thinning function, etc. of the two Antarctic ice cores. Authors provide with all assumptions necessary for the method implementation and outline its limitations and shortcomings.

Reviewer is right to say that there is a methodological aspect and a practical aspect. We tried to be as accurate as possible for the methodological aspect, so that the method can be applied to other drilling sites or with other parameters.

Presented paper is a valuable contribution to the palaeo-climatic and related studies. Specific comments. P20, L20: Length of the EDC ice core is pointed to be "Ÿ800 kyr" while further a more exact value of 740 kyr is used. Rounding of 740 kyr will be closer to Ÿ700 kyr, that is why I suggest to use more or less exact figure of 740 kyr. Here also the age of the Dome Fuji core indicated as "Ÿ330 kyr". I think it is useful to comment here that the interpreted part is far from the bottom and expected basal ice can be much more older. This comment can also be attributed to EDC, though not yet interpreted segment of the core is much shorter in this case.

For EDC: 740 kyr is what has been published in 2004, but an additional 60 m of ice has been retrieved and the length of the climatically interpretable record is currently estimated at 800 kyr. See the cited works of Jouzel et al. (submitted) or Dreyfus et al. (submitted). For DF: we added a footnote to make that clear.

P22, L2: A short comment or at least reference is necessary for "Lliboutry type ice flow model".

We now define the expression "Lliboutry type velocity profile" during the intro-

duction.

P22, L4-6: It is not clear whether “a simplified model of variations” just used an output of a 3-D model or somehow interacted with it. Though this is explained in Section 2.5 here authors could also be more precise.

We replaced “based on” by “fitted onto the results of”.

P22, L14-15: “Present-day accumulation rate” is listed among “poorly constrained parameters”. A clear comment is desirable for this statement, because normally present-day accumulation rate is thought to be well-constrained - from shallow cores etc. If this parameter was tuned to specific age markers then one could start to doubt how precise these markers were.

The present-day accumulation rate is always estimated by using the depth of well-dated horizons (like the beta radioactive horizons), so this is exactly what we do here. Moreover, we are more interested in the average Holocene accumulation rate than on the last decades (which may not be representative of the whole Holocene), and it is why we did not use any published accumulation rate estimates.

P24, L13: “shows larger bedrock relief than at Dome C”. Since relief is not a quantity, has one to understand this expression like “larger bedrock relief variations”?

Implicit in the phrase "bedrock relief" is the idea of bedrock elevation variations. Thus, it would be tautology to use "relief variations".

P26, L11-14: Activation energy of creep Q=60 kJ/mole is used, though below authors mention, that a much higher value was used by Lliboutry and Duval (1985). At the same time they use a shape function of Lliboutry (1979) to parameterize p. Since, as it is mentioned further, Q is larger in the basal layers, where the most of ice deformations concentrate, it will be helpful for clear understanding the reasoning for using another Q.

The Lliboutry model is valid with any value of Q. It will only change the value of the p exponent. In any case, this exponent is not fixed in the rest of the manuscript, but rather reconstructed from the inverse method. So we did not use any particular value of Q. We made this point clearer in the text.

P26, L17-23: "Tests with a full Stokes model" - a reference on the modeling work (paper, personal communication) should be added or, otherwise, the modeling procedure described in brief. It is not clear, whether the above modeling is the case study of the Dome C (Dome Fuji) core or just a conceptual study. It is not clear, how the Greenland results of Thorsteinsson (1997) were applied for the Antarctic case. In the phrase "an increase of the exponent by 1.4" an explicit reference on the particular expression (formula) will be helpful. "SIA hypotheses" = "SIA hypothesis" ?

The modeling procedures use a full-Stokes model and are described in the PhD thesis of Fabien Gillet-Chaulet, which is now referenced in the manuscript. We rephrased the use of the GRIP measurements. There are several hypotheses to make the SIA valid: negligible bedrock reliefs, horizontal variations of ice mechanical properties negligible in front of the vertical ones, etc.

P27, L5-6: "Tests with a full Stokes model" - as on the previous page, a reference must be added.

Done.

P29, L5-6: "Loulergue, this issue" or "Loulergue et al., this issue"?

Corrected.

P34, L1-2: The same remark as above (Page 22, line 14-15) concerning poorly constrained present-day accumulation rate, which is "tuned to a apriori an apriori information on the ice age at certain depth". Please, comment.

See answer above.

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P34, L16-26: Authors provide with the arguments explaining the choice of the limited number of age markers to constrain the model ice-core chronology by the inevitable limited accuracy of the model. Here, to my mind, two aspects must be separated. First, there are certain computational (numerical) limitations in any model. Using the terminology of the paper, even if the "perfect" age markers were used as an input for modeling, the output result would have certain numerical inaccuracy. It would be interesting to know, how precise is the model itself, because it is based on Monte Carlo approach. Second, natural processes and their history are not perfectly known. Authors mention about it. To avoid over-tuning the model they use a certain set of age markers and (probably) reject some other markers. Selection among markers inevitably causes a question about the criteria of such selection. By definition, an age marker is an absolute dating of some event with some error bounds. In case some markers are rejected it means that either they are wrong, or contradict each other (which means, that all of them can be wrong), or the markers are true, but the modeling approach is insufficient to provide a chronology satisfying as many markers as possible. It would be convenient if authors provided with arguments (or at least with brief reasoning) why one or another marker was used and others we rejected. Otherwise, one might think that the markers were chosen in the way to fit the model and not vice versa.

As the reviewer said, there are two types of uncertainties/assumptions: the numerical ones, and the physical ones. The numerical ones are very small, as we said section 2.4. We measure it by comparing two different numerical schemes, which give the same result within 0.5 %. The physical ones are clearly dominant here, because some physical mechanisms are missing. This makes the errors on the model results too much correlated: e.g., the error of the modeled age at 400 m is NOT independent from the error of the modeled age at 450 m. In the current inverse method, the modeling error is neglected, and thus using too many age markers during some periods will bias the results by over-tuning some poorly known parameters. You can be assured that the choice of the age markers has not been done to fit the model! We agree it is a subjective part, and we did not

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try to hide that weakness in the manuscript. We are currently working on a new inverse method to take into account modeling errors. This new method will avoid the subjective choice of the age markers (all age markers will be selected). But it is beyond the scope of this manuscript.

P37, L14-25: In the referred paper [Parrenin et al., 2004] the most probable value of beta for Vostok lies in the bounds 0.03-0.04 (fig. 8). In this paper, the Vostok value is mentioned as beta=0.0102. In the above paper (formula 7) and in the current manuscript (formula 6) beta is defined differently. So authors have to address this problem to avoid confusion.

In section 2.3, we mentioned that the present formulation is different from the one found in Parrenin et al. (2004). We now are more precise in section 4.1, saying that it "is roughly equavalent to Eq. (6) with b=0.0136ś0.0024.

P39, L18-19: It is not clear, what means the phrase "a full-Stokes model under SIA conditions", since by definition SIA assumes only horizontal shearing in the vertical planes.

SIA conditions means for us "conditions where the SIA applies". This is now corrected.

P40-41 "4.5 Thinning function and annual layer thickness" Authors conclude, that bumps in the calculated thinning function are due to the local variations in ice thickness. This conclusion seems me strange and not correct. Thinning function is the annual layer thickness in the core related to the accumulation rate in the corresponding year. So it is pure dynamical factor, and local accumulation rate is excluded from it. That is why I do not understand, why glacial and interglacial accumulation rate affected ice thinning in the different way. In their earlier paper [Parrenin et al., 2004] authors examined factors influencing thinning function. In that case, bumps in the Vostok thinning functions reflected spatial variations of the factors affecting the ice flow (flow divergence etc.), which we do not have in the case of EDC or Dome Fuji.

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The annual layer thickness is indeed the accumulation rate multiplied by the thinning function. So it is true that the variations of the accumulation rates are principally "removed" in the thinning function. However, variations of accumulation rate (and surface temperature and sea level) have a second effect on the thinning function through the ice thickness which is more subtle. We believe this is a robust feature of our simulations, as we checked it is visible either using the pure Lagrangian scheme, or using the "Lagrangian-thinning Eulerian-age" scheme. We can understand this feature intuitively. For simplicity, let us consider an ice sheet without basal melting, and with a plug flow velocity profile. Dividing Eqs. (10) and (1), we can deduce the rate of compression per unit of vertical displacement following an ice particle:

$$\frac{d\epsilon_{zz}}{dz} = \frac{1}{\bar{z}},$$

that is to say, it only depends on the distance of the particle to the bedrock. Now, let us consider a particle at e.g. 500 m depth and let us assume it deposited when the ice thickness was 100 m larger than today. Its actual vertical displacement in the ice sheet is not 500 m, but 600 m, so that this particle encountered more thinning than if the ice thickness had been constant. We modified the justification of this point in the manuscript.

Technical corrections. P21, L5-10: Here the paragraph is not clearly written. First "Several experiments were performed", next "They consist of 4 parts: 1) a mechanical model" etc. Experiments cannot consist either of a model or of "some age markers".

It is not "or" but rather "and" that we meant. We did not write "they fall into four different parts". We rephrase this sentence as: "They use four different ingredients: ..." to prevent any misunderstanding.

P21, L19: "a empirical trial" => "an empirical trial"

Corrected.

P22, L8: Excessive usage in the expression “recent new developments” - “recent” cannot be ”old”.

Corrected.

P25, L2 and 5: “reduced vertical coordinate” = “non-dimensional vertical coordinate” ?

Corrected. We sometimes say “reduced”, but we agree “non-dimensional” is a better expression.

P29, L3: J/K is the wrong unit of alfa (see L11).

Corrected.

P34, L10: “giving access to” = “providing with?”

Wording changed.

P33, L20: “guaranty” => “guarantee”

Corrected.

P36, L14-16: “markers that unknown coefficient” - something is missed here. “of the value of a particular age markers.” => “of the value of the particular age markers.”

“That unknown coefficient” replaced by “than there are unknown coefficients”, but the rest of the sentence is correct in our opinion.

P38, L26: definitive => definite?

Both are acceptable in English.

P39, L5-6: “but this time this value is significantly positive” - basal melting is always positive, so expression “this time” is not necessary to use. Sentence modified. We wanted to say that there is a significant basal melting. P46, L19: “french” => “French”

Corrected.

In the reference list: Watanabe, O., Shoji, H. 2003: please, check, whether it is the original typing - “dome fuji” and “antarctica”?

Corrected.

Figure 6: Unit of the horizontal axis is “cm-of-ice/yr”, which in fact is the unit of velocity; “cm of ice” or “cm of ice equivalent” will be he right one.

Corrected.

There are 3 different papers of Watanabe et al. [2003] in the reference list, which are not marked as “a”, ”b”, and ”c”. In the text, some references are marked with a letter, and some of them are not marked.

Corrected.

Text must be checked once more before the final submission to avoid typing mistakes, improper use of prepositions, and tenses (like, e.g. P38, L18-21).

Text has been checked carefully by a native English speaker.

Would it be more expedient to remove footnotes and to mention submitted papers in the reference list?

Corrected.

Summary. 1. Submitted paper addresses scientific questions within the scope of Climate of the Past. 2. Manuscript summarizes, organizes and develops the earlier ideas. Presented in the current paper methodological approach for ice dating, based on integrating past accumulation rates, thinning function, variations of the isotopical content of the ice core with the set of independent age markers, provide with an objective and self-consistent estimates of ice chronologies. Applied to EDC and Dome Fuji ice cores, this approach benefits in ice-core dating and related thinning functions. 3. Presented results are substantial for interpretation of the ice-core proxy records. 4. Scientific methods and assumptions are in general valid. Though it would be expedient, if authors would argue for their choice of the particular age markers, since they themselves mention that they used not all possible markers, but only selected ones. See above. 5. Interpretations and conclusions are supported with good arguments, except one concerning influence of varying ice thickness on the thinning function. I would thing that this conclusion is wrong. In case authors insist on this conclusion, they should argue their point of view. See above. 6. Description of the performance of the numerical experiments is clear. These can be reproduced, if necessary. 7. Though methodologically the paper continues the previous work of the authors, application of the methodology designed for the Vostok ice core to the EDC and Dome Fuji ice cores witnesses about the general applicability of the approach. 8. To my mind, the title of the manuscript is too general. The paper would benefit if the title would clearly reflect its contents, emphasizing on its goal (e.g. see the first sentence of the Abstract). "1D" added to the title. 9. Abstract provides with a good summary except the conclusion concerning the influence of the varying ice thickness on the thinning function (see above). See above. 10. Paper is well structured, and from this point of view does not need to be revised. 11. Except several technical corrections, the text has to be revised only taking into account referee's notes. Conclusion: manuscript can be accepted for publication after clarifying issues addressed in the review.

We thank you for your thorough review of the manuscript.

Interactive comment on Clim. Past Discuss., 3, 19, 2007.