

Interactive comment on “Ice thinning, upstream advection, and non-climatic biases for the upper 89% of the EDML ice core from a nested model of the Antarctic ice sheet” by P. Huybrechts et al.

P. Huybrechts et al.

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Our response to the comments in bold

This paper presents the result of a modelling exercise which seeks to explain the agedepth curve at EPICA. This has previously been dated independently (apparently) by Ruth et al, although the methodology is not well described in the paper here. The age-depth model is computed using the Huybrechts multiple interglacial cycle, forced by some realistic climate parameters, and the Pattyn model around the core site. Some attention has been paid to matching the flows at the boundaries Ages are computed by integrating along trajectories. This paper is valuable confirmation that the EDML1 time-scale is consistent with glacial theory and should be published. The glaciological

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discussion is a bit skimpy, and needs strengthening in order to make this paper satisfying to both climate scientists and glaciologists. My comments appear below. Apologies that some of my comments refer to fairly recent work by me and colleagues on how topographic disturbances affect particle paths. I think that this work is of relevance; if the discussion group can find earlier references to the same effects, please tell me and the authors.

We thank the referee for his thoughtful comments. We were aware of the 2 papers he mentions and have now incorporated some of their conclusions more explicitly in the discussion.

1 MAJOR POINTS

1. The EDML1 chronology is not well described. They need a paragraph such as the following one I lifted from the EDML1 paper 8220; A chronology called EDML1 has been developed for the EPICA ice core from Dronning Maud Land (EDML). EDML1 is closely interlinked with EDC3, the new chronology for the EPICA ice core from Dome-C (EDC) through a stratigraphic match between EDML 5 and EDC that consists of 322 volcanic match points over the last 128 ka. The EDC3 chronology comprises a glaciological model at EDC, which is constrained and later selectively tuned using primary dating information from EDC as well as from EDML, the latter being transferred using the tight stratigraphic link between the two cores. Finally, EDML1 was built by exporting EDC3 to EDML.8221; - though what exporting means in this context I don't know. The point is that there is apparently already some model input to EDC3.

Done. We incorporated elements of the above sentence to make our sentence more readable.

2. It is not clear what increase in accuracy the Pattyn model is expected to provide. It will of course compute the velocity field better in response to topographic disturbances (see e.g. Hindmarsh et al. 2006), but given that particles are advected in, and experiencing such disturbances all along their travel path, then what increase in accuracy is

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expected from use of the Pattyn model just locally?

First of all we note that particles are not 'advected in'. FSM comprises all of the area where all particles originated so the Pattyn model is not just applied locally. On the other hand, perturbations in bedrock inevitably lead to variations in the ice flow field. These variations are exaggerated in a SIA model, especially at high grid resolution and for important bedrock variability. To overcome this one has to use either a coarser grid or smooth the surface topography by taking surface gradients over longer horizontal distances. The higher-order model is insensitive to this effect and long stresses develop where gradients are more pronounced. A discussion can be found e.g. in Pattyn F., M. Nolan, B. Rabus and S. Takahashi (2005) Localized basal motion of a polythermal Arctic glacier: McCall Glacier, Alaska, USA, *Annals of Glaciology* 40: 47-51. With our 'anomaly scheme' in which LSM differences at time t and the present time are superimposed on the observed topography, SIA often gives a rather irregular velocity field due to small-scale variations in ice thickness and surface elevation. These variations are effectively smeared out in the higher-order model because of the 'long stresses'; mentioned above. Meaning that we could not have exploited the better quality of the input data on the 2.5 km grid when using the SIA. The text in §2.2 and §2.3 have been modified to express these ideas more clearly.

3. Topography with wavelength roughly the same as the ice thickness and much greater than the ice thickness affects particle trajectories in different ways; this can only be seen by using higher order models (Hindmarsh and others, 2006). Presumably the resolution of the model is not high enough to capture all this topography, and anyway the details are not being properly computed in the FSM region. Does this matter? Given that topographic errors from BEDMAP underresolution are sometimes positive, sometimes negative, is the net effect computed better, than local variations which might be quite poorly represented?

It is quite difficult to answer this question as we have no quantitative handle on

the bedrock underresolution in the FSM domain. One is constrained by the data as well as by the lack of it. We can only state that we are using the best available data from BEDMAP amended with the most complete collection of flight lines from AWI obtained afterwards. For these latter data, the flight line separation over the relevant area of FSM where the trajectories are located is better than 10 km and crossover differences smaller than 10 m for the majority of cases. Near to Kohonen station the flight line separation is even smaller than 1 km, justifying the compromise 2.5 km grid used in FSM. Since we do use a higher-order model we expect that we have also captured the physics to our best knowledge. The combination of the best data and the most suitable model make us believe that we have done a better job using a higher-order model on a higher resolution grid than we would have by simply using the output from the LSM on the 20 km grid for the backtracing. Actually, when using the LSM only for the backtracing we obtained ice core ages and upstream corrections which varied from the ones presented in this paper by up to 20 percent at 90 percent depth. Hence, it does matter to use a higher-order model on a higher resolution and the net effect is a more correct calculation. A few sentences have been added in §2.3 to clarify this matter.

4. Recent studies (Parrenin and others, 2006) show that topographic influences can effect isochrones downstream for long distances. By inference, the errors from poorly represented topography can result in errors a long way downstream. Did the authors consider any sensitivity experiments with respect to poorly known bedrock topography?

No, we did not perform such experiments within the framework of this paper. We believe this should best be done in a more schematic way and here the Parrenin et al. (2006) paper is particularly illuminating. We have responded to this remark by adding a reference to the Parrenin paper and noting that we cannot do better than the best topographic data currently available allows.

5. The mismatch between EDML1 and the paper is attributed to anomalously high

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thinning due to bedrock sills. This seems to be arguing that certain elevations are influenced more strongly by bedrock highs than others - a possibility, but how this operates needs to be more explicitly outlined. What are the physical mechanisms for the anomalous thinning - is it computed by the Pattyn model (and therefore presumably due to mechanical effects) or by the Huybrechts model (and therefore presumably due to thermomechanical effects).

The small mismatches between EDML1 and the modeled chronology in the paper occur at 1760 and 2110 m depth. At those depths the thinning function is not 'anomalous'. Also the larger mismatch below 2380 m does not correspond to any irregular behaviour of our thinning function. The 'anomalously high' values for the thinning function mentioned in §5 between 1600 m and 1720 m depth are not matched by mismatches in the dating. Hence, both issues are not correlated contrary to what the reviewer seems to suggest. The reason for the gentle wiggling of the thinning function between 1600 m and 1720 m depth is not entirely clear at this stage. That requires a more detailed consideration of the evolving bedrock topography, ice thickness, accumulation rate pattern and ice flow velocity field along the trajectories of the ice particles. This problem is being examined within the scope of the more general task to describe and to explain the change of the thinning function patterns along the flow line from Kohnen to Dome Fuji following the theoretical considerations by Parrenin et al. (2004, 2006). For now we suspect that this deviation from the linear pattern is the result of the bedrock highs at around 60-70 km upstream from Kohnen as this is the only irregular feature corresponding to the approximate time and place of deposition at this depth in the core. Besides, we cannot really separate 'mechanical' from 'thermomechanical' effects. Both FSM and LSM consider thermomechanical coupling although the ice temperature is calculated in LSM and interpolated to FSM. The LSM is the main driver and therefore we expect thermomechanical effects to dominate. Moreover, mechanical effects of FSM are probably more constant over time, as they are primarily influenced by bedrock topography, while

thermomechanical effects are more likely to change over a glacial/interglacial contrast. So any time-dependent discrepancy should be related to thermomechanical effects.

6. The matching of boundary conditions between the nested models is not well described (699:23-700:23). The discussion is framed in terms of anomalies (which are not defined, and I only vaguely know what they are) and I didn't really know what they are doing. Slightly worrying was the discussion of mass conservation; if the FSM and LSM models are not conserving mass evenly over the column, then the computation of particle trajectories, which assumes conservation, is going to concentrate or dilute particle densities. Does this affect the overall result or just the accuracy of the result?

The text stated (p. 700, line 13) that mass in the second anomaly scheme is not necessarily conserved, but in practice it is. That is because the 'smoothed' velocity field of FSM is very close to the LSM velocity field and corrections are applied to the vertical velocity component, as was also written in the same sentence (p. 700, lines 14 and 15). In effect the vertical velocity in FSM is obtained from vertical integration of the continuity equation from the bottom to the top (page 699, lines 5 and 6). A correction is then linearly applied along the vertical to satisfy the kinematic boundary condition at the top, but this is usually (and fortunately) very small. These additional explanations have now also been added in §2.3. In the climatological literature 'anomalies' are 'differences' of a modeled variable between two states (usually modeled at time t minus modeled at time zero). The text of §2.3 has been modified to (hopefully) make it more clearly also to the glaciological community. Moreover, we have a manuscript in preparation which describes the effects of the various anomaly schemes to much larger detail than is possible in a paper like the present one that only aims at illustrating our best run.

2 MINOR POINTS 1. Equation (7) should be referenced.

Done. This equation is easily derived from general principles. However, we have included a reference to Reeh (1989) and Parrenin et al. (2004).

3 REFERENCES Hindmarsh, R.C.A., G.J-M.C. Leysinger Vieli, M.J. Raymond and G.H. Gudmundsson, (2006), Draping or Overriding: The Effect of Horizontal Stress Gradients on Internal Layer Architecture in Ice-Sheets; J. Geophys. Res., 111, F02018, doi:10.1029/2005JF000309. Parrenin, F., R.C.A. Hindmarsh and F. Remy (2006), Analytical solutions for the effect of topography, accumulation rate variations and flow divergence on isochrone layer geometry; J. Glaciol. 52(177), 191-202

Both references were included as they are indeed useful in the context of our paper.

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