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Climate of the Past Discussions

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Interactive Comment

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 editor comments in bold

The comments from the two reviewers are generally positive and suggest that it will be possible to prepare a paper that is acceptable for CP. The manuscript is very well written, but details on the models and sensitivity experiments are missing. I strongly suggest to add a supplementary material with all those details. So I would ask the authors to consider and answer all the points made by the reviewers, and to be especially careful:

Thank you for the generally positive evaluation of our paper and the thorough suggestions for more material. We also appreciate the suggestion to give all the

details of the underlying models FSM and LSM. However, that would basically amount to repeating earlier work by the two model developers, P. Huybrechts and F. Pattyn. Fortunately, details of the models and basic sensitivities have long been published elsewhere and are part of the established literature. For the FSM, all the details of the model are in Pattyn (2003). For the LSM, all the details are in Huybrechts (2002) and Huybrechts and de Wolde (1999) and references therein. We think it is more useful to mention the (minor) changes that have been made with respect to these publications in the current text. In our perception of the literature, it is not customary to repeat full model descriptions in every study making use of models for which good references exist and which models have been applied in many other studies already.

Much of the other additional material asked for by the editor, concerning sensitivity experiments and many other pictures, have indeed been looked at by us, but are the subject of other manuscripts, some of which have been submitted elsewhere or are still in preparation, or have long been published and are referenced as such. We are a bit reluctant to include, and perhaps bury, all our additional material in annexes in just this one paper, which would make this paper at least 3 times longer and not give the additional material the prominence of an independent article it would deserve. The material presented here is in fact only the highlight of 5 years of intensive calculations, which work needs more than 1 paper + x annexes to fully communicate to the community.

In this paper it was our intention to concentrate on our best (standard/reference) result which was also used in the EPICA DML paper in Nature. We think we have documented our standard result sufficiently well here to understand how the modeling was performed, and to evaluate its consistency. Where necessary, however, we have provided more details and amended the text as further specified below:

To give more details on forward models. This includes:

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Physical parameters, such as the viscosity (including temperature dependency), the enhancement factor, the basal sliding, the thermal conductivity, etc.

This is fully described in Huybrechts and de Wolde (1999) and Huybrechts (2002) and references therein, as was already mentioned in the text. These models are standard in the community, have been widely tested and compared (and copied by others) within the framework of EISMINT and ISMIP, and so it seems not required to repeat all the details in every paper making use of them. Only the enhancement factor was slightly changed to 1.2 with respect to previous work, and this is now mentioned explicitly in §2.1 (previously we only mentioned it was slightly retuned to give the exact ice thickness at Kohnen without giving the exact value). Another clarification is that FSM uses the same flow laws for ice deformation and basal sliding than LSM and that is now also mentioned explicitly.

Numerical parameters, such as the time steps.

We already mentioned that in the text in §2.1: 1 year for LSM, p. 698, line 13, 100 years for FSM (p. 703, lines 13 and 25), and 100 years or less for the Lagrangian backtracing (p. 703, lines 13 and 25, and p. 709, line 8)

Boundary conditions: What is the boundary condition at surface in FSM? I suppose it is a stress condition (P = Patm), but in this case, the present day surface may not be in steady state. It would be interesting to put a graph with the vertical movement of surface for the present.

That's right. This surface evolution is of course accounted for in the vertical velocity. The spatial pattern is quite similar to maps published previously in Huybrechts (2002) and Huybrechts and Le Meur (1999) and is therefore not shown again here. As a general feature, the elevation changes over the FSM domain where Kohnen ice traveled mainly scale with the accumulation rate, and are largely synchronous with the result for Kohnen as accumulation rate variations

are the main driver.

Topography (Cf. Richard's comment): How well is it known, (not only in EDML area, but also all along the particles paths)?

Pretty well as the flight line separation over this area is mostly below 10 km, and closer to Kohnen even below 1 km. The ice thickness at Kohnen itself is known to within a few meters. Ice thickness differences at crossover points are mostly below 10 m (Steinhage et al., 2001).

Did you perform sensitivity experiments? In this case, please put a graph in the supp. geothermal heat flux: Did you perform sensitivity experiments with recent maps by Shapiro et al. or Llubes et al.?

Yes, we did, also with the Fox Maule (2005) map; these findings are however to be presented elsewhere, both for all of Antarctica and for its effect on the basal 10 percent of the EDML ice core. The editor is probably aware of the posters we presented on this subject in Il Ciocco and EGU2007. This additional work goes far beyond the scope of this paper and therefore does not deserve to be included as an annex to the current article.

Coupling schemes It is not very clear (at least for me) how this transition zone for scheme 1 does work. Are bedrock and surface elevations those of the LSM, ie a smoothed version of the data?

Inside the FSM grid the bedrock and surface elevation are a 2.5 km interpolation directly from available flight lines and available BEDMAP data. In a zone 40 km wide around the margin, a 2D spline transition is made from the 2.5 km FSM grid to a 2.5 km interpolation of the LSM grid.

It is not very clear how the coupling scheme 2 does work (p700, l13-23). Please add more details (and sensitivity experiments) on: correction of the vertical velocity to satisfy the mass conservation

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We already explained that, cf. p. 699, lines 4-6, p. 700, lines 14-15. Nevertheless, some additional clarification has now been added.

influence of horizontal velocity at boundary condition

Not inside the FSM grid far away (> 100 km) from the boundaries, which is the case for our trajectories. This was already mentioned on p. 700, lines 18-20.

correction for other variables: temperature, basal melting, sliding velocity, dH/dt, dB/dt, etc.

This information comes from LSM. Basal sliding and basal melting is slightly adjusted on the FSM grid using the FSM ice thickness and/or surface gradient. Vertical fields such as temperature are linearly rescaled for the slightly different ice thickness of FSM as compared to LSM.

In addition, we have revised §2.3 to be more specific about the coupling scheme. Although we have done extensive testing on the various ways to couple FSM and LSM, including numerous sensitivity tests, this is again the subject of another publication to be submitted in the near future. In the current paper we only report about the results obtained under our best method.

You may show some snapshots of the LSM surface for several typical climates, compared to the simulation for the present, and also compared to the surface elevation data. I have the impression that your approach is roughly equivalent to a spatially homogeneous translation of the surface in the FSM model. That means that the EDML flow line is roughly constant during time. Could you have a discussion on these results of the LSM? Which parameters may have an influence on the EDML flow line? (eg., grouding line position in the Filchner ice shelf) How well are they known for the past? This is a critical part of your modelling results.

We already mentioned the stability of the flow on p. 704, line 6, and showed the constancy of the flow line over time in Fig. 5. Indeed, surface elevation

changes are rather similar over FSM (they scale with the accumulation rate) and are almost synchronous. This is now mentioned explicitly on p. 707. The LSM behaviour is very similar to the pictures shown in Huybrechts (2002), and the extensive discussion in that paper. We now explicitly refer to that paper on p. 707, line 13. Because of the damming effect of the coastal mountain range in this part of DML, the surface primarily responds to local accumulation changes, and to a lesser extent to changes in basal deformational temperature. The effect of grounding-line changes in this area is rather minor, in contrast to West Antarctica. Similar behaviour was also discussed in earlier papers by the first author, e.g.: Huybrechts P. (1990). A 3-D model for the Antarctic ice sheet: a sensitivity study on the glacial-interglacial contrast. Climate Dynamics 5, 79-92, and Huybrechts P. (1990) The Antarctic ice sheet during the last glacial-interglacial cycle: a three-dimensional experiment. Annals of Glaciology 14, 115-119. We feel there is no need to repeat all these results in detail again here. Also, the main findings were already mentioned on p. 707, lines 6-7 and a few words were added to make that even more clear on p. 707.

Mention more clearly the possible limitations of the current approach, and draw some perspectives on them: Surface accu pattern is assumed constant in time. But accu pattern seems correlated to the temperature field (Oerter et al., AG, 2000), so that the accu pattern may be very different for a different climate. Isochronal layers may help constraining this parameter.

The large scale pattern is correlated to temperature, however the smaller scale variations have another origin, most likely bedrock. Again, we have studied this in another paper to be submitted shortly.

As usual, rheological properties of the ice may be a lot more complicate than in our models (Cf. Durand et al., this issue).

That's why we restrict our analysis to the uppermost 90 percent where the influ-

ence of rheology is not crucial. The paper already contains sufficient caveats to point to this (largely unknown) issue.

You mention clearly the uncertainty of basal melting. But the uncertainty on basal sliding is also very important. As we know that there is basal water at EDML, there is a good chance that basal sliding is not zero.

We agree. At the same time we note that we have restricted our analysis to the uppermost 89 percent because of these basal uncertainties that only seriously affect the lowermost 10 percent of the ice core.

The temperature field comes from the LSM, which is OK for the present study, but will definitely be a limitation if we want to look in more details at the basal layer.

Agreed, cf. supra.

Title is a bit long. What about: 'Dating and interpretation of the EDML ice core with a nested ice flow model'?

We like the current title more. The specificity of the title is meant to distinguish the results from forthcoming publications providing more details, other parameters, sensitivities, etc... Changing the title at this stage may also cause confusion to other papers in the special issue which already refer to the current paper title.

I do not agree with the justification for beta=0.046. Cf. discussion at EPICA meeting, and Parrenin et al., JG, 2006: it is not possible to reconstruct temporal variations of accu from the isochronal layers (just temporal variations of the accu pattern). Mention you used beta=0.046 but remove this explanation.

Done, a paper to be submitted shortly will provide more details on how we did this.

Other minor points p698, l18: why 'near to the surface'?

Thanks for remarking. Near to the 'margin' was meant here.

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p698, l19: 'They are also required' They are not really required, but the SIA does not produce realistic results below this resolution

Done. Addtional sentences have been added

p700, l20: what is the 'rate factor'?

That is the multiplier in the flow law (the flow law parameter). It gives you the rate of deformation for a given stress. Text in §2.3 was modified accordingly to avoid any misunderstanding.

Interactive comment on Clim. Past Discuss., 3, 693, 2007.

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