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Climate of the Past Discussions

Interactive comment on "Change of the ice rheology with climatic transitions – implication on ice flow modelling and dating of the EPICA Dome C core" *by* G. Durand et al.

G. Durand et al.

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Answers to reviewers remarks.

N. Azuma.

General comments

We do not clearly understand the referee comment. We believe that his recommendation to perform comparisons for transient runs under biaxial deformation and simple shear would apply for a fabric development model. But, here, the results presented in section 5.2 are obtain with an anisotropic ice flow model, where the calculations of the flow and the fabric evolution are fully coupled. Then, the ice in an ice core located at

the (perfect) divide is only submitted to pure shear (i.e. biaxial compression) and the fabric evolution along the divide is only a function of the thinning. The thinning curve along the divide change from the stationary run to the transient run, but as we plot the fabric as a function of the thinning, the curves obtained for the stationary run, the transient run (or even by calculating the fabric for a biaxial compression with the fabric evolution equation) are exactly the same.

In an ice core located at 30km from the divide, the ice has been submitted to a complex strain-rate and stress history with both shear and longitudinal terms, so that the fabric is really a function of this history and not only of the thinning. This explain why the fabric profiles are different for the transient and the stationary runs. In more fluidic layers the shear terms are higher and for a given thinning the fabric is more concentrated. This really needs a fully coupled anisotropic ice flow model to show this point. This point has been clarified in the new version of the paper.

Following N. Azuma comment, we have added the thinning curves for the transient and stationary runs in the ice core located at 30 km, and the discussion has been consequently adjusted. Because there is no extrusion flow, we don't obtain differential thinning between soft and hard layers, but our results clearly show that the thinning curve is affected by the anisotropic behaviour of ice. We have underlined in the new version of the paper that these effects (and their amplitudes) depend on the local flow conditions so that it is not possible so far to give general trends (and correcting terms) to help the ice core dating community. We recall that these effects are not taken into account properly by the current dating models and progress in this direction would have to be done in a coming future.

Specific comments

1.

We fully agree with the comment. We have specified the model which was used to calculate the thinning function and have added the corresponding references : the 2

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papers of F. Parrenin in the same issue.

2.

Indeed, there was an important mistake on the label of the 3rd arrow of figure 2 and corresponding explanations in the text. The arrow corresponds to event 5d. Figure and text have been modified. Concerning the remark of N. Azuma “In the median inclination profile of c-axis of DF core this corresponding depth (1560 m) does not show any exceptional weakening of c-axis concentration (azuma 1999)”. After a careful check on the corresponding publication, we were first surprised to not observe a weakening of the median inclination. Then, we have checked the file kindly send by N. Azuma presenting the evolution of the eigenvalues along DF. The weakening is definitively observed in the eigenvalues, there is no mistake in the data. We believe that the median inclination is not an objective parameter, in the sense that its value depends on the reference frame in which it is calculated. Then, the median inclination can be insensitive to some changes of the fabric : variability of the c-axes, a tilt of the symmetry axis of the fabric... Detailed comparisons of raw data would be needed to clearly highlight this point.

3.

We apologize for the incorrectly cited work in our original version. This has been corrected following the recommendation of the reviewer.

4.

Following the recommendation, we have enlarged our reference list to other well-known works and earlier studies than that we originally proposed. We apologize for some obvious and important missing citations.

5.

“ Dxx$<$0. Is this right ? ” It was an error and it has been corrected.

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6.

“ the model used in his work is one of the anisotropic modelĚ ”. Yes we totally agree, but there is not so many models with a coupling between the anisotropic ice flow and the fabric development (Gagliardini, Gödert and Staroszczyk). It has been shown that these models give similar results, so it is difficult to discuss this point. Concerning only the fabric development all the models available predict the same kind of fabric for a given strain history, and only the clustering rate varies from one model to another, so that we think that the trends presented here will be obtained with most of the models.

T. Thorsteinsson

All the typological mistakes mentioned, as well as the title, have been corrected following the suggestions of T. Thorsteinsson and the manuscript has been carefully read. We hope that the spelling and wording have been improved. Some of the proposed references have also been added.

About some specific remarks.

1.

“the linear model gives a 10-fold enhancement in shear, impressive.” The linearity is not a limitation to obtain a 10-fold enhancement factor in shear. With the VPSC model (Meyssonnier and Philip, 1996) the enhancement factor can vary from 1 to infinite. The limitation appears only with the (linear or non-linear) homogeneous stress model, which has often been used for ice.

2.

“The discussion of the model results is short and somewhat unclear. Also a few more word about the possible effects of large viscosity contrast between layers is needed (is the value always interpolated, or can there exist sharp boundaries, very sharp!)”

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We acknowledge that the discussion can be uneasy to follow, as we talk about model and measurements. But we also believe that it is one of the major interest of the paper, as it is, to our knowledge, the first time that texture measurements and ice-flow model are so complementary. Anyway, as mentioned previously, the discussion has been slightly improved, and a figure has also been added. About the viscosity contrast between layers: this is the aim of all the paper. About the sharpness of theses variations: experimentally, we do not have any information below our sampling rate. This is obvious. From a modeling point of view, it is interpolated between grid points. We do not clearly understand the point mentioned by the reviewer.

3.

“modification of viscosity. Is it really the viscosity?” The word “viscosity” has been changed to the expression “effective viscosity” all along the manuscript.

4.

“Is the temperature -10 really magical, or is it more a function of accumulated strain” Here, we simply summarized the different recrystallisation processes occuring in ice as it is understood today (see for example Duval and Castelnau, 1995). We also acknowledge that some recent observations at Siple Dome have shown recrystallized texture for lower temperature. But, as there is no evidence of recrystallization in the studied depth range along EDC we do not go further in details. It is clearly not the aim of our work to reformulate the present understanding of recrystallization processes in ice.

5.

“P1193, L10 : What is a higher order anisotropic ice flow model?” “higher order” is the general expression conventionally used to describe models which resolve the stokes equations without any approximation (by opposition to zero (first, second, ...) order models, which are the models constructed using the Shallow Ice Approximation expansion). A whole section is dedicated to a brief description of our model, and references

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to previous publications are proposed. We do not really see what can be improved.

6.

We have added the references proposed by T. Thorsteinsson except for one concerning the folding. The subject of the paper concerns the global effects of anisotropy in general and we do not speak about folding at all. Moreover it appears to be very difficult to obtain folding with an ice flow model where there is a coupling between equilibrium and mass conservation, which is not respected in the paper proposed by T. Thorsteinsson.

7.

“What is the measure of how far fabric is from orthotropy”. Durand et. al. 2006a have proposed a method to determine the gap from orthotropy. We acknowledge that is has not been widely used so far, and the measured fabric looks like to be not far from orthotropy. Moreover, note that most of the parameters used to describe the fabric (median inclination, degree of orientation,Ě) assumes implicitly that the fabric is transversely isotropic, and that the eigenframe corresponds to the reference frame of the thin section.

Interactive comment on Clim. Past Discuss., 2, 1187, 2006.