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Climate of the Past Discussions

Interactive comment on "Anomalous flow below 2700 m in the EPICA Dome C ice core detected using δ^{18}O of atmospheric oxygen measurements" *by* G. B. Dreyfus et al.

G. B. Dreyfus et al.

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We respond to the issues raised using the same numbering as in the referee comment.

1) There is not much mathematics in the manuscript, but the notation is an awful mixture of the verbal and symbolic. One shouldn't use "Acc" to represent accumulation rate in an equation, for example. Why not use b for surface mass balance, which is really what is preserved in the ice core. The discussion of "delta depth markers" is another example. Also "T" is commonly used for temperature so maybe some other symbol should be used for net thinning. How about epsilon, which is the usual symbol for strain?

The notation in the manuscript has been revised throughout to be more consistent with

the notation used in the companion paper: Parrenin et al., The EDC3 chronology for the EPICA Dome C ice core, Clim. Past Discuss., 3, 575-606, 2007. We have substituted the symbol *a* for *Acc*. We maintain *T* to express the thinning function for consistency. The caption to Figure 5 has been modified to avoid using "delta-depth". We agree that Δdepth is inelegant, however, it is consistent with common usage.

2) The discussion on the top of page 11 defining C(z) says that a "value greater than 1 represents compression of the age scale". It isn't immediately clear if that means compression of the original expected age scale or if it means a compressive correction is to be applied to make the new age scale.

We have added the words "original expected" to the phrase, so that it now reads: "a value greater than 1 represents compression of the original expected age scale." The words have also been added to the caption of Figure 5.

3) Section 3.4 and Figure 5c. The fact of the matter is that neither assumption works consistently.

The assumption of anomalous thinning more consistently captures the amplitude and direction of the observed Δdepth variability. To be clear, we have added the phrase "the amplitude of" to "the scenario assuming a pure thinning anomaly better captures the amplitude of these Δdepth variations" (p. 13).

4) Top of page 13, discussion of the fabric and its relationship to the age scale distortion: This is possibly true. But compression of a layer moving "up a hillside" is unusual as ice will generally have to undergo horizontal extension to surmount a hill, and hence enhanced vertical compression, which is the opposite of the picture I think the authors are conveying. (though this depends on where along the flowline we are looking: deep layers will first compress horizontally as they approach the hill and then extend horizontally later). In any case, such a deformation would probably involve the ice all the way from the bed up to a height more than double the topographic relief. The focusing of the anomalous deformation in a 100m section rather strongly suggests it is related to

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anomalous viscosity of that layer instead (i.e., a stiffer layer that resists the pure shear).

The scenario proposed by the reviewer (stiffer layer) is a reasonable explanation, even if it is not fully coherent with all our observations. For example, it does not explain the observed tilt in the crystal orientation. Moreover the objections made by the reviewer about the horizontal compression scenario are also valuable.

The discussion has been modified taking these comments into consideration and now presents both scenarios. We note that the physical processes at the root of the heterogeneities are not fully understood and more work needs to be done. A more in-depth analysis of the physical causes of the anomaly is beyond the scope of this paper, which present the deduction and correction methods.

Modified text (p. 15):

"It is difficult, however, at this stage to find a scenario fully coherent with these observations. The corrected thinning profile indicates that the layer spanning 2920-3000 m has experienced approximately half as much thinning as expected. This could correspond to a stiffer layer that resist the vertical compression. In this case, we would expect to see a clear correlation between fabric variations and the correction function, which is not the case. Moreover, this scenario would not explain the tilt of the fabric orientation maximum. Another plausible scenario could be as follows: a layer presenting a well marked vertical single fabric is subjected to an horizontal compression, the c-axes will then progressively rotate towards the horizontal. Such horizontal compression could account for lower than expected thinning, hence stretching in the age-scale. This is conceivable if the ice layer had to move up a hillside. This is not unrealistic considering that the dome at Dome C may have moved in the past. The ice column beneath the dome coupled to this motion could then have been subjected to the effects of bedrock irregularities, which vary by up to 500 m within 50 km of the EDC drill site (Remy and Tabacco, 2000). We note that such a deformation would probably involve the ice column all the way from the bed up to a height more than double the topographic relief.

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While there are indications of anomalous flow over 500 m in the core, the deviation from the expected thinning profile varies in direction. Additional work is needed to understand the physical processes that are behind the observed heterogeneities, but this is beyond the scope of the present work."

5) In the appendix, it is not clear to me why the "cost function" gives equal weightings to all these terms. Is there some justification for this? It's probably fine but I'd like to know what the authors are thinking. And why punishing a large second derivative? If there really are variations in viscosity associated with distinct layers, the true second derivative may in fact be very large.

It is true that this choice is partly subjective, as is the choice of any cost function in an inverse problem. Because we don't know if the 'real' thinning function is expected to be smooth or not, we chose the 'smoothest' thinning function that still gives agreement with our age markers. This is a classical practice for inverse problems. There is also a practical reason for this choice. Punishing only a large first derivative would have produced an optimal thinning function with artificially sharp minima and maxima, with the danger that some scientists may have interpreted them.

Interactive comment on Clim. Past Discuss., 3, 63, 2007.