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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.

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This is an important contribution, as it changes the way we must interpret the older, more interesting part of the EPICA Dome C core. The general interest here is obvious – this ice core record is the oldest available, and extends our view of the earth system from ice cores back through several additional glacial cycles. The specific interest of this manuscript concerns the age-scale of the deeper, older part of this record, and how the initial estimate for the age-depth relationship appears to have been incorrect in some important details.

The manuscript makes a strong argument that the original EPICA Dome C age scale (used in the 2004 publication apparently) is corrupted at the multimillennial scale by unexpected variations in either accumulation rate or cumulative strain. The argument relies on isotopic composition of diatomic oxygen as a chronometer that can be tuned against precession. Although it would be best to understand this remarkable relationship better mechanistically, I think the evidence is good enough at this point to assume this relationship can be used. This is the first manuscript that presents 18-O of diatomic oxygen data for this long record.

The authors derive a corrected age scale from this analysis. The important immediate result is that the long "interglacial" stage 15.1 is reduced in duration by a factor of two, and the apparent gas-temperature phase relationship at stage 14.3 is found to be similar to that for the rest of the record, whereas before it appeared there was an anomalous lead of gas changes with respect to isotopic temperature.

The second argument is weaker – that the age scale deviations are due to strain variations rather than accumulation rate deviations. It is weaker but nonetheless still consistent with the weight of evidence discussed.

I think the paper can be published with minor revision. The few issues I would like to see improved are as follows. (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? (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. (3) Section 3.4 and Figure 5c. The fact of the matter is that neither assumption works consistently. (4) top of page 13, discussion of the fabric and its relationship to the age

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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 focussing of the anomalous deformation in a 100m section rather strongly suggests it is related to anomalous viscosity of that layer instead (i.e., a stiffer layer that resists the pure shear). But it is hard to know for sure. Time-dependent changes can cause all kinds of structural features. (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 punish 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.

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