tc-2013-66: Answer to anonymous Reviewer #2

Authors

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General Comments:

As far as we understand the first issue of the reviewer, he criticizes that our ice flow model is made for places far away from ice domes and divides where horizontal flow occurs (which he implies is included in the ice flow model), but we simulate only vertical diffusion. Actually, the ice flow model we use here regards only vertical ice flow and not horizontal flow as it is assumed by the reviewer. The reason why the vertical flow at domes and divides must be distinguished from areas far away of them is that the viscosity of the ice is dependent on the deviatoric stress and, hence, the parametrisation of the vertical thinning function of the ice is different for these two different areas. The reason why we use this parametrisation for the vertical flow is that it is widely used to determine ice core agescales, in particular also for the EDC ice core (Parrenin et al., 2007a), and that the tuning parameter can be estimated based on the boundary conditions of a potential ice core. Since this estimate might be wrong if the drill site will be a dome or divide, we have done the corresponding sensitivity test for the Oldest Ice Core simulations which showed that it is of minor importance in regard to the other uncertainties.

The second issue of the reviewer regards the evaluation of the permeation parameters. He claims that the parameters we use are determined for scales in the range of micrometers or less and we simulate processes in the order of centimetres to metres. In the case of the FS parameters there is indeed a large difference between the scales of the original work (molecular scales) and the scales we use the parameters here (few centimetres). However, the authors of the original work themselves seem to believe that their parameters can be applied for centimetre scale effects since they present calculations of ice core outgassing in their work using their parameters (Ikeda-Fukazawa et al., 2005). Furthermore, in our manuscript we state that the way the FS parameters have been derived does not regard the heterogeneity and layering of polycrystalline ice in ice cores which might be a reason for the discrepancy. In the case of the SS parameters we do not agree that the scales of the parameters are much different. In the corresponding work (Salamatin et al., 2001) the exchange between neighbouring inclusion in the ice is modelled to derive the parameters. The typical distance of neighbouring inclusions is in the order of a few millimetres (Uchida et al., 2011) at which scales also the heterogeneity of the ice should be relevant. The model of Salamatin et al. (2001) averages the diffusional exchange over a large amount of analysed inclusions which are trapped over a range of several hundred meters in the ice core where they are exposed to different types of ice layers. For these reasons we argue that the SS parameters include the heterogeneity of natural ice. Therefore, we believe our model can be used to evaluate the parameters, in particular for the SS parameters.

Specific Comments:

Units of Temperature (K vs C): It is not fully consistent throughout the manuscript. We will change this.

References to Haeberlin (M.S. thesis): Unfortunately, we did not find an useful alternative. We can provide this work to the reviewer or any other person if requested. Be aware that it is written in German. If the reviewer is aware of a peer-reviewed publication in which an experimental setup for gas permeation measurements through ice is described and analytical challenges are discussed, we look forward to implement this into our reference list.

Figures 7+8: This is correct. In Figure 8 the x-axis labelling is wrong. It should be Myr instead of kyr. We will change this accordingly.

We thank the reviewer for the constructive criticism and the help to improve our work.

References in this text can be found in the Manuscript.