tc-2013-66: Answer to Reviewer Jeff Severinghaus

Authors

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Major Comment:

The main concern of the reviewer regards the two orders of magnitude disagreement in permeabilities and their treatment in the model. The question is whether the actual architecture of the model misses the two-phase characteristic of the ice or not, which could roughly explain the two orders of magnitude difference. To refer to this point we describe here quickly the relevant architecture, which is also found in Bereiter et al. (2009).

At each time step, two calculation steps are performed. First, based on the 1-D diffusion equation, a new amount of dissolved gases (first phase) is calculated for each box using the diffusion parameter of Ikeda-Fukazawa et al. (2004), ignoring the second phase of trapped gases. After that, in each box the new amount of dissolved gas is equilibrated with the amount of trapped gas using the solubility of different studies (Ikeda-Fukazawa et al. (2005) and Salamatin et al. (2001) (for O_2 and N_2) and Ahn et al. (2009) (for CO_2)). This second step of equilibration between the two phases (dissolved and trapped gases) reverses much of the changes in the dissolved part of the first step since much more gas is in the trapped phase compared to the dissolved phase. In this way the model architecture considers the buffer effect within a two phase material as described by the reviewer, which causes in the transient case a dampening of the net change. Therefore, we believe our architecture is not an issue in regard to the two orders of magnitude difference of the parameters.

The question remains whether the origin of the different parameter sets consider the two phases in the ice or not. In the case of the parameters of Ikeda-Fukazawa et al. (2004 and 2005), the diffusion and the solubility in the ice are determined separately of each other in a molecular simulation, hence, they are not influenced by the different phases and amounts in the ice. So their values are not suffering the two phase issue. In the case of Salamatin et al. (2001) the method includes both phases, but also in their model - as far we understand - they account for the two phases and the different amounts in them. Accordingly, we also do not think that the original works missed the two phases in the ice.

In this context we want to mention some other points. The solubility parameters of Ikeda-Fukazawa et al. (2005) got never published officially, as far as we know. In this publication the values are referenced to a publication marked as "submitted for publication", however, the corresponding paper got never published.

In our model it is assumed that in each time step the whole amount of the trapped phase (regarded as clathrates) gets equilibrated with the dissolved phase. This could be wrong when only the surface layer of the clathrate would equilibrate in one time step and the internal diffusion within the clathrate would matter. We have tested this effect in an experiment where the inclusions were

assumed to have a spherical shape with a radius of 0.06 mm (reasonable value for clathrates (Uchida et al., 2011)) while the internal diffusion factor were set equal to the ones in the ice. The internal diffusion factor needed to be scaled down by another factor of 10^5 in order to see an effect. It is not expected that the diffusion within the clathrates and in the ice are so different, for which reason this effect is believed to be insignificant.

Minor comments:

Figure 1: For better comparison, we have shifted the depth of the Oldest Ice Core downwards by 453 m such that the bedrock is at the same depth for both EDC and Oldest Ice Cores. This information is missing in the figure caption. We will resolve this in a revised version.

Line 149: We will change this as proposed.

Line 182: We will change this as proposed.

Line 202: We will change this as proposed.

Line 294: We will change this as proposed.

Line 313+315: We will change this as proposed.

Line 336: Indeed, this might be a possibility and is an interesting idea we will keep in mind for future work. For this work we think this idea is beyond the scope. In addition, there exist no $^{18}O(O_2)$ measurements at the deep ice of EDC yet.

Line 341: We will change this as proposed.

Line 345: We will change this as proposed.

Line 371: We will change this as proposed.

Line 372: We will change this as proposed.

Line 426: We will change this as proposed.

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

References in this text can be found in the Manuscript.