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The Cryosphere
Discussions

Interactive comment on "Can we retrieve a clear paleoclimatic signal from the deeper part of the EPICA Dome C ice core?" *by* J.-L. Tison et al.

Anonymous Referee #2

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I strongly recommend this paper for publication in The Cryosphere. The paper contains interesting data and conclusions, and the scholarship is at a high level. I am mainly interested in commenting on aspects of the geochemistry and dating. I think the use of the term "soluble" to refer to the ions is unfortunate, because, as the authors explain at the end, gases are also soluble. I think the useful distinction is between ions forming soluble salts, ions forming insoluble salts, and gases. Gases should be included in soluble constituents at the beginning. The authors discuss the fact that minor melting rearranges the distribution of ions but not gases, and they refer to the poorly known behavior of gases during partial melting. I should think that melting would primarily attack grain boundaries that are rich in salts, whereas clathrates are in the interior of ice crystals and less likely to intercept a melt zone. So it seems that one can certainly say that gases are less susceptible to redistribution than salts.

The discussion at the end of the paper describes or implies several possible scenarios for the age and age distribution of the deep and bottom ice. I think that the data only allow a single interpretation, and that is that the ice must date to a single cold climate event. The interpretation rests on small cycles in the d18Oatm record, each of which is attributed to an orbital cycle. Without more information, this interpretation appears plausible because the d18Oatm record in the deep/bottom ice looks a lot like the record between about 700-760 ka. The similarity includes the fact that d18Oatm is isotopically light when Dome C is cold. However, this interpretation invokes the presence of interglacial or interstadial ice from the warm periods in the orbital cycle. This presence is improbable because the isotopic temperature of the deep/bottom ice is glacial. There does not seem to be any way to separate the ice and its trapped gases. So it seems to me that the deep/disturbed ice is all glacial. Either it represents a single glacial period, or it represents the mixing of ice from several glacial periods without any incorporation of interstadial or interglacial ice. Of these options, the first seems more likely. For one thing, the continuous record includes the end of the glacial period ending at 800 ka. The d18O of this glacial ice is low, blending right into the d18O record of the underlying deep/bottom ice. So it seems to me that the most likely interpretation is that the deep/dirty ice represents the earlier part of the 800 ka glacial interval. Any alternative requires a mechanism for mixing glacial ice from different periods with minimal inclusion of warmer ice, as noted above. The new gas data needs to be tabulated in the paper or in the supplemental material. The Ar/N2 and O2/N2 ratios need to be reported because these ratios might be fractionated if gases were in fact transported by meltwater in the deep and bottom ice.

The authors seem to conjecture that respiration may have consumed some O2, and that this consumption may have affected d18Oatm. However given that the CO2 concentration is so low, and around the value expected based on the isotopic temperature, it is very unlikely that respiration has consumed enough O2 to significantly modify the d18Oatm value.

I recommend that the paper be published after 3 actions. First, the authors need to include a table with all gas data either in the SOM or the paper. Second, the discussion of respiration needs to be revised to include low CO2 concentration of the trapped gases (which to me rule out respiration). Third, the authors should respond to the comments about the origin of the ice, although they can keep their favored interpretation.

Interactive comment on The Cryosphere Discuss., 9, 567, 2015.