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Climate of the Past Discussions

Interactive comment on "Glacial-interglacial dynamics of Antarctic firn columns: comparison between simulations and ice core air-δ^{15}N measurements" *by* E. Capron et al.

Anonymous Referee #3

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Review of Capron et al.

(FYI I have not seen other reviews and replies) This manuscript presents new d15N data from Antarctic ice cores and discusses origins of discrepancies between the data and model predictions (assuming no change in convective zones) during the last glacial maximum and subsequent climatic transition into the current interglacial. The problem of firn thickness in glacial periods is one of unsolved issues in ice-core paleoclimatology and glaciology for the last few decades. The topic is well suited for CP and this special issue, and the authors made good efforts in collecting data from different ice cores. However, interpretations of the data and discussion of mechanisms for firn thickness

variations are sometimes weak and difficult to follow, thus the conclusions are not well supported at least in the current form. Before the manuscript can be published, the authors should analyze the data and conduct model experiments more extensively to draw solid and useful conclusions, or they should greatly reduce the manuscript to simply present the data and describe the (in)consistency between data and model, and make basic discussion/speculations for each sites without bold statements.

Major comments: There are a few major points in the abstract and associated text. P6053 L12. This sentence does not make sense. d15N during the last termination at EDC and EDML are qualitatively consistent with model outputs where dD increases, but the modeled magnitudes are underestimated. Modeled DCH, under the assumption of no change in convective zone, drifts away during relatively stable climatic conditions, making the overall change of firn thickness over the termination opposite to estimations from d15N. It would be necessary to investigate what caused the slow and large changes of modeled DCH in ACR and EH by conducting sensitivity tests of the model with only temperature or accumulation change (while fixing the other), or different coefficients for converting dD to the climatic variables. In the current manuscript such exercises are done for very limited cases, thus they don't support the authors' arguments.

The observation of small convective zone is only made for MIS 3, which is not exactly LGM, and only for EDML. This cannot really support the statement that convective zone did not develop at EDML in LGM which is the studies period for d15N. Even with additional published suggestion for EDC, the generalized conclusion that there were no changes in the size of convective zone at all sites is not supported by these observations. I think it is fine to include the speculation that the origin to may lie within accumulation rates, but it should only be written as speculation (not as definitive conclusion as the current manuscript reads) because I don't think they provide enough support for it. Maybe the authors could strengthen the case for EDML in LGM by conducting similar delta-depth exercise as done by Parrenin and colleagues for EDC.

A significant conclusion is made against the hypothesis of major effect of impurities on firn densification rate, but I don't think that the materials in this study are strong enough to reject such hypothesis. In fact the text describes uncertainty associated with magnitude and scaling of dust content, which makes the quantitative discussion of dust effect almost impossible. When plotted on a log scale, the profiles of dust content and dD become very similar, making it hard to separate the effects of temperature, accumulation rate and dust (as acknowledged in the text) without knowing the actual physics of dust effects. The presented study does not provide information to solve this, thus the statement in the abstract is too bold.

Minor comments: It should be made clear in the abstract and text that this study ignores any temperature effects on the d15N signal. This may be not correct (read papers by Severinghaus and colleagues).

In general, the manuscript is too long and unfocused with observations and arguments scattered around.

Fig 8 presents the steady state solutions of the Goujon model for different climatic parameters, so they don't reflect real (transient) change in d15N. So there seems little meaning to present the figure.

Interactive comment on Clim. Past Discuss., 8, 6051, 2012.