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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.

E. Capron et al.

ecap@bas.ac.uk

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2. Specific Comments

I don't think you present enough evidence to support the last paragraph of the abstract, namely that firn densification dynamics may be driven mostly by accumulation rate changes. Specifically, you have shown for the TALDICE core that the measured δ15N is intermediate between simulations that allow only for the influence of surface temperature or accumulation rate. Furthermore, Figure 8 clearly shows the dependence of simulated δ15N on both surface temperature and accumulation rate. Therefore, clearly surface temperature is also very significant. I think the first sentence of last paragraph

of the abstract needs to be altered to something more like “We conclude that accumulation rate changes may play a more significant role on firn densification dynamics during deglaciation than current firn models simulate”

->We have changed the abstract in the revised manuscript and we now say that “the influence of the accumulation rate on firn densification dynamics might be more important than currently simulated in the firn model”.

Introduction: It could be made clearer (possibly just after P6055 L5) that there are two competing mechanisms influencing the fractionation of Nitrogen (in the absence of rapid temperature changes) represented by the barometric equation. 1) The firn temperature directly impacts fractionation and 2) any factor that changes the diffusive column height, which will include factors such as firn temperature (again), accumulation rate, initial snow density and firn permeability.

->Done. We have changed the new manuscript accordingly in the paragraph following the description of the isobaric equation (Equation 1).

P6056 L25-29 These two sentences need expanding. In particular, need to justify the statement that firn models still should predict LID correctly even though they don’t simulate permeability. In addition need to clarify what is meant by the statement “only δ15N should be affected” affected by what, the model shortcomings discussed in the previous sentence, or the changes in permeability discussed two sentences ago.

->We feel like it is beyond the scope of the paper to expand on the statement that firn models still should predict LID correctly even though they do not simulate permeability. Also, we wish to keep the introduction as concise as possible to meet the reviewer 2’s comment on writing up a shortened and more focused manuscript. However, our statement that firn models still should predict LID correctly even though they do not simulate permeability can be validated by the fact that firnification models are able to correctly represent the LID of modern firns in Greenland and Antarctica even if the permeability is not represented. In the new manuscript, the two sentences have been

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removed. Instead we have written a new sentence which is hopefully clearer on how the measured δ15N would be affected if a deep convective zone develops during glacial time.

P6059 L19-24 Suggest adding some text to explain that porosity is not directly modelled, but calculated from the modelled firn density, so that the selection of a closed porosity corresponds directly to the selection of a firn density for bubble close off.

->We have followed this suggestion and have added a few sentences in the new manuscript. We have also added a few sentences to better explain how we choose the closed porosity threshold in the Goujon model to define the LID.

P6061 Equation 4. This equation appears to be incorrect, and "δD" should be "$\Delta\delta$D"

->We have corrected the equation in the revised manuscript.

Give units for both α and β, throughout the manuscript including Table 2.

->Done.

There is a lack of consistency in the units used for accumulation. Throughout the text "ice eq.yr-1" has been used yet Figures 1 and 8 use "we.yr-1" which is presumably "water equivalent per year".

-> We have now used only the unit "water equivalent per year" throughout the text for consistency with the unit used in the figures.

P6064 L14-17 The statement concerning the sensitivity of BI scenario A to variations in accumulation or temperature needs clarifying. In particular, scenario A is not more sensitive than scenario B to both temperature and accumulation variations. In fact compared to scenario B, scenario A will produce stronger response in δ15N for a given temperature variation but a weaker response due to accumulation variations.

->The key message in these two sentences was that it is not clear at all how to expect the firn thickness (and thus δ15N) to react when we combine the effects of both surface

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temperature and accumulation variations. It is illustrated for example when we observe in Figure 8 from the CPD manuscript that accumulation rate in Scenario A describes a larger amplitude change over the deglaciation than the accumulation rate in Scenario B, however the amplitude change in δ15N over the deglaciation is stronger for Scenario B than for Scenario A. However, since we do not present two scenarios anymore for Berkner Island, we removed the discussion and we simplified the text in the revised manuscript.

My preference when giving multiple citations is to list them in chronological order. Suggest changing the ordering of citations to reflect this, for example P6064 L23-24 would change the order to Kawamura, 2000; Caillon et al., 2001; Dreyfus et al., 2010).

->Done throughout all the revised manuscript.

To help lead the readers through Section 5, suggest changing P6067 L1 from “First, Hypothesis C” to “First, in Sect. 5.1, Hypothesis C”.

->Done.

Should make it clear throughout the manuscript when you are given numerical values for Δdepth if this is ice equivalent metres or actual metres (will obviously be the same for deep layers).

->By definition, the Δdepth is the depth difference observed along the ice core record between two synchronous events recorded in the ice phase and the gas phase respectively. Thus, by definition, Δdepth corresponds to a quantity given in real meters that are also meters of ice and one would never discuss about Δdepth in the firn. Hopefully, we made this point clearer in the revised version from the introduction.

There is no real evidence to support the statement on P6070 “The deglacial increase in Antarctic accumulation rates is underestimated, especially at the end of the deglaciation” The “arguments supporting this view below” are âĂć That δ may vary considerably over time at a given site âĂć Good agreement between layer thickness bases ac-

cumulation estimates and thermodynamically based accumulation estimates at EDML âĂć Accumulation variations uncorrelated with water isotope changes at EDC Together these do not provide sufficient support for above statement.

->We removed the sentences “The deglacial increase in Antarctic accumulation rates is underestimated, especially at the end of the deglaciation. We give arguments supporting this view below” in the revised manuscript. We have also reformulated this discussion.

3 Technical corrections

->All the technical corrections have been taken into account in the revised manuscript for the affected sentences that are still in the revised manuscript.

Thanks a lot to Jason Roberts for his recommendations and comments that helped improving the manuscript.

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