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Interactive comment on "Present and future variations in Antarctic firn air content" *by* S. R. M. Ligtenberg et al.

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My recommendation is that this paper could be published in the Cryosphere after some modifications.

This study considers the volume of air stored in the void spaces of snow, firn and unconsolidated ice that comprise the upper layers of the Antarctic ice sheet. Using a model of snow and firn compaction the authors investigate how this volume of air varies in time and space. These variations are important because they can lead to errors in estimates of how the mass of ice is changing in Antarctica, and hence estimates of the contribution that this ice sheet presently makes to sea level.

Two particular sources of error in estimates of changes in the mass of the Antarctic

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ice sheet are considered here. The first is the conversion of volume changes to mass changes that must be performed in any estimate of the mass change that relies upon satellite altimetry. The second is the conversion of elevation to ice thickness based on the assumption that the ice is floating in hydrostatic equilibrium. Thickness estimates derived using the hydrostatic assumption are often used together with ice velocities to estimate ice discharge across the grounding line, an important component of the mass budget of the ice sheet. This study shows that the impact of changes in firn air content on both the altimetric and mass-budget approaches can be significant.

The study goes further to simulate the likely changes in firn air content that may occur in future. By forcing the firn compaction model with climate projections from a General Circulation Model, the authors find that increased snowfall will likely lead to increases in the amount of air within the void spaces of Antarctica. At lower elevations, two effects act in opposition to the effect of increased accumulation. Warming promotes faster compaction and increases melt and percolation of meltwater into the snowpack. Both these effects expel air from the firn. All of these effects will need to be corrected for in altimetric studies, and the study presented here give valuable insight into the magnitude of errors that might arise if they are not corrected for.

I think the biggest weakness of the paper is the formulation of the firn compaction model. Although the model is used in a time-dependent calculation, there are aspects of the model that are fundamentally based upon an assumption of steady state. In simulations of the future, several key parameters in the model are identified using an assumption that the compaction has equilibrated to the climate of the past 40 years. There is no guarantee that this is true. In fact it would be highly unlikely for the characteristic response time to be uniformly 40 years across Antarctica, given the spatial variations in temperature and accumulation rate across Antarctica.

It would definitely be preferable to include a more physically-based approach to the temporal variations of firn compaction that does not rely on equilibration to reference values for accumulation rate and surface temperature. Expressions B1 and B2 in Appendix B of Arthern et al. 2010 provide an example of a system that could be solved to dynamically evolve the relevant quantities.

Failure to include the dynamical evolution of relevant quantities stops this from being a full, physically-based model of the processes that control air content and this should be pointed out more clearly than it presently is. However, I do not think the above concerns completely invalidate the results of this paper, so I would not insist on these changes to the model being included in this particular paper.

Rather, I think the potential weaknesses of the assumptions that have been made need to be clarified and some attempt should be made to quantify their consequences. Also, some of the assumptions that have been made implicitly should be pointed out explicitly. For example, it should be highlighted that the approach used here will only work well if the changes in climate forcing are sufficiently small that the climate of the previous 40 simulation years does adequately represent the climate on whatever timescale the process of firn compaction actually responds.

There are several different timescales involved in the response of the firn column to climate. The timescale for adjustment to a new accumulation rate can be different from the timescale for adjustment to a new temperature because the latter must propagate to deeper layers through conduction and advection of heat. The paper should point out that different timescales operate for these different processes. The paper should also note that some aspects of the equilibration, such as equilibration of the grain-size profile, may take place on an advection timescale that could easily be measured in many thousands of years in East Antarctica, not 40 years as assumed here.

Taking the above two points in combination, there are likely to be some locations where the present formulation the compaction model is effectively using an assumption that the climate of the previous 40 simulation years is the same as the climate over many thousands of years. This should be noted in the paper as an explicit assumption of the approach that is used to model regions of very low accumulation.

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It may be that the above assumption is not so bad, especially since it is only being used to provide a representative reference state for certain model parameters, however the paper should include some assessment of how different the climate might be over the 40 year window from the climate over the actual response time (perhaps by taking the time for advection to the pore close off depth as an upper limit for this timescale). A rough order of magnitude for variability in temperatures and accumulation rates could be assessed from ice-core based reconstructions of Holocene climate.

Further, some quantitative estimation should be given of how different the results of the model might be if a different reference climate had been used. This would not need to be done everywhere, but some offline simulations at representative locations could provide enough information to judge the importance (or not) of this effect. Even better would be to include a few offline simulations that included the full dynamical evolution of relevant quantities such as grain-size and snow load, to show that results from the current model are consistent with a more physically-based approach.

Minor points

P424. Line 9. The term ‘air content’ is sometimes used in glaciology, especially in ice core research, to refer to volume of air recovered per gram of ice (e.g. Martinerie et al., J. Geophys. Res., 99, D5, 10,565-10,576, 1994). There is also a precedent for the term as used here (e.g Holland et al., Geophys. Res. Lett., 38, L10503, 2011). These two uses have slightly different definitions, so it would be good to add a clarifying sentence to explain any difference in usage. An alternative would be to use a different phrase, such as ‘integrated firn air content, or IFAC’.

There is a possibility for some fraction of water within the firn. This does not seem to be acknowledged by equation (1). For water-saturated firn, FAC as defined by equation (1) could even become negative. There is a precedent for the convention of negative FAC indicating presence of water (Holland et al. 2011), but some words are probably needed here to clarify what is meant by FAC if liquid water is present. If it is being assumed that no liquid component is present this should be stated.

P430. Line 26. Assuming no vertical shearing, ice discharge is the product of ice thickness, depth-averaged density, and velocity. Errors in the density profile will affect estimates of ice thickness based on floatation, but will also affect the depth-averaged density. It should be made clearer whether this effect has been included. In other words, does the figure 0.7% refer to errors in thickness or in ice-equivalent thickness? The difference may not be too important, but it would be good if the text was clearer about which calculation was done.

P439. Line 23. The wording could be clearer here:'next to a mass increase' could be replaced by 'in addition to a mass increase' if that is the intended meaning.

Interactive comment on The Cryosphere Discuss., 8, 421, 2014.