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

Anonymous Referee #1

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The manuscript (MS) by Ligtenberg et al., presents the results from an off-line-coupled firn compaction model forced by the regional climate model RACMO. The firn model itself has been described in previous publications and the focus of the MS is two new model runs; one for present-day conditions and one for a future forcing scenario until 2200. A time-dependent (TD) model simulation is compared with a steady-state (StSt) published in a previous publication. The authors find a significant difference between the StSt and the TD with direct implications for mass balance studies for the Antarctic ice sheet. The description of dynamic changes in the firn air content (FAC) is an important result. In addition, the future projection shows that the firn dynamics remain important in a warming climate and is of equal value. The conclusion of the study directly implies to mass balance studies of ice sheets and is therefore suited for "The Cryosphere".

General comments: The MS is well written and contains important quantifications of the climate sensitivity of firn, which have to be considered for remote sensing mass balance studies. The MS adopts the setup of Ligtenberg et al. (2011) and without this paper in fresh memory the model description and following results may be hard to follow. The authors clearly state the model description not to be the scope of the MS. With this in mind, it is hard to follow the comparison between the StSt and the TD. I will get back to this inter-comparison between the two models and its problems. I think the MS should focus on the output of the TD and not so much on the difference to an "outdated model". The reference given from Antarctic studies and maybe a couple from Greenland would support the conclusion from the inter-comparison of the two models.

Based on the bias correction of the StSt model and its missing meltwater, I think that the TD model evaluation for present day condition to the StSt does not add insight in to firn dynamics. The need for dynamic-firn modeling is well established in the literature. It seems that the authors agree when they use 5 pages on the results of these simulations but only 10 lines in the discussion. In addition, the 10 lines of discussion are mainly references to prior work by the authors, which is more suited for an introduction. I think the importance of the future simulations can be emphasized if section 3.1 was shortened.

If the comparison between the two models is kept for the final MS, a quantification of their basic differences has to be done. The MS uses much time in reporting the difference between the StSt (Ligtenberg et al. 2011, fig8a) and the time-dependent (TD) firn densification model output in terms of air content for present-day conditions. It is clear that the two models results in different structure of the firn, especially in areas of water percolation. Additional, the StSt model applies a bias correction to fit the depth of the 550 and 830 densities observed in firn cores by Ligtenberg et al. (2011) and this gives raise to differences in the FAC. It would be interesting to see how much of the difference is accounted for by the bias correction and how much by the missing melt water account for differences. It might give valuable insights to rerun the TD without

melt water to quantify some of the differences or to focus on the area with out melt in the presented model runs to quantify the bias correction. The MS references the supplement of Depoorter et al. 2013, and estimate 10% errors for the FAC, however much of the difference between the StSt and TD is within 10%.

Following the focus on the top firn, which is of particular interest for remote sensing altimetry, brings me to the surface density applied in the models. Based on the level of references given in the MS, it may be hard to follow which exact surface densities are used in the StSt and in the TD, and at what time resolution the surface density is prescribed? In Ligtenberg et al. 2011 the surface density is following Helsen et al. 2008, however investigations into surface densities were done using the RACMO model in Lenaerts et al. (2012). Could the authors comment on the difference in the density parameterizations and the implications for the comparison of the two models, also manifested in the applied bias correction?

The MS tries to quantify the effects of the introduction of melt water to the Antarctic firn. However, I find little validation of the "snowmelt" module, in the given references. The only reference would be Kuipers et al. 2013 who states "The tipping-bucket approach performs well against other models of firn hydrology (Wever and others, 2013)". Neither, Kuipers et al. (2013) or Wever et al. (2013) are cited in the model set-up. Since the inclusion of refrozen melt water is of concern in this MS I would like more discussion about this either in the model set-up or in the discussion part. In addition the right amount of retained water only accounts for half the story as the location of ice lenses and water aquifers may be as important for remote sensing applications as in mass balance studies.

Specific comments: Page 426, l. 14+ the spin-up: How does the two different spin-up strategies for the spin-up affect the modeled firn? Why is the same strategy not chosen for both? From comparing lines 6-8 page 427 with line 3-5 page 428 there seems to be a slight difference in the spin-up procedure for the present and future simulation. Is there any difference in the profiles of 2012 when they are compared?

Page 426, l. 20+ subsurface temperatures: The Arthern equations apply an annual mean temperature Tav, which was estimated at each site from the average temperature of the deepest thermistor, at approximately 10âĂŘm depth. How come the present day simulations use a fixed average temperature and not evolving as modeled by the heat transfer in the firn? The same could be done for the future simulation where the past 40 years is used. This should not make a significant difference, however I'm not sure the 40-year average makes more sense other than being convenient for the present-day simulation and limiting the computational demand.

Page 429, l. 15-17: The bias correction overestimates the top density of the StSt profile. In fig 2a; how much of the FAC is accounted for in this overestimation? The effect of bias correction has to be investigated in order to compare the two simulations.

Page 429-430: The authors are pointing out that the StSt model is missing melt percolation. However, I wonder how many firn cores from melt areas were used in deriving the parameterization in Ligtenberg et al. 2011? Some of the arguments are circular when having no melt in the StSt and possible little validation of percolation models.

The discussion: I would like to see more discussion of the surface density for present conditions. More clarity in what are results, discussion and conclusion is needed.

Page 438, l.9-11: This statement is referring to the introduction and may not be entirely true. It seems that the literature has evolved into dynamic models in mass balance studies (eg. Zwally et al. (2011), Sørensen et al. (2011) and Pritchard et al. (2012)), selected references should be added somewhere.

Page 438, l.20-23: Again, the comparison with an obvious outdated model does not justify anything.

Figure 5, it is a very interesting figure, however the firn densification is driven by a combination of the subsurface temperatures and the overburden load. It might be more intuitive to split the firn densification (b) into two: the temperature and the surface

density.

Technical corrections: Page 422, l. 14: “within the 33 yr period” which period is this? 1979-2012? Page 422, l. 23-24: The elevation change split in air and ice (mass) would give more insights. Page 423, l.4: This has been investigated for the GrIS, maybe one or two more references would be beneficial. Page 424, l.17: The abbreviation AIS might be confused with the later definition of ice shelf (IS) especially Amery IS. I can’t think of an elegant solution but maybe shorten the Antarctic Ice Sheet as (AntIS) or something similar. Page 426, l. 26: The RACMO2 model is presented as known; a couple of references might help other readers. Page 427, l. 8: How long time does it take for the firn to be refreshed (maybe range). The same could apply to page 428, l. 4. Page 432, l. 26-27: “which should be in agreement” Is it? Be more precise. It is hard to compare with the different time periods in mind (1960-1999 vs. 1979-2012). Page 433, l.12: The authors give multiple references to locations in the paper, but no geographical location is given for Amery IS. Page 436, l. 12: remove “crystals” Page 436, l.25-26: In which of the previous work. Give a reference for the statement. Page 437, l. 1: I agree with the statement but the bias correction play a role in the FAC, which is not evaluated in Ligtenberg et al. 2011.

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