

Response to reviewer comments:

First, we would like to thank all three reviewers for their positive and constructive comments, which improved the MS. Re-running our firn model (FDM) simulations with slightly different settings in order to perform a sensitivity analysis is not feasible due to the computational time (several months). Two main points were raised by all reviewers; 1) comparing/evaluating the present-day simulations with observations and 2) discussing the weaknesses of the semi-empirical FDM. We first discuss these two major points and weigh the different opinions of the reviewers, before addressing the remainder of the comments point-by-point. Our response to the comments is written in blue and changed/added text to the MS is in *italic*.

1). Reviewer #1 comments that the comparison between the steady-state (StSt) and time-dependent (TD) simulations might be “out of focus” as the StSt model is “outdated”. He/she feels that there is too much emphasis on the present-day results, while the more interesting future results are perhaps overshadowed. Reviewer #3 states that the lack of model evaluation with ground observations is weakening the model results, which contradicts with the statement of Rev1. Rev3 feels that the TD results in 3.1 are suitable for such a model-obs. comparison.

We agree with Rev3 on this point.

First, we do not agree that the StSt results are “an outdated model”. The StSt simulation of firn density and air content have until very recently been used in high profile papers, e.g. to estimate the mass balance of the Antarctic ice sheet (Shepherd et al. 2012), the mass balance of Antarctic ice shelves (Depoorter et al., 2013 and Rignot et al., 2013), and in the BEDMAP2 project (Fretwell et al., 2013). Therefore it is important to show the differences between this and the new TD solution. The latter includes melt, which changes the firn pack characteristics especially along the ice sheet margins. Moreover, the TD solution shows that firn depth, density and firn air content cannot be taken as a constant quantity. We feel that the importance of using a TD simulation over a StSt simulation is currently introduced properly in the 4th paragraph of the introduction. In Section 3.1 we elaborate on the main differences between both simulations.

We agree with Rev3 that a model evaluation against in-situ observations would strengthen the MS. He suggests to either use the firn core measurements from an earlier paper (Ligtenberg et al. 2011) or to use density profiles from different times to assess the temporal variability in the simulated firn density profiles. As far as we know, the latter do not exist, and if they do they are likely not at a high enough vertical resolution to assess small difference in the density-depth profile. Therefore, we implemented his first suggestion and compared the time-dependent (TD) FDM simulation with the results from Figure 4 and 6 from Ligtenberg et al. 2011. We included a new Figure (Figure 3) and a new paragraph to section 3.1 to discuss Figure 3. It shows that the here presented TD simulation compares very well with the StSt solution. The differences in RMSE between the StSt and TD are small for both z550 and z830. The reason is that melt is small or non-existent at the observational sites.

New paragraph:

“The latter statement is accentuated by the comparison of depth-density characteristics with in-situ observations from dry firn cores and earlier model results. Figure 3 shows the depth of two critical density levels (550 and 830 kg m⁻³) as observed in 48 firn cores (van den Broeke, 2008) compared with four model simulations; steady-state simulations by the Barnola et al. (1991), Arthern et al. (2010) and Ligtenberg et al. (2011) models and the time-dependent FDM simulation used here. The time-dependent FDM results show good agreement with observed depths (z550 and z830); both the magnitude and range are similar. Moreover, the steady-state (black square) and time-dependent (red circle) FDM results agree well for almost all locations. The difference between both is generally below 2 m, which is lower than the uncertainty in most firn core

measurements (van den Broeke, 2008). The slightly higher/lower RMSE for the 550/830 kg m⁻³ density level, compared to Ligtenberg et al. (2011), indicates that the surface-density bias correction introduces a small error into the density profile of the steady-state simulations. The difference in FAC between the two FDM simulations originates from the bias correction in surface density and can be separated into the obvious mismatch in the density near the surface and a smaller mismatch in the entire depth-density profile. The seasonal differences in temperature are only felt in the top 3-5 m and can cause FAC differences up to 0.2 m (note: the FAC difference in the top 3 m in Figure 2a is 0.04 m). The second process is more important, as it affects the entire firn column. For example, a mismatch of 5 kg m⁻³ in the entire firn column (100-120 m) leads to a FAC difference in the order of 0.5 m. For locations with significant melt, the effect of this bias correction is generally smaller than for no-melt locations, although it is difficult to quantify. Especially the mismatch due to the second process is smaller because of meltwater refreezing and the subsequently higher firn density.”

In the last sentences of the newly added text, we also addressed another point of Rev1; discussion of the influence of the bias correction in surface density on the resulting depth-density profile, both with and without melt (see above). We quantified the difference in FAC between the StSt and TD simulations for locations without melt and added a discussion on the partitioning of the difference on locations with melt.

2). All 3 reviewers comment on the lack of discussion about the use of a semi-empirical model for this study and its disadvantages.

- There are more physically-based FDMs available and one suggestion is to use such a model or its equations to identify differences in the results. This is however not feasible due to the high computational time/cost of simulating the firn layer of the entire Antarctic ice sheet. Also, with the inclusion of the new Figure 3 it is shown that the present-day results compare well with observations and earlier model studies. To emphasize the (dis)advantages of using a semi-empirical FDM, we included an extra discussion paragraph.

- All 3 reviewers comment on the choice of the 40-year running window for the determination of the average accumulation/temperature. To clarify, we included an extra paragraph in the discussion section.

- Rev1 suggests to add discussion about the used surface density approximation.

We combined these 3 comments in the following new discussion text:

“The FDM is a semi-empirical model and contains a few aspects that are based on the assumption of steady state. Strictly speaking, this makes the model unsuitable for time-dependent applications, such as future simulations. The densification equations (Eq. 1) depend on the annual average temperature and accumulation rate. To account for this, we used a running average over the previous 40 years. This time span was chosen because the future climate forcing contains a 40-year period (1960-1999) that is comparable to the present-day average climate. This way, the steady-state assumption is valid during the entire spin-up period and from 1960-1999 during the final simulation, while the future climate change simulation commences in 2000; similar to the set-up of GCM simulations. The choice of this 40-year running average introduces an additional uncertainty, because it assumes a similar response time for firn layers across the AIS. Also, it assumes that the adjustment of the firn layer to a change in temperature and accumulation is similar. However, an accumulation change is felt instantly as it adds pressure to the top of the firn layer, while it takes time to advect and diffuse a temperature change to greater depths. By using a 40-year running average window, the increase in annual average temperature and accumulation, and hence the densification rate, is more gradual.

The response time of a shallow firn layer in combination with high annual accumulation rate is much smaller than for thicker firn layers with low accumulation, as it takes less time to fully adjust to a new climate. The response time of the firn layer can be defined as the time needed to advect

firn from the surface to the pore-close off depth (z830), which varies between 20-2000 years across the AIS (Van den Broeke, 2008). However, most of the firn densification occurs in the upper part of the firn layer (10-20 m), so the effective response time will be lower. For example, using the average Antarctic annual accumulation rate (160 mm yr⁻¹, from Lenaerts et al., 2012) it would take ~60 years to refresh the upper 20 m of firn. This is similar to the chosen 40-year window, making this a reasonable choice. At locations with a higher annual accumulation rate, the firn layer will have a smaller response time and hence a shorter climate memory, while for low-accumulation sites the opposite holds.

The simulated temporal evolution of a firn layer in a changing climate is difficult to evaluate, since hardly any observations of temporal firn changes are available. Moreover, it is challenging to measure the small variations in firn density and depth that result from changes in the climate. Especially when no melt occurs, the increase in firn density, densification rate and depth are rather subtle and likely to be smaller than the measurement errors.

A definite improvement would be to use a firn model (e.g. Spencer et al., 2001) that calculates the firn densification rate using a physical relation between density, overburden pressure, snow grain size, and temperature (e.g. Appendix B in Arthern et al., 2010). For instance, in the future simulation this would make the reaction of the firn layer to additional mass input instantaneous instead of depending on the chosen length of the running-average window. Also, in the current FDM the surface density is assumed to be constant, while it is uncertain whether and how the density of fresh snow will change in the future. For the present-day, this approximation introduces uncertainty as not every accumulation event is the same, but on average the calculated values agree well with observations (Kaspers et al., 2004). The meltwater percolation and refreezing scheme in the current FDM does not include heterogeneous percolation ('piping'), a process that is known to be quite widespread on the Greenland ice sheet (Marsh and Woo, 1984; Harper et al., 2012). Due to this rapid transport of mass and heat to greater depths, the density and temperature profile are influenced. Apart from some isolated locations on the Antarctic Peninsula, melt amounts across the AIS are generally too small to cause this type of vertical transport in Antarctica. However, with future increasing melt rates this could potentially occur more regularly in warm areas and on ice shelves.

Ultimately, the results are not expected to be greatly influenced by the use of a different model. Most of the inter-annual(/transient) changes in the present-day(/future) simulations are initiated by variations in the forcing climate (e.g. Figure 5a), especially accumulation. For the present-day, the magnitude and timing of seasonal and decadal climate variations as simulated by RACMO2 (1979--2012) agree relatively well with observations (e.g. Horwath et al., 2012; Medley et al., 2013). The magnitude and trend of the simulated future Antarctic climate change also agree well with earlier published results (Ligtenberg et al., 2013). Combined, this gives confidence that the presented variations and future changes in Antarctic FAC are realistic despite the use of a semi-empirical FDM."

Review #1 (Anonymous)

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.

[Response: To remedy this, we extended the firn model description in Section 2.1 Amongst other things, we included the firn densification equation from the FDM.](#)

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.

[Response: Concerning the present-day model evaluation, we refer to the general discussion at the start of this response. The results section of both the present-day and future simulations is approx. 4-5 pages and this reflects the two important subjects of this manuscript; both the present and future variations. This is also reflected in the title: “ Present and future variations “.](#)

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. Additionally, 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%.

[Response: As we mentioned earlier, it is computationally not feasible to re-run the TD simulation. We added a couple of extra paragraphs to the discussion, where we discuss the FAC uncertainty. Please see the main response \(first page of this response\) for more details.](#)

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?

Response: The surface density in the current FDM is calculated using the parameterization of Kaspers et al. 2004, which was adapted by Helsen et al. 2008. The equation depends on average annual temperature, accumulation and wind speed, and is described in detail in Ligtenberg et al. 2011. We added a sentence to the model description to note this.

The FDM does not use snow or firn densities from the climate model RACMO2, only the meteorological forcing.

We added discussion about the use of the surface density parameterization to the discussion (see main response for more detail).

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.

Response: We included a short description of the melt module in the method section (2.1): *“This module uses the simple ‘tipping-bucket’ method (i.e. liquid water is stored in the first available layer and only transported downwards when it exceeds capillary forces), which performs relatively well in comparison with other –more sophisticated– firn hydrological methods (Wever et al., 2014). Moreover, most of the Antarctic meltwater refreezes instantly due to the cold firn pack conditions.”*

Also in the discussion we added some lines about the currently used melt module and possible disadvantages and improvements: *“The meltwater percolation and refreezing scheme in the current FDM does not include heterogeneous percolation (‘piping’), a process that is known to be quite widespread on the Greenland ice sheet (Marsh and Woo, 1984; Harper et al., 2012). Due to this rapid transport of mass and heat to greater depths, the density and temperature profile are influenced. Apart from some isolated locations on the Antarctic Peninsula, melt amounts across the AIS are generally too small to cause this type of vertical transport in Antarctica. However, with future increasing melt rates this could potentially occur more regularly in warm areas and on ice shelves.”*

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?

Response: Two slightly different spin-up methods are adopted to allow for the different time periods covered by the present-day and future climate data. We assume that the present-day simulations obey steady state (see 2.1), so the entire forcing data period (1979-2012) is used as spin-up. For the future simulation however, we expect a reaction of the firn layer as a

consequence of a warmer and wetter future climate, so we do not assume steady state over the complete forcing period (1960-2200). Instead, we assume that the historical simulation (1960-1979) is representative and this period is used as spin-up. After sufficient iterations, the future simulation (from 1980 onwards) is allowed to evolve freely in order to study the reaction of the firn layer. We added a phrase to section 2.2.1 to highlight the difference in spin-up procedure: *“To obey the steady-state assumption, and to create a realistic initial firn profile, the same procedure as in Ligtenberg et al. (2011) is used; the FDM is run iteratively until the complete firn layer is refreshed (100-5000 years), after which the final simulation starts.”*

The future climate simulations by RACMO2 (from Ligtenberg et al., 2012) use synthetic global climate model data as input for their simulations. Therefore, the year 1985 in this simulation does not represent the “real” year 1985 in the present-day simulations. This means that comparing the profiles for 2012 would be comparing apples and oranges. To clarify that the future simulations are performed with synthetic data, we added a sentence to the last paragraph of 2.2.2: *“It should be noted that the FDM future simulation uses synthetic climate data from a global climate model, in contrast to re-analysis data for the present-day run, making a direct comparison with the present simulation impossible; i.e. “1985” data from the future simulation are not comparable with real 1985 data from the present-day simulation. Therefore only the long-term averages (1979-2012 for the present-day simulation and 1960-1999 for the future simulation) are compared. “*

Page 426, l. 20+ subsurface temperatures: The Arthern equations apply an annual mean temperature T_{av} , which was estimated at each site from the average temperature of the deepest thermistor, at approximately 10 m 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.

Response: The present-day simulations use the ‘steady-state’ principle. In other words, the firn layer is assumed to be in equilibrium with the long-term average climate. In theory, the deep-firn temperature is equal to the long-term average surface temperature in the absence of melt. Since we assume that the firn layer is in steady-state, the deep-firn temperature is fixed to the average surface temperature.

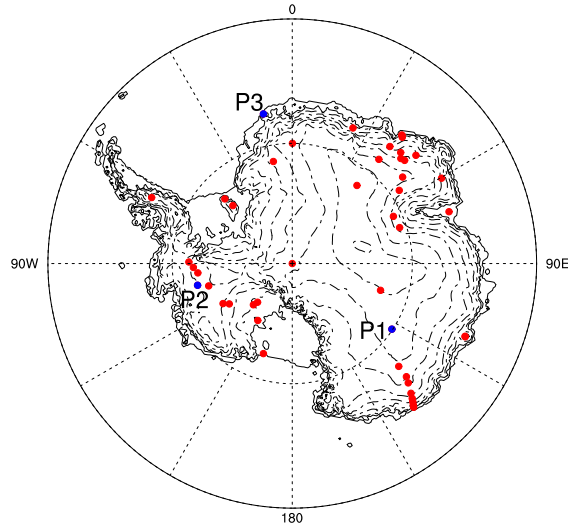
Please find the response regarding the 40-year average on the first pages of this document.

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.

Response: See response to your third comment.

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.

Response: In Ligtenberg et al., 2011, 48 firn core observations from across the Antarctic ice sheet were used, see Figure below. Only 3 of them experience some annual melt (Neumayer, Amery IS and Ross IS). The majority of the firn cores that were used to tune the StSt model do not experience any melt, so it is fair to say the StSt model and this comparison with firn cores does not include any significant melt.



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.

Response; we added this to the new discussion text, see the general response for more details.

Page 438, I.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.

Response: The Pritchard et al. 2012 paper uses the same FDM as used here, while the other two are at least comparable. Neither of the three papers look at firn layer (e.g. FAC) variability, all focus on the effect of firn changes on surface elevation change in order to correct satellite altimetry data. To our knowledge there is currently no publication that describes temporal Antarctic firn layer variations with the use of a dynamic firn model. We added the references to the introduction.

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

Response: We feel that the StSt model is not outdated as described on the first page of this response: the StSt simulation of firn density and air content are very recently used in high profile papers that estimate the mass balance of the Antarctic ice sheet (Shepherd et al. 2012), the mass balance of Antarctic ice shelves (Depoorter et al., 2013 and Rignot et al., 2013), as well as in the BEDMAP2 project (Fretwell et al., 2013). We find it therefore important to show the differences between the StSt and average TD solution.

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.

Response: It would indeed be interesting to separate the influence of the temperature and overburden pressure on the densification rate. However, in the current model equations, the

overburden pressure is scaled with the average accumulation and therefore constant in time. Figure 5b therefore solely represents the effect of sub-surface temperature variations. To clarify this, we added a sentence in the last paragraph of 3.1: “... is explained by variations in firn densification, which are caused by sub-surface temperature variations. In reality, variations in overburden pressure also add to firn densification rate variability, but in the current model equations this is scaled with the average annual accumulation (Equation 2) and therefore constant in time.”

Technical corrections:

Page 422, l. 14: “within the 33 yr period” which period is this? 1979-2012?

Response: Added the specific time period: 1979-2012.

Page 422, l. 23-24: The elevation change split in air and ice (mass) would give more insights.

Response: Split the sentence into two: “Integrated over the AIS, the increase in precipitation results in a similar volume increase due to ice and air (both $\sim 150 \text{ km}^3 \text{ yr}^{-1}$ until 2100). Combined, this volume increase is equivalent to a surface elevation change of $+2.1 \text{ cm yr}^{-1}$, which shows that variations in firn depth remain important to consider in future mass balance studies using satellite altimetry.”

Page 423, l.4: This has been investigated for the GrIS, maybe one or two more references would be beneficial.

Response: Removed the GrIS from the first two sentences as the manuscript only focuses on Antarctica. “The most common method to determine the effect of climate change on the Antarctic ice sheet (AIS) is to calculate the change in mass over time. Due to its remoteness, adverse climate conditions, and sheer size, it is difficult to measure the mass balance of the AIS directly.”

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.

Response: AIS is a very common abbreviation for the Antarctic ice sheet, so we prefer to retain this acronym. In our opinion, the difference between “Amery IS” and “AIS” is sufficient.

Page 426, l. 26: The RACMO2 model is presented as known; a couple of references might help other readers.

Response: Added three appropriate references.

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.

Response: Added a time range “100-5000 years”.

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).

Response: we have cut the sentence in two: “Figure 7a shows the average FAC at the start of this period (1960-1999), representative for the present-day climate. Therefore, it should be

in agreement with the present-day FAC as presented in Figure 1a.”

Page 433, l.12: The authors give multiple references to locations in the paper, but no geographical location is given for Amery IS.

Response: Added a geographical location for Amery IS when we first mention it (in the second-to-last sentence of Section 3.1).

Page 436, l. 12: remove “crystals”

Response: Removed

Page 436, l.25-26: In which of the previous work. Give a reference for the statement.

Response: In the next three sentences, eight appropriate references are given for this statement, which we feel is sufficient.

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

Response: Your comment is valid, so we rephrased the sentence to: “... agrees well with depth-density observations from firn cores ...”

Reviewer #2 (R. Arthern)

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 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.

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.

[Response: It was difficult to assess the above comments point-by-point, as most of the](#)

points were mentioned in multiple paragraphs. The main comment is that we have to include more discussion about the FDM and the assumptions made. This was also suggested by the other reviewers, so please find a general response at the start of this document.

We added discussion about the following topics:

- Semi-empirical nature of the used FDM
- The 40-year running average
- Different response times of the firn layer
- The advantages of more physically-based models
- Reasons why the results presented here are still valuable.

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.

Response: Including grain size evolution in the FDM would be a large operation and is deemed outside the scope of the current manuscript. The current MS focuses on the spatial patterns in firn layer characteristics (such as FAC) and shows that the T-D solution agrees well with the published StSt solutions, which in turn agree well with observations. Therefore, we are confident that the results presented in this manuscript are robust. We agree that the next step in firn modelling is more towards solving the full dynamical process. Therefore, it might be interesting to mention that we are currently in the process of comparing the current FDM with a more physically-based model (SNOWPACK). However, it will take some time before the first results are available.

We feel no need to discuss the choice of reference climate further in this MS. We use the longest Antarctic climate time series available, which has shown to agree well with observations in multiple earlier published papers. This is mentioned in the first paragraph of the discussions. In the methods section, we also mention the previous papers that use the same present-day RACMO2 simulation as yielded realistic results. The GCM-forced simulation (1960-1999) is compared with this present-day RACMO2 simulation in previous papers (Ligtenberg et al. 2013 and Kuipers Munneke et al. 2014). The cold and dry bias that was found has been removed. Using these two different reference climates, the results are fairly similar. We feel that adding discussion about the chosen reference period would only repeat previously published work.

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.

Response: To clarify the definition and calculation of the FAC used in this manuscript, a few lines are added to the part where FAC is introduced. Also, this part is moved from the introduction to a separate section in the Methods (Section 2.4): *“The firn air content (FAC) is a measure for the pore space fraction of the firn layer and is defined as the change in thickness (in m) that occurs when the firn column is compressed to the density of glacier ice:*

Equation 1

where ρ_i is the ice density, here assumed to be 910 kg m^{-3} , and z_s and z_{ρ_i} indicate the surface and the depth at which the ice density is reached, respectively. Often 917 kg m^{-3} is taken as the ice density; here we use a slightly lower number, as the density in the FDM will never exactly reach 917 kg m^{-3} due to the asymptotic nature of Equation 1. The definition of the FAC (following Holland et al. 2011) is slightly different than the one commonly used in ice core research, where air content is expressed as air volume per gram of ice (Martinerie et al. 1994). This latter definition is more convenient when analyzing vertical differences in air content, but since this paper focuses on the air content of the entire firn column, the integrated firn air content as in Equation 1 is used. Throughout the manuscript the effect of liquid water stored in the firn pore space is neglected when calculating the FAC. Most melt water on Antarctica refreezes immediately, so no significant liquid water bodies are expected.”

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.

Response: By using the integrated FAC over the entire firn column, we correct both the ice thickness and the depth-averaged density at the grounding line. One can correct the measured ice thickness using two methods: 1) subtracting the FAC and multiplying by the ice density (917 kg m^{-3}) or 2) calculating the depth-average density of the ice and firn column. By using 1), the difference in density between firn and ice is explicitly taken into account. By using 2), the amount of air is taken into account by using the density of the firn layer. To clarify this, we added *“0.7% of the total ice-equivalent thickness ...”* to the sentence.

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.

Response: Incorporated the suggestion.

Reviewer #3 (G. Picard)

This modeling study aims at evaluating the future changes of the firn air content (i.e. the height decrease caused by an hypothetical compaction of the firn into ice), its causes and consequences on the volume versus mass change of the Antarctic ice-sheet. This subject is very important to correctly estimate the ice-sheet mass changed with the methods using satellite altimetry/volume change and ice flux through the grounding line. The firn densification model developed in this paper is an improved version of the steady-state densification model described in a previous study (Ligtenberg et al. 2011). This clear, well-written and well-illustrated paper is a

significant contribution to this question. I recommend that it is published in The Cryosphere after the following comments are addressed:

1- The overall lack of comparison of the model outputs to observations weakens the credibility of the model and results. Such comparison could be introduced at two levels:

1-a The observations used Ligtenberg et al. 2011 for the validation of the steady- state model should be used again to validate the time-dependent model which provides different results. At least in the Section 3.1, the difference between the models should be put into perspective with the available observations.

Response: This is a very valuable suggestion and we agree that the manuscript would benefit from a comparison with observations. Therefore we included the new Figure 3. Please find a detailed response on the first page of this response letter.

1-b Ideally, the evolution of the time-dependent model need to be validated as well using density profile measured taken at different time during the last years or decades. If not possible or in addition, I suggest to use the model to provide recommendations on the necessary measurements accuracy required to validate the model on a short period (i.e. accessible future for a research program) . In other words, is it possible to validate the model by collecting density profiles every year over the next 5 years ?

Response: We agree that it would be very valuable to compare the time evolution simulated by the model with firn cores from the same locations but drilled at different times. However, as far as we know, they are not available. We are currently working on a manuscript (submitted to Annals of Glaciology) in which we compare simulated firn densification rates at different depths with measured firn densification rates from airborne radar. Although we only looked at a relatively small region, there is good agreement in vertical, horizontal and temporal variations in firn densification rate.

Your suggestion of collecting density profiles every year over the next 5 years would be very valuable. However, this is very expensive and time-consuming. Moreover, there are a few things to be aware of; 1) the firn density differences from year to year that you try to measure are very small, likely smaller than the measurement error. 2) It is impossible to drill a firn core at exactly the same location during the next year, as ice core drilling is a destructive measurement technique, introducing an extra uncertainty due to spatial heterogeneity. I expect that this heterogeneity is also larger than the density difference you try to measure. Concluding, it will be quite challenging and expensive to accomplish this kind of measurements to a degree that they prove to be useful. We included a few sentences in the discussion to note this: *“The simulated temporal evolution of a firn layer in a changing climate is difficult to evaluate, since hardly any observations of temporal firn changes are available. Moreover, it is challenging to measure the small variations in firn density and depth that result from changes in the climate. Especially when no melt occurs, the increase in firn density, densification rate and depth are likely smaller than the measurement errors, making it hard to collect reliable temporal firn evolution data.”*

2- As Robert Arthern, I suggest to clarify, early in the text, that the time-dependent model is a semi-empirical approach. I also suggest to test an alternative to compute the running average with a window size modulated by the local averaged accumulation and firn depth to account for the "sinking" rate of the firn, instead of the 40-year constant window.

Response: please see the second point of the general response at the start of this document, regarding the discussion of the 40-year averaging window.

3- A minor question: does the assumption of the density of ice (here chosen to a constant value of 910kg/m³) at depth have an influence on the results or not ? If yes, this should be addressed in the text.

Response: Taking the ice density to be 910 kg m⁻³, instead of 917 kg m⁻³, obviously has an effect on the magnitude of the FAC. We added this to description of FAC in Section 2.4: *“Often 917 kg m⁻³ is taken as the ice density; here we use a slightly lower number, as the density in the FDM will never exactly reach 917 kg m⁻³ due to the asymptotic nature of Equation 1.”*

Minor typo:

l12 p 422: remove the parenthesis, it is an important information l25 p 422: idem. (satellite) -> satellite l1 p435: can be become -> can become

Response: Inserted the suggestions.