Reply to reviews on "An improved semi-empirical model for the densification of Antarctic firn" by S. R. M. Ligtenberg et al.

First, we would like to thank both anonymous referees for their valuable input and constructive comments. They certainly helped to improve the quality of this manuscript. In this response, we will address all their comments point by point.

Furthermore, we must mention that during the evaluation time of this manuscript the ERA-Interim dataset was extended to 31 years (1979-2009) instead of 21 years (1989-2009). As we want the paper to be as up to date as possible, we decided to replace all the numbers and figures that are based on 21-year period with values based on the new 31-year dataset. Therefore we changed:

- Throughout the text "period 1989-2009" into "period 1979-2009" and "21-year period" into "31-year period".
- Atmospheric variables in Table 1 and 2. The averages changed only marginally.
- Figure 3, although the differences are not visible with the naked eye.
- Figures 4, 5, and 6. Leading to a very minor change in the regression equations 8 & 9.
- Figures 7 and 8. Only small spatial changes can be noticed.
- Figure 9. Some minor differences, especially in d), where the amplitude and size of the error bar increased.
- Figure 10. The time period on the x-axis is increased to 1979-2009 and the evaluation on Page 1932 is adapted: "At P3, the more rapid lowering of the surface is often caused by strong melt seasons in combination with less than average precipitation (e.g. 1981-1983), while upward motions are associated with wetter periods in combination with a weak melt season (e.g. 2008-2009)."

Anonymous Referee #1

General comments are:

(1) The major drawback to the approach used here is that all data used to tune the model are again used to validate it. Moreover, the time-dependent solution is not validated with any observational data (which is perhaps hard to find). There should be some discussion in the "summary and conclusions" – an additional paragraph – about the need to test the model against other datasets and suggestions for potential opportunities.

Answer: We agree with the reviewer that this is a drawback of the process used. However, with only 48 points available the result would probably not have been better if the dataset was split into a tuning and a validation dataset. Also, the Antarctic climate is very diverse and some climate zones are only represented by a few points.

For the time-dependent model it is indeed hard to find a validation dataset. There are height measurements from AWS, but these are typically spanning only a few years. Also it is not always clear what the reference base of their height measurement is: the surface, or a few meters in the firn (the mast foot). By accumulation or melting, this reference height may change. There are also satellite surface height measurements, but these are only useful when studying trends. The time resolution of satellite measurements is too poor to make a direct comparison with our model output.

So in summary we agree and added the following paragraph to the "summary and conclusions" section: "Several improvements to the model can be made. The accumulation dependence that has been introduced in the densification expression of Arthern et al. (2010) is tuned and validated with the same dataset of 48 Antarctic firn cores. Ideally, a part of the dataset should be used for tuning and another part for validation. However, the Antarctic climate is very diverse and some climate zones are only represented by a few points, so splitting the dataset of only 48 firn cores is no option at present. The time-dependent model can at present not be validated with observations, as long-term reliable surface height measurements at high temporal resolution are scarce. Satellite height measurements do not have the right time resolution and are often very noisy, while in-situ observations typically span less than a year. Moreover, to perform a thorough comparison with in-situ observations, the height measuring instrument should be fixed at a reference level outside the firn layer, which is virtually impossible."

(2) Here, a semi-empirical correction is made. Another option would be to improve the physically based model, i.e. by changing the exponent of the annual accumulation rate, as mentioned on p. 1929 (line 5). Perhaps the authors could also comment on the benefits and drawbacks to this approach vs. the one used here in the "Summary and conclusions".

Answer: The model by Herron and Langway (1980), mentioned in p1929, line 5, is also a semi-empirical firn densification model. Most of the commonly used models (Zwally and Li, 2002, Helsen et al.,2008, Arthern et al.,2010 and this manuscript) are based on their principles and therefore also semi-empirical. Models based on physical principles are much more complicated and therefore less used, also because the main processes of firn densification (grain settling, sintering and packing) are still not fully understood and therefore hard to use for the current application. Our goal was to combine previous models into one new model that is able to simulate the steady and transient state of the firn layer in a satisfying way. To reflect this, we added to the final section: "This is also the reason for choosing to improve a semi-empirical model instead of using a more physically-based firn densification model. The physics of firn densification are still not fully understood and mainly observed in laboratories. This explains our choice for an empirical model that describes firn densification in relation to temperature and accumulation."

Additional relatively minor changes are suggested to improve the clarity of the manuscript below.

Specific Comments

1. P.1922 Lines 6-8: It is not clear what the "two applications" are. Possibly add "First, the steady-state. . ." and "Second, the time-dependent model. . ." or something to this effect to clarify.

Answer: changed according to reviewer's suggestion.

2. P. 1923 Lines 1-4: This sentence is a bit awkward. Change "state" to "states" and "provides" to "provide". Possibly, split the sentence in two, and add the word "needed" after "crucial parameters".

Answer: changed according to reviewer's suggestion.

3. Figure 1: The introduction indicates that firn is the intermediate product of the transition between snow and ice, but no snow layer is included in the figure. If the firn layer is assumed to include both firn and surface snow (if present), note in the caption that the "firn layer" includes snow at the surface.

Answer: the snow layer is indeed assumed to be included in the firn layer. We added this explanation to the caption and also on Page 1925, line 11. Added: "New added surface snow is instantly treated as the upper layer of the vertical firn column. The fresh snow density for each grid point is determined by a parameterization of Kaspers et al. (2004), … "

4. P. 1923 Lines 27-28: for clarity, revise to read: "van den Broeke (2008) showed that the steady-state solution of Barnola et al. (1991), forced with regional atmospheric climate model output from van de Berg et al. (2006), is in good agreement with observations from firn cores."

Answer: changed according to reviewer's suggestion.

5. P.1925 Lines 10-11: Following on comment #3, it would be good to note here that the model simulates both snow at the surface and higher density firn below it, and that the "vertical firn column" includes snow at the surface.

Answer: see answer to comment #3

4. P. 1925 Lines 12-13: Kaspers et al. (2004) used annual averages. Is the same true here? If annual averages are not used, it would be best to change to Ts for consistency. If they are, change “average” to “annual average” on line 12. It seems that using the annual averages could have an impact on the performance of the time- dependent model. Was this corrected for in the time-dependent model? It would be useful to include an additional sentence or paragraph describing the potential impact of this parameterization when used in the time-dependent scenario, or whether there are any changes made to formula 2 for the steady-state or time dependent model.

Answer: annual averages are used, as in Kaspers, 2004. We changed ‘average’ in ‘average annual’, also see answer to comment #6. Your comment on a time-dependent surface snow parameterization is justified, as also we tried to improve this. However, other parameterizations for fresh snow are often only used in alpine regions with much lower densities (80-100 kg m-3) than found in Antarctica. This alpine snow densifies almost instantly into denser snow/firn and the time step of our model is not able to represent this. Therefore, we decided to keep the Kaspers-parameterization. In future research however this could be a point of improvement, as the density of fresh snow is probably not constant in time. Moreover, almost no observations on surface snow density are available; most observations are averaged over the first 0.5-1 m. Therefore a paragraph is added to the summary: “The fresh snow density parameterization of Kaspers et al. (2004) is now used in both the steady state and the time-dependent firn densification model. As the density of fresh snow is most likely not constant in time, this could be a point of improvement in future research of the time-dependent model. However, almost no observations on Antarctic fresh snow density are available: most observations are averaged over the first 0.5-1 m.”

5. P. 1926 Lines 15-17: It might be useful to state the length of the spin-up period.

Answer: The spin-up period is different for every grid point, since it depends on the average accumulation and the firn column depth. This is probably not clear from the text and is therefore clarified.

Added: “As the average annual accumulation and depth of the firn layer vary widely in space, the spin-up period is determined uniquely for every grid point. To obtain the input for the spin-up period, the 1979-2009 period is repeated into a time series long enough to refresh the entire firn layer.”

6. P. 1927 Line 8: Should be referred to as the “average accumulation rate”? If so, is it the annual or 6-hourly rate, or does it change based on the scenario?

Answer: b is the average annual accumulation. We will change this throughout the paper for clarification.

7. P. 1928 Line 3: Mention the source of the data from the 48 drilling sites.

Answer: added the source (van den Broeke, 2008).

8. P.1928 Lines 10-11: Is there an explanation for the success of the model at the South Pole? Is this associated with low accumulation rates there? It might enhance the arguments presented later to suggest this possibility somewhere in this section.

Answer: The SP- and MBL point were chosen to highlight that at some points there is good agreement between both models and the observations and at some points there is not. The success at South Pole could be coincidence, but is indeed most probably caused by the lower accumulation values (56 mm yr^{-1}) leading to MO-ratios around 1. We added this at the beginning of P1929, where we introduce the accumulation.

Added: “This also explains the agreement of Ar10S with observations in Fig 3b at South Pole, where low average annual accumulation leads to a MO-ratio close to 1. The higher average annual accumulation at MBL in Fig 3b on the other hand, leads to a MO-ratio < 1 and thereby shows an overestimated densification rate in Ar10S."

9. Figure 5: Add r2 values to the figure if possible.
Answer: done

10. P. 1929 Line 4: Change "coefficient" to "exponent".
Answer: done

11. P. 1930 Lines 25-28: The sentence would be clearer if it read "...the pattern is somewhat different; while z550 is small, z830 shows relatively high values. This is caused by. . ." Change comma after "regions" to a semicolon.
Answer: changed according to reviewer's suggestion.

12. P. 1931 Line 11: I suggest adding a sentence for clarification along the lines of: "...edges of the ice sheet (not shown). Therefore, a deeper firn column is not necessarily denser, and will contain more air."
Answer: added this sentence.

13. P. 1931 Lines 20-22: Note the source of the "total ice column" thickness data. Define "BEDMAP"; it is not defined earlier.
Answer: source of the ice thickness data is Lythe et al., 2001 (Page 1932, line 14). We removed 'BEDMAP' and added the source.

14. P. 1931 Line 24: Clarify that the time dependent model is also corrected with the updated expression used for the steady state model.
Answer: that is already indirectly noted on Page 1929, line 17, but we will mention it also at the start of 3.2 for clarity. Added: "The new densification expressions can also be used in the time-dependent FDM. By forcing the time-dependent FDM with climate model time series, …"

15. P. 1932 Line 3: Change "Table 2:" to "Table 2."
Answer: done

16. P. 1933 Line 22: Change "outspoken" to another word, such as "obvious" or "evident".
Answer: changed into 'evident'.

17. P. 1934 Line 11: "Everywhere in Antarctica" might be an overstatement based on the results presented here; replace with "within three very different climatic zones in Antarctica" or a similar sort of statement.
Answer: Is indeed somewhat of an overstatement, we inserted the reviewer's suggestion.

Anonymous Referee #2

Abstract, Line 9: Clarify whether surface lowering etc includes effects of accumulation/melting as well as densification
Answer: We added to the abstract: "Surface height changes are caused by a combination of accumulation, melting and firn densification processes."

Equation 1: Vice appears to represent vertical velocity caused by creep and motion, assumed here to be proportional to accumulation, which is appropriate for steady state, but not for regions where the ice sheet is thinning or thickening. This could have important ramifications for model results in such areas.

> Answer: This is true, but not the scope of this paper. Here, we assume a steady state ice sheet, in order to exclude the processes that determine firn layer thickness changes from the other processes that play a roll in surface height changes, such as dynamical thickening or thinning. In other words, the FDM used here is not used to simulate total surface heights changes, but only the surface height changes that are caused by firn layer processes.

Equation 2: presumably applies only for selected units, such as for temperature and accumulation rates. These should be defined for this and for later equations where appropriate.

> Answer: Eq.2 applies for temperature and accumulation ranges found on Antarctica. For every grid point the averages are different, and therefore also the surface density. For clarity, we add a sentence to section 2.2: "The fresh snow density for each grid point is determined by a parameterization of Kaspers et al. (2004), based on average annual accumulation (b), 10 m wind speed (V_{10}) and surface temperature (T_s), with a slope correction by Helsen et al. (2008):"

Equation 3: What are units of W_{mi}? And is this parameter used to include effects of melting in the model? If so, it is not clear to me how it is used.

> Answer: W_{mi} is in %. As stated on P1925, line 22: it is the maximum amount of pore space used to store liquid water. We added units (%) to the example values on Page 1926, line 1-2.

Page 1925, line 16 indicates that the new model takes account of densification by melt and rain, and later model descriptions (lines 17-25) partially explain how, but fail to explain how rain rates and melt rates are calculated, but perhaps I simply missed this!!?? And does equation (2) represent the "previous model" or the improved one??

> Answer: Rain and melt rates are not calculated in the FDM, but prescribed by the RACMO2 output. This is stated indirectly in section 2.1, but is now noted more explicitly: "In the new model, these processes are implemented by allowing liquid water from rain and/or snowmelt, as prescribed by RACMO2 output, …" Also the previous model of Helsen et al. (2008) is indicated as a time-dependent model.

Page 1926, line 24: This appears to apply to the dry snow zone; surely melting etc can be dominant in wet-snow zone?

> Answer: True. All previous models did not take melt into account, so this paragraph refers to the dry snow zone. In melt regions, refreezing of melt water is also a large contributor to the densification process. A sentence to state this is added: "Obviously, outside the dry snow zone refreezing of melt water and/or liquid precipitation also cause firn to densify."

Equations 8 and 9 apply only when b is expressed in mm/yr, and it should be specified what the mm/yr are: snow, ice, or water equivalent.

> Answer: b is the average annual accumulation in mm w.e./yr, which is given as 'the sum of solid and liquid precipitation minus sublimation', as stated on Page 1925, line 4-5. Following comment #2 of referee1, throughout the manuscript we clarified that b is the average annual accumulation. Also added units to the variables introduced in Eq. 2,5,6 and 7 for clarification.

Discussion on p 1930 etc: It would be useful also to include estimates of the air content in the firn column. For ice shelves, this can be checked against estimates inferred from measured ice thickness and surface elevation. I see that air content is discussed on p 1931. Line 11 mentions an average air content of 22.6 m, but fails to state what this is based on??? It seems rather high to me. Line 20/21 mention air content as percent of ice-shelf thickness, but it is not clear how useful this is.

Answer: The average air content of 22.6 m is the ice sheet average of Fig. 8a, which is now stated more clearly in the text. By showing the air content as a percentage of the total ice thickness, it becomes clear that highest air content values on East Antarctica are in fact of relatively little importance (<1%) when calculating the total ice mass from ice thickness. On ice shelves however, a small amount of air can make a significant difference (>5%).

Page 1932 and elsewhere mention regions of "significant melt", but it is not clear to me how its effect was included in the model, as mentioned in my comments above??

Answer: See answer on 'Page 1925, line 16'. The words 'significant melt' are probably not chosen wisely and are replaced by "… where melt occurs annually …".

Page 1933, line 20 etc: this discussion of the "firn correction" could perhaps be more clearly expressed by making use of the air content of the firn column, as suggested above. The "firn correction" is simply the air content, expressed best as a length of the air column, rather than as a percent of ice thickness.

Answer: The 'firn-depth correction', the amount of air in the firn column and the length of the firn column are all the same variable. For clarity, we removed the term 'firn depth correction' from the manuscript.

Page 1934, line 4: "deeper" than what?? Lines 5 and 6: I don't see why they cannot be modeled; page 1925 explains the impact of melt and rain on density profiles, implying that this is included in the improved model. If it is, then why can't it be modeled?? Lines 8 and 9: but the firn density between ice layers presumably is unaffected, so its densification rate should not be reduced??

Answer, line 4: Melt water will refreeze lower in the density profile. This sentence is rephrased.

Answer, line 5-6: Melt and rain are only explicitly included in the time-dependent model version. Their effect on the steady state profiles is implicitly included through the different parameter values in the densification expressions.

Answer, line 8-9: The density between ice layers is indeed unaffected, but the average density of the firn column is higher due to the presence of these ice lenses. And for a firn column with a higher average density, the average firn densification speed is lower.