

## ***Interactive comment on “An improved semi-empirical model for the densification of Antarctic firn” by S. R. M. Ligtenberg et al.***

**Anonymous Referee #2**

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This paper addresses an issue of increasing importance as we accumulate more time series of surface-elevation measurements on the polar ice sheets. But I found the paper difficult to follow, with the model poorly explained and associated equations sloppily presented.

Abstract, Line 9: Clarify whether surface lowering etc includes effects of accumulation/melting as well as densification

Equation 1:  $V_{ice}$  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.

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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.

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.

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??

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

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.

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.

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??

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.

Page 1934, line 4: “deeper” than what?? Lines 5 and 6: I don’t see why they cannot be

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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??

In summary, I would say that the paper addresses an important problem, but presentation of the modeling is poor and rather sloppy. However, their results suggest that, at least qualitatively, they are progressing towards the right answers. Although I cannot recommend publication of the paper as is, I suspect that the authors could easily respond to my concerns and tighten it up sufficiently to justify publication. In the summary assessment required above, I indicated it needed "major revisions" rather than the other option of "minor revisions". More precisely, I am sure something in between will suffice.

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Interactive comment on The Cryosphere Discuss., 5, 1921, 2011.