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Discussions

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Interactive comment on "Bayesian calibration of firn densification models" *by* Vincent Verjans et al.

Anonymous Referee #1

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The study by Verjans et al. focuses on Bayesian calibration of parameters in three widely used firn models. All three models simulate dry firn compaction; they do not consider the influence of meltwater percolation and ice layer formation in firn. The authors use a comprehensive set of firn cores from dry firn areas of Antarctica and Greenland. The majority of the cores is used for model calibration; the remainder is used for model validation. The authors find that the Bayesian calibration results in model parameters that differ from the original parameters. Validation of original and newly calibrated models shows that the Bayesian calibration improved performance of one model substantially, one model was slightly improved and the third model showed no improvement.

The study by Verjans et al. appears sound and concise to me. The presentation of the research appears of high quality. However, my expertise in Bayesian approaches is limited and thus I did not focus my evaluation on the implementation of the Bayesian

calibration. Below I provide a list of general remarks.

- The authors point out that "Results of the calibration would depend on the particular climate model used for forcing" (Line 4, Page 4). This important point is mentioned in Data and Methods, but not in Discussion and Conclusions. From reading the latter two sections, I gained the impression that the authors suggest replacing the original parameters with the MAP parameters (with the exception of the LZ model). However, could the difference in parameter values also result from different model forcing? Which parameters would the Bayesian calibration provide as output if you would use, for example, the climate conditions assumed by Herron and Langway (1980)? I understand that this is difficult to quantify, but I suggest at least to highlight this in Discussion and Conclusions or to test the stability of the calibration under different forcing.

- I appreciate that the authors investigate the impact of the new calibration on a Greenland scale. Nevertheless, I feel the comparison could be improved by showing the numbers in the context of total Greenland mass change. Furthermore, the three models are designed to simulate dry firn compaction, while the sensitivity analysis extends to the entire accumulation area. A very substantial part of the Greenland accumulation area is subject to melt and refreezing. How does this influence the informative value of your sensitivity analysis?

- Figures: I appreciate the good quality of the figures. My only suggestion is to use the same colour scale for Greenland and Antarctica in Figure 1. As it is now, and being fully aware that the two colour bars are different, it is difficult to anticipate the differences in climate at the core locations. For both maps, why does the colour bar represent a temperature range that exceed the actual range in climate conditions?

- References: I had only a brief look at the references, but noticed that the bibliography for Shepherd et al., Science, 2012, might contain some errors. Looking

up the article, I found for example a different DOI ("10.1126/science.1228102" instead of "5b0143 [pii]").

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-274, 2020.

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