

## **Editor comments on “Bayesian calibration of firn densification models” by Verjans et al.**

I would like to thank the authors for providing a thorough, detailed response to the first round of reviewer comments and for the steps they have taken to demonstrate how their results are of practical use to the wider community of firn model users. In their rebuttal document the authors provide detailed insight into the decisions made when carrying out this study and responses to all reviewers' comments are clearly justified.

Following recommendations from the reviewers, the authors have undertaken additional simulations that include a more robust treatment of uncertainty on input parameters, an appreciation of correlation in the prior distribution of model parameters, and a clearer analysis of the spread in the results. I support the decision to remove the section that seeks to apply the results to the whole of the Greenland Ice Sheet - the authors provide very clear justification that a robust analysis would require consideration of factors that are outside the scope of this study.

Reviewer 1 is happy with the edits that have been made to the manuscript. I have carried out a review of the current version of the manuscript with the aim of (i) checking that issues raised by reviewer 2 have been addressed and (ii) assessing whether the article is now ready for publication.

All technical aspects of the article appear to be robust and there is no need for any additional analysis to be carried out. However, I list below a number of minor points that should be addressed before the article can be accepted for publication. These either seek clarification on specific issues or are suggestions to improve the clarity of the text. I am not an expert in firn modelling, and some of the points may reflect gaps in my knowledge, but they are made with the aim of ensuring the article is accessible to the non-specialist. All points relate to minor text/presentation issues that should be relatively straightforward to implement. Once they are addressed, I would be happy to accept this article for publication in The Cryosphere.

Kind regards,

Pippa Whitehouse  
Associate Editor, The Cryosphere

### Minor points – page/line numbers relate to version2 of the manuscript (no track changes)

p.1 l.31: most points in this paragraph document why each application is important rather than recommending specific steps for improvement – the text on line 31 is a little misleading since it could be taken to suggest that you will investigate sensitivity to climate conditions in this study

p.2 l.16: check throughout whether it is appropriate use to use 'AIS/GrIS' or 'the AIS/GrIS'

p.4 l.5-7: text seems a little out of place here, information would fit better at the end of the final paragraph of p.3

p.4 l.25-26: the statement that you use a constant site-specific value is slightly at odds with the statement on lines 29-30 that you add random noise at every model time step. It would be useful to mention the approach used to account for uncertainty earlier in this paragraph

p.6 l.9-11: do Li and Zwally (2015) use a different formulation of the equation, or do they just determine different parameter values compared with Li and Zwally (2011)?

p.6 l.25: to data -> with data

p.7 l.23: several steps are described prior to the mention of figure 2a; please indicate how the text on lines 20-23 relates to steps shown in figure 2. Also, please clarify whether calculations are carried out for both  $\theta_i$  and  $\theta_i^*$  on each iteration

p.8 l.1: variance -> covariance (as defined on l.25 of the previous page)

p.8 l.16: 'a 500 random sample' – rephrase

p.9 l.22: the DML plots are figs. 5g-i

p.9 l.28: 'the better performance at the GrIS evaluation sites...' – make it clear that this text relates to the performance of the original model

p.10 l.7: it is not clear to me how the LZ dual model was constructed; do you determine different parameters for each ice sheet by dividing the calibration data set, or is the whole formulation of the model different? Refer to Table 2 when quoting results for the LZ dual model

p.10: if feasible, it would be useful to include a figure showing the results for LZ dual and IMAU-FDM (e.g. similar to figure 4) in the supplementary information

p.10 l.23: please include information on how uncertainty intervals were constructed in the captions to figures 4 and 5

p.11 l.5: 'indicate a weaker increase...' – weaker than what?

p.11 l.12: 'The same can be applied...' – not clear what 'The same' refers to

p.11, l.13: please refer to a figure or table when quoting correlation coefficient values

p.11 l.22: over-sensitivity -> over-sensitivity in Ar

p.12 l.25: what is the reference period? If it is 2000-2017 this should be explicitly stated

p.12 l.33: make it clearer that uncertainties in the following sentences are calculated using the CV values quote above/ in table 3

p.13 l.5: a couple of clarifications needed: (i) what does 'it' refer to, and (ii) what does 'Such numbers' refer to?

p.13 l.7: the purpose of the text (paragraph?) starting on this line is initially unclear. For example, it is not clear what you mean by 'the different sensitivities...'. You mention that compaction is sensitive to variability and 'general increases' in temperature and accumulation – can you be more explicit about the climate at the two sites, perhaps by including site-specific RACMO2 output in figure 7?

p.13 l.14: short-scale -> short-timescale

p.13 l.26: 'at most sites...uncertainty intervals do not cover observed DIP values' – this is an important result but I did not see it stated/quantified anywhere in the main text

p.14 l.12-13: this link takes you to a folder which contains several files that are unrelated to this study, are you able to list a source that just links to the firn data?

Tables 1 and S2: some terminology issues for large/small values, e.g.  $9 \cdot 10^4$  should be  $9 \times 10^4$

Table 2: explicitly mention RMSE in the caption rather than just 'The errors'

Figure 1: what is the difference between a circle and a cross?

Figure 2: in the box titled 'If i is multiple of 100' the second  $\Sigma$  should be  $\Sigma_{cov}$

Figure 3: please document what the ‘posterior samples’ are. Do they represent parameters associated with the 500 parameter sets randomly selected from the ensemble of accepted  $\theta$ ?

Figure 6: mention the difference between the left and right columns in the caption. Also, is it possible to represent AIS and GrIS data points differently, to support statements in main text?

Figure 7: legend is missing

Supp Info section S2: the GrIS RMSE value for surface mass balance flux is quoted as 69 mm w.e. in Noël et al. (2019), not 69 m w.e. – check that the units have been correctly converted when applying random noise to the boundary conditions

Supp Info section S2: equation S5 contains the term  $c_n$ , should it be  $c_p$ ?

Supp Info section S2: ‘...must not be iteration specific...’ – needs clarification

Supp Info section S2: please include a reference to justify the choice of 25 kg/m<sup>3</sup> when defining the perturbation to the fresh snow density values

Supp Info section S3: please clarify that ‘original values’ refers to parameter values from the original publications of the HL and Ar models

Supp Info section S6: start of second-to-last sentence – clarify what ‘it’ refers to

Supp Info section S7: second sentence should refer to figure S5

Supp Info, Table S1: Please clarify whether accumulation and temperature values are taken from original publications or RACMO2