

Interactive comment on “Modeling the Evolution of the Structural Anisotropy of Snow” by Silvan Leinss et al.

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General comments

Dear Reviewer #2,

thank you for carefully checking the manuscript, for your constructive comments and for the suggestions to improve the paper. Below are all answers to Reviewer #2. Answers to Reviewer #1 can be found in the other author response.

Prior to the individual answers we like to mention that a model validation suggested by Reviewer #1 could excellently confirm the effect of TGM on the anisotropy evolution. Because of the good agreement we could remove the free parameter α_2 from the model and set $\alpha_2 = 1$. As a consequence, only the parameter α_1 had to be fitted to the radar

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data which allows for a significant simplification and shortening of some sections of the manuscript.

Answers to Reviewer #2

RC 1: Could this model be adapted for an Eulerian snow model (see final question [here RC 2 and RC 3])?

AC 1: In principle yes. As shown in (Krol and Löwe, 2018)), evolution equations for microstructural parameters principally do contain material derivatives which are mainly governed by the settling velocity. By stating a Lagrangian form for the anisotropy evolution, as done here, and superimposing this formulation in SNOWPACK to individual layers, we actually assume implicitly that the Eulerian counterpart of the anisotropy equation is governed by such a Eulerian (PDE) form with a material derivative due to settling. This is the default approach for any microstructural parameter in SNOWPACK (and any other model utilizing Lagrangian layers) and thus we follow the same approach here. If in contrast a snowpack model would explicitly solve the ice mass continuity equation as an Eulerian conservation law with an advective settling term, the evolution equations for the anisotropy should follow the same procedure.

As the model describes the temporal evolution of individual snow layers we do not see much advantage in formulating it in a Eulerian coordinate system. To support this, we like to refer to the four remarks on p. 127 in (Bartelt and Lehning, 2002), especially point 4) which states: "It is impossible to track snow microstructure parameters using a Eulerian formulation since material history is lost." As we model explicitly the material history an Eulerian formulation would not be compatible with our model.

To point this out more clearly, we suggest to add the above reference to Section 2.2 where we address the Lagrangian viewpoint: "As common for snow models focusing on the evolution of properties of individual snow layers (Bartelt and Lehning, 2002), we describe(...)"

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Please note that we moved the final question of reviewer #2 here (RC2 and RC3).

RC 2: In the conclusions, p.33 lines 9 and 21-22 imply that the polarimetric radar measurements are all that are needed to monitor the snow anisotropy. However, a snowpack model will be needed to interpret the anisotropy of the layers so the text should be adjusted accordingly.

AC 2: Polarimetric radar measurements are all that is needed to *observe* the *depth-averaged* snow anisotropy. However, a snowpack model is needed to *model* the anisotropy of *individual layers* which cannot be measured with polarimetric radar systems, at least not without tomographic radar imaging methods. (See also AC20 for Reviewer #1)

We suggest to make clear in line 9 that with radar remote sensing systems only the *depth-averaged* anisotropy can be measured but we like to point out that with in-situ systems even the depth-resolved evolution of the anisotropy could be measured. In the current words "(...) to monitor the structural evolution of the snow pack", "structural" might be misunderstood as layer-wise. To clarify we suggest to write: "(...) to monitor the evolution of the structural anisotropy the snow pack." And add "Depending on the system geometry the anisotropy can be measured only depth-averaged (remote-sensing systems) or even depth-resolved with in-situ systems (Fujita et al., 2009)." With these text adjustments also line 21-22 should read clearer.

RC 3: Extending that [RC2] a little further, is the role of the CPD then to adjust the relative α_1 and α_2 per season (cannot be used operationally), is the seasonal fluctuation in these parameters significant (CPD could be used operationally but model needs to be adjusted for Eulerian snowpacks) or are CPD observations needed in the short to medium term to look at different snowpacks / seasons until there is high confidence the snow model can be used to simulate anisotropy without it?

AC 3: As mentioned in the general comment, the validation of the TGM equation of
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the model with CT data confirmed that $\alpha_2 = 1$ is very likely valid for all kinds of snow. However, due to the lack of data sets including anisotropy time series of fresh snow under settling we need to restrict the validity of the parameters α_1 to the test site in Sodankylae and the years 2009-2013. However, the similarity of obtained values of α_1 for individual seasons (Figure 7 and Table 5, TCD) indicates that the same values can be used for all seasons. All our main results (Fig. 4, 5, 6, TCD) are based on constant values for α_1 and α_2 (in the revision, this will only be α_1) which value(s) have been determined from the complete set of data spanning all four seasons.

To clarify this, we suggest to adjust p.33, line 2 (TCD) to "free parameters (...) calibrated by globally minimizing the difference between (...) and four years of anisotropy data (...). The radar data were acquired in Sodankylä, Finland between 2009 and 2013." In addition, as requested by Reviewer #1, we will include an explicit validation of the temperature gradient term in the evolution equation that confirms (independent of CPD measurements) the correctness at least of one part of the model.

RC 4: Pg 8 line 24 add in reference to section 4.1.

AC 4: Fine. We suggest to adjust the text to: "SNOWPACK was calibrated (...) by snow temperature (Sect. 4.1)."

RC 5: Add in introduction (p.2, line 20) that model is evaluated against micro-CT derived anisotropy.

AC 5: Sure. Additionally we will add the validation against independent CT data. We suggest to add: "The TGM formulation of the model is validated with independent CT laboratory experiments. The full model is evaluated against full-depth micro-CT derived anisotropy profiles from the field".

RC 6: Would be useful to put Fig 8 after p.14, line 10 where it is referenced so it becomes Figure 4.

AC 6: In principle, I would agree. However, Section 5.1 (Results: CT validation) and especially Section 6.1 (Discussion of model results) refer quite frequently to Figure 8. Therefore, I prefer to keep it close to Section 6.1 and accept the not-in-order reference to Figure 8.

RC 7: P.14, line 24: What does it mean to enforce the snow height? P.30, line 20 states that the snow height is enforced yet is too large.

AC 7: we suggest to adjust the text to: "(...) enforced snow height, i.e. SNOWPACK models the amount of precipitation based on measured snow height". To make clear, that SNOWPACK failed to enforce snow height in Nov 2012, we suggest to adjust P.30, line 20: "Because snow height was enforced, the too large snow height end of Nov implied forcing with too low precipitation for mid of Dec which resulted in less fresh snow with a positive anisotropy and in turn explains why the simulated anisotropy is lower than the radar-measured anisotropy, Fig. 5(d)." to "Because SNOWPACK struggled enforcing a decreasing snow height in late Nov, the subsequent amount of fresh snow required to match the enforced snow height early Dec was underestimated. The missing effect of settling of fresh snow explains why the simulated anisotropy in Dec 2012 is lower than the radar-measured anisotropy, Fig.5(d)."

RC 8: Is the 'best' snowpack in Table 3 the one that performs the best over all seasons i.e. same simulation configuration in 2009/2010 as 2010/2011 etc, or the best from each season?

AC 8: It's the best over all four seasons. To make this clearer, we suggest to adjust p. 14 line 20 to "(...) we run for all four seasons more than 5000 simulations with each time different settings (but keeping the same settings for all four seasons)..." and also p. 14, line 25 to "Details (...) and the definition (grading) of the 'best' set of simulations (with same settings for all four seasons) are described in Appendix A3.

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RC 9: The purpose of including p_c results on p.21, line 6-13 isn't clear and breaks up the flow of the results. Consider removing them, putting them below line 18 and/or stating here that this is relevant to the discussion.

AC 9: As the discussion (6.4) goes quite into detail of uncertainties of microstructure characterization we think that it's worth to mention the comparison to p_c . However, we moved (and slightly shortened) this paragraph below line 18 and added a reference to the discussion.

RC 10: Figure 7: could the value of α_3 be added to caption to indicate its value relative to α_1 and α_2 ?

AC 10: We suggest to add to the caption "Other parameters ($A_{min} = -0.6$; $A_{max} = 0.3$, $A_{ini} = 0.05$ and $\alpha_3 = 3 \cdot 10^{-5}$) were kept constant." Please note that A_{min} has been changed from -0.3 to -0.6 to provide a better agreement with the independent CT validation data.

RC 11: It is very hard to distinguish between T_{air} and T_{soil} in Figures 9 and 10. Please change colours and/or line type.

AC 11: We suggest to change the style of T_{soil} to dotted lines, the colors to black/gray and will draw them on top of T_{air} which makes T_{air} and T_{soil} much better distinguishable.

technical corrections:

RC 12: Please correct the following typos:

AC 12: The 13 typos listed by Reviewer #2 will be corrected.

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References

- Bartelt, P. and Lehning, M.: A physical SNOWPACK model for the Swiss avalanche warning: Part I: numerical model, *Cold Regions Science and Technology*, 35, 123 – 145, [https://doi.org/http://dx.doi.org/10.1016/S0165-232X\(02\)00074-5](https://doi.org/http://dx.doi.org/10.1016/S0165-232X(02)00074-5), 2002.
- Fujita, S., Okuyama, J., Hori, A., and Hondoh, T.: Metamorphism of stratified firn at Dome Fuji, Antarctica: A mechanism for local insolation modulation of gas transport conditions during bubble close off, *Journal of Geophysical Research: Earth Surface*, 114, 1–21, <https://doi.org/10.1029/2008JF001143>, 2009.
- Krol, Q. and Löwe, H.: Upscaling ice crystal growth dynamics in snow: Rigorous modeling and comparison to 4D X-ray tomography data, *Acta Materialia*, 151, 478 – 487, <https://doi.org/https://doi.org/10.1016/j.actamat.2018.03.010>, 2018.

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