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Discussions

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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 #1,

thank you for your constructive comments and suggestions for improving the paper. Below we will answer all comments by Referee #1. Answers to Referee #2 can be found in the other author response.

In summary, we will include an explicit validation of the evolution of anisotropy under temperature gradients from CT experiments that allows us to eliminate one of the free parameters. This requires a slight extension of the paper in this direction but at the same time we can shorten the paper at many other places. Detailed answers are given

below.

Answers to Reviewers #1:

RC 1: (...) It [the paper] is well written, although it would gain clarity by being shortened or more to the point in some paragraphs.

AC 1: We will check the paper carefully and shorten it where ever possible and will concisely rewrite some paragraphs. As we suggest below, the validation of the TGM term of the model makes the fit-parameter α_2 superfluous and in consequence a lot of text about determination of the two parameters α_1 and α_2 can be removed which should significantly shorten the paper. Additionally to that, we try to shorten the following paragraphs:
- Results based on pc (pg 21, line 3-12) will be reformulated/moved. See also answer to RC9 by Reviewer 2.
- The discussion section will be shortened.
- The appendix will be shortened.

Specific comments (major).

The following major comments RC 2 and RC 3 are split in several subcomments which are answered separately.

RC 2: Robust evaluation is missing:
The validation step of the anisotropy model is too little addressed in the paper. The evolution of structural anisotropy has been only observed in some cases, and is not fully understood yet, especially the evolution during settlement and wet snow meta-morphism. The proposed model relies thus partly on hypothetical processes. It seems thus crucial to provide a rigorous evaluation.

AC 2: Generally we agree with Reviewer1, that the individual contributions of the model (TGM, settling, melt metamorphism) should be validated before running the full model on long time series where effects of individual contributions cannot be assessed. However, as validation data is rare we can only validate the TGM equation and follow the Reviewer's suggestion to distinguish more clearly what is based on assumptions and on observations.

For example, we suggest to clearly identify the equations for snow settling and melt metamorphism as a *hypothesis*.

AC 2.1: To address the missing robust evaluation we will comment separately on the individual contributions below, keeping in mind that the additive superposition is already a simplification and suggest to cite (Wiese and Schneebeli, 2017b) on p.4, line 12 (TCD) who showed that the strain rate is coupled to temperature gradients (We suggest also to comment on this observations when stating that "Naturally, in snow all these processes are coupled (section 2.1)").

AC 2.2: Validation of anisotropy evolution during TGM:
For temperature gradient metamorphism (TGM), we will include an explicit validation using different sets of existing laboratory data (TGM-2, TGM-17, DH-1, DH-2) analyzed by (Löwe et al., 2013) and additional data labeled C-1 from (Calonne et al., 2014), which were acquired for snow at different ice volume fractions (0.22, 0.33, 0.19, 0.29, 0.35), temperatures (-10, -8, -20, -20, -4C) and temperature gradients (100, 50, 50, 50, 43 K/m). For all these data the growth of vertical structures during TGM can be compared with the model. Fig 1(a) shows the evolution of the observed anisotropy, dashed lines indicate modeled results. Interestingly, in an early stage a few days after sample preparation, Fig 1.(b) shows that the anisotropy seems to be quite stable before vertical structures grow. When ignoring the limiting factor $(A - A_{min})^2/A_{min}^2$ in Eq. (9, TCD) and setting $\alpha_2 = 1$ one obtains the anisotropy evolution by time integration $A_{TGM}(t) = A(0) + |J_v|/(\rho_{ice} * f_\mu)t$, which already agrees well with the experimental data as shown in Fig. 1(c). This figure indicates that the growth of vertical structures

is proportional to the water vapor flux J_v as modeled by Eq. (13, TCD) for different temperatures and temperature gradients (will be listed in a Table, here Fig. 2). Adding the limiting factor $(A - A_{min})^2/A_{min}^2$ to the model and using setting $A_{min} = -0.6$ further improves the correlation with the laboratory data, Fig. 1(d).

Because of the good agreement shown in Fig. 1(d) the "free" parameter α_2 (for TGM) can be eliminated and set constant to 1.0 as confirmed by the CT time series. As in this case we do not require any extended discussion of the parameter set α_1 and α_2 and because α_1 (for snow settling) is then the only fit parameter, the paper can be simplified and shortened at many places.

AC 2.3: Validation of anisotropy evolution by snow settling:
For the evolution of anisotropy under settling presently no conclusive validation can be given without extensive re-evaluation of existing data or conducting new, tailored experiments: As stated in the TCD paper (Sec 6.4) the evaluation of the anisotropy for the dielectric tensor in the present form (based on the correlation lengths) relies on the assumption of spheroidal symmetry of the correlation function. Strictly this assumption is apparently violated in new snow under settling as a direct consequence of the observed differences in the evolution of different length scales in the correlation function (Löwe et al., 2011). This implies that presently we can only effectively cast the settling contribution into the given form, when using the exponential correlation lengths to evaluate the anisotropy. This will be stated more explicitly.

For the settling induced "growth" of horizontal structures we will point out clearly, that the evidence from lab-experiments is ambivalent anyway:. e.g. (Wiese and Schneebeli, 2017b) did not observe an increase of initially existing horizontal structures in sintered snow of relatively high density (200...300 kg m^{-3}) during isothermal settlement, but others observed either a squeeze in fresh snow of low density (100 kg m^{-3}) (Schleef and Löwe, 2013) or an increase of horizontal structures a few days after fresh snow deposition (Leinss et al., 2016). We will point out this more clearly and suggest to rewrite the first paragraph of section 2.3 (gravitational settling), p. 4, line 20 - 24 as

suggested below:

The first term in Eq.(3), A_{strain}(t), accounts for gravitational settling and densification of snow which have *assumed* to be the cause of horizontal structures in polarimetric radar data. Interestingly, in these data the horizontal structures did not appear instantaneously with fresh snow but with a time delay of a few days after snow fall, thereby suggesting a settling effect (Leinss et al., 2016, Sect. 5.4). However, such growth of horizontal structures during compaction could not be observed in snow which has sintered for several months after initial sample preparation by sieving (Wiese and Schneebeli, 2017b). Nevertheless, in this work most samples showed a slight horizontal structure at the beginning of the experiment which, in combination with the observation in (Leinss et al., 2016), rises the *hypothesis* that a horizontal anisotropy grows only in an initial phase after snow deposition, likely due to settling. In contrast to (Wiese and Schneebeli, 2017b), (Schleef and Löwe, 2013) avoided any sintering with the aim to study new snow and confirmed "the anisotropic nature of densification" by attributing density changes "solely to a squeeze of the structure in the vertical direction, i.e. to axial strains". Such a squeeze can be observed in (Schleef and Löwe, 2013, Fig. 4) where the "displacement of characteristic maxima and minima of the density profile" at different times is indicated by arrows. The relative position of the arrows to each other shows that "the structure is strained during 2 days of densification". From that we draw the *hypothesis* that the ice matrix is squeezed, at least in an initial state of fresh snow.

Additionally, because of a lack of CT data showing the anisotropy evolution of *fresh* snow during settling (the snow in (Wiese and Schneebeli, 2017b) has sintered for several months) we suggest to point out in the discussion that for a proper evaluation of this model part further experiments would be required which is far beyond the scope of this study.

AC 2.4: validation of wet snow metamorphism
For wet snow metamorphism the situation for independent experimental validation is even worse and we suggest to provide a comparison in the supplementary figures of

the model when the wet snow metamorphism term switched off. (Figures 3 and 4). Additionally we like to add a short explanation to the paper "that the surface tension of water should cause a rounding of ice grains by melt metamorphism and that an anisotropic structure would be driven therefore towards isotropy." Similar to (Lehning et al., 2002) and (Brun et al., 1992), who both use similar equations, we will clearly stress that this part is presently based on an unconfirmed assumption.

RC 2.5: Thus, before evaluating the model in the frame of a full detailed snowpack model, for longtime period, and for "complex" conditions as encountered in nature, it seems relevant to first assess the model alone, for simple cases of evolution (restricted conditions).

AC 2.5: See evaluation and comments (AC 2.1 - AC 2.4) above. We hope that comparably high standards will be applied for any parametrization in snowpack models in the future.

RC 2.6: To do so, there are few studies on structural anisotropy referred in the paper that would be suitable: controlled experiments where conditions imposed to snow are known and often restricted to few parameters. These experiments could thus be replicated by the anisotropy model, itself, without implement it in a full snowpack model. The works of (Schneebeli and Sokratov, 2004; Wiese and Schneebeli, 2017b) or (Calonne et al., 2014), for example, could be used. I can see that a difficulty might be to deal with the different estimates of anisotropy from the different works, not always based on correlation lengths; solutions could still be find to make relevant comparison.

AC 2.6: As describe above, we used data analyzed by (Löwe et al., 2013) acquired by (Kaempfer et al., 2005; Riche et al., 2013; Löwe et al., 2013) and data from (Calonne et al., 2014) as suggested by the Reviewer. The data of (Schneebeli and Sokratov, 2004) was not used because of the difference in anisotropy metric (it provides the "degree of anisotropy" (without sign)) and the experiments are similar to the other listed

studies. The work of (Wiese and Schneebeli, 2017b) could not be used for a quantitative analysis because of i) the difference in the anisotropy metric and ii) the used sintered snow of significantly higher density which is not comparable to fresh snow. However, the work of (Wiese and Schneebeli, 2017b) shows an important limitation of the anisotropy model as well as of SNOWPACK which both do not consider the strong coupling of TGM and the settling rate as observed by (Wiese and Schneebeli, 2017a). To account for this, we suggest to comment on this in the discussion:
SNOWPACK does not consider the coupling of TGM and the settling rate as observed by (Wiese and Schneebeli, 2017a). A modification of the settling rate in SNOWPACK would affect the anisotropy evolution of fresh snow.

RC 2.7: Alternatively, computations based on the set of images of the above mentioned studies (or others) could have been re-do to obtain anisotropy as defined in this paper and allow comparisons.

AC 2.7: We like to suggest this idea in the discussion by addressing settling of snow and will cite the work of (Schleef and Löwe, 2013) and (Schleef et al., 2014). However, re-analyzing this entire dataset would be to time- and cost intense and therefore definitely beyond the scope of this paper.

RC 3: Description of the model
As it is the core of the presented work, the model should be described in more details and evolution laws should be illustrated. More care should be given when presenting previous studies from which the authors partly relied on to built the model.

AC 3: Several references have been checked as suggested by Reviewer 1 (see comments below).

RC 3.1: I strongly encourage the authors to include a figure that illustrates the anisotropy model by showing how do Astrain, ATGM and Amelt evolve with time for

different values of strain rate, temperature gradient, temperature and liquid water volume fraction (typical min., mean, and max. values for example). This would greatly help apprehending the model: relative contribution of each process, constraints (threshold values) of the model...

AC 3.1: Though even lengthening the paper, we suggest to add Fig. 5 and the corresponding table to the result section. Fig. 5 would also help to reproduce our model results and could act as a verification when re-implementing the model. In the figure, each line is labeled with a number; the corresponding parameters are listed in the table below. We will add some discussion to the figure which illustrates on which time scales different processes happen: e.g. TGM runs on completely different time scales (longer than 10 days) compared to settling and melt metamorphism (faster than 10 days). Additionally, Fig. 5(a) shows that the water vapor flux varies by about 1 order of magnitude for snow temperatures between -20 and 0C and depends linearly on the temperature gradient.

RC 3.2: The present model simulate the development of horizontal structures with snow densification. To support such a modeling, the authors provided notably two papers, which seems however a bit over-interpreted or at least would deserve to be described in more details (Section 2.3). Maybe the description of the anisotropy evolution by settlement should appear more clearly like a still hypothetical snow process (since it has never been clearly shown?).

AC 3.2: Yes, we totally agree and will clearly identify it as a *hypothesis*. See also AC 2.3 and AC 3.

RC 3.3: (Schleef and Löwe, 2013) p.4 l.23: "gravity causes an uniaxial squeeze of the snow structure in the z-direction (Fig. 3 and 4 in (Schleef and Löwe, 2013)) which increases A". I checked the mentioned figures and, if I am not mistaken, they do not support the above statement (structural anisotropy is not discussed at all in the paper,

above mentioned figures highlight the densification process only).

AC 3.3: The paper of (Schleef and Löwe, 2013) supports the statement of an "uniaxial squeeze" which should increase the anisotropy according to our model. We explained our conclusion from this paper already in detail in AC 2.2.

RC 3.4: (Wiese and Schneebeli, 2017b) Looking at the results of this study (Fig. 6), it is actually not really clear how does densification influence anisotropy. For example, why does anisotropy toward horizontal structures develop more in the case of temperature gradient condition with no loading (exp.6) than in the case of isothermal condition with loading (exp.3 and 4)? Why does Exp. 3 and 4 show very little evolution, although the effect of densification should be significant as it is not competing with the opposite effect of temperature gradient? It seems that a more detailed descriptions of (Wiese and Schneebeli, 2017b) paper would be useful here.

AC 3.5: Indeed, Figure 6 in (Wiese and Schneebeli, 2017b) does not support our hypothesis that densification influences the anisotropy. However, as discussed in AC 2.3 (Wiese and Schneebeli, 2017b) studied sintered snow of a relative high density which cannot be compared to fresh snow. We therefore suggest to restrict our assumption "that settling increases the anisotropy" to fresh snow which is supported by (Schleef and Löwe, 2013), and the observations in (Leinss et al., 2014) and (Leinss et al., 2016).

RC 3.6: Regarding the evolution of the anisotropy by densification: what is the role of the initial structure/shape of snow crystals that deposit? Do you expect that plates-shaped crystals (e.g. dendrites) and graupel (I take in purpose two extreme cases) will show the same anisotropy evolution for a given strain rate? The underlying question is: does densification can create horizontal structures in microstructures that were initially isotropic, or only in microstructures that initially present anisotropic crystals shapes. If relevant, it might deserve a comment in the paper.

AC 3.6: As mentioned in the last paragraph of AC 2.3 and AC 2.7, no CT data about

densification of fresh snow and the corresponding anisotropy is available to us. This includes of course any microscopic information about snow type or crystal shape and the related behavior of the anisotropy under densification. Therefore it is beyond the scope of this study to answer this question. However, we will come back to this point later (AC 4.3 and AC 4.7a) where we provide arguments that the initial anisotropy should not very too much (less than a range of about $A\pm0.05$) and that we ignored any microstructure by setting the microstructural parameter in our model fixed to $f_\mu = [1]mm$.

RC 3.7: A model of the evolution of anisotropy in the case of wet snow is presented. However, the modeling of this specific case is basically only based on assumptions: neither supported by the measurements presented in the paper, which were done only on dry snow, neither by literature studies (no references are given). As a result, there are no evaluations at all of this part of the model, so reader have no clue about the pertinence of the suggested formulation for wet snow metamorphism (eq 14). Thus, the question is, is it relevant at this stage to present wet snow anisotropy at all?

AC 3.7: I think we should comment about the expected behavior of the anisotropy of wet snow under melt metamorphism as suggested in AC 2.4 but will clearly state that this part of the model is completely based on reasonable assumptions. In our dataset, the influence of melt metamorphism affects almost only the snow melt period for which no radar data is available. Still, the short melt event in April 2012 after which the snow pack refroze provides a few data points which support the *qualitative* observation that melt metamorphism decreases the anisotropy. We will therefore make clear in the discussion that further studies are needed to provide *quantitative* data on melt metamorphism. To show that melt metamorphism does not affect our model we like to provide a comparison of our model with and without the melt metamorphism part (Fig. 3 vs. Fig. 4) and suggest to add the figure with melt metamorphism switched off to the supplementary material. Note that in both figures we set $\alpha_2 = 1.0$, $A_{min} = -0.6$ and determined α_1 by fitting the model to the radar data.

RC 4: Other specific comments (minor)
Some ideas are discussed but it is difficult to follow the author's thoughts; they should be reformulated. References are often missing, or it should appear clearly that the authors are talking about hypothesis.

AC 4: We will answer below the specific comments to address this general comment.

RC 4.1: p.31 l.4: "Nevertheless, it may surprise that the model completely neglects any dependence on grain size. (...) We still think that neglecting the microstructure could be the main reason why the model was not able to simulate the fast decay of horizontal structures in Jan-Feb 2012."
not clear - Which effect of the grain size do you expect on structural anisotropy? Two microstructures with different mean grain sizes but subjected to the same conditions will have different anisotropy rates?

AC 4.1: Yes, we would expect that under the same conditions, e.g. the grain size should affect the anisotropy change rate. However, we could not find any improvement of the model results when implementing a grain size dependence. Notably, when using A_{min} = -0.6 instead of $A_{min} = -0.3$ as used in the TCD manuscript, the model agrees well with the CT validation data and is able to reproduce the fast decay of the horizontal structures in Jan-Feb 2012 which was not possible in the TCD manuscript. We will adjust the relevant section accordingly. (see next comment)

RC 4.2: p.31 l.11: "Beyond the dimensions of the microstructure, we ignored the crystallographic fabric of snow, i.e. the orientation of the c-axis of the hexagonal ice crystals which compose the microstructure. For the radar data it was ignored because the snow fabric anisotropy affects only very weakly the dielectric anisotropy (Appendix A in (Leinss et al., 2016)). For the model, we neither consider the evolution of the snow fabric anisotropy nor the influence of crystal orientation on the evolution on the structural anisotropy. This, because only very few studies exist which provide experi-

mental insight about the orientation of the snow fabric (Calonne et al., 2016) or even the temporal evolution of the snow fabric anisotropy (Riche et al., 2013). Furthermore, the dominant growth direction of snow crystals depends on temperature (Lamb and Hobbs, 1971; Lamb and Scott, 1972) and is not necessarily parallel to the temperature gradient (Miller and Adams, 2009) as it can be clearly observed in the supplementary movie in (Pinzer et al., 2012). Motivated by the competing effect of crystal orientation, structural disorder and structural optimization to increase the vertical thermal conductivity (Staron et al., 2014) we simply introduced a lower limit of the anisotropy A_{min} under TGM."
this paragraph should be reformulated to get clearer.
- Again, readers need to understand which influence do you expect of the crystalline orientation on structural anisotropy. What is "structural disorder", etc.
- Beside, this point appears to be a "detail" compared to other assumptions or simplifications of the model. Or do you expect a significant effect?

AC 4.2: This point is indeed a detail but still important to mention. Therefore we suggest to reformulate it in such a way that it is clear to the reader why we ignore the crystallographic fabric.

RC 4.3: p.16 l.21: "For the initial anisotropy, we neglected any temperature dependence due to lack of representative data. Stronger cohesion between crystals at temperatures close to zero could lead to a more isotropic structure (but with faster settling) compared to cold temperatures were crystals align according to gravity without being influenced by stronger cohesion forces or settling. A temperature dependence for the shape of snow crystals growing in the atmosphere could also influence the initial anisotropy."
here you discuss about potential effect of temperature and crystalline anisotropy, while you do not provided the first basic information that reader would expect, in my view: how "strong" is the assumption of a same initial value for all new layer, i.e. what is the variability of anisotropy of fresh snow (observed/reported)? + references are missing

to support the described processes.

AC 4.3: We suggest to provide the basic information first that "we expect that the initial anisotropy after snow fall should not vary much around zero but assume that it should be slightly positive because of some initial settling. Due to lack of representative data we ignore any temperature dependence..." I think this paragraph can also be shortened. For the temperature dependent shape of crystals we suggest to cite (Libbrecht, 2005).

RC 4.4: p.6 l.27: "Additionally, we assume that horizontal structures in fresh snow decay significantly faster than the growth speed of vertical structures in old snow and add an empirical, quadratic weighting function"
Why? Please explain and/or give references. Besides, the sentence is not clear (do you mean that vertical structures develop faster in fresh snow than old snow? what is old snow?) + incorrect formulation ("structures" cannot decay faster than a "growth speed").

AC 4.4: We suggest to reformulate this sentence to: "Because small ice grains in fresh snow evaporate faster than large ice grains in older snow, we assume additionally that horizontal structures decay significantly faster than vertical structures can grow and add therefore an empirical, quadratic weighting function."

RC 4.5: p.6 l.10: "The absolute value $|J_v|$ is used because vertical structures can grow independent on the sign of J_v
add references

AC 4.5: We suggest to make clear that this is an assumption based on the argument that "Snow crystals always grow in the opposite direction of the vapor flux Pinzer et al. (2012); Yosida (1955). Because the anisotropy does not contain information about the growth direction but only on the growth orientation, we assume that the growth of

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vertical structures can be modeled proportional to the absolute value $|J_v|$." (See also next answer for RC 4.6)

RC 4.6: p.6 l.12: "In contrast, temperature gradients changing their direction on a daily scale seem not to increase the anisotropy but cause a rounding of grains (Pinzer and Schneebeli, 2009)."
I could not find any comments on structural anisotropy in (Pinzer and Schneebeli, 2009). Please provide justifications why oscillating temperature gradient would not cause structural anisotropy (while oscillation longer time period would do).

AC 4.6: Our current formulations may read a bit like an over-interpretation of the results in (Pinzer and Schneebeli, 2009). Therefore we suggest to reformulate this sentence as: "In contrast, temperature gradients changing their direction with a daily cycle seem not to cause the growth of faceted crystals: according to (Pinzer and Schneebeli, 2009) the morphology of the snow structure evolves much slower under alternating temperature gradients and did not show any sign of TGM. Therefore we exclude the effect of daily alternating temperature gradients."

RC 4.7a: p.7 l.8: "We found, that the model best predicts the measured anisotropy evolution by simply setting $f_\mu(\cdot) = 1$ mm, constant, instead of considering any grain-size dependence. A more physical approach would be to characterize each grain type and size by its potential velocity to transform into vertical structures by a more sophisticated definition of $f_\mu(\cdot)$. Interestingly, any simple, empirical relation could not produce better results compared to the fixed factor $f_\mu(\cdot) = 1$ mm."
this part is not clear. I do not understand why the authors are interested by modelling the individual growth speed of grains, while willing to describe the structural anisotropy of a layer.

AC 4.7a: Without any limitation, we would expect that the time a structure needs to transform into a more vertical structure should depend on its microstructure, which

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can be described e.g. by grain size, SSA, or other structural parameters. Therefore, the anisotropy change rate should be somehow related to the grain size growth rate because both depend on the transport of water molecules. We will try to point this out more clearly.

RC 4.7b: p.6 l.28: "A faster decay rate of fresh snow compared to old snow partially compensates the fact that any grain size dependence was neglected in the model: the lifetime of small grains in fresh snow should be significantly shorter than the lifetime of large crystals in old snow."
(linked to some above points on influence of grain size) I do not understand this comment.

AC 4.7b: We hope, that AC 4.7a already clarified that a potential connection between the anisotropy change rate and the grain size growth rate should exist. We suggest therefore to rephrase "A faster decay rate of fresh snow" to "A faster decay rate of the anisotropy of fresh snow".

RC 4.8: p.31 l.4: "It is remarkable how well the model reproduces the radar-measured anisotropy time series."
It is maybe not that remarkable since model is actually calibrated based on the radar measurements to which it is here compared to.

AC 4.8: The agreement between model and radar data needs to be seen in the context that four years of radar time series, consisting of over 3000 different data points, could be reproduces by fitting the limited set of around 2-5 parameters. With the additionally provided validation using independent CT data (see AC 2.2 and the corresponding figure 1) we could even reduce the number of free parameter by one. At the same time our results still agree with the radar data and even better with the in-situ CT validation data. We think that this is very remarkable.

RC 4.9: p.33, l.6: "First the detailed agreement between radar-measured anisotropy and the anisotropy modeled (...) demonstrates that polarimetric radar measurements (...) can be used to monitor the structural evolution of the snow pack".
I have a hard time understanding for which application it would be useful to obtain a bulk structural anisotropy (as most snowpack are not homogeneous). The authors should provide concrete ideas of the usefulness.
In the same idea, I am not sure I understood the radar measurements correctly: does a snowpack made of 20 cm of vertical structures (let's say A=-0.2) and 20 cm of horizontal structures (A=0.2) would show a bulk structural isotropy with A=0?

AC 4.9: With standard polarimetric radar systems only the bulk anisotropy of the snow pack can be observed, such that your example above would indeed result in A=0 (see also answer to RC 2 by Reviewer #2). However, as shown in (Leinss et al., 2014) by observing time series of the anisotropy some information about the amount and the timing of snow fall events can be extracted. With in-situ radar systems which have antennas moving on a rail, the snowpack could be scanned layer by layer, either by using methods of radar tomography or with antennas transmitting horizontally through different layers of the snow pack. We will provide such examples. (See also answer AC 2 to Reviewer #2).

RC 4.10: p.5 l.24: "water molecules diffuse from the bottom up through the ice matrix" through the pore space?

AC 4.10: Yes, we suggest to write that they will "diffuse through the pore space surrounding the ice matrix".

RC 5: Technical corrections

**AC 5:** All technical corrections will be applied.

References

Brun, E., David, P., Sudul, M., and Brunot, G.: A numerical model to simulate snow-cover stratigraphy for operational avalanche forecasting, Journal of Glaciology, 38, 13–22, 1992.

Calonne, N., Flin, F., Geindreau, C., Lesaffre, B., and Rolland du Roscoat, S.: Study of a temperature gradient metamorphism of snow from 3-D images: time evolution of microstructures, physical properties and their associated anisotropy, The Cryosphere, 8, 2255–2274, https://doi.org/10.5194/tc-8-2255-2014, 2014.

Calonne, N., Montagnat, M., Matzl, M., and Schneebeli, M.: The layered evolution of fabric and microstructure of snow at Point Barnola, Central East Antarctica, Earth and Planetary Science Letters, 2016.

Kaempfer, T. U., Schneebeli, M., and Sokratov, S. A.: A microstructural approach to model heat transfer in snow, Geophysical Research Letters, 32, n/a–n/a, https://doi.org/10.1029/2005GL023873, 2005.

Lamb, D. and Hobbs, P. V.: Growth rates and habits of ice crystals grown from the vapor phase, Journal of the Atmospheric Sciences, 28, 1506–1509, 1971.

Lamb, D. and Scott, W. D.: Linear growth rates of ice crystals grown from the vapor phase, Journal of Crystal Growth, 12, 21 − 31, https://doi.org/https://doi.org/10.1016/0022-0248(72)90333-8, 1972.

Lehning, M., Bartelt, P., Brown, B., Fierz, C., and Satyawali, P.: A physical SNOWPACK model for the Swiss avalanche warning: Part II. Snow microstructure, Cold Regions Science and Technology, 35, 147 − 167, https://doi.org/http://dx.doi.org/10.1016/S0165-232X(02)00073-3, 2002.

Leinss, S., Parrella, G., and Hajnsek, I.: Snow Height Determination by Polarimetric Phase Differences in X-Band SAR Data, IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing, 7, 3794–3810, https://doi.org/10.1109/JSTARS.2014.2323199, 2014.

Leinss, S., Löwe, H., Proksch, M., Lemmetyinen, J., Wiesmann, A., and Hajnsek, I.: Anisotropy of Seasonal Snow measured by Polarimetric Phase Differences in Radar Time Series, The Cryosphere, 10, 1–28, https://doi.org/https://doi.org/10.5194/tc-10-1771-2016, 2016.

Libbrecht, K. G.: The physics of snow crystals, Reports on progress in physics, 68, 855, 2005.

Löwe, H., Spiegel, J., and Schneebeli, M.: Interfacial and structural relaxations of snow under isothermal conditions, Journal of Glaciology, 57, 499–510, 2011.

Löwe, H., Riche, F., and Schneebeli, M.: A general treatment of snow microstructure exemplified by an improved relation for thermal conductivity, The Cryosphere, 7, 1473–1480, https://doi.org/10.5194/tc-7-1473-2013, 2013.

Miller, D. and Adams, E.: A microstructural dry-snow metamorphism model for kinetic crystal growth, Journal of Glaciology, 55, 1003–1011, https://doi.org/doi:10.3189/002214309790794832, 2009.

Pinzer, B. R. and Schneebeli, M.: Snow metamorphism under alternating temperature gradients: Morphology and recrystallization in surface snow, Geophysical Research Letters, 36, 1–4, https://doi.org/10.1029/2009GL039618, 2009.

Pinzer, B. R., Schneebeli, M., and Kaempfer, T. U.: Vapor flux and recrystallization during dry snow metamorphism under a steady temperature gradient as observed by time-lapse microtomography, The Cryosphere, 6, 1141–1155, https://doi.org/10.5194/tc-6-1141-2012, 2012.

Riche, F., Montagnat, M., and Schneebeli, M.: Evolution of crystal orientation in snow during temperature gradient metamorphism, Journal of Glaciology, 59, 47–55, https://doi.org/doi:10.3189/2013JoG12J116, 2013.

Schleef, S. and Löwe, H.: X-ray microtomography analysis of isothermal densification of new snow under external mechanical stress, Journal of Glaciology, 59, 233–243, https://doi.org/https://doi.org/10.3189/2013JoG12J076, 2013.

Schleef, S., Löwe, H., and Schneebeli, M.: Hot-pressure sintering of low-density snow analyzed by X-ray microtomography and in situ microcompression, Acta Materialia, 71, 185 − 194, https://doi.org/https://doi.org/10.1016/j.actamat.2014.03.004, 2014.

Schneebeli, M. and Sokratov, S.: Tomography of temperature gradient metamorphism of snow and associated changes in heat conductivity, Hydrological Processes, 18, 3655–3665, https://doi.org/10.1002/hyp.5800, 2004.

Staron, P. J., Adams, E. E., and Miller, D. A.: Nonequilibrium thermodynamics of kinetic metamorphism in snow, Cold Regions Science and Technology, 97, 60–71, 2014.

Wiese, M. and Schneebeli, M.: Snowbreeder 5: a Micro-CT device for measuring the snow-microstructure evolution under the simultaneous influence of a temperature gradient and compaction, Journal of Glaciology, 63, 355–360, https://doi.org/10.1017/jog.2016.143, 2017a.

Wiese, M. and Schneebeli, M.: Early-stage interaction between settlement and temperature-gradient metamorphism, Journal of Glaciology, 63, 652–662, https://doi.org/10.1017/jog.2017.31, 2017b.

Yosida, Z.: Physical Studies on Deposited Snow. I.; Thermal Properties., Contributions from
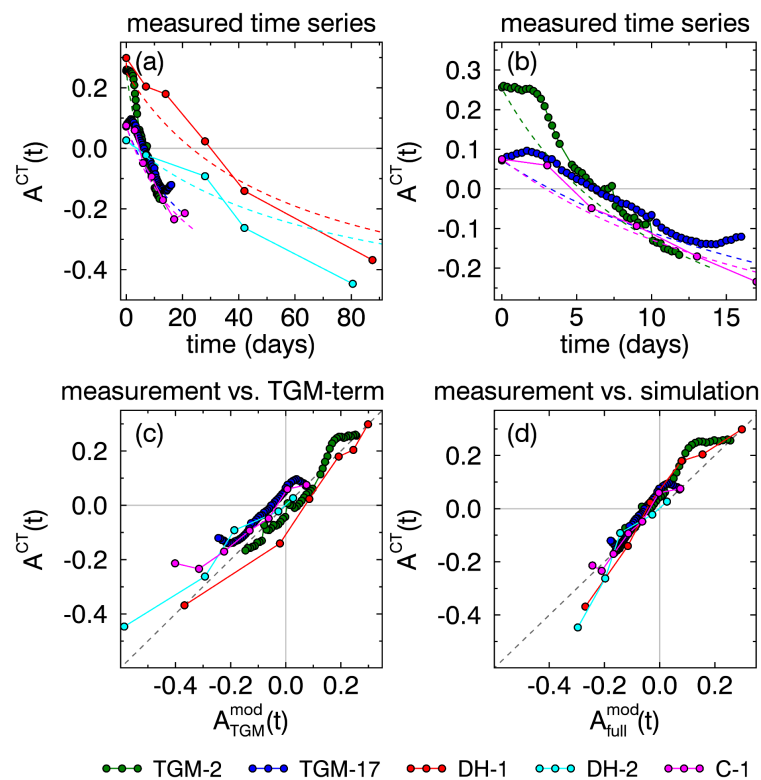
Fig. 1. (a) anisotropy time series from lab experiments; (b) first 15 days of anisotropy evolution; (c): CT data vs. time integration of Jv; (d): CT data vs. full model.

sample	T	∇T	$f_v(0)$	type	SSA	Δt
	°C	K/m	-	-	m² kg^{-1}	days
TGM-2	-10	100	0.22	DFdc	29.0	11.7
TGM-17	-8	50	0.33	RGsr	21.7	16.0
DH-1	-20	50	0.19	DFdc	22.1	87.5
DH-2	-20	50	0.29	DFbk	20.0	80.5
C-1	-4	43	0.35	RG	20.8	27.7

Fig. 2. Experimental conditions for TGM experiments.
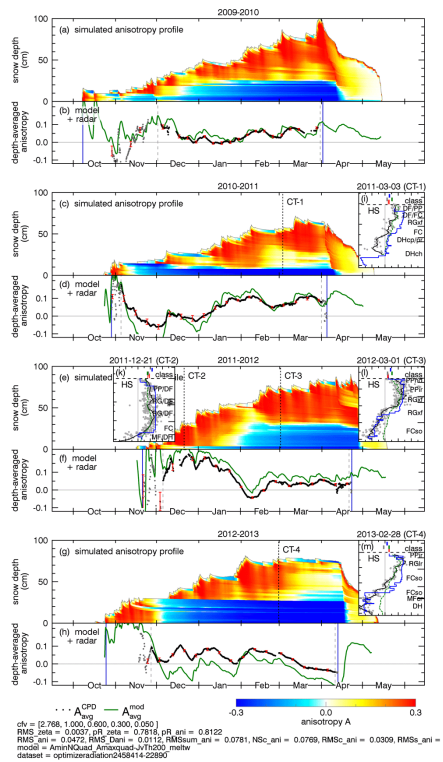
Fig. 3. Results of the full model with $\alpha_1 = 1.0$, $\alpha_2 = 2.8$, $\alpha_3 = 3e\text{-}6$, Amin=-0.6, Amax=0.3

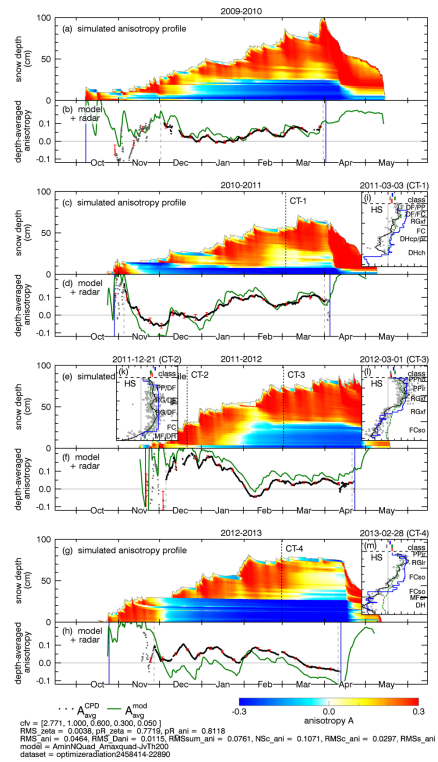**Fig. 4.** Same as Fig. 3 but without melt metamorphism.

labels in (a)	1	2	3	4	5	6	7
∇T (K/m)	5	10	25	50	100	200	500

labels in (b)	1	2	3	4	5	6	7
T (°C)	0	0	-10	-20	-20	-40	-40
∇T (K/m)	100	50	50	50	20	50	10

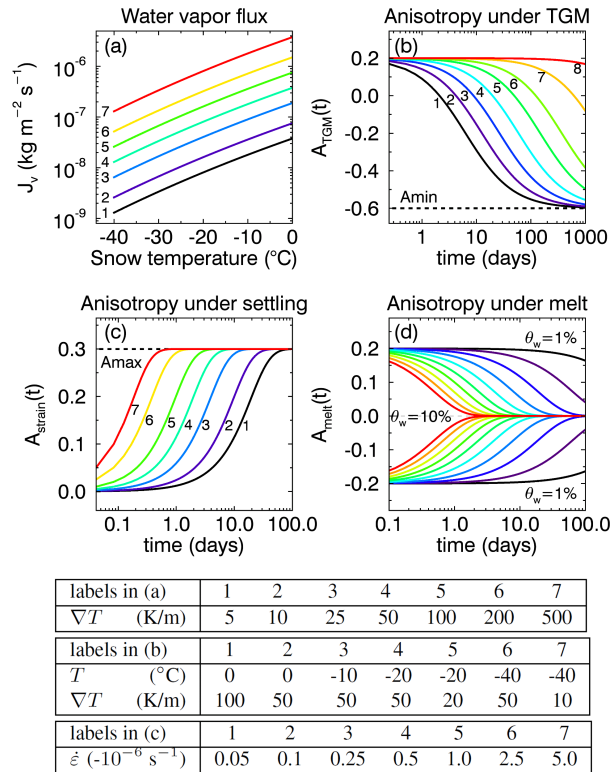| labels in (c)	1	2	3	4	5	6	7
$\dot{\varepsilon}$ (-10^{-6} s^{-1})	0.05	0.1	0.25	0.5	1.0	2.5	5.0

Fig. 5. The evaluation of different parts of the model show that TGM and settling evolve on different time scales.