**** General comments**

Snow sometimes presents a structural anisotropy which induces an anisotropy in the physical and mechanical properties of snow. Although its influence can be significant, anisotropy is currently not taking into account in snowpack models. Contributing to address this gap, the paper presents a macroscopic model of the structural anisotropy of snow.
The model relies on physical concepts partly supported by observations from literature (field studies and cold-lab experiments). However, these observations are limited, so the physical concepts of the model also rely on some hypothesis. The model is empirical and includes free parameters that are calibrated from measurements.
Briefly, the model consists in empirical descriptions of three processes that influence structural anisotropy: temperature gradient metamorphism, settlement, and wet snow metamorphism. These three contributions are balanced by empirical parameters that are calibrated based on a large dataset of bulk structural anisotropy (average over the full-depth of the snowpack) obtained from radar measurements performed at Sodankyla, Finland, during 4 winter seasons, on dry snowpacks. The inputs of the model are: layer temperature, temperature gradient through the layer, strain applied on the layer, and liquid water volume fraction of the layer. There is one initial condition: all new snow layers have the same structural anisotropy that corresponds to a slight anisotropy towards horizontal structures.
Concerning evaluation, the model is assessed based on 4 vertical profiles of structural anisotropy at Sodankyla, Finland, at different dates between 2010 and 2014. To do so, they implemented the anisotropy model in the detailed snowpack model "SNOWPACK", run this version of the SNOWPACK model at Sodankyla with an adequate, tuned set of forcing, and compare the simulated profiles of anisotropy to the ones derived from computer-tomography measurements (Fig. 6 and Table 4 of the paper). They conclude on an overall good agreement, except for pronounced vertical structures that are significantly underestimated by the anisotropy model.

The paper addresses a very interesting topic with potential great outcomes for the snow science community. It is well written, although it would gain clarity by being shorten or more to the point in some paragraphs. The topic is clearly suitable for a TC publication. The studies is based on a large variety of methods (field measurements, simulations, analytical work). The work is highly novel since it is an attempt to the first model of structural anisotropy.
I have however some major comments that concern:
1/ The evaluation of the model is very limited, which do not allow to be really convinced about the presented work; yet, there are options to perform a more robust, more "instructive" evaluation.
2/ The physical processes involved in the anisotropy model are, for some of them, justified based on incorrect references (wrong interpretations or over-interpretation of previous studies) and more care should be given to presenting results from previous related studies. The distinction between what is based on assumptions and on observations should appear more clearly.
Besides, several unsupported comments can be found throughout the paper, that should be re-written (in minor comments).

I would like to strongly encourage the authors to address these main issues, as, I believe, with an improved evaluation and some efforts to better describe and justify the model design, the present paper would be very beneficial for the snow community.

**** Specific comments (major)**

1/ 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 metamorphism. The proposed model relies thus partly on hypothetical processes. It seems thus crucial to provide a rigorous evaluation.

- The validation approach describes above only evaluates the anisotropy model “diluted” in a bigger detailed snowpack model. Thus, simulated anisotropy values inherit of all the potential errors from the detailed model. Such evaluation does not allow to identify the causes of deviations, especially what is the contribution of the anisotropy model itself to these deviations.

- No evaluation of the anisotropy model itself is provided. The model consists in three formulations to describe the anisotropy evolution by settlement, by temperature gradient metamorphism, and by wet snow metamorphism. These formulations, although partly based on assumptions, are not assessed. Evaluation is even more crucial for the anisotropy evolution by settlement and melting, processes that have never been clearly shown/supported by quantitative studies, as mentioned by the authors.
  What are the relative contributions of these formulations to the anisotropy evolution? How do they compensate each other? How do they “perform”, i.e. what is the individual contribution of each formulation to the observed simulation-measurements errors?
  Those are some examples of questions to which the paper should definitively be able to answer.

- Thus, before evaluating the model in the frame of a full detailed snowpack model, for long-time 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). 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 2017 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. 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.

2/ 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.

- I strongly encourage the authors to include a figure that illustrates the anisotropy model by showing how do $A_{strain,}$ A_{TGM} and A_{melt} 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…

- The present model simulate the development of horizontal structures with snow densification. To support such a modelling, 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?).

  - 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).
  - Wiese and Schneebeli 2017
    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 paper would be useful here.

- 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.

- A model of the evolution of anisotropy in the case of wet snow is presented. However, the modelling 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?

**** 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.

  - p.31 l.4: “Nevertheless, it may surprise that the model completely neglects any dependence on grain size. However, we found that no simple grain size dependence,like weighting the TGM-term by the inverse grain size (by setting the microstructural parameter equal to grain size, f μ = r g in Eq. 9), could produce reasonable simulation results. Using the relation f μ = r g caused a strong vertical variability of the anisotropy combined with too positive values for the anisotropy of depth hoar (Fig. S24). 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?

- 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 experimental 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?

- 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

- 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”)

- 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

- 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).

- p.7 l.8: “We found, that the model best predicts the measured anisotropy evolution by simply setting f μ (·) = 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 μ (·). Interestingly, any simple, empirical relation could not produce better results compared to the fixed factor f μ (·) = 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.

  - 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.

- 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.

- 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?

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

**** Technical corrections**

- throughout the paper:
  - snow pack → snowpack
  - many errors in the format of citations (brackets or not). Please check.

- p.1, after the 1st sentence, it might be good to recall briefly what is meant by structural anisotropy. "In some cases, snow microstructure can develop a significant structural anisotropy, i.e a structure of ice and air elongated in a particular direction, being often the vertical or horizontal direction." for instance.

- p.2. l.20: add where measurements were done.

- p.3 l.2: "preliminaries" could be replaced by "Defining anisotropy", or similar, to better reflect the content of the paragraph.

- p.1 l.7: "The model implements..." is not correct. Maybe use "includes"

- p.1 l.23: "and also" → "as well as"

- p.2 l.8: "For snow, the microstructure can be obtained ..."

- p.2 l.14: “The model act as a link” → the formulation seems not correct.
- p.2 l.19: mistake in citation format
- p.3 l.7: mistake in citation format
- p.4 l.1: “...are larger than the vertical scale“ → “are larger than the vertical ones”.
- p.5 l.11: “for horizontal anisotropies...” → it should be “vertical”?
- p.5 l.16: always
- p.13 l.29: “snow/air images” → “ice/air images”

References

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, http://www.the-cryosphere.net/8/2255/2014/, 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, http://dx.doi.org/10.1002/hyp.5800, 2004.

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, 2017.