

## ***Interactive comment on “Anisotropy of seasonal snow measured by polarimetric phase differences in radar time series” by S. Leinss et al.***

**S. Leinss et al.**

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Dear Reviewer 1,

We thank for the careful examination of the manuscript and the numerous comments. All comments have been carefully answered.

The answers are attached below.

The Latex-Diff file shows changes which have been made to the manuscript. I think, the revised manuscript will be uploaded in an additional step.

The following main points have been addressed: - The need for additional information about snow depth and snow density to determine the anisotropy has been clarified

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in the abstract, the conclusion as well as in the body of the paper. - The fact that ice has an anisotropy which is related to the crystal orientation has been carefully distinguished from the structural anisotropy of snow which is the focus of the paper. - Numerous references have been corrected and references to many excellent papers of Matsuoka and Fujita have added.

We would like to point out, that an additional computer tomography dataset has been added to the paper. The updated figure is attached.

We hope that our answers satisfy the reviewers comments.

best regards, S. Leinss

Answers to the comments of the reviewer:

General Changes: Author: A forth validation data set from computer tomography has been added which shows similar results as the existing three datasets.

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General comments: ————— R1: Leinss et al. presented a method of microwave remote sensing to detect birefringence of electromagnetic waves that propagate through thicknesses of snow. First, dielectric mixture theory that relates between the geometrically anisotropic features of snow and the anisotropic dielectric permittivity is given. Then, microwave propagation model for the oblique incident angle and scattering at the bottom of the snow thickness was given. The authors performed radar measurement of snow at a test site in Finland. The authors demonstrated the copolar phase difference (CPD) had temporal variations in four winter seasons. With sets of data for snow thickness and snow density that was manually measured, the authors converted the CPD to the geometrical anisotropy of snow. The geometrical anisotropy of snow was verified by direct measurements of the snow microstructure using X-ray absorption micro-tomography. The authors demonstrated that settling of the snow particles that occur in several days after the deposition could be observed. The authors

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suggested that detection of the CPD variations are indicator of the fresh snow. In addition, the CPD observed from the satellite showed the same temporal variation that was observed at the test site.

R1: I evaluate that this is a nice paper that opens use of the birefringent features of snow for microwave remote sensing. Handling of the dielectric mixture theory seems sound and fair to me. Experimental settings, processing of the data and interpretation for the data were almost properly presented, with which I could agree. Overall, the readers of this paper can learn a lot about a remote sensing method that can provide progress in snow science related to metamorphism and remote sensing of snow metamorphic properties.

R1: I have relatively minor criticisms/concerns at points as listed below as the specific comments.

Comment: Frequent use of a term "recrystallization" does not seem proper to me to express metamorphism where vapor sublimation and condensation play major roles.

Answer: The term "recrystallization" has been removed from the paper and was replaced by the more appropriate term "metamorphism".

Comment: For explanation of the method in the abstract and conclusion, the authors tend to mention little about the principle of radio wave birefringence, whereas it is a key of the method. It should be explicitly mentioned.

Answer: The "principle of radio wave birefringence" is now mentioned in both, abstract and conclusion.

Comment: In the experimental method, to derive the snow anisotropy from the CPD, we need to know both thickness and density of snow independently. The authors tend to mention little on this point when the authors wrote summary of their experiment, for example, in the abstract and in the concluding remarks. Without providing such information, many readers may think that the snow anisotropy can be precisely determined

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by the CPD measurement alone, without any additional measurements. It seems more fair to me that prerequisite of the method is given.

Answer: The need for thickness and density/SWE has been added to the abstract, the conclusion and also the introduction of the method-section (sect. 2).

Comment: Besides, there are some misleading citations for the history of the measurement of the dielectric anisotropy of snow and firn, which should be fairly repaired.

Answer: We thank the reviewer for checking carefully the citations. We think, by answering your comments below, this point should be addressed adequately.

Comment: Though it is not mandatory, I recommend that author contributions are given at the end of this paper as a good habit of the collaborative paper.

Answer: This section has been added to the paper. A few people which also contributed to the paper were added to the acknowledgements.

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Specific comments —————

Abstract in general Comment: It seems to me that there is a big step of the context between lines 2-8 and lines 9-19. In the former, the authors' statement is that snow anisotropy can be remotely sensed. In the latter, the authors' statement is about observation. There is no mention for the basic principle in this abstract. A sentence should be given to fill the gap. My suggestion is something like "Snow is dielectrically anisotropic medium that has an axis of symmetry in the vertical due to anisotropic distribution of ice matrix and pore spaces. Such dielectric anisotropy can be detected by microwave remote sensing using a principle of radio wave birefringence and oblique wave propagation." This kind of mention for the basic principle will better lead readers, I think.

Answer: Large parts of the abstract have been rewritten and your suggestions have

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been thankfully taken into account.

Abstract, L.2. Comment: It seems to me that the term "recrystallize" is not proper. As many snow scientists have used, it seems better that the author choose a term "metamorphose". In crystal physics, "recrystallize" means more about migration of molecules within solid ice, from a domain of molecules (crystal lattice) to another. Such diffusion of molecules should exist in snow metamorphism. However, sublimation and condensation play major role in initial changes of the snow properties in particular 3D geometry. "recrystallize" is only one phase of various phenomena. Considering this, I suggest that a more general term "metamorphose" is a better choice.

Answer: Thanks for this nice explanation. We fully agree that the term "recrystallization" should be replaced by "metamorphism". This was done where necessary.

Abstract, L.3. Comment: A term "oriented ice crystals" has vague meaning. Readers who know that ice crystal has c-axis and a-axis will wonder if this paper talks about either crystal axis orientation or macroscopic shape of ice matrix, or both. I understand, in this paper crystal axis orientation is not discussed. If it is so, some different expression seems better. It is mostly 3D geometry of ice and pore spaces that give such anisotropic effects (an anisotropy in mechanical, thermal, and dielectric properties). Not crystal-axis orientation.

Answer: The term "oriented" has been removed from the abstract and has been replaced by "spatially anisotropic distribution of the ice matrix".

Abstract, L.4-8. Comment: I felt that the contrast or comparison was a bit strange here. Anisotropy of snow have been observed by many methods using snow samples. In this paper, the authors discuss microwave remote sensing method that can detect changes in average anisotropy over thick ice. A problem in the expression is that the authors wrote a context as if microwave remote sensing were an alternative method to detect anisotropy in microscopic manner. It is a method to detect dielectric properties of the target (snow in this case) in macroscopic manner. I agree that such macroscopic

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properties in the media reflects microscopic properties in snow. However, the remote sensing method itself cannot clarify what is going on in microscopic scales. I suggest the authors to avoid to give an impression to readers that this paper gives an alternative method to detect microscopic features.

Answer: We replaced "alternative method (...) to determine the anisotropy" by "method to determine the depth-averaged anisotropy on macroscopic scales". This should clarify that microwaves cannot be used to determine the microstructure but that only the macroscopic effect due to the microstructure can be measured.

Main text: \_\_\_\_\_

L.23 in P.6062 Comment: I suggest that "metamorphism processes" is better than "recrystallization processes" with a reason same as abstract L.2.

Answer: agreed and changed.

L.24 in P.6062 Comment: The authors used a term "electromagnetic". I suggest that a term "dielectric" is better here. This word is more directly related to what the authors observed.

Answer: agreed and changed.

L.26 in P.6062 . L.2 in P.6063 Comment: The authors give introduction like this to show a contrast between sample measurement and the microwave remote sensing. However, microwave remote sensing is a method to detect macroscopic nature of the targets. It does not seem proper that the authors give an impression to readers that sample measurements have a problem of actions of sampling. Both are necessary scientific approaches. Advantages of the microwave remote sensing include (i) repetitive measurements for inaccessible locations using satellites or airplanes, and (ii) the measurement can cover very wide area. Disadvantage is that it cannot be as detailed as the ground measurements. I suggest the authors to tell these aspects to readers. Destruction of samples by snow sampling does not seem to matter.

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Answer: A short section about advantages and disadvantages of microscopic measurements vs. microwave remote sensing techniques have been added. The sentence about destructive sampling has been removed.

L.3 . 12 in P.6063 Comment: I felt a bit odd to find that an example of the polar snow first appeared in this paper. In the long history of seasonal snow studies, are there no studies that investigated anisotropy in the deposited snow?

Answer: This paragraph has been rewritten and early examples for anisotropy measurements of seasonal, deposited snow are cited. eg. - Photographs by Kojima, Kenji (1960), in "Thin Section of Snow Cut by a Heated Wire.", Contributions from the Institute of Low Temperature Science, vol. 16. - a review about dielectric measurements: Evan, S (1965) "Dielectric properties of ice and snow" review", Journal of Glaciology vol 5. - and the reference to the anisotropic thermal measurements of (Izumi, 1975). "Studies of metamorphism and thermal conductivity of snow, 1", Low Temperature Science Series A, vol. 33. After that, the reference to (Alley, 1987) is given.

L.8 in P.6063 Comment: I suggest "metamorphism" rather than "recrystallization".

Answer: agreed and changed.

L.14 in P.6063 Comment: (Pfeffer and Mrugala, 2002) should be (e.g., Pfeffer and Mrugala, 2002) because there are earlier examples that these authors cited. In addition "driven by a vertical water vapor flux under temperature gradients" seems better. I suggest to add "under temperature gradients".

Answer: agreed and corrected.

L.10 . 12 in P.6063 Comment: For the vertical anisotropy of geometry, the authors mentioned that it was driven by a vertical water vapor flux. The authors did not mention any cause of the horizontal anisotropy of geometry here. I think that a short mention will help readers' understanding.

Answer: This point is addressed later by the paper of Schlee (2013) and Löwe (2011).

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L.13 . 16 in P.6063 Comment: The anisotropy of snow was determined from the computer tomography data by a paper Fujita et al. (2009) below as well. To be fair, I suggest this paper should be naturally added to citation with (Lowe et al., 2011, 2013). 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, J. Geophys. Res.-Earth, 114, 1.21, doi:10.1029/2008JF001143, 2009

Answer: The references to Lowe are given for the method of the statistical analysis. However, a reference to the paper of Fujita(2009) has been added later where anisotropies in polar firn are mentioned. This paper is indeed very interesting.

L.17 in P.6063 Comment: Vertical structures have been found in samples of polar firn (the same paper above and the paper below), too. To be fair, Fujita et al. (2009) and Fujita et al. (2014) should be naturally cited here. Fujita, S., Hirabayashi, M., Goto-Azuma, K., Dallmayr, R., Satow, K., Zheng, J., and Dahl-Jensen, D.: Densification of layered firn of the ice sheet at NEEM, Greenland, J. Glaciol., 60, 905-921, doi:10.3189/2014JoG14J006, 2014

Answer: agreed and added.

L.2 . 3 in P.6064 Comment: The authors wrote "The origin of horizontally aligned structures has been discussed with respect to settling of fresh snow (Schlee and Lowe, 2013)". However, I did not find such a context in the cited paper. Perhaps I did not read this paper deep enough to detect the cited context. However, the other readers may find the same problem. I suggest the authors to point out where readers should see in the citation.

Answer: We agree that here the wrong citation was given. The correct citation should be "Lowe (2011)" which is already given in the next sentence. We also added two references which indicate that snow crystals fall already with a preferentially horizontally aligned orientation. - Garrett, T. J. and Fallgatter, C. and Shkurko, K. and Howlett,

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D. "Fall speed measurement and high-resolution multi-angle photography of hydrometeors in free fall", in Atmospheric Measurement Techniques (2012), vol 5 no 9. - Matrosov, Sergey Y. and Reinking, Roger F. and Djalalova, Irina V. "Inferring Fall Attitudes of Pristine Dendritic Crystals from Polarimetric Radar Data", Journal of the Atmospheric Sciences (2005), vol 1 no 62.

L.2 . 6 in P.6064 Comment: The authors used several lines here to explain a relation between the isothermal metamorphism and the horizontally aligned structures. However, cited papers are all for artificial snow in the laboratory. Please mention how plausible such horizontally aligned structures of snow really appear in natural snow.

Answer: References have been added which show horizontal structures in natural snow (Mätzler, 87, Fig. 2.15) and Calonne (2012). Both papers are already referenced at other places in the paper.

L.7 in P.6064 Comment: I wonder why dielectric anisotropy was suddenly introduced here. Please provide an explanation. Why not mechanical properties, thermal properties or optical properties? It seems too sudden.

Answer: A paragraph has been added to the introduction to prepare the reader for different methods to determine the anisotropy of snow (microscopic, macroscopic in the field, and by remote sensing). I think this prepares the reader now that different scales are addressed. Additionally, we added "e.g." to the sentence to indicate that dielectric measurements are not the only method to determine the anisotropy. The sentence was changed to "On macroscopic scales, the anisotropy of snow can be characterized by macroscopic properties of snow like e.g. the anisotropy of the dielectric permittivity".

L.9 . 11 in P.6064 Comment: The authors wrote, "the di-electric anisotropy can be measured with different polarizations of the electromagnetic field in microwave resonators filled with snow (Jones, 1976)." Readers will surely read this sentence as if Jones (1976) had measured snow. It is not true. Note that "Jones (1976)" is a method paper and that only crystal quartz was measured. The authors sentence make readers

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misunderstand that Jones (1976) measured snow. Fujita et al. (2009) and Fujita et al. (2014) are real applications of the method to snow. Matsuoka et al. (1997) was the real application of the method to ice crystal. Please provide precise citations. Ignorance is insult to earlier studies. Reference: Matsuoka, T., Fujita, S., Morishima, S., and Mae, S.: Precise measurement of dielectric anisotropy in ice Ih at 39 GHz., J. Appl. Phys., 81, 2344-2348, 1997.

Answer: I totally agree that this sentence is not clear and that this reference is misleading. See also comment/answer below.

L.13 . 15 in P.6064 Comment: Lytle and Jezek (1994) did not use open microwave resonator. They measured wave propagation speed. I find that the authors are sometimes misleading readers in terms of citations. I suggest description something like below. "Using open microwave resonators, different permittivities in the vertical and horizontal direction have been found in multi-year firn on the Greenland ice sheet (Fujita et al., 2014) and Antarctic ice sheet (Fujita et al.,2009). Using a method of microwave propagation, Lytle and Jezek (1994) also detected different permittivities in the vertical and horizontal direction in multi-year firn on the Greenland ice sheet . These anisotropy measurements were performed in conjunction with photographic (Lytle and Jezek, 1994) and computer tomographic analysis (Fujita et al.,2009).

Answer: Thanks for the suggestions. Your suggestion has been added in a slightly adapted form to mention the design of Jones (1979). I'm not sure if the paper of Matsuoka et al. (1997) should be mentioned here as it focuses on the crystal anisotropy of ice and not the structural anisotropy of snow. However, I cited the paper Matsuoka et al. (1997) in the section about the definition of the "structural anisotropy". Here it is indeed relevant to consider the anisotropic properties of ice with respect to the crystal orientation/c-axis.

L.16 in P.6064 Comment: It seems to me that there are big steps in the context in this introduction here. It is the same problem that I pointed out for the abstract. The

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authors skipped introduction of the physical principle and earlier examples that used the principle. First, if the media has dielectric anisotropy, in principle, electromagnetic waves propagating through the medium have polarization effects due to birefringent nature of the medium. Rather than suddenly introducing satellite-based observation, the authors should mention this basic physical principle to readers. Second, it seems to me that the authors should tell to readers that there is no real measurement of dielectric anisotropy in seasonal snow.

"seasonal snow", "radar" and "satellites " are big steps in introduction, I felt. I suggest that the authors should provide introduction something like below.

"Snow is dielectrically anisotropic medium that has an axis of symmetry in the vertical due to anisotropic distribution of ice matrix and pore spaces, as it has been observed. Such dielectric anisotropy can be detected by microwave remote sensing using a principle of radio wave birefringence of the electromagnetic wave propagation. The use of principle of the birefringence to remote sensing has been used to explore internal structure of the ice sheets and glaciers with radio wave (e.g., Hargreaves (1977, 1978), Fujita et al. (2006) and Matsuoka et al. (2009)). As for detection of birefringence of seasonal snow, Leinss et al. (2014) determined the anisotropy of seasonal snow with radar satellites; they analyzed propagation differences of differently polarized microwaves within snow."

References Hargreaves, N. D.: The polarization of radio signals in the radio echo sounding of ice sheets, *J. Phys. D. Appl. Phys.*, 10, 1285-1304, 1977. Hargreaves, N. D.: The radio-frequency birefringence of polar ice, *J. Glaciol.*, 21, 301-313, 1978. Matsuoka, K., Wilen, L., Hurley, S. P., and Raymond, C. F.: Effects of birefringence within ice sheets on obliquely propagating radio waves, *IEEE Trans. Geosci. Remote Sens.*, 47, 1429-1443, 10.1109/TGRS.2008.2005201, 2009. Fujita, S., Maeno, H., and Matsuoka, K.: Radio-wave depolarization and scattering within ice sheets: A matrix-based model to link radar and ice-core measurements and its application, *J. Glaciol.*, 52, 407-424, 2006. The authors did not invent the use of principle of the birefringence

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to remote sensing. I suggest that earlier examples for snow and large ice masses should be naturally introduced to readers.

Answer: Thanks for this extended explanation for the history of remote sensing of anisotropic properties of ice sheets and also for the numerous references. I added your suggestion with very small modifications. The following paragraph gives also historical reference for remote sensing of seasonal snow as Leinss (2014) was also not the first who observed birefringent effects in seasonal snow.

L.23 . 25 in P.6064 Comment: It seems to me that "a contactless, destruction-free" are not something to be emphasized. This aspect is clear if the authors tell it is radar remote sensing. I suggest something like below. "Polarimetric radar remote sensing methods can provide information of the dielectric anisotropy of snow from large distances. Areas of many thousands of km<sup>2</sup> can be observed with air- and space-borne sensors repeatedly if it is observed from orbit of the satellite. They provide a complementary tool to detailed ground sampling/measurements such as computer tomography or dielectric anisotropy as large areas and volumes of natural snow can be observed as an averaged manner." I suggested here to mention the dielectric anisotropy. Indeed it is a measurable quantity and this is the very quantity that causes the birefringence. Between  $\mu$ CT measurement and the microwave remote sensing, a quantity dielectric anisotropy is necessary.

Answer: I agree and thankfully took your suggestion into account. I made also clear, that only "area-, depth- or volume-averaged properties are measured".

L.29 in P. 6064 . L. 3 in P.6065 Comment: The authors wrote "Currently, polarimetric radars are only used to characterize the anisotropy of falling snow or rain". To be precise, it is not true considering the radar remote sensing of the ice sheets and glaciers with radio wave (e.g., Hargreaves 1977, 1978, Fujita et al. 2006 and Matsuoka et al. 2009).

Answer: Considering your references concerning ice sheets which have been added, I

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changed this sentence to "Today, polarimetric upwards looking radars are used (...)"

L.3 . 5 in P.6065 Comment: This is repetitive statement about the dielectric anisotropy. The authors already gave statements snow is dielectrically anisotropic material. It is equivalent to the propagation speed difference. I suggest that the statement here should be removed or rewritten.

Answer: as many examples are already given for the vertical dielectric anisotropy ( $\epsilon_V > \epsilon_H$ ) this sentence has been removed.

L.5 and L.11 Comment: "opposite effect" meaning is unclear to me.

Answer: It has been clarified that for fresh snow a larger horizontal permittivity has been found (The "opposite effect" to firn, where  $\epsilon_V > \epsilon_H$ ).

L.9 Comment: "both effects" meaning is unclear to me.

Answer: "both effects" means vertical and horizontal anisotropies. This has been clarified.

L.10 Comment: TerraSAR-X" appeared suddenly. The authors should give a short basic information for this instrument. Not all readers are familiar to this.

Answer: "satellite time series of TerraSAR-X" has been replaced by "time series of polarimetric radar measurements using the satellite TerraSAR-X (Stangl et al., 2006; Werninghaus and Buckreuss, 2010)" References: M. Stangl and R. Werninghaus and B. Schweizer and C. Fischer and M. Brandfass and J. Mittermayer and H. Breit (2006) "TerraSAR-X technologies and first results", Radar, Sonar and Navigation, IEE Proceedings, vol 153, no 2. Werninghaus, R. and Buckreuss, S. (2010) "The TerraSAR-X Mission and System Design", IEEE Transactions on Geoscience and Remote Sensing, vol 48 no 2.

L.13 Comment: "negative values" meaning is unclear to me.

Answer: ( $\epsilon_x < \epsilon_z$ ) has been added.

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L.14 . 16 in P.6065 Comment: Meaning is unclear to me. It seems that the statements are for detectable resolution. However, I did not understand. Why does this statement of the resolution in introduction? It does not seem important at all at this stage of this paper.

Answer: This paragraph has been clarified to point out that the CPD can be used to precisely measure the dielectric anisotropy of the snow pack. The dielectric anisotropy is now defined as  $\Delta\epsilon = \epsilon_z - \epsilon_x$ . It has also been added (as suggested for the abstract) that snow depth and density are required to measure the dielectric contrast  $\epsilon_x - \epsilon_z$ .

L.17 in P.6065 Comment: What is dielectric anisotropy? Definition was not given so far anywhere. Does it mean something measurable with a resolution of 0.0001? It seems unnecessary precision in practice. Can the authors provide?

Answer: See answer above.

L.27 in P.6065 Comment: "TanDEM-X" appeared suddenly. Please provide introductory information for this instrument.

Answer: "TanDEM-X" has been removed from the paper. This is not 100% correct, however it avoids a lot of confusion. TanDEM-X is often referred to a satellite formation consisting of two almost identical satellites, TerraSAR-X and TanDEM-X (TanDEM-X = TerraSAR-X-Add-on for Digital Elevation Measurements). The measurements of both satellites are (with respect to the application in the paper) identical. Therefore, I think it is justified to claim that a few datapoints which actually were measured by the "other" satellite TanDEM-X were measured by TerraSAR-X.

L.11 in P.6066 Comment: It was written as "different choices for the length scales". It is not clear that the authors have shown two or more different choices. Different from what? What does a choice of the exponential correlation length mean as compared to the other correlation? An explanation to readers seems to help. What "choice" do the

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authors suggest to use for studies of the snow?

Answer: A reference (Loewe 2011) was given which describes different correlation lengths. We use the exponential correlation length as it has been commonly used in microwave modeling. ("microwave modeling" has been added). It has further been added, that the exponential correlation length is conveniently fitted to the correlation functions, but that for scattering effects limitations occur. New reference to (Löwe, 2015) added. Reference: Löwe, H. and Picard, G., "Microwave scattering coefficient of snow in MEMLS and DMRT-ML revisited: the relevance of sticky hard spheres and tomography-based estimates of stickiness", *The Cryosphere* (2015), vol. 9, no 6.

L.14 . 16 in P.6066 Comment: It seems to me  $a_x$  and  $a_z$  are dimensions in the horizontal axis and in the vertical axis, respectively. Then, the magnitude of  $A$  for grains with given ratio between longest and shortest length seems dependent on whether the longest length is vertically or horizontally oriented. Is there my misunderstanding by me somewhere?

Answer: "magnitude of  $A$ " means " $|A|$ " (without sign). According to Eq. (1), when e.g.  $a_x = 2$  and  $a_z = 1$ , then  $A = 0.66$ . Swapping  $a_x$  and  $a_z$  leads to  $A = -0.66$ . (Same magnitude, opposite sign.). This is not the case in the definition  $A'$  where for the same numbers ( $a_x = \{2, 1\}$  and  $a_z = \{1, 2\}$ )  $A' = 2$  and  $A' = 0.5$  follows which has a difference of 1 and 0.5 to the isotropic case of  $A' = 1$ . "the magnitude of  $A$ " has been changed to "the magnitude  $|A|$ ". The given example has been added.

L.6 in P.6067 Comment: The authors wrote "In the following we define the coordinate axes such that  $z$  is parallel to the normal vector of the earth surface and the  $x$  and  $y$  plane is parallel to the flat earth surface." It seems that this was already assumed in eq. 1. I am confused to see that this definition appeared only here.

Answer: I moved this definition before the definition of the anisotropy. Thereby,  $z$  is defined by gravity (analog to the definition of vertical) and  $x, y$  is defined as horizontal.

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L.20 . 22 in P.6067 Comment: The authors wrote "However, the relative permittivity,  $\epsilon_{\text{eff, MG}}$  calculated with the Maxwell-Garnett formula underestimates the measured permittivity." Does this mean that the measured permittivity of the isotropic snow was higher than the model calculations or opposite? This point is unclear to me. Please clarify.

Answer: To me "underestimate" means that the MG result is lower than the measured values. I added: to "the measured permittivity (Mätzler 96)" "which is slightly higher" to clarify this.

L.26 in P.6067 . L.2 in P.6068 "We found...." Comment: Meaning of this sentence is unclear to me and probably to the other readers. Do the authors intend to claim that the weighted average of the Maxwell-Garnett formula and the "inverse" Maxwell-Garnett formula agree with the empirical data of the permittivity of snow measured with the resonator method (Matzler, 1996). Is it correct? Please clarify to readers.

In addition, the authors wrote that deviation was within 0.7 %. 0.7% of what? I suggest authors to develop their analysis in the appendix of this paper or as supplementary information. Otherwise, I am afraid that this part of the analyses are left as a black box for readers, which readers cannot digest only by reading this paper.

Answer: This has detailed in Appendix A: Effective permittivity from weighted average of Maxwell-Garnett equations

Comment: As for the footnote #2 in P.6068, it is not understandable for me, too. What are  $\frac{3.171}{3}$  or  $\frac{3.171}{3}$ ? What is  $\frac{3.171}{3}$ ?

Answer: This has also been clarified in Appendix A.

Equation 3 Comment: Please provide physical meaning of this equation to readers if it is possible.

Answer: Eq. (3) considers that for a low ice volume  $f_{\text{vol}}$   $\epsilon_{\text{eff, MG}}$  should provide better results as here, a few ice particles are embedded in a matrix of air. For

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large snow densities, it is better to model the permittivity by air inclusions in ice. The equations provides therefore a smooth interpolation of both boundary cases. (This arguments have been added to the appendix).

L.4 in P.6068 Comment: Please provide unit of  $\epsilon_{\text{ice}}$ .

Answer: This is also mass per volume and can be  $\text{g/cm}^3$  or  $\text{kg/m}^3$ .

L.7 . 9 in P.6068 Comment: Please indicate temperature range that this study is applied. It seems that temperature range for this study is above about -10 degrees C. Is it correct? How did the authors handle temperature dependence of the permittivity? Did the authors approximate values of the permittivity?

Answer: For the entire paper, a fixed permittivity for ice of 3.179 was used. This corresponds to -10 degree C. The temperature dependence of the permittivity is about 1% in the range of the experiment (-30 ... 0 degree C) and therefore smaller than the uncertainty in density of a few percent.

Comment: In addition, nothing is mentioned for the fact that ice crystal has dielectric anisotropy with a size more than 1 % of the ice permittivity (Fujita et al., 1993; Matsuoka et al., 1996). Did the authors think that effects from this is negligibly small? Please explain to readers.

Answer: This is an important point and has been added in a separate subsection (including the reference to (Fujita et al., 1993; Matsuoka et al., 1997). Currently, it is not clear how large the crystal anisotropy of natural snow is, but evidence exists that the crystal anisotropy could have a small effect on the dielectric anisotropy (Riche et al, 2013). However, the dielectric anisotropy due to crystal orientation would have the opposite sign than the dielectric anisotropy due to a structural anisotropy. Reference: Riche, Fabienne and Montagnat, Maurine and Schneebeli, Martin "Evolution of crystal orientation in snow during temperature gradient metamorphism" (2013) in Journal of Glaciology vol. 59 no 513.

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EQ.7 in P.6069 Comment: "s" is not defined or explained here. In addition, what is physical meaning of this assumption? Please provide explanation to readers if possible.

Answer: Here, I refer already to (Sihvola, 2000) which provides this equation. "s" is a integration variable which can be substituted by a dimensionless quantity  $u = s/a_x^2$  as mentioned later.

L.24 in P.6070 Comment: Meaning of "spatially anisotropic microstructure" is unclear to me. Does it mean that anisotropic microstructure is variable from one location to another? If so, please write so.

Answer: "spatially" has been removed. We mean only "anisotropic microstructure".

L.24 . 26 in P.6070 Comment: The authors wrote "The effective permittivity can be measured when snow is observed with a polarimetric radar system by analyzing the Copolar Phase Difference, CPD." It does not seem true to me. How can we detect the permittivity by microwave remote sensing?

Answer: This sentence has been corrected to " The difference of the vertical to the horizontal permittivity of snow can be measured when snow is observed with a polarimetric radar system by analyzing the Copolar Phase Difference, CPD, when snow depth and density are known."

L.1 . 2 in P.6071 Comment: To be precise. I suggest the authors to express "measuring the vertical anisotropy of snow". Nadir-looking radar systems can still measure the horizontal anisotropy of snow if there are such structures.

Answer: Thanks for the suggestions. "vertical" has been added.

L.3 in P.6071 Comment: The authors mentioned "a requirement". It seems to me that another requirement is that microwave signals that were scattered at the distinct boundary with snow, such as soil, should be detected. The authors need to analyze CPD form such distinct target. Propagation "through" snow is an important experi-

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mental setting. In case of very cold glaciers or ice sheets without such clear "bottom" of snow, it seems that a method described here cannot be used. Please clarify such points to readers.

Answer: Thanks for pointing this out. Indeed, this is an important requirement.

L.6 . 9 in P.6071 Comment: The authors wrote here as "several GHz". However, in this paper, the authors used 10 - 17 GHz. These numbers seem more than "several". Please inform readers of what will happen if we use higher frequencies, for example, 17 - 30 GHz?

Answer: I think, this is covered in the section "Generalization for scattering multilayer systems".

L.10 in P.6071 Comment: The authors wrote "The dielectric anisotropy can precisely be measured with the CPD". It does not seem true to me. It is CPD that can be measured precisely in a condition that there is a clear scattering object behind the snow as propagation path. Average of the dielectric anisotropy over propagation paths can be calculated only if observers can determine lengths of the propagation paths and density of snow. Even if the radar system is capable of detecting precise CPD, it does not necessarily mean that precise dielectric anisotropy can be detected.

Answer: I think this point has been addressed above where I mention, that ground is required below the snow pack.

Comment: In addition to this aspect, I did not see in this paper any discussion about effects from footprint. It seems that footprint width can give some averaging effects for the wave propagation of the side-looking radar.

Answer: Generally, radar systems have a very good range resolution of a few meters therefore, within one pixel, there is almost no variation in incidence angle. In our paper, we indeed averaged the CPD over the footprint of the antenna which covered an incidence angle range of about 5 - 12 degree (depending on frequency). It has been

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added to the paper, that 1) the incidence angle within the area of interest should be considered, and 2) (in the data processing section) that the CPD within the antenna footprint has been averaged. However, the effect of averaging is small as the CPD depends sufficiently linear on the incidence angle within the range of incidence angles of 5 - 8 degree.

L.5 . 6 in P.6072 Comment: The authors wrote "Hence, the H-polarization is delayed by the ordinary refractive index  $n_0$ ". Meaning of this sentence is unclear to me. What does this delay mean? Delay as compared to propagation in air or delay as compared to the extraordinary wave?

Answer: The sentence has been rephrased to "Hence, the propagation velocity of the H-polarization is determined by the ordinary refractive index  $n_0$ ".

Figure 2 Comment: This figure seems to show slightly tricky geometry. It seems untrue that paths of the VV wave and HH wave meet at the same point of the snow/soil boundary.

Answer: This seems indeed a bit arbitrarily chosen. However, it is a strict requirement otherwise the copolar-coherence is lost. Only common scatterers within one range resolution cell of the radar contribute to the CPD. When the delay between the HH and VV polarization is larger than the range-resolution of the radar, this condition is not anymore fulfilled and the coherence is lost. This has been discussed in detail in a new section "Copolar phase difference of polarimetric radar systems".

L.1 in P.6076 Comment: The authors are using approximation that ice has no dielectric anisotropy. Please clarify it to readers.

Answer: We added a sentence: "We note here, that  $\Delta \zeta$  vanishes only for isotropic ice which is not generally the case for ice on glaciers and ice sheets where the crystal axis of ice (c-axis) has a preferential orientation (e.g. Matsuoaka97, Fujita14)".

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L.12 in P.6076 Comment: "Delta epsilon" should be minus (-0.05) if we exactly follow the definition of "Delta epsilon" in this paper ( $\epsilon_x - \epsilon_z$ ). As observed in Fujita et al. (2014) at a site in Greenland ice sheet. I suggest to add this.

Answer: Thanks for finding this typo. From this value, a negative CPD of  $-70^\circ$  per meter would be expected. This has been corrected and "at a site in Greenland ice sheet" has been added.

L.16 . 17 in P.6076 Comment: "Similar anisotropy values have been observed in Alley (1987); Schneebeli and Sokratov (2004)." It was not clear to me similar to what. I read these two papers but I could not identify what was really cited here. In addition, the authors should mention what kind of snow they are talking about. The former is the Antarctic firn. The latter seems to be artificial snow under temperature gradient. In contrast the authors' major topic in this paper seems seasonal snow. It seems that all these types of snow and firn are treated equally.

Answer: It has been added, that Antarctic firn was used by (Alley 1987) and natural and sieved seasonal snow by (Schneebeli 2004). "similar" refers to the range of the determined structural anisotropy. This has been clarified by citing numbers for A' as given in the two papers.

L.23 in P.6077 Comment: What does "SDvar" stand for? Snow Depth variability or something like that? An explanation will help readers.

Answer: The abbreviation "SDvar" for Snow Depth variability course is now given.

L.17 in P.6078 Comment: "sectors can be found in (Leinss et al., 2015)." This way of citation occurred at many points in this paper. I think that "sectors can be found in Leinss et al. (2015)" is correct. If so, please repair many such points in this paper.

Answer: I think, both styles are correct. On [http://www.the-cryosphere.net/for\\_authors/manuscript\\_preparation.html](http://www.the-cryosphere.net/for_authors/manuscript_preparation.html) it is written: In general, in-text citations can be displayed as "[...] Smith (2009) [...]", or "[...] (Smith, 2009)

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[...]". To me "can be found in Leinss" sounds a bit like it can be found inside the body of Leinss. Of course, correct would be "can be found in the publication of Leinss (2015)" but I see "can be found in (Leinss, 2015)" as a shorter version of "in the publication of ~". Please correct me if I'm wrong here.

Section 3.2 Comment: A lot of abbreviations started to appear, such as SSI, SDTA1, SMT etc. It is hard to remember everything for readers. I suggest that a list for abbreviations is provided.

Answer: A table listing the abbreviations is now provided.

Section 3.3 First line Comment: The authors wrote "Snow density was manually measured in the snow pit once every week." Was it measured over the entire thickness? If so, please inform readers of it already here. In addition, in this paper, it is important to inform readers that manual measurement of density and independent measurement of the snow thickness is necessary to derive snow anisotropy from the CPD.

Answer: Both, the depth-average and the vertically resolved density were measured. However, for this paper, no vertical density profiles are required, therefore we wrote "The depth-averaged snow density was measured manually..." Your second point is already addressed in comments above.

L.3 in P.6079 Comment: Please let readers know what SWE means when it is used first in this paper.

Answer: "snow water equivalent" was added.

L.5 in P.6079 Comment: What is GWI? Please explain to readers briefly. Is there any good citation for this instrument? What is the measurement principle?

Answer: It has been added that SWE was determined "from measurements of gamma ray absorption within the snowpack using the Gamma Water Instrument". The measurement principle is described in the given reference about SWE determination (Leinss, 2015). Unfortunately, there is no reference for the GWI.

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L.9 in P.6079 Comment: "where" → "were" ?

Answer: corrected.

L.10 . 12, in P.6083 Comment: The authors wrote "For three dates anisotropy measurements are compared with anisotropy data from computer tomography." It should be clarified for which three dates. It took time for me to understand. Perhaps it should be as follows. For the three dates when the muCT measurements were done, anisotropy measurements are compared with anisotropy data from computer tomography.

Answer: This has been clarified by writing: "The anisotropy obtained from the computer tomography analysis of the four profiles CT-1 to CT-4 were compared with the anisotropy obtained from the CPD for the four corresponding dates when the samples were taken in the field". Note, that we added a fourth anisotropy profile.

L.19 . 20, in P.6083 Comment: The authors wrote "The snow density was determined by dividing SWE, as determined in (Leinss et al., 2015), by the snow depth measured by the sensor SDAT1." Readers will not understand this sentence unless they know physical meaning of SWE.

Answer: The definition of SWE = average snow density x Snow depth is given to provide the reader the physical meaning of SWE.

L.24, in P.6084 Comment: "Lemmettyinen et al. (2013, p. 399(49))" Is this paper publicly available and accessible? If it is not to access for readers, perhaps the authors provide the data in the appendix or as the supplementary information.

Answer: The NOSREX report (Lemmettyinen et al, 2013) is available on the ESA datahub for campaign data. The URL is provided now in the references.

L.29, in P.6084 Comment: The authors wrote "melt-refreeze events caused the formation of a crust at the bottom of the snow pack". It seems crust should appear at the surface of the snow pack. Like me, the other readers may not imagine a crust at the bottom of the snow pack.

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Answer: Of course, the crust appeared first at the top of a very shallow early winter snow pack but was then covered by snow therefore, the crust is situated at the bottom of the snow pack. The sentence has been rephrased to: "Due to warm temperatures, depth hoar was largely absent and melt-refreeze events in early December caused the formation of a crust in the shallow snow pack which was later covered by snow."

L.25, in P.6085 Comment: Again I suggest that "recrystallization" should be replaced by "metamorphism".

Answer: done.

L.12, in P.6086 Comment: Citation of Bormann et al. paper is a bit confusing. It is not very clear if the citation is for the density range or the density dependency of the CPD. Please clarify.

Answer: This paragraph has been rephrased: "The average anisotropy of the snow pack can be estimated, because the CPD shows only a weak density dependence for the density range of seasonal snow. For seasonal snow, densities of 0.15 and 0.4 g cm<sup>-3</sup> have been reported (Bormann13). Within this range, the CPD varies by less than 20% as shown by Fig. 3a."

L.14, in P.6086 Comment: There seems no "Figure 3a". It is Figure 3 (right).

Answer: yes. This has been corrected.

Section 4.2 Comment: So far when I read this paper, I had an impression that the dielectric anisotropy could be calculated purely by remote sensing (contactless, destruction free, according to the authors). However, I noticed that an important point of the method is that the data of the ice thickness and the snow density should be observed independently.

Answer: This has been clarified in several points; also here it has been added that the CPD "can be inverted with the additional information of snow depth and a good approximation of snow density to get a CPD based estimate for the depth-average

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anisotropy."

Comment: Also, it seems unclear that observations using multiple angles of theta and multiple frequencies are necessary for data processing. I think that such information should be provided in the abstract and the conclusion. Otherwise, until readers reach this section 4.2, readers will think that the dielectric anisotropy of snow may be observed by a method of microwave remote sensing alone. In reality, ground observations for density and snow thickness are necessary. In addition, it seems that the readers should know what will happen if the remote sensing data with only single theta value and single frequency is available.

Answer: Only snow density and snow depth are required. Different frequencies provided redundant estimates for the anisotropy. Different incidence angles could in principle be used to eliminate either snow depth or density due to the nonlinear dependence in incidence angle. However, I think the nonlinearity is too small to be used. The anisotropy was determined independently for all different incidence angles and the results for different incidence angles were compared. This requires the assumption that the snow properties do not vary spatially for different antenna footprints. The careful preparation of the test site and the snow depth variability measurements support this statement.

Comment: The authors showed that the standard deviation of the anisotropy of snow is very small. But this small standard deviation can be attained based on multiple settings of  $\theta$  values and frequencies. Is it so?

Answer: I'm not sure, if I understand your comment correctly. The standard deviation is the standard deviation based on multiple settings of theta and frequencies. I rephrased the sentence to: "The standard deviation for each time is given by the scatter of the individual estimates  $S_A(\theta_0, f)$  for each  $\theta_0$ , and  $f$ ." I hope this clarifies / answers your question.

L.24 . 25, in P.6087 Comment: I did not understand meaning of an expression "wave-

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lengths "fit" into the snow volume".

Answer: I hope, "wavelengths "fit" into the propagation path through the snow volume" is more clear.

L.16, in P.6089 Comment: Please specify +4% and -8% relative to what.

Answer: Relative to the anisotropy measured by computer tomography. This has been corrected.

L.1 . 2, in P.6090 Comment: "as it is expect for snow recrystallized by temperature gradient metamorphism" I suggest as follows. "as it is expected for snow geometry modified by temperature gradient metamorphism" Here, I consider definition of recrystallization in metallurgy or ice crystal. If the authors feel that this term can be still used for sublimation and condensation, please explain basis for it.

Answer: As in other cases, also here the term "recrystallized" has been replaced by "metamorphic" and the sentence has been rephrased to: "as it is expected for the geometry of metamorphic snow which was exposed to temperature gradients"

L.12 . 14, in P.6090 Comment: "Further, the CPD decreases during periods of cold temperatures due to temperature gradient metamorphism." To clarify more, I suggest to modify "due to growth of the vertical anisotropy by temperature gradient metamorphism."

Answer: Thanks for the suggestion. It has been clarified as suggested.

Equation 26 Comment: Please provide readers what "SD" means. I think it is snow depth.

Answer: SD = snow depth has been added.

In the equation 26 and Figure 18 Based on my poor understanding, I did not understand what "tau" meant and how I should see Figure 18 right top and right bottom. Please better explain meaning of these to me and to readers.

C3375

Answer: The parameter "tau" is the temporal offset between the time series snow depth, SD(t), and the time series CPD(t). This is now explained more in detail.

L.11, in P.6091 Comment: fresh "snow"?

Answer: thanks, "snow" was missing here.

L.12 . 14, in P.6093 Comment: The authors wrote as "The propagation delay difference of orthogonally polarized microwaves was measured by the CPD which was then used to determine the structural anisotropy of snow." I suggest to clarify that the method is for snow with known thickness to derive average anisotropy over the thickness. For example, geometry of the optics is clearer if it is written as follows. "The propagation delay difference of orthogonally polarized microwaves through known thickness of snow was measured by the CPD which was then used to determine the structural anisotropy of snow averaged over the snow thickness."

Answer: This suggestion has thankfully been included together with the comment below: This sentence reads now: "The propagation delay difference of orthogonally polarized microwaves through known thickness of snow was measured by the CPD which was used to estimate the dielectric anisotropy of snow to determine then the structural anisotropy of snow averaged over the snow thickness."

L.16 . 19, in P.6093 Comment: I would suggest that the authors explicitly tells to readers that the CPD values were converted to the dielectric anisotropy of snow using the snow thickness that were determined independently. Otherwise, many readers may misunderstand that the CPD alone can determine the precise snow anisotropy.

Answer: see answer above.

Comment: The authors wrote that the standard deviation of 0.005 as small numbers. However, this evaluation is a result of measurements using multiple theta and multiple frequencies. In addition, snow thickness and the density should be known independently. These experimental settings should be mentioned.

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Answer: I see your point. I added: "The standard deviation is based on measurements of different incidence angles and frequencies and confirms the provided theory though does not represent systematic errors due to uncertainties in snow depth and density."

L.19 . 20, in P.6093 Comment: The authors wrote, "Copolar phase differences ranging from -30 to +135° were measured for 50-60 cm deep snow at a frequency of 13.5 GHz." Readers may wonder why these numbers are specifically given here. Are these numbers symbolic for the present study? In addition, actual snow depth ranged up to ~100 cm. Readers will wonder why 50 - 60 cm deep snow was highlighted.

Answer: "The large variation of CPD values shows that the anisotropy of snow must be considered when the CPD is analyzed in polarimetric studies of snow covered regions." (this sentence has been added.)

L.22 . 24, in P.6093 Comment: The authors wrote, "Only small deviations of 5 - 10 degree" Readers will not understand whether this angle is for incidence angle or the CPD. In case this is the CPD, readers feel hard to understand how deviations of 5 - 10 degree mean as a size of uncertainty.

Answer: It has been clarified that here I mean the deviations of the CPD from the modeled data. Further, the 5-10 degree have been put into relation with the range between -30 and +135 degree in order to provide an idea about the relative accuracy.

L.25, in P.6093 Comment: I hope to find one of keywords "dielectric anisotropy" somewhere in this line, to tell basic principle of the birefringence.

Answer: the keywords "dielectric anisotropy" have been added: "The linear frequency dependence verifies our assumption that the CPD is a volumetric property of snow which is determined by the dielectric anisotropy and is which is related to the structural anisotropy of the ice matrix and pore spaces of snow." In the second sentence of the conclusion, the keyword "birefringence" has been added.

L.6 . 10, in P.6094 Comment: "A weak correlation was found and an optimal acqui-

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sition interval of 8.15 days was determined to detect the depth of fresh snow . It was observed that the evolution of the CPD shows a delay of about 2.3 days compared to the evolution of snow depth, which indicates an average settling time of a few days." Due to my poor understanding, I did not understand the relation between tau and the optimal acquisition interval in this paper.

Answer: As answered on your comment further above, I hope that this has been clarified now. The parameter tau has been described more detailed there.

Around L.18 in P.6094 Comment: The authors wrote "The possibility to observe the anisotropy of the snow pack by remote sensing techniques". This technique require independent determination of the snow thickness and the snow density. Readers should know how this requirement can be satisfied in the practical remote sensing. A short paragraph to discuss this point will help. Otherwise, some readers may think there is no such requirement.

Answer: A short paragraph providing some information about independent determination of snow depth and snow density using remote sensing data has been added.

L.20 . 23, in P.6094 Comment: I did not understand at all what kind of principle was meant here.

Answer: The paragraph has been rephrased.

L.14, in P.6095 Comment: "Dielectric anisotropy" should be explicitly stated here, because it was exactly used in experimental principle used in this paper.

Answer: "especially the dielectric anisotropy" has been added.

Technical corrections Comment: L.19 in P.6064 (Li et al., 2008) should be Li et al. (2008). Answer: corrected.

Interactive comment on The Cryosphere Discuss., 9, 6061, 2015.

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Date: 11 April 2016

## Anisotropy of seasonal snow measured by polarimetric phase differences in radar time series

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**Abstract.** Snow settles under the force of gravity and recrystallizes-metamorphism is driven by vertical temperature gradients. Both effects ~~are associated to snow-related ice crystals which~~ cause a spatially anisotropic distribution of the 3D ice matrix and pore spaces of snow. The spatial distribution has its symmetry in the vertical axis and induce an anisotropy in the mechanical, thermal, and dielectric properties of the snow pack. On microscopic scales, the anisotropy ~~could be isotropic-determined-only~~ can be determined from stereology or computer tomography of samples taken from snow pits. In this paper we present an ~~alternative method and show how the anisotropy of a natural snow pack can be observed-contact and destruction-free with polarimetric radar measurements~~. The ~~scattering phase differences~~ method based on radio wave birefringence to determine the depth-averaged anisotropy on macroscopic scales, and without the need for in-situ sampling. Using polarimetric radar instruments, the birefringence allows precise determination of the anisotropy of seasonal snow if snow depth and density are known. The anisotropy is determined by measuring the copolar phase difference (CPD) of polarized microwaves transmitted through dry snow ~~were analyzed for four winter seasons (2009-2013)~~ from the SnowScat instrument installed at a test site near the town of Sodankylä, Finland. An electrodynamic model was established linearly polarized microwaves which propagate obliquely through the snow pack. We established an electrodynamic model based on anisotropic optics and on Maxwell-Garnett-type mixing formulas to provide a link between the structural anisotropy and the measured CPD. The anisotropy ground based radar instrument "SnowScat" was used to measure the anisotropy of a natural snow pack over

four winter seasons (2009-2013) at a test site in Finland. Anisotropy values derived from the CPD were compared with in-situ anisotropy measurements obtained by computer tomography. In addition, we show that the CPD measurements obtained from SnowScat show the same temporal evolution as space-borne CPD measurements from the satellite TerraSAR-X. The presented dataset provides a valuable basis for the future development of snow models capable of including the anisotropic structure of snow.

### 1 Introduction

Deposited snow is a porous and highly metamorphic material which continuously undergoes ~~recrystallization~~ metamorphic processes to adapt to the external thermodynamic forcing determined by the atmosphere and the underlying soil. The microstructure of the sintered snow crystals constitutes the thermal, mechanical and electromagnetic-dielectric properties of snow. Hence, an ~~anisotropic microstructure~~ spatially anisotropic distribution of the microstructure (composed of the ice matrix and pore spaces) leads to a macroscopic anisotropy of snow properties.

Characterization of the microstructure is difficult and requires work intensive sampling ~~which can disturb the natural snow structure~~ ~~measurability by snow pits using sample-taking or cutting of samples~~, sample preparation, and data processing. However, it reveals a unique richness of details with microscopic resolution. Macroscopic methods applied in the field measured depth- or volume-averaged snow properties on scales of cm to meters. Microwave

Fig. 1.

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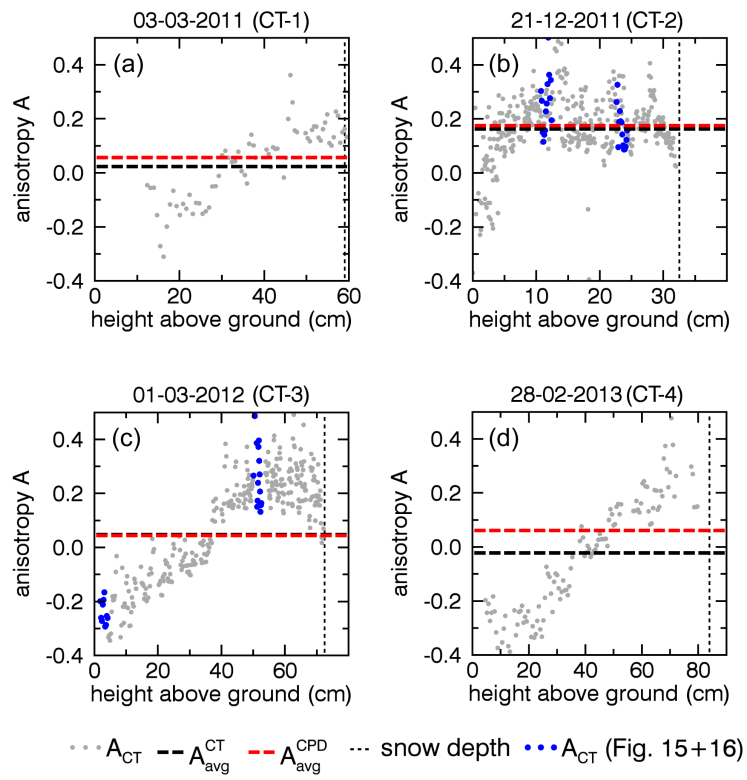


Fig. 2.