General comments

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 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.

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.

I have relatively minor criticisms/concerns at points as listed below as the specific comments. Frequent use of a term “*recrystallization*” does not seem proper to me to express metamorphism where vapor sublimation and condensation play major roles. 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. 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 by the CPD measurement alone, without any additional measurements. It seems more fair to me that prerequisite of the method is given. 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. 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.

Specific comments

Abstract in general
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.

Abstract, L.2.
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.

Abstract, L.3.
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.

Abstract, L.4-8.
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 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.

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

L.24 in P.6062
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.

L.26 in P.6062 – L.2 in P.6063
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.

L.3 – 12 in P.6063
I felt a bid 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?

L.8 in P.6063
I suggest “metamorphism” rather than “recrystallization”.

L.14 in P.6063
(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”.

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

L.13 – 16 in P.6063
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 (Löwe 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

L.17 in P.6063
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

L.2 – 3 in P.6064
The authors wrote *“The origin of horizontally aligned structures has been discussed with respect to settling of fresh snow (Schleef and Löwe, 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.

L.2 – 6 in P.6064
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.

L.7 in P.6064
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.

L.9 – 11 in P.6064
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 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.

L.13 – 15 in P.6064
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)."

L.16 in P.6064
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 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 to remote sensing. I suggest that earlier examples for snow and large ice masses should be naturally introduced to readers.

L.23 – 25 in P.6064

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 km2 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 μCT measurement and the microwave remote sensing, a quantity dielectric anisotropy is necessary.

L.29 in P. 6004 – L. 3 in P.6065

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).

L.3 – 5 in P.6065

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.

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

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

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

L.13 “negative values” meaning is unclear to me.

L.14 – 16 in P.6065
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.

L.17 in P.6065
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?

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

L.11 in P.6066
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 authors suggest to use for studies of the snow?

L.14 – 16 in P.6066
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?

L.6 in P.6067
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.

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

L.26 in P.6067 – L.2 in P.6068 “*We found....*”
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 resometer method (Mätzler, 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.
As for the footnote #2 in P.6068, it is not understandable for me, too. What are ε_h or ε_s? What is $3.17^{1/3}$?

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

L.4 in P.6068
Please provide unit of ρ.

L.7 – 9 in P.6068
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? 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.

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

L.24 in P.6070
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.

L.24 – 26 in P.6070
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?

L.1 – 2 in P.6071
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.

L.3 in P.6071
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 experimental 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.

L.6 – 9 in P.6071
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?

L.10 in P.6071
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. 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.

L.5 – 6 in P.6072
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?

Figure 2
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 target of the snow/soil boundary.

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

L.12 in P.6076
$\Delta\varepsilon$ should be minus (-0.05) if we exactly follow the definition of $\Delta\varepsilon$ in this paper ($\varepsilon_x - \varepsilon_z$).

"as observed in Fujita et al. (2014)" <u>at a site in Greenland ice sheet.</u> I suggest to add this.

L.16 – 17 in P.6076
"*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.

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

L.17 in P.6078
"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.

Section 3.2
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.

Section 3.3 First line
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.

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

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

L.9 in P.6079 "where" → "were" ?

L.10 – 12, in P.6083
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 μCT measurements were done, anisotropy measurements are compared with anisotropy data from computer tomography.

L.19 – 20, in P.6083
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.

L.24, in P.6084
"Lemmetyinen 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.

L.29, in P.6084
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.

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

L.12, in P.6086
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.

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

Section 4.2
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 in that the data of the ice thickness and the snow density should be observed independently. Also, it seems unclear that observations using multiple angles of θ 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 θ value and single frequency is available.

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 θ values and frequencies. Is it so?

L.24 – 25, in P.6087
I did not understand meaning of an expression “ wavelengths “fit” into the snow volume”.

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

L.1 – 2, in P.6090
“*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.

L.12 – 14, in P.6090
“*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.”

Equation 26
Please provide readers what “SD” means. I think it is snow depth.

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.

L.11, in P.6091
fresh “snow”?

L.12 – 14, in P.6093
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.”

L.16 – 19, in P.6093
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 pricese snow anisotropy.
The authors wrote that the standard deviation of 0.005 as small numbers. However, this evaluation is a result of measurements using multiple θ and multiple frequencies. In addition, snow thickness and the density should be known independently. These experimental settings should be mentioned.

L.19 – 20, in P.6093
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.

L.22 – 24, in P.6093
The authors wrote, "*Only small deviations of 5–10◦* "
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 mean as a size of uncertainty.

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

L.6 – 10, in P.6094
" *A weak correlation was found and an optimal acquisition 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.

Around L.18 in P.6094
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.

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

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

Technical corrections

L.19 in P.6064

(Li et al., 2008) should be Li et al. (2008).