

The Cryosphere Discuss., author comment AC1
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Reply on CC1

Joëlle Voglimacci-Stephanopoli et al.

Author comment on "Potential of X-band polarimetric synthetic aperture radar co-polar phase difference for arctic snow depth estimation" by Joëlle Voglimacci-Stephanopoli et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-314-AC1>, 2022

Dear Georg,

Thank you very much for the useful comments regarding our work and especially all the suggestions regarding the polarimetry. They helped a lot to improve the manuscript. We deeply appreciate your conscientious review. For a better readability of our response, the answers to the reviews and the corrections in the manuscript are shown in orange.

CC comments 1: It's clear that you did a lot of analyses on a lot of data, but I found it sometimes difficult to follow all the analyses across different measurements and vegetation classes. For instance, when do you speak about the long 5-year TSX time series and when about the short interval with the 3 orbits? You write that you have snow measurements which are revisited for each TSX acquisition, but that seems to apply only to the 2019 data.

We agree this structure might be difficult to follow. In order to improve readability, we modified the objectives and add a third one (modification at lines 64-67):

*[...] (1) investigate SD and DHF variability between different vegetation classes in the Ice Creek catchment (Qikiqtaruk-Herschel Island, Yukon, Canada) **using in situ measurements collected during a field campaign in 2019; (2) evaluate linkages between snow characteristics and CPD distribution over 2019 dataset and (3) Determine CPD seasonality considering meteorological data over 2015-2019 period.***

We mentioned the period of acquisition for the snow profiles (line 150) and their utility (line 158). We also suggest a modification for the sentence at line 158 to improve the understanding of the reader:

"Detailed snow profiles were acquired in spring 2019 (mid-April to early May)".

*"The snow depth and mean density of each layer classified were compiled in a linear regression analysis with TSX data **from the same period. Regression analysis are used to reach objective (2) if this paper"***

We took the 5 years TSX timeseries as it allowed reporting on the seasonality of CPD signal over the years. Unfortunately, no snow information was available starting 2015

nonetheless, the objectives stated were reached.

CC comments 2: A thorough check, which results are important and which could be removed for conciseness, could also help the reader and better tailor the paper towards the objective(s). For instance, in section 4.2.1, the reported seasonal values for CPD are very important, but annual means might be not so meaningful with such distinct snow and snow-free seasons. Similarly, I'm not sure how the paragraph on "Comparison over Snow Classification" contributes to the objective of the paper (but this impression could be just because of my radar perspective).

Sentences on line 303-305 were meant to report seasonal values over each year. To improve clarity the text was changed to:

"For the 2015-2019 period, the mean CPD value during the snow season was -8.59° . The means of each winter are ranging between 13.41° (2014-2015) and -6.42° (2017-2018). During the snow-free condition, the average CPD over the same period increased to -0.87° (2015-2019). Maximum and minimum values during snow-free conditions ranged between -0.44° (2015) and -1.32° (2015-2016)."

Regarding the paragraph "Comparison over snow classification", we have the feeling that this paragraph is necessary to contextualize our dataset to the newly updated snow types classification proposed by recent work from our group by Royer et al. (2021) following results from Sturm (1995) which demonstrates the applicability of arctic snow classifications in Western Canada. As such, to improve the flow of the manuscript, we suggest moving this paragraph to the Conclusion section and moving Table 2 in appendix.

CC comments 3: The analyses of the wealth of in situ data and co-polar phase measurements is for sure highly valuable for the scientific community, but I'm wondering if the derived conclusions could be clearer and more elaborate. I think 3.5 lines of conclusions about CPD and snow depth could be a bit more when looking at the title of the paper. For instance, conclusions about which scenarios do not give a correlation between snow depth and CPD measurements and the underlying reasons (some ideas: shallow snow at exposed topography, maybe related to certain snow structures like wind crusts or depth hoar, which don't have the required anisotropy to give CPD. Generally small sensitivity to shallow snow. Certain ground conditions, even though I don't understand what you mean there, see questions below). Maybe also some thoughts about how to overcome these limitations could be of interest.

Thank you for the suggestion. We included a paragraph 'Future work' and detailed some thoughts:

*"Future studies should focus on the threshold sensitivity to TWI and the incidence angle of snow depth retrievals in order to map snow depth in such environments. This would also allow an evaluation of the potential of using interpolation techniques to bridge spatial observational gaps in SD information at the watershed scale. **First, SD variability within a TSX pixel should be studied further, especially in hummocky areas where the highest variability was found, which could suggest a variability in the TWI as well. Statistical approaches, using the coefficient of variation of snow depths (CVsd), as suggested by Winstral and Marks (2014) and Liston (2002) could be an interesting avenue in the development of a representative mapping of the terrain. Meloche et al. (2022) demonstrated recently the effectiveness of the coefficient of variability of snow depth (CVsd) to improve passive microwave SWE retrievals in similar environment found on Herschel island (i.e., arctic snowpack with tundra vegetation type).***

References added:

Liston, G. E.: Representing Subgrid Snow Cover Heterogeneities in Regional and Global Models, Journal of Climate, 17, 1381-1397, 10.1175/1520-8, 2014. 0442(2004)017<1381:RSSCHI>2.0.CO;2, 2004.

Meloche, J., Langlois, A., Rutter, N., Royer, A., King, J., Walker, B., Marsh, P., and Wilcox, E. J.: Characterizing tundra snow sub-pixel variability to improve brightness temperature estimation in satellite SWE retrievals, The Cryosphere, 16, 87–101, <https://doi.org/10.5194/tc-16-87-2022>, 2022.

Winstral, A., and Marks, D.: Long-term snow distribution observations in a mountain catchment: Assessing variability, time stability, and the representativeness of an index site,13 Water Resources Research, 50, 293-305, 10.1002/2012WR013038, 2014.

CC comments 4: The correlation between CPD and SD shown in Table 6 gives higher correlation for some vegetation classes, while the correlation results in Appendix A give only low correlation for SD (H_tot). Do I understand it right that this is because all vegetation classes are combined in Appendix A? And how does this relate to line 325 "No significant correlation was found other than SD.." ?

Yes, the understanding is correct. Our samples contain 15 observations or less for each snow characteristics, which we feel is to divide the sample by vegetation classes. Appendix A (Changed to appendix B) shows no significant correlations in the snowpack characteristics at each orbit. Only 2 correlations are possible at orbit 152 (incident angle=24°, on the cumulative thickness of horizontal layers (meltfreeze crust, ice lens, in cm) and mean density of the snowpack) but they are not significant because of the sample size (8 observations for each characteristics).

CC comments 5: The discussion about TWI is interesting, but beyond the potential difference in soil moisture, isn't also the question of freezing of soil relevant? In my understanding, any level of soil moisture will give surface scattering from the ground below the snow, which is the desired scattering scenario for the CPD model. Isn't the question rather what happens when the soil freezes?

High moisture in the soil will have the effect to delay the freezing process at first, and then keep the ground temperature stable longer than soil with low moisture (e.g.: Romanovsky and Osterkamp, 2000). On the other hand, Burn and Zhang observed a delay on active layer freeze back in area where "snow may accumulate in early winter" (from section 5.5. of their paper). Active layer in these areas freeze back in mid-December, or a month later than other location (between 2003-2007).

Considering these two different scattering mechanisms:

1) dry snow over wet soil: the SAR signal penetrates through the snowpack and is reflected away by the wet soil. Based on the Romanovsky and Osterkamp (2000) theory and in situ observations from Burn and Zhang (2009), we could suggest that could enhance surface scattering processes of specular reflectance depending on surface roughness. This could explain why stronger correlations with snow depth are observed in area with high TWI, no matter the snow depth as good correlations were observed in Coltsfoot (mean SD: 126.0 ± 67.6) and Lupine areas (mean SD: 38.9 ± 22.3).

2) dry snow over ice: the SAR Signal penetrates the snowpack and scatters on the ice layer. As detailed on lines 390-393, Dedieu and al. (2018) monitored the phenomenon that the SAR signal is not able to penetrate ice layers thicker than 5 cm. The scattering mechanism on ice is mostly specular reflectance given the flat nature of ice layers. Hence, both an ice layer and a wet soil supports the CPD measurement.

In our case, ice layers in the snowpack were less than 2 cm. It is possible that, in preferential area for water accumulation, ice layers developed at the snow-ground interface which would enhance the surface scattering in the season i.e. after January as observed in figure 5, where the active layer should be frozen. Unfortunately, this was not documented on the 2019 field campaign. A more "in depth" study on freeze up process including in situ data and observations at different vegetation classes would be of great interest to have a better understanding of the processes in place during changes in the CPD signal throughout the season as observed on figure 5.

Reference cited:

Romanovsky, E. and Osterkamp, E.: Effects of Unfrozen Water on Heat and Mass Transport Processes in the Active Layer and Permafrost, 2000.

CC comments 6: In the discussion and conclusion about TWI, there is potentially an unclear causality. Maybe the good correlation between CPD and SD for high TWI is rather related to the fact that high TWI values are found in the depression areas which are naturally with high SD (and are apparently the Coltsfoot class)? Similarly, the good correlation between CPD and SD for Coltsfoot could be just because Coltsfoot is predominantly in valleys. I'm wondering if the larger SD (in valleys with coltsfoot) is required to have a certain sensitivity of CPD to SD and the high TWI and related soil moisture is just a correlation but not the cause of the CPD to SD correlation.

We agree that future works should study more precisely the snow depth variability within a vegetation class, as its location is dependant to the topography. We found the best results of snow depth and CPD in Coltsfoot valley, but also in Lupine class, where the mean snow depth is 38.9 ± 22.3 cm which is more than 80 cm different from the mean snow depth in Coltsfoot class (see Table 3). Although there is no certainty that there is no causality between TWI and snow depth, our results show good correlation between a variety of snow depth. Future works should address on the variability of snow depth within a SAR pixel by vegetation class would greatly improve our comprehension at this point. This topic is now addressed in the conclusion. Please refer to our answer above in comment #3.

CC comments 7: Line 230: What do you mean by the presence of ice leads to better reflection conditions for the microwave? Do you consider the mentioned moisture content to be frozen or liquid? As you mention somewhere else, larger moisture gives higher dielectric contrast and thus more backscatter, therefore I'm not sure how ice (with less dielectric contrast) leads to a better reflection. And do you maybe mean backscatter instead of reflection here? (forward reflection would reduce backscatter for a side looking SAR)

The presence of an ice layer in the snowpack simply provides a nice surface for specular reflection of the radar signal, especially in the horizontal polarization thus reducing backscatter. To improve clarity the sentence was changed to:

"High moisture content at the soil surface would potentially improve the performance of SD retrieval, given that the penetration of the signal into the soil would be limited by the high dielectric constant of the soil."

CC comment 8: A few statements about the scattering scenario (scattering only from ground) are unclear to me:

Line 424: Isn't the signal penetration through the entire snowpack and only scattering from the ground exactly the desired scenario for the approach of Leinss et al?

Leinss et al.(2014) showed a correlation with cumulation of fresh snow. In our case, our correlation is with the total snow depth, which is a complex snowpack with a diversity of snow type (depth hoar, ice lenses, wind slabs, etc.)

Please refer to lines 109-113 form the manuscript:

"A relationship was found between CPD and snowfall by Chang et al. (1996) and Leinss et al. (2014) which induces a propagation delay among horizontal and vertical phases due to horizontal alignments of fresh snow crystals. Recent studies focused on the boreal region (Leinss et al., 2014, 2016) or were applied in arctic region with no or sparse vegetation (Dedieu et al., 2018) so the application of the CPD method in the Arctic remains poorly documented. It could be hypothesized that the CPD can describe the entire snowpack in such cold and dry environments."

CC comments 9: Line 403: In my understanding, the approach requires all backscattering to come from the ground, which reads the opposite in this sentence? I'm not sure if you mean the contribution of the ground on the backscattered intensity or the CPD by "backscatter signal". It seems to me that you indicate at some occasions that scattering from the ground at low moisture could influence the CPD, but I couldn't find an explanation why. Leinss et al., 2014 mention that potential CPD contributions from rough surface scattering from the soil are small and have the opposite sign. Is this meant here?

The reviewer is right, this is simply a wording mistake, so the sentence was modified as followed:

"Thus, CPD captures snow accumulation well across winter in areas of higher potential of soil moisture, while soils with lower potential moisture are likely to contribute much less to the CPD signal, thus reducing the correlation between snow depth and CPD."

CC comments 10: Line 385: What do you mean by "the small CPD decrease during winter for Lupine and Dryas indicates an influence from the ground" if the general scenario is that all scattering comes from the ground anyway? I miss an explanation how ground can influence the CPD measurement.

Our results suggest that high soil moisture would enhance the contrast between ground and snow. Therefore, a low soil moisture would probably lead to signal noise at the interface from vegetation and ground variability.

Sentence in lines 385-388 is modified accordingly:

"The small decrease observed at Lupine and Dryas classes during the snow season (Fig. 5) could indicate an influence from the ground, as the snow depth measured is less than 30 cm and highly stratified. However, the effect from inhomogeneities within the snowpack does not support this case, as the CCOH is greater of 0.5 for each pixel. Dryas is characterized by the lowest TWI, which could lead to less backscattering at the snow-ground interface hence decrease the change in the snow season. High DHF in Lupine vegetation class indicate a potential of higher TWI in the tussock's hollow, which might not be captured by the TWI. Hence, the TWI variability within a TSX pixel at this vegetation class area could also explain the low decrease of CPD observed in Fig. 5."

CC comments 11: Line 393: I don't understand why "the shrubs may explain the best correlation" and how this is related to the canopy and the size of the shrubs.

Sorry this was a shortcut. Warmer ground temperature was measured under shrubs (e.g.:

Domine et al. (2016) and Myers-Smith and Hik (2013)) which could delay the freezing process during the shoulder season (as suggested and detailed on comment #5).

line 393 is changed for the following sentence:

*"A high level of moisture in the ground will lead to major dielectric contrast at the snow-soil interface, hence limiting the penetration depth of the radar signal (Duguay et al., 2015). Thus, the sensitivity of the signal to ground conditions decreases. Duguay et al. (2015) also showed a strong saturation of TSX signal in the areas with shrubs greater than 50 cm. **Warmer ground temperature were previously observed in permafrost are (e.g. Myers-Smith and Hik (2013), Domine et al. (2016)). which could delay the freezing process and enhance the contrast at the snow/ground interface. In the case of the study area, Myers-Smith et al. (2019) report an increase of the canopy where the measured shrubs at the bottom of the valley were more than a meter.***

Reference added :

Myers-Smith, I. H., & Hik, D. S. Shrub canopies influence soil temperatures but not nutrient dynamics: An experimental test of tundra snow-shrub interactions. Ecology and Evolution, 3(11), 3683–3700. <https://doi.org/10.1002/ece3.710>, 2013.

CC comments 12: Retrieving HHVV* phase with $\arctan()$ of the Kennaugh elements is only partly correct. Values above $+\pi/2$ and below $-\pi/2$ give a phase jump that causes ambiguous values. I assume the correct functionality of typical programming languages is used to derive the phase, but it might be worth checking to avoid related errors. For the equation, the angle symbol \angle comes to mind here instead of the $\arctan()$.

You are right and thank you for your notice. We have used the atan function in idl using two arguments. Hence, the result is between $-\pi$ and π and ambiguous values were intercepted. To avoid misunderstanding, we changed the equation as follows:

(see pdf for equation)

CC comments 13: The use of the Kennaugh elements could be clearer. The conventional notation of the full-pol Kennaugh matrix follows $K_{11} \dots K_{44}$, even though I see the single digit notation, with e.g. K_7 , in Schmitt et al., 2015. Furthermore, eqs. (2) and (3) are for a Dual-pol Kennaugh matrix, which might confuse readers who are familiar with the conventional Kennaugh matrix. Maybe I am just not aware of this kind of formulation in other literature, but for me it is a particular dual-pol Kennaugh formulation. This can be easily solved by just explicitly stating that you follow the dual-pol Kennaugh matrix formulation of Schmitt et al., 2015, but it might be also related to my finite overview of Kennaugh matrix theory.

Thanks for your comment. To avoid misunderstandings, we will point out to the reader that we have used the Kennaugh notation according to Schmitt et al. (2015).

Therefore, we added following explanation (line 211):

"Note that the notation of the Kennaugh matrix is labelled according to Schmitt et al. (2015)."

CC comments 14: A detail on eq. (4): The expression of the HHVV* coherence in terms of Kennaugh elements only gives the real valued coherence magnitude, but the left hand

term is the complex co-pol coherence (with the CPD phase). Maybe this is meant by the approximate equal sign, though. You could appropriately extend the Kennaugh expression with eq. (1) to integrate the phase and make also the Kennaugh right hand term complex. Alternatively, you remove the CPD phase term and make the entire equation real-valued and only about the coherence magnitude. This would then fit to the way you describe and use CCOH.

That's correct, we lost the complex term on the right part of the formula. We corrected the equation as followed:

(see pdf for equation)

CC comments 15: Eq. (4): Since you use HH-VV phase, meaning negative values for fresh snow, see eq. (1), I suggest to switch the VV and HH subscripts in eq. (4), to make the sign of the CPD phase consistent with eq. (1).

Suggestion accepted

(see pdf for equation)

CC comments 16: Line 297 should start "Figures 5a and 5b", I guess.

Thank you. Suggested change was done

Please also note the supplement to this comment:

<https://tc.copernicus.org/preprints/tc-2021-314/tc-2021-314-AC1-supplement.pdf>