The Cryosphere Discussions

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Comment on tc-2021-314

Georg Fischer

Community 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-CC1, 2021

I read this preprint with a lot of interest and realized that my personal notes, my questions, and a few technical issues I have, could help to improve the paper. Therefore, I decided to contribute here with some selective thoughts and points which cover only parts of the paper.

Some thoughts:

First of all, kudos to the authors for trying to interpret the Co-polar Phase Differences (CPD) to snow depth (SD) relationship in such a diverse and challenging environment. The investigation of the CPD to snow relationship in such a complex environment provides valuable insights, which are important for further developments towards snow retrievals with CPD measurements. The analysis of CPD correlation to various snow parameters provides an interesting result that a correlation could be only found with snow depth. This could even indicate a certain robustness of the method to varying snow properties. Interesting is that the expected decrease of CPD with increasing snow is only obvious for the Coltsfoot (and Shrub) vegetation class, which is characterized by larger SD. The lack of correlation in all other classes and the potential relation to vegetation or snow characteristics is discussed and raises challenging questions for future research. The presence of vegetation makes the understanding of the CPD to snow relationship very challenging, since vegetation increases the complexity of the scattering scenario, which deviates from the snow-over-ground scenario assumed by Leinss et al. for the CPD model.

General comment:

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.

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).

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.

Questions (which don't necessarily require an answer, but hopefully help to understand what might be unclear to the reader):

- 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.." ?
- 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?
- 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.
- 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)
- 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?
  - 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?
  - 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.

- 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.

Technical points:

- 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 ∡ comes to mind here instead of the arctan().
- The use of the Kennaugh elements could be clearer. The conventional notation of the full-pol Kennaugh matrix follows K_11 … 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.
- 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.
- 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).
- Line 297 should start "Figures 5a and 5b", I guess.