

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2022-19

Anonymous Referee #2

Referee comment on "Snow properties at the forest tundra ecotone: predominance of water vapor fluxes even in deep, moderately cold snowpacks" by Georg Lackner et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-19-RC2>, 2022

In "Snow properties at the forest-tundra ecotone: predominance of water vapor fluxes even in thick moderately cold snowpacks", the authors present an observation dataset and modelling with CROCUS of snowcovers in a forest-trundra eco zone in north eastern Canada.

Overall the manuscript is well written. The ability of modern snow models to accurately resolve snow depth and microstructure including accurate density estimates remains a key challenge. This paper presents some interesting observational and modelling results that, after tightening should make it a contribution.

My major criticism is that the treatment of the canopy and canopy impacts, e.g., canopy interception and canopy sublimation, are not clearly articulated. This is especially the case in the modelling description. I would like to see this more readily described. Without, it makes the forest results difficult to interpret. I understood the forest site to be similar to the Ménard, et al site where the shrubs are buried by snow (~1m tall), however the black spruce (~5m) are almost certainly not buried and thus have canopy affects. Given the canopy dynamics can impact shortwave transmittance, longwave (in/out), albedo, etc I feel the manuscript is missing this critical section.

More of a "this surprised me" rather than a criticism, I was very surprised at the total lack of SWE observation and comparison with the model. I realize the focus of this manuscript is on the depth and density estimates. However, as snowdepth includes the uncertainty in both snow mass and density, it is a bit difficult to attribute differences in SD as entirely due to density errors versus more systematic snow mass errors. For example, biases in snow loss due to surface sublimation cannot be diagnosed with snowdepth results alone. I would strongly suggest a small comparison of model v. obs SWE so-as to allow the reader to confirm snow mass is being correctly estimated.

Lastly I think there needs to be a better treatment of the uncertainty of the parameters in the snowmodel and in the observations (specifically the conductivity). I realize the authors are not interested in calibrating the model. However, for example, how impactful are the decisions on line 160? Although I get the sense these are chosen by evaluating the physical processes in play, they are still somewhat arbitrary and may dramatically impact the interpretation of the results.

Specific comments:

L5 The TUNDRA and FOREST sites being all-caps surprised me. I'm fine with this, but I am wondering what the motivation is versus proper names such as "Tundra" and "Forest"?

L16 "models leads to an inadequate representation" of snowdepth or SWE? The distinction matters w.r.t policy. E.g., if the mass is still right, then at these scales that is often sufficient: "we will still have $X \text{ m}^3$ water input to reservoirs under policy Y".

L34 "precipitation are typically higher" Given this study focuses on the transition, certainly they remain somewhat similar 1km apart. The boreal forest is a large region. Where, exactly, is this transition point?

L38 "similar to alpine snow" This is below-tree-line alpine snow?

L 55 I would like to see the authors directly specify the research questions. This has a good start, but I would like this clearly stated and then answered in the discussion+conclusion

L 72 Figure 1 I really like this figure

L 95 This section needs a description of how canopy interactions (mass + energetics) are handled

L180 "based on estimates" are these from just musing on it, or were these informed from soil pits, etc?

L181 Based on the met data availability I had expected a simulation period of 2012+ with a spin up period pre-2012. Why was the model not spun up prior to 2012 and run for evaluation 2012 onward? It would be good in this section to explicitly note "met data available for 2012-XXXX, model spin up was YYYY-ZZZZ, and evaluation was PPPP-QQQQ".

L182 Does this not contradict the 2012 statement on line 172? "Observations of these variables at each of the two sites have been collected since 2012, except for atmospheric pressure,"

L182 "corrected the precipitation" is this the ERA5 data? Or the obs? Please explicitly state.

L184 "fixed threshold of 0.5" There are a plethora of threshold methods that are physically based and indeed the choice matters significantly

1.Harder, P. & Pomeroy, J. W. Hydrological model uncertainty due to precipitationâphase partitioning methods. *_Hydrol Process_* **28**, 4311–4327 (2014).

2.Jennings, K. S., Winchell, T. S., Livneh, B. & Molotch, N. P. Spatial variation of the rain–snow temperature threshold across the Northern Hemisphere. *_Nat Commun_* **9**, 1148 (2018).

This would be a good candidate for inclusion in additional uncertainty estimates to understand how impactful this was in the fall and spring seasons.

An adjacent question is how was precipitation temperature estimated due to its impact on developing snowpack cold content?

L188 "specific humidity" this one may not be identical

1.Flerchinger, G. N., Reba, M. L., Link, T. E. & Marks, D. Modeling temperature and humidity profiles within forest canopies. *_Agr Forest Meteorol_* ****213****, 251–262 (2015).

L198 "mean difference" And I assume gusts too, important for blowing snow

L199 "downwelling shortwave" is this sub canopy? Shrubs or in the forest proper? If the latter I would have expected substantially more difference late season. It may be worthwhile breaking this into a few periods as the long, dark winters will heavily bias the mean.

L201 "remained comparable throughout the winter" This seems expected due to low solar angles?

L202 Or due to the canopy + higher solar angles?

L205 Figure 3, note the site and add units

L217 "depending on the maximum snow height" isn't this somewhat a tautology such that deeper, colder snow packs take longer to melt out than small snowpacks? Noting the impact of cold content development might help make this section stronger.

L241 "similar environments" unclear what this is referring to.

L241 Figure 6 these captions are Proper Name case Tundra and Forest. Either change the text to proper name case or change these to TUNDRA/FOREST for consistency.

L253 "for the upper 80%" It looks closer to 50-40%?

L248 Figure 7 Suggest adding uncertainty regions for these observations to match the 29% noted in the text.

L281 Best remind the reader quick what was adjusted

L283 "between the two versions" adjusted v. non-adjusted?

L286 Canopy impacts?

L295 Without doing a falsification experiment of one with and one without can you know this 100%. Please describe how it is known with such high confidence that it is related to these events.

L321 "as for the low[...]there." Is this not canopy as well?

L397 This water vapour transport finding seems to be a major conclusion I think you should better highlight

L403. Same as above

L423 "Arctic-like" this is unclear, previously you had noted Alpine-like. Is that what you mean here?

L435 by publication-time I hope?