Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-348-RC1, 2019

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Snow processes in mountain forests: Interception modeling for coarse-scale applications" *by* Nora Helbig et al.

Anonymous Referee #1

Received and published: 23 September 2019

Review of: Snow processes in mountain forests: Interception modeling for coarse-scale applications by Nora Helbig et al.

This paper presents a development and validation of a simple snow interception model that can be applied in large scale modelling efforts. Snow interception can be a significant part of cold-region forest water balances and novel approaches are needed to more robustly estimate this term especially in large scale modelling activities. Overall the paper articulates the challenge well and provides a simple solution. I would recommend publication with some minor revisions.

Major comments:

Throughout the paper forest structure is parameterized from a Digital Surface Model

(DSM). If I am reading this correctly this is a DSM that is normalized to terrain to give a DSM in terms of vegetation height? (Z-axiz of Figure 2)? To be consistent with literature this should rather be termed a canopy height model (CHM) rather than a DSM. https://www.earthdatascience.org/courses/earth-analytics-python/lidar-raster-data/lidar-chm-dem-dsm/

Can you clarify the type of model you are presenting in context of the existing models? There was much discussion of other existing models and a clear compare/contrast of what you are presenting would be beneficial. This is simple empirical relationship with 1 or two input variables rather than a physical/mechanistic parameterization. This is critical to clarify so that future users can determine how to use this moving forward.

A clearer description of what was being measured at the various sites is needed. It was not immediately apparent that all of this was based on snow depth difference only and ignored density. The assumption that density of new snow accumulation is the same between open and forested areas is critical. Is it reasonable to assume that the standard error of 9.31 kg/m2 of new snow estimates is greater than observed snow density differences between open and forested? Even immediately after snowfall events there will be differences in density associated with unloading/compaction on the dripline of tree crowns versus the influence of blowing snow redistribution/erosion or not in clearings? At these locations is it reasonable to assume snowfall is the same between forest and clearing locations – any preferential deposition patterns evident? Variable blowing snow deposition/erosion in clearings versus forests? In the end do you have any observations that you could demonstrated that density differences are negligible or provide these values in terms of SWE? Any errors in density differences could lead to relatively large errors in interception ratios, especially for small events, and this needs to be clarified.

The transferability of this model is tested by applying the Swiss parametrization to French and US sites. While results are promising for between these sites I would temper some of the speculation (339-353). Relative to the large range of climatic con-

ditions of cold-regions forests globally these sites represent relatively warm locations. As expressed elsewhere there has been variability in interception model performances between maritime and continental locations not to mention more temperature cold regions versus cold arctic treeline/tundra locations. Before recommending this for universal and widespread applications this model should be tested if possible at other locations that represent more end members.

The approach implemented is to parameterize an empirical relationship. This will not work perfectly for all scenarios/locations obviously. Is it possible to quantify the uncertainty of the parameters and how they may vary between sites / vegetation types? How stable are these parameters?

Specific comments:

First sentence on abstract and line 19-21 are a little contradictory.

26-33: Transition to discussing surface albedo is abrupt. While snow interception/albedo is a critical feedback it is not extensively discussed hereafter? Can this section be simplified?

41-42: Awkward sentence

102: define more clearly what indirect interception measurements are.

151-154: what may the influence of different point cloud densities be upon the CHM derivation? Are there any recommendations you could make on what should be collected in future for proper a parameterization of your models?

205: how are you getting from SP a ta point to mean sky view factor. How is the space being discretized? Are you computing SP on a fine scale grid and averaging values over a coarser scale of interest?

248-250: can you clarify this reversed response?

279: Why do we want to know the standard deviation of snow interception. Can you

articulate a broader reason to calculate this?

286-287: As canopy gets to be more homogeneous the spatial variability of interception increases? How?

322-324: Could a global product (between 51.6° N and 51.6° S at least)for these metrics be derived from the GEDI platform? https://gedi.umd.edu/

365: Typo “ASifferences”

381-383: Deciduous will have very different behavior than coniferous vegetation. Could you reoptimise your model for deciduous specific sites? Would be interesting to know if the same scaling laws were applicable to know if a separate deciduous scaling parameterization is needed or not.

386-388. Why? Can you justify this a bit more?

412-416: The full summary of the various interception efficiencies would be better presented in the results rather than in the conclusion for the first time.

423-425: long challenging sentence

Figure 1: Can scale bars and north arrows be consistently sized and located on edge of orthophotos? Where snow depth measurement setups the same for each point in the respective sites?

Figure 2: what is grid resolution of DSM (aka CHM)? What UTM zone is applicable for the respective easting/northing? Correct sig figs on the easting northing?

Figure 4-6: ”Parametrized“ or “Modelled” interception on y-axis label?

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