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Interactive comment on "How Much Cryosphere Model Complexity is Just Right? Exploration Using the Conceptual Cryrosphere Hydrology Framework" *by* Thomas M. Mosier et al.

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General comments

The authors describe the development and application of a modular framework combining several conceptual cryosphere process modules. In the paper, they focus on the modules for simulating heat transfer and mass and internal energy of snow and ice. The model is calibrated for two glacierized catchments in Alaska and validated (in the respective other catchments) for seven model combinations using MODIS snow cover products, stake measurements, and runoff observations. Results show that the more physically based methods tend to be more reliable, however no single best module combination can be identified.

I like the concept of a modular open source framework for simulating cryosphere pro-

cesses in data-sparse environments very much and also like the general structure of the paper. However, there are some issues in the manuscript which should be addressed by the authors.

Probably the biggest issue I see, which possibly also affects the conclusions, is the way the MODIS validation procedure is performed. First, both the spatial and the temporal resolution of the chosen MODIS product is, in my opinion, unnecessarily coarse. 0.05deg are approx. 5 km, hence I assume only very few MOD10CM pixels are within the catchment boundaries? On top of that, by additionally averaging the monthly MODIS SCA pixels over the catchment area, even more of the original information is lost (as no pixel-by-pixel comparison is performed). A more valuable validation strategy would have been to use the daily (or possibly 8-daily) 500 m MODIS products (while applying a cloud cover threshold), and to use another skill score (e.g. the fraction of correctly classified pixels) besides just comparing watershed-averaged SCA values. Otherwise, the distributed nature of both the MODIS data and the model results is neglected by lumping the results together into a single number. Additionally, in my experience, the MODIS snow mapping algorithm generally also classifies ice surfaces as "snow-covered". Since large parts of both of the investigated catchments are glacierized, I would assume that in these pixels MODIS and the model results always match, leading to a positive bias in the SCA validation results?

Additionally, the authors should check the units of the variables more carefully. For example, throughout the manuscript several times units of "m^{-1}" are used (e.g., for M, CC, SWE, snlr, iclr, SM, SMC), which should in fact be m (or m s^{-1}). Also, in several equations the units do not work out.

Specific comments

- Is there a reason for the comparatively coarse spatial resolution (30 arcseconds, i.e. approx. 1 km?), considering that the investigated catchments are relatively small? This might be at least one reason for the generally quite poor skill scores

for the stake measurement comparison (especially with regard to the bias, as shown in Table 3), due to the considerable scale differences between a single point on a glacier and a 1 km^2 model pixel.

- I like the approach of evaluating the robustness of the model by calibrating it for one watershed and validating it in the other, however it would have probably been very insightful if the calibrated model would have also been applied and evaluated in the same catchment (using a split-sample test) prior to transfering the parameters to the other one. I assume this has not been done due to the lack of sufficiently long validation data time series?
- The authors state that the CCHF is open source and available to interested parties, which I very much appreciate. However, there is no mention on how/where to obtain the source code. I would suggest to add this information to the manuscript.
- Section 1.2: Besides the description of the climates, possibly add some more general information about the two catchments (e.g. area, elevation range, glacierization, ...).
- Section 2: In the introduction of the section, I would suggest to add a sentence about the temporal (i.e. daily) and spatial resolutions the model is/can be applied on, as this information appears only later in section 3.1. Besides, some remarks about the meteorological variables that are used in the model (especially which variables are required as input data (minimum/maximum/mean temperature and precipitation?) and which are calculated/parameterized (shortwave and longwave radiation?)) could be added, as this is not immediately clear from the manuscript. Additionally, what is missing from the model description is information about the precipitation-phase partitioning method(s) available in the model, and if any kinds of precipitation adjustment functions (e.g. for gauge undercatch) are implemented?

- Eq. (13): It is not immediately clear to me why the scaling of potential ice melt with $c_g H_{ice}/H$ is necessary. I would assume that the differences in energy required to melt snow vs. ice are already accounted for by the differences in albedo, which is taken into account in all heat transfer formulations except SDI (and in this case this could be overcome by introducing two separate degree-index factors for snow and ice, respectively)?
- Eqs. (14-15): Please introduce r_f after eq. (14) (where it first appears) instead of after eq. (15). The units of r_f do not work out in eqs. (14) and (15) (in eq. (14) it would have to be in m, while according to eq. (15) it is in m^3). Additionally, in eq. (14) $r_{f,i}$ should likely be $r_{f,i-1}$, otherwise there would be a circular reference?
- P17L3: Stating that glacier models are commonly evaluated only for a few days (!) is probably an exaggeration – I think it is well established that multi-year evaluation periods are necessary for glaciological purposes.

Technical corrections

- There is a typo in the title (“Cryrosphere”)
- P4L10: $[f_s]$ should be $[f_m]$, in the units again m instead of m^{-1}
- P4L24: ti → to
- P5L1: latent heat → latent heat of fusion
- P5L3: i.e → i.e.
- P5L29: (Simpson et al., 2002) → Simpson et al. (2002)
- Table 1: α_o (from the table heading) never actually appears in the table

- P9L3: m$^-2 \rightarrow$ m^{-2}
- P9L24: I would move this sentence ("... where the negative bounds on CC is zero") a few lines up (after eq. (9)).
- P10L17: "the the"
- P11L20: c_t should probably be t_l?
- P15L6: their $\rightarrow$ there
- P18L21-22: "has the predictive skill" $\rightarrow$ "has the best predictive skill"? (2x)
- Table 8 (heading): vise $\rightarrow$ vice

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