The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-257-RC1, 2017

Interactive comment on "Observations and simulations of the seasonal evolution of snowpack cold content and its relation to snowmelt and the snowpack energy budget" *by* Keith S. Jennings et al.

Anonymous Referee #1

Received and published: 22 December 2017

The authors present a study that uses a long term observational data set to validate simulated snowpack cold content. The authors attribute the largest increase in cold content to new precipitation mass. Validating a complex, multi-layer snowpack model that is frequently used in the literature is a substantial contribution, especially given the uniqueness of the long-term snow pit data. However, as currently presented, this manuscript needs substantial revision and polish. Below I explain my reasoning for this, and I hope the authors can use it to improve this manuscript into the contribution that is hiding under the surface. As is, I recommend accept pending major revisions.

Printer-friendly version

Discussion paper

My first issue is that these conclusions are specific to a deep snowpacks in a warmer climate. Thin, shallow snowcovers have a long record in the literature as being difficult to simulate due to the substantial radiative cooling of the snowpack resulting in sharp gradients and maximum cold content being exceeded. It is important that all these results are very clearly stated to apply to the deep snowpacks herein.

Second, is that I'm not entirely convinced by the results. As I understand it, the authors assert via Figure 3 that cold content of the snow pack is explained by cumulative precipitation. A statistically significant trend line is show for the subalpine site; however, it has an rˆ2 of 0.17. Cold content is effectively an instantaneous, integrated snowpack temperature expressed as energy required to bring it to zero-degree isothermal. Cold content will, by definition, become greater (more negative) as below zero-degree mass is added to the snowpack. An rˆ2 of 0.17 is a poor correlation and does not, at least to me, act as strong evidence for the authors conclusion. Perhaps the rˆ2 for the alpine site is acceptable, however given cold content will by definition increase as cold mass is added, it seems to be a circular result that does not add any new knowledge nor should be unexpected. With these results, the authors then proceed to the model step, effectively trying to duplicate the observed results. Stepping back, the message I feel like the authors are trying to present are: "there is no substantial radiative cooling of the snowpack, thus the precipitation temperature (and associated cold content) is the principal control on the total snowpack temperature, and therefore cold content." I suspect this is where Figure 8 becomes important, showing a small, negative total Qnet. However, something feels off about these results. In Figure 8a, the only real difference between day and night is the shortwave radiation and a slightly dampened latent heat flux. It seems odd to me that the mean response is identical, especially for the sensible heat flux. I'm just highly skeptical of an almost entirely similar surface energy balance between night and day. I would like the authors, upon confirming these results are correct as presented, to describe in more detail what is going on here, and if this is a site-specific effect or not, as my impression is it may be. Stepping back to Figure 6, I feel like this further highlights my issue with this conclusion. Full energy balance models use the balance of the energetics to simulate internal layer temperatures and energetics. Using cumulative mean air temperature feels very temperature-indexy and not really appropriate in this context – it supposes that the entirely of the snowpack energetics could potentially be explained by a mean air temperature, when in reality it's really the associated processes that would impact it.

Third, precipitation temperature and phase is unaddressed and is a critical component of this work. The simulations shown in Figure 9 c and d suppose the precipitation temperature and phase are correct. I'm assuming you used the default temperature-threshold in Snowpack for phase? These results could be quite different if phase was wrong (i.e., rain instead of warm snow) or precipitation temperature was biased. There is substantial uncertainty associated with phase partitioning methods and snowfall temperature (e.g., Harder, et al. 2014), and these have significant implications for this work. How sensitive are these results to various phase and falling hydrometeor temperatures?

Fourth, despite reading through this a few times looking for it, it is unclear to me what kind of clearing this sub-alpine site is in. The site is specifically stated as a clearing, but the Snowpack canopy routine is enabled. This will significantly change the surface fluxes as well as precipitation at the snow surface; e.g., canopy interception. In my mind, this undermines the results presented herein – maybe it explains the poor result in Figure 3? – and needs to be detailed and the effects and impacts explained. Site photos would go a long way towards helping orient the reader. However as is, this is a major detail that is omitted.

Fifth, A discussion on the role of Qg on cold content is needed and the assumptions behind your Qg simulation flux. These results show a treatment of the surface fluxes on cold content, but neglect discussion of soil-snowpack interactions, e.g., conditions that lead to frozen soil or refreezing of active layers.

Lastly, the authors assert that increased peak cold content and total spring precipitation control snowmelt onset. But this seems by-definition – doesn't this imply more mass and refreshed albedos? Isn't this just what you'd expect with increased cold content being a function of snowpack mass?

In summary: As I understand the results presented, the story is that the authors found limited evidence for sustained energy loss from the snowpack and that the cold content of the snowpack was mostly a result of mass inputs. However, there are many confounding factors that make it difficult to accept this at face value. Given the circular reasoning in the results (more snow -> more cold content, but that is by definition), it is difficult for the reader to accept the results. That being said, validating the model against these observations is quite interesting and diagnosing snowpack energy loss during the winter is a useful contribution. However, I think the overall message needs to be refined to more clearly articulate the site-specific nature of this study, the uncertainties in key aspects of the analysis (e.g., precipitation, canopy), and the text improved for readability.

References Harder, P., and J. W. Pomeroy (2014), Hydrological model uncertainty due to precipitation-phase partitioning methods, Hydrol. Process., 28, 4311–4327, doi:10.1002/hyp.10214.

Specific points Throughout: The authors introduce (para 25) increase/decrease for cold content, but proceed to use gain/loss. I think it should be consistent throughout Figure is used in the text but Fig. when used in brackets. Ideally should be consistent. Units should be separated with a cdot instead of spaces, e.g., WâŃĚmˆ(-2) Unclear what wet and dry days mean. Wet implies rain to me, but I suspect that's not what you mean. I would reword, or at least clearly define.

P1, Para 20: "cold content ... associated with reduced snowmelt" this needs to be reworded as snowmelt should be happening when CC = 0. Which melt rate is being considered? P2, Para 20: "the authors" which authors? P2, Para 25: "Furthermore. . ." I'm not sure I agree with this statement. CC needs to be = 0 for melt to occur, so isn't this known? Do you have a citation? P2, Para 30: "saturate", word choice P2, Para 30: "However...", I'm unclear what you're trying to say, please clarify. P3, Para 10, 15, 30 Need to be indented. P3, Para 20, use "10 m/s to 13 m/s" instead of how it is written. P4, Para 1, "Snow" incorrect capitalized P4, Para 14, "downwelling longwave" I would put a quick note as to what method you used. P5, Para 20, remove "proposed in Sect. 1" P5, Para 20 "We then quantified" I found this section unclear P5, Eqn 3 Consider writing 86,400 as a variable and showing in the text the units. Either way, you need units. P5, Para 15 "in order to improve" Using a model doesn't improve obs, it just compliments them. I think you should reword to make this distinction. P5, Para 20 "number of finite elements" change to layers P5, Para 25 remove "the numerical model in" P5, Para 5, the canopy module stuff comes out of nowhere, especially given you say the site is in a clearing. This needs to be much clearer. P5, Para 20 "Output from snow model simulations" I don't follow. Do you mean the comparison is more robust w/multiple outputs to validate? P6, Para 20 Any EC observations considered? P7, Eqn 4 The form for the energy balance equation given in Equation 4 is not a standard form. Generally, the change in internal energetics are given as a dU/dt and Qm is on the LHS. Qnet and Qm together are redundant in the energy balance as the energy available for melt is the net energy. P7, Para 1, "time scales" -> temporal scales P7, Para 25, as I said above, I don't buy that an r^2=0.17 demonstrates a primary control P8, Para 5, Probably should note these are depth averaged P8, Para 10, -2.2 should have units after it P8, Para 15, How is this working with the canopy module? Intercepted snow has massive sublimation losses, but that doesn't seem to be reflected here. P8, Para 20, Monotonically is either monotonic or not. There is no in-between. Reword P8, Para 25, "simulations confirm" change to "support" or similar P9, Para 10, So how are you calculating Qg? Maybe I missed it? I think you need a reasonable treatment on the assumptions behind however you do this. Did you couple snowpack with the soil? Constant flux? Constant ground temp? Qg is important for a conduction heat flux into the snow pack, and needs to be addressed if you go after cold content. Often Qg is taken to be 0-4W/m^2, but this flux can be important for stopping a numerical model from simulating absurd cold contents. P12, Para 10 “continued snowfall” But this is just more mass, so you’d expect snowmelt timing to be delayed P12, Para 20, “future work. . .” Lots of work on this already. . .. P16, Para 30 Given Snowpack is forced hourly, this longwave estimate seems like a massive source of uncertainty, especially within the context of an energy balance model. There are many incoming longwave formulations that take into account various proxies for non-clear sky. You seem to do this for your emissivity, but it’s not clear how that exactly works. With such low r^2 this needs to be detailed and expanded upon. The large error in a critically important mid-winter energy flux may have substantial implications for this work.

Figures

All figures – It would certainly aid readability to have them labeled as alpine/sub alpine without having to constantly refer to the caption.

Figure 1, difficult to determine differences at high elevation. Figure 2, can you change the DOY to dates for easier parsing? Figure 5a,b Should have same axis extents Figure 8abcd would benefit from having the same y- (ab) and x- (cd) axes to aid in comparison. Also, please expand the y-axes of (ab) so-as to understand what the limits are. Figure 9, needs legend

---

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2017-257, 2017.