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
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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.

Thank you for the detailed, critical review and the suggestion to publish pending the revisions. In the below response, our comments are in blue text.

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.

Given our geographic setting (Colorado Rocky Mountains), we framed this work in the context of western US snowpacks, which are essential to regional water resources. In this regard, the alpine and subalpine snowpacks are considered shallow and cold relative to the deep, warm snowpacks of the Sierra Nevada and Cascade mountains (Armstrong and Armstrong, 1987; Serreze et al., 1999; Trujillo and Molotch, 2014). We note in the discussion section of the original manuscript that these results are specific to the studied sites:

> "Firstly, we have only presented results from two sites within a single snow-dominated research catchment. Seasonal snow cover in the western United States spans a large elevational gradient and includes both maritime (e.g., the Cascades and Sierra Nevada) and continental (e.g., the Rocky Mountains) snowpack regimes (Serreze et al., 1999; Sturm et al., 1995). Therefore, an avenue for further research is to examine differences in cold content development across seasonally snow covered areas, with a particular focus on disentangling the effects of precipitation and air temperature during snowfall at sites with different snowpack characteristics."

However, we agree this framing overlooks the spatially extensive cold, shallow snowpacks of the Canadian Prairies and Arctic, and we have added text to the discussion mentioning these other snowpacks (p. 15 lines 17-21). We also reframed the last paragraph of the introduction (p. 3 lines 4-5) and research question 1 (p. 3 lines 9-10) per the recommendation of Reviewer 2 to emphasize that this research is specific to our study site.

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 r^2 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 r^2 of 0.17 is a poor correlation and does not, at least to me, act as strong evidence for the authors conclusion. Perhaps the r^2 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.

We agree that an r^2 of 0.17 is low and we note this in the original manuscript:

> "The relationship was statistically significant at the 99% level at both sites despite the low coefficient of determination in the subalpine."

We use significant portions of the text to evaluate the differences between the alpine (precipitation explains the majority of the variance in cold content development and contributes over an order of magnitude more cold content than negative energy fluxes) and subalpine (precipitation explains a small portion of the variance but still contributes nearly an order of magnitude more cold content than negative energy fluxes). We have included additional text to reiterate the differences in between the alpine and subalpine (p. 8 lines 14-19; p. 9 lines 17-21).

Additionally, we frame in the introduction how little work has been done examining how cold content develops in seasonal snowpacks and that one of the only papers to do so suggests that it was primarily through a largely negative surface energy balance. Our conclusion is not that snowfall adds cold content (which is obvious and known), it is that snowfall is the dominant pathway through which the snowpacks at our two study sites develop cold content. This finding is interesting on its own in that the alpine site should have a high potential to develop cold content through a negative energy balance due to high rates of sublimation and net longwave emission from the snowpack. Surprisingly, despite this fact, precipitation exerts a stronger control on cold content in the alpine than subalpine.

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.

We thank Reviewer 1 for their critical evaluation of figure 8. This caused us to go back into the simulation data and take a closer look at why the values would be so similar, and we found another interesting facet of cold content development during non-snowfall days. When subsetting the data to all hours of cold content gain without snowfall, we found that midday gains (0900 h to 1400 h) were practically negligible. Thus, the energy balance results were similar for the day and night periods because the daytime gains occurred primarily in the early morning and late afternoon hours. We added Figure S1 (histograms showing the frequency of cold content gains without snowfall at each site, binned by hour) to the supplementary material and related text to the results section (p. 10 lines 4-11).

Because daytime hours with cold content gains were so uncommon, we decided to redo figure 8 to present the daily energy balance for non-snowfall days with cold content gains. Figure 8 now shows the energy balance for the entire day and mean Q_{net} by hour. Interestingly, even on days with flux-driven cold content gains, Q_{net} is positive during the midday hours. As noted in the paragraph above, midday hours with negative Q_{net} were rare, thus the average Q_{net} for these hours was positive.

Additionally, while taking a closer look at the data we found several instances of simulated cold content spiking up and down (0.3 MJ m^{-2} h^{-1} $\leq \Delta CC \leq -0.3$ MJ m^{-2} h^{-1}) before returning to approximately its

previous value. We removed these spikes from the analysis (representing less than 0.2% of the dataset) and added a note on p. 7 (lines 10-12).

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.

We state our motivation for including air temperature in the introduction from the original manuscript:

> "Cold content can be estimated using at least one of three primary methods: 1) As an empirical function of air temperature (e.g., Anderson, 1976; Seligman et al., 2014; United States Army Corps of Engineers, 1956); 2) As a function of precipitation and air temperature (e.g., Cherkauer et al., 2003; Lehning et al., 2002b; Wigmosta et al., 1994) or wet bulb temperature (Anderson, 1968) during precipitation; and 3) As a residual of the snowpack energy balance (e,g., Andreadis et al., 2009; Cline, 1997; Lehning et al., 2002b; Marks and Winstral, 2001). In general, simple temperature-index models employ method 1, while both 2 and 3 are utilized in physics-based snow models. These methods suggest that cold content develops through both meteorological and energy balance processes, but few direct comparisons to observed cold content exist. This is likely due to the inherent difficulty in measuring cold content, which requires either time-intensive snow pits or co-located snow depth, density, and temperature measurements (Burns et al., 2014; Helgason and Pomeroy, 2011; Marks et al., 1992; Molotch et al., 2016). The lack of validation data introduces significant uncertainty into the dominant process by which cold content develops. Thus, it is not known whether cold content is primarily a function of air temperature (method 1), snowfall (method 2), or a negative surface energy balance (method 3)."

Given that air temperature is still used in current literature (e.g., DeWalle and Rango, 2008; Mosier et al., 2016; Seligman et al., 2014) to estimate cold content and that no research has shown the process (meteorological or energy balance) behind cold content development, we believe its inclusion appropriate.

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 temperaturethreshold 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?

Regarding precipitation phase, we increased the standard SNOWPACK rain-snow air temperature threshold from 1.2°C to 2.5°C to better represent phase partitioning at our high-elevation continental location (a paper of ours in press at Nature Communications shows the Rocky Mountains have some of the highest rain-snow air temperature thresholds in the Northern Hemisphere; Jennings et al., In Press). To test the effect of our threshold selection, we compared the annual snow frequency using the 2.5°C threshold (alpine = 76.4%; subalpine = 61.5%) to a bivariate binary logistic regression phase prediction model (alpine = 76.7%; subalpine = 62.8%). This model predicts precipitation phase as a function of relative humidity and air temperature, and it was shown to the best precipitation phase method in a Northern Hemisphere comparison (Jennings et al., In Press). We have added this information to p. 6 (lines 10-16).

The temperature of precipitation is a likely shortcoming of SNOWPACK as the model sets precipitation temperature equal to air temperature. In independent work we have performed, we found wet bulb temperature to be a better predictor of new snow temperature, which was also noted in Harder and Pomeroy (2013). This should be included as an update in the SNOWPACK model considering wet bulb temperature could be easily estimated from the already standard forcing data. However, because SNOWPACK prescribes new snow temperature to be equal to air temperature, our estimates of the cold content added by precipitation are on the conservative side. The use of the colder wet bulb temperature (relative humidity is often below saturation, even during snowfall on Niwot Ridge) would lead to a greater amount of cold content added by precipitation. We have added this to the new modeling uncertainty discussion section (Sect. 5.2).

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.

This is an excellent point and we have added site photos to Figure 1. We have also changed the text from "small clearing" to "stand of lodgepole pine" to be clearer.

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.

We have added information to the manuscript discussing how Q_G is simulated (covered in detail in Reviewer 1's other Q_G comment on page 8 below). As to the rest of the comment, we do include Q_G in our analysis of the snowpack energy balance, noting that it is typically positive even during periods of cold content gain. Frozen soil is more relevant to runoff processes and is outside the scope of this work. Refreezing of active layers leads to cold content losses (latent heat is released as the phase of water changes from liquid to solid). While this process is important to snowpack ripening and snowmelt generation, we only consider the empirical relationships between cold content and snowmelt rate/timing in this work.

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?

This is noted in the discussion of the original manuscript:

> "These results all suggest later seasonal snowmelt onset and faster snowmelt rates are primarily a function of persistent snowfall. While snowfall events can add significant cold content to the snowpack, they also change other fundamental properties that can delay snowmelt timing, such as increasing surface albedo (Clow et al., 2016) and adding dry pore space that must be saturated (Seligman et al., 2014)."

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.

We again thank Reviewer 1 for their critical review of this work. We have added text to the manuscript to illustrate our results are specific to our two study sites in addition to the discussion section that covered this point in the original manuscript. We have also addressed their specific comments below to improve the readability of the text and more clearly outline the project's uncertainties.

Additionally, we would like to reiterate the novelty of this work. There is relatively little previous literature assessing the meteorological and energy balance controls on cold content development. We used a combination of observed data and validated simulation output to show precipitation was the dominant source of cold content development at our two sites. This finding was particularly surprising at the cold alpine site considering its high rates of snowpack sublimation and net longwave emission.

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

We have changed p. 5 (lines 4-6) to include gain/loss and increase/decrease.

Figure is used in the text but Fig. when used in brackets. Ideally should be consistent.

This usage is in accordance with The Cryosphere's style guide (https://www.the-cryosphere.net/for_authors/manuscript_preparation.html) and will be kept:

> *"The abbreviation "Fig." should be used when it appears in running text and should be followed by a number unless it comes at the beginning of a sentence, e.g.: "The results are depicted in Fig. 5. Figure 9 reveals that...".".*

Units should be separated with a cdot instead of spaces, e.g., Wâ´NEˇmˆ(-2)

The Cryosphere does not specify the use of c-dots. The only example units we found in the manuscript prep instructions showed the units with a space as we have in the manuscript:

> *"Units must be written exponentially (e.g. W m^{-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.

Yes, this is confusing. We have changed all relevant text to reflect this (snowfall/precipitation and non-snowfall/non-precipitation days instead of wet and dry days).

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?

Reworded for clarity.

P2, Para 20: "the authors" which authors?

Reworded for clarity.

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?

The citations are in the following paragraphs and they indicate they provide differing perspectives on the control exerted by cold content on seasonal melt rate and timing. However, our text was not clear enough (yes, melt occurs when Q_{net} is positive and CC = 0) that we were referring to winter cold content magnitude and we have edited p. 2 (line 29) for clarity.

P2, Para 30: "saturate", word choice

Saturate is commonly used in the snow hydrology literature in reference to satisfying the irreducible liquid water content of a snowpack, but we have edited p. 3 (lines 1-2) to be more specific and be applicable to other hydrologists to whom the word saturate may indicate all pore space is filled.

P2, Para 30: "However: : :", I'm unclear what you're trying to say, please clarify.

Edited for clarity.

P3, Para 10, 15, 30 Need to be indented.

Changed

P3, Para 20, use "10 m/s to 13 m/s" instead of how it is written.

Changed, but units are left in exponential form to be consistent with The Cryosphere style.

P4, Para 1, "Snow" incorrect capitalized

Snow begins an independent clause, meaning it can (or should, depending on your preferred style guide) be left capitalized.

P4, Para 14, "downwelling longwave" I would put a quick note as to what method you used.

Added this information to p.4 (lines 21-22), with full methodology detailed in the appendix.

P5, Para 20, remove "proposed in Sect. 1"

Changed

P5, Para 20 "We then quantified" I found this section unclear

Rewritten for clarity.

P5, Eqn 3 Consider writing 86,400 as a variable and showing in the text the units. Either way, you need units.

Changed.

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.

Yes, changed.

P5, Para 20 "number of finite elements" change to layers

Changed. Although SNOWPACK is a finite element model, that is not important here.

P5, Para 25 remove "the numerical model in"

Removed.

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.

We changed the site description to note the pits are dug in a "stand" of lodgepole pine and have included a site photo in Figure 1.

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?

We are noting that the output of snow model simulations has greater fidelity when validated on more than just SWE, based on the work of Lapo et al. (2015). We are stating that we can make conclusions on the simulations of snowpack cold content because we have actual measurements of cold content to which we can compare the model output. We added text to p. 7 (lines 4-5) to clarify.

P6, Para 20 Any EC observations considered?

We did not consider using the EC observations on Niwot Ridge because the subalpine AmeriFlux tower measures fluxes above the canopy (21 m) and not near the snow surface. There are EC measurements in the alpine, but the records are short and the instruments are located near areas of snow scour.

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.

The form presented in the Equation 4 is used frequently throughout the snow hydrology literature (Cline, 1997; Marks et al., 2008; Marks and Dozier, 1992, to name just a few examples). However, we have changed the notation to the suggested form in order to avoid confusion. Additionally, we have changed Q_{net} to refer to the sum of the radiative, turbulent, and ground heat fluxes in order to avoid writing out the full energy balance each time we are referring to the net surface and ground fluxes.

P7, Para 1, "time scales" -> temporal scales

Time scale is appropriate and left as-is.

P7, Para 25, as I said above, I don't buy that an rˆ2=0.17 demonstrates a primary control

Please see our previous note. We also clarified to say "of the two meteorological quantities evaluated here" to note we are referring to precipitation and air temperature. As mentioned previously, we devote an entire discussion section to the topic of subalpine cold content development and why precipitation exhibits a reduced effect relative to the alpine.

P8, Para 5, Probably should note these are depth averaged

Fixed.

P8, Para 10, -2.2 should have units after it P8,

Fixed.

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.

We are only concerned with snow surface sublimation as canopy sublimation does not directly lead to changes in snowpack internal energy.

P8, Para 20, Monotonically is either monotonic or not. There is no in-between. Reword

Correct. We removed that sentence and included a new sentence to clarify precipitation exerts a stronger control in the alpine than subalpine (p. 9, lines 17–21).

P8, Para 25, "simulations confirm" change to "support" or similar

Changed.

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/mˆ2, but this flux can be important for stopping a numerical model from simulating absurd cold contents.

We used version 3.3 of SNOWPACK, which assumes a ground temperature of 0°C when there is snow cover. Simulations showed Q_G was typically between 0 and 4 W m^{-2} when snow depth exceeded ~20 cm, which is similar to other values reported in the literature. Thus, Q_G provides a small, but consistently positive to the snowpack energy balance. Cline (1997) noted ground heat flux was negligible at the alpine site using a flux plate in the soil. Snow pit data from the alpine and subalpine are consistent with this in that the warmest snowpack temperatures are observed at the bottom until ripening begins. We added this information to p. 6 (lines 20-21) in the methods and new Sect. 5.2 in the discussion.

P12, Para 10 "continued snowfall" But this is just more mass, so you'd expect snowmelt timing to be delayed P12,

Mass additions do not lead to consistent snowmelt responses because mass in and of itself is not a physical property that delays snowmelt (i.e., a given amount of warm, wet snow has a much different

effect on snow energetics and runoff than the same amount of cold, dry snow due to the amount of liquid water vs. dry pore space, changes to surface albedo, and cold content). Furthermore, we note other hypotheses in the discussion of the original manuscript:

> "These results all suggest later seasonal snowmelt onset and faster snowmelt rates are primarily a function of persistent snowfall. While snowfall events can add significant cold content to the snowpack, they also change other fundamental properties that can delay snowmelt timing, such as increasing surface albedo (Clow et al., 2016) and adding dry pore space that must be saturated (Seligman et al., 2014)."

Para 20, "future work: : :" Lots of work on this already: : :.

We have rewritten to clarify that most previous work focuses on single, well-instrumented sites (such as our manuscript) or vast networks of SNOTEL-like sites with only air temperature, precipitation, and SWE observations (e.g., Trujillo and Molotch, 2014). A spatially explicit, energy balance treatment has yet to be applied to the different snow categories of the western United States despite the irreplaceable contribution of snowmelt to the hydrologic cycle and regional water resources. We have added text to this discussion section to clarify our statement (p. 14 lines 29-32).

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.

This is an important point and we have added a note on this limitation to the new discussion section on modeling uncertainty (Sect. 5.2). Downwelling longwave radiation is under-sampled relative to other standard forcing data (Raleigh et al., 2016) and associated errors propagate into SWE and snow temperature biases (Lapo et al., 2015). Schlögl et al. (2016) showed little sensitivity in Alpine3D SWE simulations to a selection of empirical longwave estimates and Lapo et al. (2015) indicated the largest effects of longwave uncertainty were simulated at perturbations greater than ± 10 W m^{-2}. Thus our small mean bias likely indicates the total amount of incoming longwave radiation is correct while the low r^2 suggests the timing of subdaily fluctuations is not well simulated. Our multi-variable validation shows that SNOWPACK performs well relative to snow pit observations of SWE, depth-weighted snowpack temperature, and cold content.

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

Agreed. Changed on all relevant figures.

Figure 1, difficult to determine differences at high elevation.

We added contour lines to the figure along with site photos per an earlier recommendation.

Figure 2, can you change the DOY to dates for easier parsing?

Yes, changed for easier interpretation on this figure and others that previously showed DOWY.

Figure 5a,b Should have same axis extents

Axes left as-is.

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.

Changed per this suggestion and our notes on the energy balance in pages 2 and 3 above.

Figure 9, needs legend

Changed.

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