**Review of :**

**Observations and simulations of the seasonal evolution of snowpack cold content and its relation to snowmelt and the snowpack energy budget, by Jennings et al.**

The authors address the issue of the drivers of cold content evolution based on the exemple of two seasonal snowpacks from a single observation catchment in the Western US. They also assess, in a much shorter part, the effect of cold-content on snow-melt timing and rates.

The paper is well written and well illustrated. The take-home message is clear and the objectives assessed in Introduction are achieved with scientific quality. I found the paper both an appreciable synthesis of existing litterature on the topic, and enlightening regarding the conclusions achieved.

We thank Reviewer 2 for their suggestions and thoughtful review of the manuscript. Our responses are in blue text throughout this document and we have made changes to the manuscript in regard to their comments.

In addition to the few suggestions made below, there is in my opinion one minor contradiction in the Result section (point 9 below), that I would recommand the authors to adress with priority as it affects the consistency of the paper. Note that this apparent contradiction may come from misunderstanding from my side, or an edit mistake from the side of the authors. Comment addressed in point 9 below.

1. Introduction-p2 Ll-6 : not the snowmelt itself is critical for the cited applications, but the timing of surface/subsurface runoff from Snowmelt, which may not be the same when surface melt occurs and refreezes (as just mentionned earlier in the manuscript). For consistence I suggest changing« snowmelt » into« runoff from snowmelt » in the current sentence.
   Yes, this is a good distinction. We have changed the text to reflect this (p. 2 lines 2-3).

2. Introduction-p2 L16-18 : Aren't the « dominant processes »well-resolved by Method-3 (residual of the energy balance) at places where energy-balance models like SNOWPACK are routinely validated against data regarding most components of the energy balance ? In that case, isn't it rather a lack of investigation into the process of cold content development, than a lack of validation data, that limits knowledge of the prevailing processes ?
   Given the adequate performance of advanced energy balance models (Etchevers et al., 2004; Rutter et al., 2009), it is somewhat reasonable to assume they are accurately simulating the evolution of snowpack cold content. However, recent work has shown the utility in validating snow model output on more than one state variable (Lapo et al., 2015) in order to ensure we are not "getting the right answers for the wrong reasons." In this case we stand by our assertion that having measurements of snowpack temperature and cold content are necessary for making conclusions on how cold content develops in seasonal snowpacks. I.e., previous work could have tested hypotheses using energy balance model output alone, but a lack of cold content validation data would have limited the strength of their conclusions.

   Furthermore, the dominant processes involved in cold content development likely depend on the climatology of the investigated sites. Event though it is well stated in the Discussion, specifying the perimeter of validity of your study should be done here already. Typically, I suggest transforming research question #1(p3 L6) into « What are the meteorological and energy balance controls on cold content development **at two alpine and sub-alpine sites from the Western US ? »**

Yes, we agree with this point and have changed research question 1 to reflect Reviewer 2's suggestion (p. 3 lines 9-10). This point was also noted by Reviewer 1, so we have added text wherever possible to note the results are specific to our study sites.

3. Introduction-p2 L22-26 : conduction fluxes within the snowpack, and from snowpack to the ground, can mitigate the impact of the intense negative fluxes reported here. If a gradient around 100 W/m develops within the snowpack as a result of intense surface cooling, around 20 W/m2 propagates downwards (upon hypothesis of a 0.2 W/m/K conductivity for Snow), which should somewhat prevents the snowpack from locally reaching unreaslitic temperatures ( ?)
In the cited work, the authors note that their reported values are for ΔQ, or the change in snowpack internal energy, (Marks and Dozier, 1992). Although their paper has provided many significant contributions to the state of knowledge of the snowpack energy balance, it also underlines how little we previously knew about cold content development processes. We hope our manuscript provides a small step in the right direction.

4. Introduction-p3 LÍ : uncertainties->unknowns(suggestion)
Changed.

5. Methods - p5 L28 : « vapour diffusion » in SNOWPACK is actually only calculated to compute Snow grain/bounds growth rates. There is no mass redistribution between different snow layers as a result of vapour diffusion in current versions of SNOWPACK. I therefore suggest to suppress this item from the list of existing SNOWPACK routines, as it would be otherwise misleading.
Thank you for clarifying. Given this paper is not about grain metamorphism, we have removed this part.

6. Methods- p6 L7-10 : could you specify here or in appendix the result of your calibration procedure for the parameters leaf area index, vegetation height, direct canopy throughfall, and wind speed reduction ? Note that these parameters are usually estimated from field data, and that any observation-based estimate of them would help assess the soundness of the calibrated parameter or of the canopy model.
Yes. We have added this information to the Supplemental Material (Table S2).
Additionnal, the rough size of the clearing where sub-alpine snowpits were made, should be specified (p4LI) to justify the use the canopy module of SNOWPACK, instead of an open- area version SNOWPACK with just wind attenuation.
We have included site photos for both locations (Figure 1) in the updated manuscript and removed the "small clearing" part from the text as this caused some confusion.

7. Results-p5 L15-16 : « Peak cold content and peak SWE respectively occurred 33 d and 10 d later in the alpine than subalpine ».Add « on average » to this sentence and the next.
Changed.

8. Results-p7L25-27 : « This is likely due to the increased variability of winter precipitation, the coefficient of variation of which is 2.9 and 2.7 times greater than that of air temperature in the alpine and subalpine, respectively ». I assume that by « increased »you mean« higher» ? I would suggest that snow-atmosphere heat transfers occuring during cold air temperatures periods are less efficient in cooling the

snowpack, than the direct addition of cold Snow from fresh snowfall.

Correct, we have changed *increased* to *higher*. Additionally, we have added text to Sect. 4.1 to note that the air temperature is less effective at producing cold content than precipitation.

9. Results- p7L29-30 : « During periods of SWE accumulation, $Q_{net}$ was typically near 0 W $m^{-2}$ (Fig. 4a), indicating a large negative energy balance was not responsible for cold content development. » First, here, you infer $Q_{net}$ from the variation in CC between 2 snowpit dates, so where is the link to energy balance ? Second, based on Eq 3, $Q_{net}$ ~0 W $m^{-2}$ indicates no cold-content increase, meaning there is no visible snowfall-driven cold-content increase in the snowpit data. In my mind this contradicts the other results of the study, e.g. Fig 3 and 7 - please justify, or explain me where I am wrong. May I suggest using different names for $Q_{net}$ in Eq. 3 and $Q_{net}$ in Eq. 4 ? Like $Q_{net\text{-}pit}$ and $Q_{net\text{-}EB}$ respectively.

   This is a good point and similar to the one Reviewer 1 brought up regarding our energy balance notation. In the quoted lines we were attempting to convey that the snow pits showed no direct evidence of a large negative surface energy balance like the one reported in Marks and Dozier (1992). We computed $Q_{net}$ as a function of the change in cold content and the time between pit observations. To be more consistent, and clearer, we have changed our notation throughout the paper to have dU/dt be the change in internal energy of the snowpack ($dU/dt_{pit}$ for the snow pit data) and $Q_{net}$ to represent the sum of $SW_{net}$, $LW_{net}$, $Q_H$, $Q_{LE}$, and $Q_G$ (per the recommendation of Reviewer 1).

   In regard to your point "$Q_{net}$ ~0 W $m^{-2}$ indicates no cold-content increase, meaning there is no visible snowfall-driven cold-content increase in the snowpit data", we have clarified in lines 20-27 (p. 8) the way we presented these data. Because cold content is a relatively small value in terms of W $m^{-2}$, decreases in dU/dt will always be small, whether cold content gains come through precipitation or a negative surface energy balance. For example, if two snow pits were dug exactly one day apart, the computed dU/dt for a 0.2 MJ $m^{-2}$ increase in cold content (2 cm of new SWE at -5°C) would be just -2.4 W $m^{-2}$ (assuming all other energy balance components summed to zero). Thus, a fairly significant cold snowfall event would show up as a very small dU/dt value.

10. Results- p8 L7 : could overestimated densities be the reason for cold-content overestimation at alpine location ? (as Snow temperature tend to be overestimated ?) Maybe a line on that could be added to the Result or Discussion section

   We have added this point in the results section (p. 9 lines 3-6) and we have also added a new discussion section on the model shortcomings (Sect. 5.2).

## References


Etchevers, P., Martin, E., Brown, R., Fierz, C., Lejeune, Y., Bazile, E., Boone, A., Dai, Y.-J., Essery, R., Fernandez, A., others, 2004. Validation of the energy budget of an alpine snowpack simulated by several snow models (SnowMIP project). Ann. Glaciol. 38, 150–158.

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