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The Cryosphere Discussions

Interactive comment on "Changes in seasonal snow liquid water content during the snowmelt period in the Western Tianshan Mountains, China" *by* H. Lu et al.

Anonymous Referee #1

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The authors present an interesting dataset of measurements of liquid water content (LWC) in a seasonal snow cover. Measurements from different states of the snow cover are presented, from both pre-snowmelt conditions and snowmelt conditions. Also, there is some special interest for Rain-on-Snow events (ROS events). The authors claim the dataset is able to show seasonal and daily variations in vertical profiles of liquid water content. Furthermore, the authors claim the results show that ROS events alter LWC distribution profiles only for a short period of time.

Major points

As the paper is presented now, I think it will require at least major revisions. The major problem I find is that the paper lacks any discussion about the accuracy and errors of the liquid water content measurements. There are 3 reasons why this discussion of measurement accuracy is of crucial importance for the paper:

1. The paper has a strong focus on the measurements of liquid water content and many conclusions are drawn from these measurements. However, the interpretation of the conclusions is impossible with the absence of a discussion about measurement accuracy and errors.

2. There are signs in the dataset that measurement errors are far from being negligible small. One major problem I have with the data is that in seemingly dry, below-freezing snow, a liquid water content of about 0.3% is measured. See for example: p 4146, L13: It's very strange to call the period "pre-snowmelt" and still observe an LWC > 0. Also the daily profiles don't show 0% LWC in the top layer in the morning, which should be there when snow refreezes over night. I did not find a single measurement with 0% LWC in the paper. As far as I know, the current knowledge is that in snow that is below 0 degC, all water is frozen and only a very tiny fraction (a layer of a few molecules) of liquid water is present at the interface between the ice grains and the pore space. However, this quantity is not measurable with the technique used by the authors. So I think the observation of liquid water in snow of below 0 degC temperatures is pointing to (at least) a bias in the measurements. This issue is not discussed at all in the paper, which I find very problematic. It should be mentioned, and taken into account in a discussion about measurement errors and accuracy.

3. Due to the fact that the snowpack will be destroyed after making a snow profile, one can never do measurements in the same place. The authors made snow profiles 30cm apart from each other. This means that the authors have not only sampled temporal variations in liquid water content, but also spatial variations. This is never mentioned in the paper, but it is a source of variation (error) in the

measurements. It would be nice of the authors would have made several profiles at the same time, to sample the spatial variability. If the authors have such a dataset, it should be presented in the paper. If they don't have such a dataset, it should be discussed along with a discussion about measurement errors and accuracy.

I hope the authors will be able to amend the paper with a thorough discussion of these 3 issues, as in my opinion they are crucial for the paper.

Two other major issues I had with the paper regard the Introduction and Conclusions.

- The Introduction should not only mention relevant literature, but should also relate the citations to the work presented in the paper and should support the choices the authors made for measurement type, sensor type, measurement protocol and study area. As it is discussed now, this connection is not made. Some examples:

    1. p 4140, L6-15: So if all these methods to measure LWC are available, why did the authors choose the Snow Fork? This should be discussed here.
    2. p 4140, L43-5: The mentioned literature seems to have done simulations. Why are they mentioned here? The authors only work with measurements. Or did the cited literature show that more measurements were needed? Then this should be mentioned.
    3. p 4139, L14, L24, L29: What are the results and conclusions from the cited studies? Why are they relevant for the study the authors performed?
    4. p 4139, L17-18: Why are these values for LWC so different from the ones presented in this paper in p 4146, L13-14?

- The conclusions drawn in the results section are sometimes not based on the results presented, are inconsistent and sometimes even invalid. Examples:

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1. p 4146, L 21-24: The authors claim that the fact there is a scatter in LWC, which, according to the authors, suggests that the temperature indices are not fully representing the energy balance. However, it are the melt rates that should be more or less proportional to the energy balance (provided an isothermal snow cover), not LWC. Secondly, ROS events are also bringing LWC into the snow cover. This part of the LWC is not caused by snow melt, and should therefore not necessarily correlate with the energy balance. The statement that the scatter in LWC is caused by the mass balance of the snowpack needs further explanation.
2. p 4146, L17-19: From the paper, I assume that the correlation coefficients as presented in the paper in Table 1 are "standard" Pearson correlation coefficients and thus are testing for a linear relationship. The authors themselves show in Figure 2 and p 4146, L19-21 that the relationship between average air temperature and LWC seems to be exponential and between accumulated air temperature and LWC linear. It is therefore not valid to compare correlation coefficients for both sets, as done in L17-19. If the authors still want to compare the two datasets, it is better to use a rank correlation coefficient (such as Spearman's rank correlation coefficient) that does not imply linear relationships. Note that the accumulated air temperature only takes into account positive air temperatures and when I take the part in Figure 2a with average air temperature > 0, this seems to be much closer to linear too. Regarding this point, it is strange that the authors discuss the correlation between LWC and accumulated air temperature and average air temperature here, but for deriving an equation to estimate LWC (see Eq. 17, p 4151), they use the prior moving average air temperature. So this statistical approach to relate LWC to air temperature indices is in my opinion quite inconsistent and sloppy and needs a thorough revision.
3. p 4147, L19. It is not clear how the authors determined that those specific days were having a "typical" distribution of snow liquid water. It is crucial that

this is explained more thoroughly and made more objectively, as the results and conclusions strongly depend on the choice of these "typical" days. A proper way (in my opinion) is to calculate average profiles (averaged over all days in the specific period). Then, we also can get an impression about the variations from day-to-day within the snowpack (so between 08:00LT on one day to 08:00LT on the next day). We should remember that the experimental setup will also sample spatial variability. So between two 08:00LT-profiles, we have both temporal variation because of changing meteo-conditions, and spatial variation due to different profile-locations. This analysis would improve the importance of the paper. When typical profiles are still needed for the analysis, typical days may be selected by choosing profiles that resemble these averaged profiles the best.

4. p 4148, L22-23: "Thus, Anderson's formula may be suitable for all conditions". First, please refer to this Equation with the corresponding equation number (so Eq. 14). But then, I don't see how one specific measurement on one specific day can show that Eq. 14 is valid for all conditions? What is exactly meant by "conditions"?
5. p 4150, L16-17: "The liquid water from rain was rapidly discharged. Hence, most part of the energy supplied by precipitation cannot exchange with snow." This statement cannot be true. The only energy delivered by the rain water itself is the temperature difference between the rain water and 0 degC. This energy is delivered to the snow cover, until the liquid water has reached 0 degC. Then the energy exchange stops and the melt water infiltrates through the snowpack, leaving the snowpack with 0 degC. So unless the authors have measured the temperature of the snowpack runoff water to be >0 degC (which is highly unlikely), this statement is not true.
6. p 4150, L19-21: First, I don't see how the distribution of LWC is in accordance to the typical distribution. At what time is this supposed to be the case? I only see an agreement BEFORE the snowfall, but that's not what

is discussed here. It would also be strange that the distribution would look like Figure 4b, which is a typical infiltrating melt water front, which does not suit the snowfall event. In L21, it is stated that the snowmelt rate decreased during and after the snowfall BECAUSE of the changes in liquid water content. The effect of liquid water content on melt rates is very small (e.g., wet snow has lower albedo than dry snow), compared to the opposite effect: that snowmelt rates directly influence LWC.

7. The discussion of the first ROS period (3-5 April), I cannot follow. p. 4149, L29: I'm confused by the word "only" in this sentence. Moreover, the sentence seems to be contradictory to p. 4150, L4-5. And what is exactly meant by the "variation trend"? p. 4150, L5: how was it determined that the LWC distribution was not significantly different? From the text, I thought I had to compare the purple line in Figure 9 with the blue line in Figure 7 (so both during the precipitation event). However, they do seem to differ. p. 4150, L1-3 are also a bit confusing. The fact that one term is larger than the others, does not imply that the meltrate did not decrease, as for this, the sums of energy balance terms are important. Both ROS events in this section are compared to the "typical" profiles and it is concluded that the effects of ROS events on LWC profiles last only for a short period of time. It is indeed an interesting question how ROS-events alter the state of the SNOWPACK, how the snow cover alters runoff during liquid precipitation when there is no snow cover present. However, because the choice of "typical" profiles is not justified in the manuscript, it is impossible to know how well they can serve as a reference. The change in LWC in the days after the rain should be compared to melt created at the surface during these days. In my opinion, this distinction (contribution ROS and contribution snow melt) is not made clear enough.
8. I don't think the relation proposed in Eq. 17 has important predictive power. It is important to realize that the authors try to relate the average LWC of

the snowpack to the prior moving average air temperature. As I pointed out in point 2, this choice is strange, regarding the fact that the authors first investigate correlation coefficients with daily average air temperature and accumulated air temperature. Furthermore, I find it strange that the weights given on p. 4151, L16 seem to put more weight on both the beginning AND the end of the 7 days and less weight in the middle. Is this result statistically significant? I don't see a physical explanation for this, so I think this part should be more extensively discussed (significance of the correlation to arrive at the 7 days, etc). But I think there is a general problem with these types of relationships: these temperature indices are mainly indicative of the energy balance at the surface, and thus, predictive of melt rates. The effect on the average LWC in the snowpack is then dependent on the thickness of the snow cover. A thin snow cover would requires much less melt to achieve a certain value for average LWC than a thick snow cover. So I think it is doubtful if the proposed relation in Eq. 17 would hold in other years with varying snow cover thickness. Unfortunately, the authors don't seem to have a dataset to validate the found relationship.

9. I find it inconsistent to call a change from 0.37% to 0.43% "small and stable" (p. 4148, L12-13) and at the same time provide a thorough explanation for a change from 0.31% to 0.38% (as on p4148, L1). The issue here is really how accurate the LWC measurements are.
10. p 4146, L18-19: "snowpack outflow was observed". There is no description about the measurement technique for the snowpack outflow in the Methods and Data section. How was this determined?

Minor points and corrections

- Please abbreviate liquid water content with LWC throughout the manuscript.

- Abstract: L18: The authors should mean something different, because a ROS event consists of rain and snowfall.
- p. 4138, L20: not proper English.
- p. 4138, L23: "generated by air temperature". Air temperature itself won't generate melt, but sensible and latent heat do (for example).
- p. 4138, L26: "avalanche" -> please write "avalanche formation"
- p. 4140, L3: "it was not used regularly as a snowpack characteristic": I think the cited references show otherwise.
- p. 4140, L18: "Snow in this region". I guess it is meant here: "New snow in this region..."
- p. 4140, L25: "estimated" is not a correct word for this sentence and the sentence is not so clear. I guess the authors meant something like: "Thus, one day in this study was defined from 20:00 LT to 20:00 LT in the next day." However, the authors should think about this sentence too, because it leaves some ambiguity, as it is not so clear if the 20:00LT measurement from this day, or the next day is used.
- p. 4141, L14, Eq. 2: Is this accuracy (like: -1.2142857) of the coefficients in the equation justified? Please only report only the number of digits that can be justified.
- p. 4141, L24: "estimated" is not a correct word for this sentence. Please write: "The daily value of liquid water content was defined as the afternoon value."
- p. 4142, Eq 3. I suggest to call it a melt index, instead of accumulated temperature.

- p. 4142, L19: "Daily sensible (H) was calculated" -> "Daily sensible heat (H) was calculated"
- p. 4143, L15: "the value of z_0 is equal to" -> "the value of z_0 was taken equal to"
- p. 4143, L20: "the mean temperature of THE air layer".
- p. 4144, Eq. 10: There is the use of both P_0 and p_0, but only p_0 is defined. Please be consistent here.
- p. 4146, L4: "The changes in melt rate were consistent with those in liquid water content" I would say the melt rates correlate well to the LWC, but the changes from day-to-day in melt rates sometimes have opposite sign compared to the changes from day-to-day in LWC.
- p. 4146, L10: "increased to 0.54% d-1". I'm a bit puzzled by the units. Please check if you mean 0.54% per day, then it should be "sharply increase by 0.54% per day", or "increase to 0.54%".
- p. 4146, L15: Consider writing "The state of the snowpack is significantly" instead of "Snowpack condition"
- p. 4146, L15 and elsewhere: I prefer to restrict the use of the word significant in scientific literature only for cases were a proper statistical test has been used.
- p. 4146, L20: "whole layer": I guess it is meant "whole snow cover". Usually, the term layer is restricted for a part of the snow cover with the same properties.
- p. 4147, L2: Please check if it is justified regarding the accuracy of the measurements to report up to 1 digit after the comma.
- p. 4147, L5 and elsewhere: what is "astronomical radiation"? Please restrict to the common wording: shortwave, longwave and net radiation.

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- p. 4147, L10: When reporting an increase, please report the rate of increase, or the value it increased from. So write: "... increased by XXX and became 1.08 g/g" or "... increased from XXX and became 1.08 g/g". Furthermore, Eq. 6 defined LHF as a gradient, so it is not a sufficient explanation to only mention the change in specific humidity in the air.
- p. 4147, L27 and elsewhere: Please write "bottom of the snowpack" instead of "snow bottom".
- p. 4147, L27. Maybe it is better to write "(the snow type here was depth hoar)"
- p. 4148, L9. "The drastic variation". It does seem to be a steady decrease, so not really a "variation", but more a "decrease".
- p. 4148, L14-15: how can the accuracy for both melt rates be different (0.0186 vs 0.038) when the method to determine them is more or less similar?
- p. 4148, L15: "... and then changed slightly." Please state with what the melt rates changed (time?).
- p. 4148, L27: "was not less than 0 degC" sounds a bit awkward. I suggest writing: "The snow temperature below 15cm depth remained 0 degC all day".
- p. 4148, L29: Note that figure 5b shows melt rates, not outflow!
- p. 4149, L8: "one event of snow". I would suggest to write: "one event of only snow (referred to as non-ROS)". This prevents ambiguity with the term non-ROS. I think the authors point to this snow-only precipitation event, and not all the days without rain.
- p. 4149, L12, 13: Try to remain quantitative instead of using terms like "far less" and "less".

- p. 4149, L34: Please write "total energy input" or "total energy budget". The term "total energy" is not appropriate as only changes in total energy are regarded.
- p. 4150, L3-4: It is very confusing for readers to use the term "supplied" in combination with a negative energy flux term. "Supplied" would imply an influx of energy, a negative energy flux term an outflux.
- p. 4150, L8: I guess the authors mean "5 April and 6 April" here.
- p. 4151, Eq. 17: The high precision of the coefficients is not justified, I think.
- p. 4152, L5: How was the absence of discharge determined?
- p. 4152, L5-6: "Thus, the snow period was in the mature stage." Please rewrite this sentence, it is not clear now.
- p. 4152, L12: "The variation was also less and was more stable": this is saying the same thing twice.
- p. 4152, L20: "that occupied" is not correct English. Please change the sentence.
- p. 4152, L22-23: "The distribution and variation of every snow-layer": please specify, the distribution and variation of what?
- Figure 3: Please denote the 3 periods in this graph, for example with vertical lines.
- Figure 4: Mention the specific dates in figures. But even better: replace the figures with figures with average LWC profiles for the 3 periods.
- Figure 7, 9, 11: I suggest to maybe take the base of the snowpack as reference. Now, Figure 7 shows for example at -10 first old snow, and after the new snow was added on top, -10 shows new snow.

Interactive comment on The Cryosphere Discuss., 6, 4137, 2012.