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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.

H. Lu et al.

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Dear referee: Thank you very much for your careful review and valuabe suggestions, the revisions were made as follows:

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. Answer: In transitional period, LWC gradually increased but still less than liquid water-holding capacity, and could not produce outflow. The surface snow became to melt and the liquid water gradually penetrated downward. Thus, observing LWC>0 was possible in this period. Although the measurements were conducted every 2 cm depth, the value of LWC was averaged every 10 cm from the snow surface. The snow in the top layer refreezes over night, but the top layer depth may not exceed 10 cm, caused the daily profile didn't have 0% LWC in the top layer in the morning. Another reason for this case is that the measurement error and accuracy. Snow liquid water content measurement ranged from 0% to 10%, with accuracy of 0.3%. In order to check the measured accuracy of the LWC, we measured the LWC in dry snow (hand test dry and snow temperature≤-0.2âĎČ, from Dec 12, 2009 to Jan 3, 2010). The Snow Fork recorded a range from 0% to 0.67%, mean LWC=0.05%, median LWC=0%, standard deviation=0.097%, n=772. 73.6% of the LWC were 0%, 95.3% of the LWC were less than 0.3%. Kattelmann et al (1999) and Techel et al (2011) measured the snow LWC, results showed small liquid water

have been measured in snow with snow temperature below 0âĎČ (LWC<1%). Thus, the LWC measured using Snow Fork was reliable.

Kattelmann, R., and Dozier, J. 1999. Observation of snowpack ripening in the Sierra evada, California, U.S.A. J. Glaciol., 45(151), 409-416.

Techel, T., and Pielmeier, C. 2011. Point observations of liquid water content in wet snow-investigating methodical, spatial and temporal aspects. Cryosphere., 5, 1-14.

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. Answer: due to lake more LWC data in spring 2010. We used the LWC data in spring to analysis the spatial variation of LWC. The LWC datasets measured in spring 2009(from March 11 to March 30 2009) were chosen to analyze the influences of LWC spatial distribution on the accuracy of measurement, The LWC was measured using the same method, the maximum and minimum snow depth was 67.5 cm and 16 cm, respectively. At this year, two profiles of LWC was measured each time, the distance between two profiles was bigger than 0.3 m. The range of LWC was from 0% to 5.12%, the average value was 1.61%. Figure 1a shows LWC distributed near the symmetrical line in different wetness conditions. The LWC data of two profiles were analyzed using the paired-samples T test, which indicated that there was no significant difference between the LWC of two profiles (p=0.054>0.05). Figure 1b shows the average difference of LWC was 0.25%, 71.5% of the differences were not bigger than the measurement accuracy (0.3%), 17.9% of the differences ranged from 0.3% to 0.6%, 10.6% of the differences were bigger than 0.6%. The differences which

were bigger than measurement accuracy mainly distributed in the range of LWC was bigger than 1% (figure 1c). The differences which were bigger than measurement accuracy may be caused by spatial variations of LWC. The observation site was chosen in the meteorological observation field (in spring of 2009 and 2010), with the uniform underlying surface and meteorological condition, so the influence of spatial difference on LWC is very small. When the LWC was bigger than 1%, the average of differences was 0.368%, so the spatial variation of LWC was from 0.068% to 0.368%. When the LWC was less than 1%, the average of differences was 0.121%, so the spatial variation of LWC was from 0 to 0.121%.

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. Answer: These methods are difficult to perform and time-consumig, which makes them impractical for operational in the continuous and long time field observation. In addition, these methods are destructive to the snow sample. Hand test is a convenient method to estimate the snow liquid water content, but it has a strong subjectivity and depends on observer's experience. Remote sensing technology approaches usually have a low temporal and spatial resolution. The Snow Fork we used in the experiment is of dielectric principle. Compared with Denoth and Niang measurement devices, the Snow Fork is a non-destructive method for snow pack.

2. p 4140, L3-5: 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. Answer: These studies did not analyze the variation of LWCïijŇmore measurements were needed to analyze the condition of different snow pack, for example, how rain or

snow event influence on the vertical variation of LWC.

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? Answer: Techel et al's (2011) study showed Snow Fork is an efficient way to record spatial distribution of snow wetness. Li et al (2001) and Jones et al (1983)'s study did not analyze the vertical variation and distribution of LWC in different state of snowpack.

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? Answer: In p 4139, L17-18 and p 4146, L13-14, discribe the LWC of different snow period. In p 4139, L17-18, the snow period were stable period (WVOL<0.1%) and interim period (0.1%<WVOL<0.3%). In p 4146, L13-14, the snow period were snowmelt period (WVOL>0.3%), it was divided into three stages (transitional period: 0.3%≤WvolïijIJ1%, mid-snowmelt period: 1%≤WvolïijIJ2.5%, and late-snowmelt period: Wvol≥2.5%). You may misunderstand that the transitional period (pre-snowmelt period in original paper) was not snowmelt period,actually, transitional period was a part of the snowmelt period in this paper. The transitional period was in the stage that the LWC gradually increased but still less than the liquid water-holding capacity.

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. Answer: LWC comes from snowmelt generated by the energy budget of snowpack in the clear day, and partly comes from rainfall in the rainy day. It depends on snowmelt, rainfall, and discharge. However, different temperature indices could only indicate the energy balance of snow surface to a certain extent,

especially in the rainy day. The air temperature does not contain the information about the amount of precipitation and discharge. So temperature indices could only indicate part of the energy balance, but not indicate the mass balance. Therefore, it caused the liquid water content scattered widely.

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, p4151), 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. Answer: I'm very sorry for making such a mistake and thank you for your advice. Now, I compare correlation coefficients about the two datasets use Spearman's rank correlation coefficient in Table 1. The result showed the correlation coefficient between minimum air temperature and LWC was higher than that between maximum air temperature and LWC. It indicated that the influence of minimum temperature on LWC was more important than the maximum temperature. The minimum air temperature can influence snow refreezing over night and snow melting in the daytime, especially the melt-freeze crusts formed. The melt index does not contain the information about snow refreezing, thus, the correlation coefficient between average air temperature and LWC was higher than that between melt index and LWC. The equation to estimate LWC will be displayed behind.

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 08:00 LT on one day to 08:00 LT on the next day). We should remember that the experimental setup will also sample spatial variability. So between two 08:00 LT-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. Answer: Three clear days (March 5, March 31, and April 21 2010) in different snowmelt periods, also two days before these days were required to be clear, were selected as the typical distribution of snow liquid water. Select typical days by choosing profiles that resemble the averaged profiles may be a good idea, but how to conduct this average thing is a big problem, the snow depth changed with time , especially, in the mid and late snow melt periods, the snow depth decreased drastically, it's hard to choose the same layer to get the average profile. If we did do this average, the profile may have higher or lower LWC at surface, middle or bottom. The new falling snow is another problem, which has LWC almost equal to 0%, if the surface snow was averaged in certain period, in fact, the LWC was lowered.

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"? Answer: I am sorry, I uploaded a wrong graph (figure 4 in the original paper) in the original version,

and the wrong graph may confuse you. I explained more detail about the correct graph in the revised manuscript (figure 5 in the revised paper).

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. Answer: Because of laking more data about this phenomenon, this statement was less exacting and the analyses was incomplete. However, this statement may be a possible for this phenomenon. So the statement was revised as follow in the paper. The snowmelt was not similar with non-ROS (Figure 5b), especially the snowmelt in the night was bigger than that in non-ROS period. . This phenomenon may be indicated that rapid percolation rates contribute to fast stream flow response but very little energy exchange for melt (Mazurkiewicz et al., 2008).. According to the Eq. (17), energy supplied by precipitation can melt 103.99 kg water. However, the total discharged water was only 18.554 kg. Mazurkiewicz, A., Callery, D., and McDonnell, J. Assessing the controls of the snow energy balance and water available for runoff in a rain-on-snow environment. J. Hydrol.,2008, 354: 1-14.

p 4150 (4149), 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. Answer: The typical distribution was the distribution of LWC in the clear day which showed the vertical profile of the LWC, it didn't explain the absolute value of LWC. Before the snowfall (March 27) was a clear day, so you can see an agreement before the snowfall and we have the statement that "the distribution of LWC was in accordance with the typical distribution". "The snowmelt rate decreased during and after the snowfall indicated the changes in liquid water content" instead of the statement that "the snowmelt rate decreased during and after the snowfall because of the changes in liquid water content".

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. Answer: "the variation trend and distribution of liquid water content were different only from the snowfall event" should be "the temporal variation trend and distribution of liquid water content were different from the only snowfall event". The variation trend was the LWC

changing with time, especially the variation of distribution during the ROS period. In p. 4150, L5, we want to state the differences of snow liquid water content and distribution between 5 April and non-ROS period was less than that between only snowfall period and non-ROS period, especially the distribution of LWC was relatively uniform. Due to the different of idiomatic expression between Chinese and English, I wrongly used the word: "significant". In p. 4150, L1-3, we add the total energy during different ROS period. On April 4, the energy supplied by precipitation was 102.82 MJ (the total energy was 106.86 MJ), which was much higher than other heat fluxes. On April 5, the energy budget contributed by precipitation was -3.76 MJ (the total energy was 2.33 MJ). The typical distribution was just used to explain the distribution (vertical profile) of LWC. In order to analyze how ROS-events alter the state of the snowpack, we chosen one or two clear day before and after ROS-event, and compared the state of the snowpck during ROS period with the state before and after ROS period. Because the LWC influenced by energy and mass balance of the snowpck, we cannot compare the absolute value of LWC between ROS period and non-ROS period. We can only compare the LWC distribution of the snowpck during ROS period with the distribution before and after ROS period, to analyze how ROS-events alter the state of the snowpack. In future, we need measure more data about snow temperature, snowmelt water temperature, precipitation intensity, and higher temporal resolution of LWC to distinguish the contribution ROS and contribution snow melt.

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. Answer: We delete the part of using temperature to predict LWC. Because I wrongly used the "standard" Pearson correlation coefficients to analyze the correlation between LWC and average air temperature, the result showed that the correlation coefficients between LWC and average air temperature in the beginning and the end of the 7 days were bigger than in the middle, and passed the significance tests. Thus, the weights given on p. 4151, L16 put more weight on both the beginning and the end of the 7 days and less weight in the middle. According to your suggestion, I think the regression equation of LWC should contain the snow depth and temperature (prior moving average air temperature). Because of the air temperature is a good index to indicate the energy balance in snow surface and the melt water is the only source for liquid water in non-ROS period, so I try to use snow depth and temperature to predict LWC. The regression equation is as follow: $W_{vol}=a*e^{((b*h+c*T_{ma}))}+d$ Where W_{VOL} is the snow liquid water content (%) , T_{ma} is the prior moving average air temperature (°C), h is the snow depth (m), a, b, c, d are constants. The fit has an R2 of 0.82. The accuracy of the regression equation is lower than through snow depth and prior moving average air temperature. There are at least two reasons for this condition. Firstly, the regression equation contains more information about snow process, so the uncertainty is bigger than regression equation which only used a single parameter. Secondly, we do not have enough data to fit these regression equations. Thus, we do not add the regression equation in the revised paper.

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. Answer: In p. 4148, L12-13 and p4148, L1 describe the change of LWC in two different snowmelt periods. In the p4148, L1, described the change of LWC in pre-snowmelt. In the L12-13, described the change of LWC in mid-snowmelt. Because the LWC in mid-snowmelt was bigger than in pre-snowmelt period, and compare with LWC from snow surface to 30 cm depth (p4148, L6-7, LWC changed from 0.36% to 2.74%), we described the change from 0.37% to 0.43% was "small and stable".

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? Answer: In this paper, snowmelt rate was obtained through two methods: snow lysimeter (observed snowmelt rate) and calculated according to the energy balance in the snow surface (calculated snowmelt rate). The observed snowmelt rate was observed through lysimeter. A galvanized iron box of 1 m×1 m×0.05 m was placed before snowfall in the winter. A tube was welded at the bottom of a corner. The snowmelt water was discharged through the tube and was collected by a plastic kettle. To avoid the loss of water result from evaporation and splash, the inlet diameter of the plastic kettle was bigger than the tube diameter no more than 1 cm. The snowmelt water was weighed every 2 h in the daytime, 1-3 times in the night. The accuracy of the electronic balance is 0.001 kg (1 g). If the snow temperature is 0âĎČ, the calculated snowmelt rate can be computed using the following equation (Kuchment et al, 1996): S_c=Q_m/(_(i) L_li)×S×_w where S_c is the calculated rate (kg m-2 h-1), Q_m is the total energy (MJ m-2), _(i) is the ice density (917 kg m-3), _w is the water density (1000 kg m-3), L_li is the heat of fusion (J kg-1), S is the snow lysimeter area (1 m2). We add this description to the revised paper. The snowmelt rate was the observed snowmelt rate in the original version.

Minor points and corrections

Please abbreviate liquid water content with LWC throughout the manuscript. Answer: done

Abstract: L18: The authors should mean something different, because a ROS event consists of rain and snowfall. Answer: I use the “precipitation event” instead of “ROS event”, the ROS event include rainfall, snowfall and sleet.

p. 4138, L20: not proper English. Answer: done

p. 4138, L23: "generated by air temperature". Air temperature itself won’t generate melt, but sensible and latent heat do (for example). Answer: “generated by sensible heat and latent heat” instead of “generated by air temperature”.

p. 4138, L26: "avalanche" -> please write "avalanche formation" Answer: done

p. 4140, L3: "it was not used regularly as a snowpack characteristic": I think the cited references show otherwise. Answer: “It was not used regularly as a snowpack parameter” instead of "it was not used regularly as a snowpack characteristic".

p. 4140, L18: "Snow in this region". I guess it is meant here: "New snow in this region...". Answer: “Snow in this region” is correct.

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. Answer: In this study, one day was defined from 18:00 LT (GMT+6) to 18:00 LT ((GMT+6)) in the next day, 18:00 (GMT+6) LT was regarded as the end of one day (when calculated the temperature and energy balance, 18:00 was classified into the current day).

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. Answer: The equation is provided by instruction of Snow Fork gauge. I check the equation in this paper, all number are correct.

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." Answer: done

p. 4142, Eq 3. I suggest to call it a melt index, instead of accumulated temperature. Answer: Thank you for your suggestion, I have already replaced the accumulated temperature with melt index.

p. 4142, L19: "Daily sensible (H) was calculated" -> "Daily sensible heat (H) was calculated". Answer: done

p. 4143, L15: "the value of z0 is equal to" -> "the value of z0 was taken equal to". Answer: done

p. 4143, L20: "the mean temperature of THE air layer". Answer: done

p. 4144, Eq. 10: There is the use of both P0 and p0, but only p0 is defined. Please be consistent here. Answer: done

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. Answer: After a clear investigation, I found it does like what you said, so I revise the sentence according to your suggestion.

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%". Answer: it’s 0.54% per day, I have also corrected the manuscript

p. 4146, L15: Consider writing "The state of the snowpack is significantly" instead of "Snowpack condition". Answer: done

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. Answer: I have substituted the word “significant” by ”important”.

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. Answer: Your guess is right, I have changed the “whole layer” with "whole snow cover"

p. 4147, L2: Please check if it is justified regarding the accuracy of the measurements to report up to 1 digit after the comma. Answer: done

p. 4147, L5 and elsewhere: what is "astronomical radiation"? Please restrict to the common wording: shortwave, longwave and net radiation. Answer: astronomical radiation is the extraterrestrial solar radiation. I have changed the “astronomical radiation” with “solar radiation”.

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. Answer: I revise the sentence according to your suggestion. Besides analyze the specific humidity in the air, I also analyze the difference between atmosphere vapour pressure and snow surface vapour pressure to explain the change of the latent heat flux.

p. 4147, L27 and elsewhere: Please write "bottom of the snowpack" instead of "snow bottom". Answer: done

p. 4147, L27. Maybe it is better to write "(the snow type here was depth hoar)". Answer: done

p. 4148, L9. "The drastic variation". It does seem to be a steady decrease, so not really a "variation", but more a "decrease". Answer: We reinterpret the variation of the LWC in mid-snowmelt period, the sentence was deleted in the revised paper.

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? Answer: in the section 2.7 "snowmelt rate", we describe the accuracy of observed snowmelt rate. The

accuracy of the observed snowmelt rate is 0.001 kg m-2 h-1.

p. 4148, L15: "... and then changed slightly." Please state with what the melt rates changed (time?). Answer: The minimum and maximum observed snowmelt rates were 0.0186 and 0.038 kg m-2 h-1, respectively, it changed with time slightly.

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". Answer: Thank you for your suggestion, I have already make the modify.

p. 4148, L29: Note that figure 5b shows melt rates, not outflow! Answer: We measured the snowmelt water only one time in night 21 April, so we didn’t estimate the snowmelt in the night. In the revised manuscript, we convert dot graph to curve graph, which can show the variation of melt rates..

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. Answer: the non-ROS referred to as the day does not have precipitation. ROS event contain rainfall, snowfall, sleet.

p. 4149, L12, 13: Try to remain quantitative instead of using terms like "far less" and "less". Answer: I used the quantitative instead of “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. Answer: done

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 outflow. Answer: I have changed the manuscript by “energy budget contributed by precipitation”

p. 4150, L8: I guess the authors mean "5 April and 6 April" here. Answer: sorry, I miswrite "5 May and 6 May", it was revised in the manuscript.

p. 4151, Eq. 17: The high precision of the coefficients is not justified, I think. Answer: I delete the part of using temperature to predict LWC.

p. 4152, L5: How was the absence of discharge determined? Answer: the measurement of snowmelt rate was described in 2.7.

p. 4152, L5-6: "Thus, the snow period was in the mature stage." Please rewrite this sentence, it is not clear now. Answer: It was rewritten as "Thus, the snow period was in the mature stage with the LWC gradually increasing but less than the liquid water-holding capacity."

p. 4152, L12: "The variation was also less and was more stable": this is saying the same thing twice. Answer: It was changed to "The variation was more stable in this depth range".

p. 4152, L20: "that occupied" is not correct English. Please change the sentence. Answer: "the net radiation contributed more than 70% of the total energy" instead of "net radiation that occupied total energy".

p. 4152, L22-23: "The distribution and variation of every snow-layer": please specify, the distribution and variation of what? Answer: the distribution and variation of LWC

Figure 3: Please denote the 3 periods in this graph, for example with vertical lines. Answer: done

Figure 4: Mention the specific dates in figures. But even better: replace the figures with figures with average LWC profiles for the 3 periods. Answer: according to the reply about how to choose the typical distribution. I didn't revise figure 4.

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. Answer: I have modified the graphs as what your suggestion

Please also note the supplement to this comment:

http://www.the-cryosphere-discuss.net/6/C2469/2012/tcd-6-C2469-2012-supplement.pdf

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

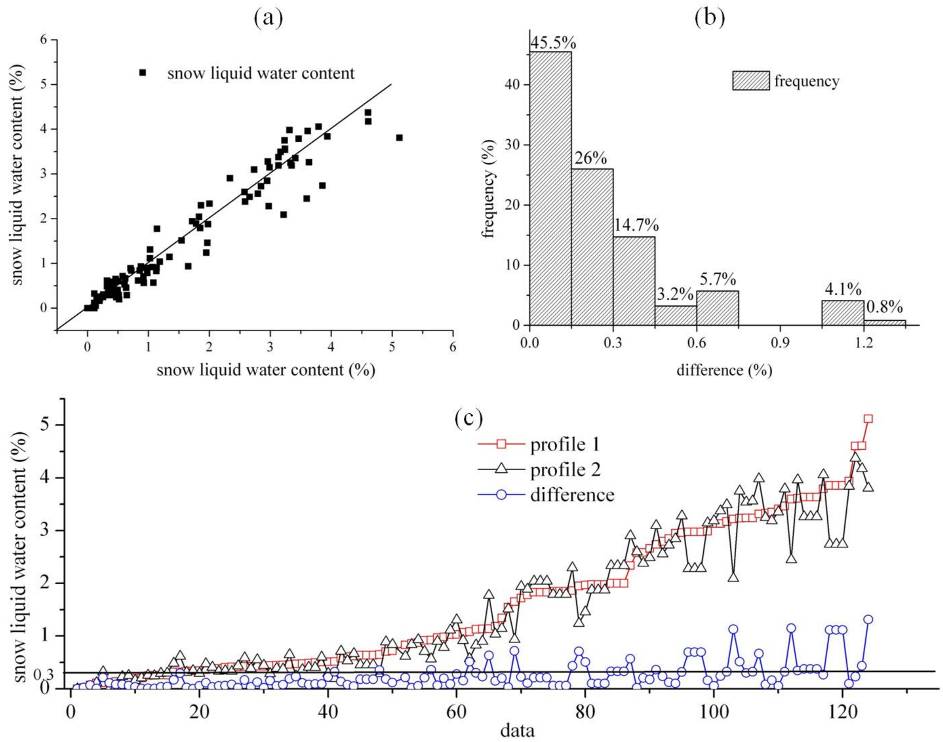

Fig. 1. Fig. 1. Comparison of measured LWC between different profiles in spring 2009

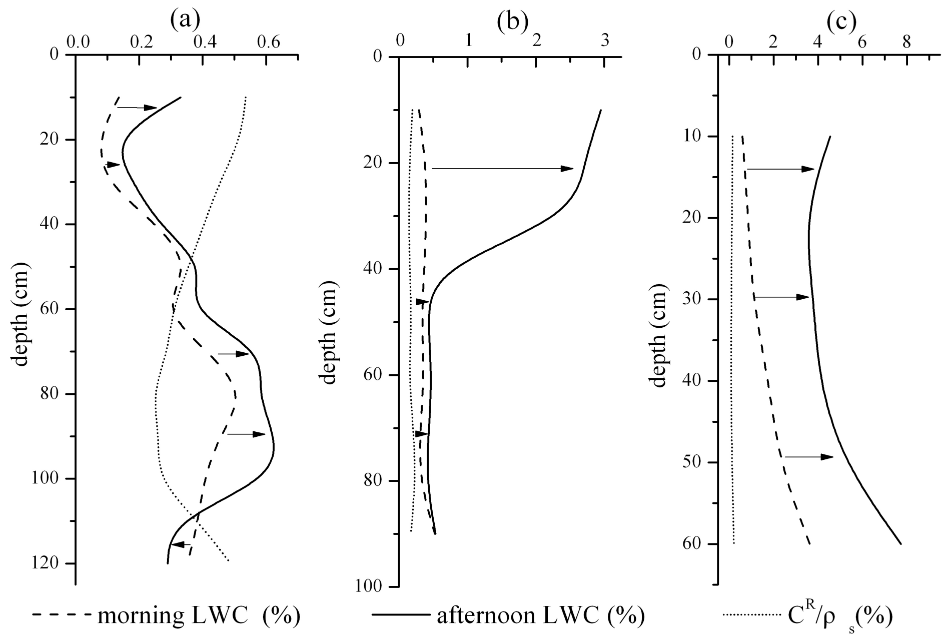

Fig. 2. Fig. 5. Difference between morning and afternoon LWC and liquid water-holding capacity