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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 #2**

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The authors present a data set of liquid water content (LWC) profiles recorded with the Finnish SnowFork over the course of two winter seasons. The recorded profiles are statistically compared to measured air temperature and snowmelt rates or basal discharge (?) during different prevailing meteorological conditions. A special focus is set on so-called rain-and-snow (ROS) events. The main outcome of the manuscript is that LWC is dependent on high air temperature, net radiation and rain-on-snow events – which is quite trivial and known since the early beginning of snow science. In addition, the authors describe how to measure and calculate the energy balance. Temporal evolution of LWC are presented and discussed based on prevailing weather conditions. Statistical models based on air temperature and LWC profiles are presented.

At present state the manuscript does not fulfil the requirements and standards for being published in The Cryosphere. Methods, data, results and discussion are erroneous and do not substantially improve our understanding on the field of melting snow covers. Many assumptions are wrong and the interpretations of the results leads to the conclusion that the authors misinterpret known and general valid physics in wet snow science (see major points). The manuscript does not fulfil the requirement of good scientific practice. Analysis, data and interpretation are not clear and comprehensible. Language is not concise which results in a text which is hardly comprehensible. The manuscript lacks a detailed and correct presentation of the used methods. In fact, I am quite surprised that the manuscript passed the relatively demanding editor process of TCD.

Major points

(1) The paper relies heavily on measurements performed with the Finnish SnowFork. Although all results are based on these measurements no error analysis was performed. Based on some results and Figures, it is obvious that the error is far from negligible. The authors mention in their Introduction the work presented by (Techel and Pielmeier, 2011) where the accuracy of the SnowFork was discussed and compared to the Denoth-capacity probe (Denoth, 1994). In that publication the authors measured dry snow using the SnowFork and obtained values ranging from 0.3% to 1.8% by vol. for dry snow. The presented Figures suggest that the authors do not know at which resolution LWC values are obtained with the presented method. The accuracy of the method is probably in the range of $\pm$1% by vol. and Figure 4a shows just dry snow conditions. The differences in the values just reflect differences in density showing snow layers with low density on top of dense snow. The basal layers which consist of depth hoar are again less dense. So I guess that the pre-melt season is simply the period where dry snow conditions prevail. A temperature profile could verify this. Figure 4b shows in a very nice the advancing of the wetting which is known to have shock-wave like pattern (Colbeck, 1972). The explanation (p. 4148, lines 6-14) for the shape of



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liquid water distribution makes no sense to me. Low air temperature is not connected to this process. The authors claim to show spatial distributions of liquid water content, but in fact they just present the time evolution in 1-D. In some sections the explanation reveal that the authors did not understand the present knowledge on water movement in snow. In section 2.5 the authors try to explain some sort of tipping-bucket model for water transport or in other words the water-holding capacity. The entire section is incomprehensible as important variables of Eq. (14) are not explained. Eq. (15) does not explain the fraction of ice, but rather some sort of porosity. What do you mean with partial density of ice on p. 4145, line 10?

(2) Modelling and measurements of the energy balance are presented, however, why these calculations were done is nowhere explained. I guess that snowmelt rates were calculated based on energy balance in combination with snow density, but this is not mentioned within the text. In some parts of the text the authors talk about snow melt in other I assume that the authors rather want to say basal outflow or discharge. The energy balance and its component are expressed as energy with the unit MJm-2 but then described as fluxes (Wm-2), e.g. sensible heat flux (p. 4142, line 15). It is a bit puzzling why the recording of air temperature, which is fairly simple, is explained in detail while other components necessary to accomplish energy balance calculations (e.g. snow surface temperature) are completely neglected. Where does the information of the snow surface temperature in Eq. 5 (p. 4142, line 21) come from? Did the authors correct z_0 for varying snow heights?

(3) In the last section of the Results and Discussion part, the authors present regressions of snow height with liquid water content. Could you please provide any explanation on the physical validity of that assumption?

(4) Throughout the paper, there are many grammatical errors and misused words. I am well aware that it can be difficult for authors whose first language is not English to prepare a paper to be published in an international journal. However, the poor language makes the paper hard to follow and leaves the reader confused. In addition, structure is



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missing in every section. Parts of the methods are presented at the end of the Results section, the manuscript stops with a short discussion and a proper Conclusion is not provided. It is hard to understand what kind of data were used for which analyses. A thorough discussion of the results is missing.

Minor points

Units are not consistent throughout the manuscript. Densities are sometimes expressed as gcm-3 and in other places as kgm-3. Please be consistent.

Equations have missing explanation

Recheck the References, there are many typos and inconsistencies with the text.

Please use the terminology given by the International Classification of Seasonal Snow and Ice on Ground (Fierz et al., 2009). Do not use “pre-snowmelt season”. Rather use the terminology given in (Colbeck, 1972) and (Jordan et al., 2008).

P. 4139, lines 3-5: I want to see a reference

P. 4139, line 14: Change to Techel et al., 2011

P. 4139, line 17: Do not use “stable” in this context. There is now mechanical or hydraulic stability within the context.

P.4140, line 6: You do not present spatial changes, all Figures are in 1-D.

P.4140, line 18: What do you mean with “dry-cold type”?

P.4140, lines 21-22: What kind of density? Snow? What do you mean with other factors?

P.4140, lines 25-26: Please specify the time-zone. In Figure 6, 8 and 10 your midday radiation peak seems quite strange.

P.4141, line1: Why did you dig out a entire column, what was your measurement setup, how did you insert the SnowFork? Any side effects?



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P.4141, line 20: How exactly did you calculate the afternoon value for LWC?

P.4142: I suggest rewriting lines 1-9.

Eq (12) and Eq (14) have the same variable, but obviously describe something different.

Colbeck, S.C., 1972. A theory of water percolation in snow. Journal of Glaciology, 11(63): 369-385. Denoth, A., 1994. An electronic device for long-term snow wetness recording. Ann. Glaciol., 19: 104-106. Fierz, C., Armstrong, R.L., Durand , Y., Etchevers, P., Greene, E., McClung, D.M., Nishimura, K., Satyawali, P.K. and Sokratov, S.A., 2009. The International Classification for Seasonal Snow on the Ground. HP-VII Technical Documents in Hydrology, 83. UNESCO-IHP, Paris, France, 90 pp. Jordan, R.E., Albert, M.R. and Brun, E., 2008. Physical processes within the snow cover and their parameterization. In: R.L. Armstrong and E. Brun (Editors), Snow and Climate: Physical Processes, Surface Energy Exchange and Modeling. Cambridge University Press, Cambridge, U.K., pp. 12-69. Techel, F. and Pielmeier, C., 2011. Point observations of liquid water content in wet snow-investigating methodical, spatial and temporal aspects. The Cryosphere, 5(2): 405-418.

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