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Interactive comment on "Solving Richards Equation for snow improves snowpack meltwater runoff estimations" *by* N. Wever et al.

N. Wever et al.

nander.wever@slf.ch

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Reply to review #2

Reply to main comments: We thank the second reviewer for his many comments and suggestions that helped to improve the manuscript. His main concerns are:

- "However, the authors do not do that well at providing a sound basis for many of their arguments about the comparisons of the models to the acquired field data. Too often they 'brush off' the differences between measured and modelled results as being due to measurement error without even documenting clearly that measurement errors were occurring. Is the reader to assume that when

the comparison between measured and modelled results are good that the measurement errors are insignificant, while when the differences are large then the measurement errors are also large? That seems to be the case in their presentation."

Reply: In most studies that discuss lysimeter measurements for snow (e.g., Kattelmann (2000) *and Morin et al.* (2012)*) it is noted that the interpretation of lysimeter data for measuring snow cover runoff is difficult and we think it is important to remain sceptical about the measurements, even when the exact source of the measurement problems cannot be determined. In fact, we are only sceptical about the measured runoff in three of the fourteen years, for clear reasons, as described in L17-25, p2387 and L17-19, p2388. All the other measurements are considered as-is. In almost all analyses that we perform, we include all years, even the three ones which seem to have unrepresentative runoff. Only for Figures 6 and 7, we omitted them, as these years showed up as clear outliers in the graph.*
As we explain in our reply to the review by Samuel Morin, we now also analyze lysimeter measurements for Col de Porte in France, together with snow cover simulations for this site. We think that now we are taking another measurement site into consideration, with basically the same model and measurement setup, we can make the results much more robust, as we do not rely anymore on a single lysimeter for validation. We will reformulate parts that were commented by reviewer 2 and provide more robust argumentation where possible and necessary. This will be pointed out below, in the reply to the comments from the marked up manuscript that was sent along with the review.

- "The characteristics of the models are not that well presented. It would be better if some comparisons between model results were made using some synthetic conditions rather than trying to model field conditions. That way one could try to explain the differences between model results based on well-founded physical principles (and approximations) rather than based on shortcomings of measurements. This might make the manuscript longer, but either that or the authors need to publish a separate manuscript where such a comparison is made."

- "The authors focus on the water flow and phase change part of the models, but they do not explain anything about the energy balance part. How is the heat transport handled, and how is the radiation balance handled?"

- "The title of the paper gives the reader the idea that using Richards equation in the modeling does lead to improvements in the comparisons between the model and the measurements. However, the reasons for this apparent improvement are not articulated in any convincing way. As mentioned above, a separate analysis comparing model features on synthetic conditions would help to clarify why there are differences in model response."

Reply: The reviewer points to a lack of model description throughout the manuscript. Please note that the SNOWPACK model has been published in a complete form in three papers (Bartelt and Lehning, 2002; Lehning et al., 2002a,b). In this manuscript, we only discuss the water balance schemes while these are under investigation. Furthermore, we provide details about how the domain that is solved by the solver for Richards equation is adapted based on snowfall events or snow melt.
Although the idea of the reviewer to use synthetic snow cover conditions is very interesting, we think there are actually more studies published with synthetic conditions or idealized snowpacks, than studies concerning field conditions. There are actually many studies regarding the use of Richards equation, or approximations of Richards

equation, for snow. They are listed in the Introduction. Some of them do studies on synthesized snow pack, for example: Illangasekare et al. (1990); Daanen and Nieber (2009). Yamaguchi et al. (2010) has shown in laboratory experiments, that the water retention curve in snow can be described by the van Genuchten model. However, we did not find any study comparing modelled snowpack runoff with lysimeter measurements for complete multiple snow seasons and multiple years.

- "The authors seem to not account for the model, SHAW, which is a fairly complete model that accounts for soil freezing, snow water balance, etc."

Reply: as far as described in available documentation, the SHAW model uses a bucket type approach for modelling liquid water flow in snow, combined with attenuation and lagging factors. In the manuscript we focus on the physical based snowpack model SNOWPACK for which the bucket and the NIED water balance schemes have already been published. As a recent addition, we implemented this Richards equation solver, which seem to improve the snowpack runoff modelling.

Comments in marked up manuscript:

- *We thank the reviewer for pointing out several typos and grammatical errors, which will be corrected in the revised manuscript.*
- "you mean in terms of prediction. Certainly the actual meltwater timing does not constitute bad timing."
  Thanks for pointing out this wrongly formulated sentence. We will revise the sentence as: "This appears to be mainly due to bad timing of meltwater release during the day in the model."

- "what about the SHAW model by Gerald Flerschinger et al. They conducting many studies on modeling of frozen soil and overlying snow for entire seasons. These were done for real field conditions with monitoring from the field including meteorological data."
  Reply: the SHAW model uses a bucket type approach for modelling liquid water flow in snow, where additional attenuation and lagging factors are used when routing the water through the snowpack. With this sentence in the manuscript, we would like to stress the fact that we now solve Richards equation for both the soil and snow, considering it as a continuous column. We will improve the formulation in the revised manuscript.

- "The effect shown by Colbeck was the kinematic wave model as opposed to the RE by neglecting capillary effects."
  Reply: For this reason, we wrote in L13 on p.2376 that "Several snowpack models that describe liquid water transport on the basis *of RE have been developed." We formulated it this way, because we are aware of the (Colbeck, 1972) study, where capillary effects are neglected. We will revise this part to prevent the apparent confusion.*

- "these also coupled heat along with the flow. The equations in some cases were not just Richards equation, but also included other terms that are not in the Richards equation (the ice term of the water balance for instance)."
  Reply: Basically SNOWPACK solves heat and water balance equations uncoupled, as described in L6-18, p2384. We argued that for the 15 minutes time steps we use in the model, it is a good approximation and it is not necessary to solve the equations coupled by using the source/sink term in Richards equation.

- "it is actually drainage at the base of the snowpack isn't it? Runoff implies to me something to do with water flowing from a larger area.)"
  "it is not really the runoff since it is just what is draining downward from above in

the snowpack. Runoff would imply that it is leaving the area, which would mean that it would have to not infiltrate into the underlying soil.)"
  Reply: please note that many papers about snowpack modelling or lysimeter measurements in snow covers call the meltwater that leaves the snowpack "runoff", for example: Brun et al. (1989); Kattelmann (2000); Bartelt and Lehning (2002); Morin et al. (2012). So for consistency, we prefer the use of the term snowpack runoff. We will add a better clarification of this possibly confusing definition.

- "the lysimeter is not circumvented, but extra water entering the lysimeter laterally is circumvented."
  Reply: we are sorry for the confusion, but we should have used the word "enclosed" here, instead of circumvented.

- "if the lysimeter is expected to catch that lateral water, then isn't this going to give you measurements that exceed the drainage that you expect to model with a 1-D (vertical flow) model?"
  Reply: this is exactly the expected effect. We will amend the manuscript at this point.

- "what about the freezing of the soil. Were you modeling that too? Nothing is mentioned about it here, but is is a very important process. If the 0 C point is at the lower boundary of the soil, then the temperature is probably subzero at the soil surface, which would mean some soil freezing."
  Reply: the soil is kept at a minimum of 0 degC in this study and soil freezing is switched off. For thick snow covers, that are well insulating, the observed effect is that geothermic heat will produce melting or close to melting conditions at the base of the snowpack. Most of the biweekly profiles at Weissfluhjoch show bottom temperatures of 0 degC. Only in the beginning of some of the seasons, with shallow snow covers, the bottom of the snowpack can become sub-zero at

the ground. However, during the season, before the start of the melt season, geothermic heat will heat the snowpack to 0 degC. The reason we didn't use soil freezing in this study is that first of all, we currently have no validation measurements of soil temperatures at Weissfluhjoch. If we don't accurately describe the soil layers, we easily over- or underestimate the thermal state of the soil, leading to a faulty estimation of the bottom flux into the snowpack. Using a 0 degC lower boundary has been found to most closely resemble observed profiles. Although we will not focus on this comparison in the revised manuscript, we will motivate our choice to prohibit soil freezing in our model.

- "This issue of sublimation vs. evaporation is not quite clear."
  Reply: The surface energy balance provides a latent heat flux in terms of W/m^2. These sentences describe that the latent heat is first used for evaporation, and afterwards remaining energy is used for sublimation. For the partitioning in deposition/condensation, this is done based on the presence of liquid water in the top snow layer. If there is liquid water in the top layer, condensation will occur, else deposition will occur. It is an important issue how to partition latent heat, as the differences in latent heat associated with the phase transition from solid to gas (or vice versa) is larger than from liquid to gas (or vice versa), causing differences in actual mass exchange between the snow cover and the atmosphere.

- "perhaps give the reader an idea of the magnitude of this maximum evaporative flux."
  Reply: The maximum evaporative flux was determined by calculating the flux according to Darcy's law between the upper layer and an imaginary layer just outside the model domain, with a pressure head equal to 0.1 times the small pressure head used for initializing dry snow layers, as described in Appendix A3, p. 2397. We will explain this procedure in more detail in the revised manuscript.

- "It is not clear how temperature distribution is modeled. In some of the references

you cite the models were coupled flow and heat transport models. You have not said anything about that for your model."
  "You do not mention how metamorphism is handled in your model or the other models you used. The process is very important with regard to grain size and liquid water retention in the snowpack."
  "I do not see the energy balance (temperature) part explained or laid out in the paper."
  Reply: the SNOWPACK model has been completely published before, where the treatment of the heat flow, energy balance and metamorphism are discussed. References are provided in the manuscript and we think it is better to point readers to the available literature, instead of repeating the information in this paper again. The water transport models are discussed in detail, as the module to solve Richards equation has not been published before and is a new extension to the model. We will improve the revised manuscript to make it clearer to readers were to find the details about the heat flow, energy balance and metamorphism.

- "but since you have three models here, which model did you use to construct the series?"
  Reply: We used the bucket model to construct these series, although differences are rather small. We will add this to the revised manuscript.

- "water balance schemes" vs "water transport schemes"
  Reply: We will change the terminology in the revised manuscript.

- "But how different is the LWC distribution for the three models?"
  "you did not show anything in the paper about the resulting LWC distributions."
  Reply: The fact that the simulations with Richards Equation show an arrival of melt water earlier in the melt season than with Bucket simulations, implies that LWC distribution are different. With the Bucket scheme, melt water stays in the top layers longer, whereas in simulations with Richards Equation, liquid water

gets distributed more evenly in the domain. We will discuss this more thoroughly in the revised manuscript.

- "The small difference in drainage does not mean that there is small difference in snow water equivalent. there are other fates for the snow; evaporation and sublimation. So this could make for larger differences in snow water equivalent, unless you of course can state that the snow water equivalents are nearly the same based on calculated values (you did not give this)."
  Reply: We will explicitly state in the revised manuscript that the precipitation input for all simulations is the same, so differences in cumulative runoff sum between the simulations arises solely from differences in sublimation (deposition) and/or evaporation (condensation). However, as can be seen in Figure 1, these differences are rather small.

- "mass gain will only occur with condensation or the reverse of sublimation. So you need to mention something like that."
  Reply: We will reformulate the sentence as follows: "Because of the difference in latent heat associated with sublimation or evaporation, mass gain or loss will be smaller in case of phase changes from solid to gas (sublimation) or the reverse."

- "lots of speculation about the differences. Can you narrow it down a bit?"
  Reply: Unfortunately, we cannot really narrow down the speculation about why the lysimeter registers generally more runoff than the model.

- "do you know that the lysimeter malfunctioned, or are you saying that this is a possibility?"
  Reply: There are two years in which the runoff from the lysimeter is either too large or too small compared to what can be expected based on the maximum snow height (assuming reasonable ranges of snow density). Maybe it is not correct to state that it is malfunctioning of the lysimeter, because the error source can also be an unrepresentative state of the snowpack above the lysimeter. Maybe

several melt paths formed that transported meltwater either in or out of the area above the lysimeter, for example over melt-freeze crusts or ice lenses inside the snowpack. So we will revise the text that it may be malfunctioning of the lysimeter, or an effect of some not-represented snowpack feature.

- "why was water held too much? It is the parameters used for the water retention. Why not use equivalent water retention functions between the NIED model and the RE model so that you can compare the results on a more sound physical basis?"
  Reply: We did use the same water retention curve as in the NIED scheme, as described in L14-15, p2378 for Richards equation and L6-7, p.2381 for the NIED scheme.

- "lower agreement with each other?"
  Reply: It was meant "lower agreement with measured runoff". We will revise the text to make this clear.

- "a lot of speculation here. it does seem that it is something other than the hydraulics that determines the differences between the models. Saying that it has to do with the energy balance though is difficult to assess since you do not give any details on the energy balance for your model."
  Reply: As we pointed out earlier, the energy balance part is not discussed as it is already published (Lehning et al., 2002b). However, we think it is important to point readers to the fact that no matter which water transport scheme is taken, there are still some years where all water balance schemes show a larger discrepancy between modelled and measured runoff than in other years.

- "what is the bias between modelled and measured? Not clear."
  Reply: As shown in Figure 1, the measured runoff is almost never equal to the modelled runoff. This implies that if Nash-Sutcliffe coefficients are calculated, this

bias influences the results. A larger bias between measured and modelled runoff causes smaller Nash-Sutcliffe coefficients.

- "is it a fence, or a lip/wall on the top of the lysimeter?"
  Reply: thanks for suggesting different terminology. We will use the term "wall", as also done in Morin et al. (2012).

- "can you come up with any estimate at all of the amount of lateral flow. This is a pretty big issue because you are modeling assuming the drainage comes only from the area vertically above the lysimeter."
  Reply: One could interpret the difference in cumulative sum of measured runoff and modelled runoff as the amount of lateral flow, as described in L10-11 on p.2387. Although there are several other factors why this difference cannot be solely attributed to lateral flow as described on the same page.

- "Is this absolutely what causes it, or are there are possible causes? want to try to avoid being too speculative."
  Reply: NSE coefficients are sensitive to wrong timing of modelled runoff compared to measured runoff. So if the daily sums of runoff have much higher NSE coefficients than hourly sums, this is mainly caused by wrong timing in the model.

- "I think that the publications by Marsh show that preferential flow in snow is more the rule than it is the exception, so I would think that it is not correct to say it is 'small'."
  Reply: we agree with the reviewer that this sentence is not correct in its present form. We wanted to reflect on the fact that just based on our study, it may be concluded that the amount of water transported in preferential flow is rather small. However, given all the evidence published in literature, this is very likely not true. In the revised manuscript, we will reorder the arguments, starting with the most likely reasons, which is our choice of residual water content θ_r, and the experiments that partly include preferential flow in estimating parameters. The pos-

sibility that the amounts of water involved in preferential flow are small will be mentioned third, and it will be made clear that this is not likely given the evidence in literature.

- "what experiments do you mean? You did not mention anything about experiments to derive the parameters."
  Reply: The used water retention curves (both Daanen and Nieber (2009); Yamaguchi et al. (2010)) are based on laboratory experiments. This is only implicitly mentioned in the manuscript in L15-17, p2375 and L17-18, p.2378, but we will revise the text to explicitly mention this.

- "You say that measurements problems are the reason, but it might be something in the driving processes that are outside the realm of your parameterization of the models. Unless you can point to definite measurement problems then you should not just speculate it."
  Reply: The problem was that when these three years were added to Figure 6, they clearly showed up as point clouds. I think the reasons mentioned in L17-25, p2387 and L17-19, p2388 are rather convincing and show that the measurements are at least unrepresentative for the actual snow cover runoff, if not indicative of malfunctioning of the device.

- "why is the runoff, actually 'drainage' is preferred, given as a negative number?"
  Reply: As we described in L14-16, p2383, we treat snowpack runoff from the snow pack mass balance perspective.

- "what is the reason though?"
  Reply: the reason is that the bucket model does not take into account travel times through the snow pack. We did not mention this in the original manuscript.

- "why then would the models have similar 24-hour results (Figure 6) if this is the case?"

Reply: We are not sure, but from Figure 4, it is clearly seen that the water is retained in the snowpack too long in the Bucket scheme. When peaks are better estimated, it may still show up as a higher r^2 value.

- "I did not see anything in your paper that would indicate that this additional study is needed. I do not disagree that it is needed, but your results never point this out in the presentation."
Reply: We raised this point, because the two van Genuchten parameterization, both based on their own data sets, provide significantly different runoff in the snowpack model. The Yamaguchi et al. (2010) parameterization provides better agreement with the observed runoff. However, the Daanen and Nieber (2009) water retention curve was also derived from experiments, so that is why we concluded that further investigation in this issue is needed. We will amend the manuscript on this point.

- "the layers will form in the simulations or they will form in the actual snow pack?"
Reply: we agree that this is an inadequate formulation. In fact, many layers form in both the actual snow pack and the simulations. We will reformulate this sentence in the revised manuscript.

- "I did not see anything in your manuscript about metamorphism being considered."
Reply: Please see p2384, L18. Details of the metamorphism are not discussed here, as they have already been published. We agree that the information about the SNOWPACK model has become rather scattered over the manuscript. In the revised version, we will add a section that shortly describes the processes that are included in the SNOWPACK model, with appropriate references.

- "While you explain in the text what you mean by the dates given, you should probably put a footnote on the table that also explains this since the table should

be able to stand by itself."
Reply: We will revise the table as suggested.

- "It is not clear how the LWC production, which is a modelled amount, is calculated differently from the four models listed herre."
Reply: We omitted to mention that we derive the LWC production from the Bucket simulations. First of all, this scheme is in the original SNOWPACK version and the model is tested most with this scheme. Furthermore, differences between simulations are rather small and the purpose of showing the LWC production is only to serve as a reference of the relation between LWC production and runoff.

- "maybe use bars instead of lines"
Reply: Also reviewer 1 commented that lines are not an appropriate plotting method, because the years are not dependent on each other. We will find another plotting method for the revised manuscript. However, for Figure 5, it can be argued that NSE coefficients for various time scales are related to each other, and lines are appropriate.

- "balance also, the coefficients I assume are for each model versus the measured."
Reply: we will change the term to water balance schemes. The NSE coefficients are indeed model versus measured. We will revise the caption in the manuscript.

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