

Dear Prof. Glade,

Thanks you for your letter and for the very helpful reviewers' comments concerning our manuscript. The comments were valuable and helpful for revising and improving our paper. We have studied the comments carefully and have thoroughly revised the manuscript. We have added a number of paragraphs in the Introduction and the Discussion and significantly modified the Methods and the Results Sections. We have spent a considerable effort in calculating the Nash-Sutcliffe model efficiency for all described model runs of the original simple and modified tank models in Section 5 and by this we could demonstrate that our modified tank model has a much higher explanatory power than the standard tank model.

We have carefully edited the revised paper including figures and made it more concise in two complete revisions. As this applied to the complete paper we have not marked all language changes and grammar changes. However, the main changes made in response to the reviewers' comments, as listed here, are marked in red in the revised paper.

Thanks for all efforts

Wen and Michael

Review1:

General commons: the main drawback in this manuscript is lack of information about frozen soils and reasons why authors not include it to the research, as well critical evaluation of using methods in the paragraph of discussion.

(1) I would advise the authors of this paper to describe climate condition in the region of investigation and rainfall patterns for the observed period that influence on the landslide initiation.

>The climate condition and rainfall description are added in section 3.1.

>Implemented (Line 2-12, Page 7): "The Aggenalm is exposed to a subcontinental climate with a pronounced summer precipitation maximum and an annually changing share of 15–40 % of the mean annual precipitation that fall as snow. Nearby meteo-stations such at the Brännsteinhaus, the Sudelfeld (Polizeiheim) and the Tatzelwurm indicate mean annual precipitation of 1594, 1523 and 1660 mm/a at similar elevations (Table 1)."

>New Table 1 Implemented:

Table 1 Mean monthly precipitation (1931–1960 and 1961–1990) for the Brännsteinhaus, the Sudelfeld (Polizeiheim), and Tatzelwurm meteorological stations (data from Germany's National Meteorological Service DWD).

Precipitation	Oct.	Nov.	Dec.	Jan.	Feb.	Mar.	Winter
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Brünnsteinhaus	[mm]	89.6	109.2	115.7	102.9	100.5	103.2	621.1
(1345 m)	[%]	5.6	6.9	7.3	6.5	6.3	6.5	39.0
Sudelfeld (Polizeiheim)	[mm]	84.7	98.3	113.1	82.7	82.2	95.5	556.5
(1070 m)	[%]	5.6	6.5	7.4	5.4	5.4	6.3	36.5
Tatzelwurm	[mm]	109.9	106.1	99.1	123.2	118.7	110.9	667.9
(795 m)	[%]	6.6	6.4	6.0	7.4	7.1	6.7	40.2
Precipitation		Apr.	May.	June.	July.	Aug.	Sep.	Summer
Brünnsteinhaus	[mm]	121.9	138.4	194.3	208.6	193.0	116.8	973.0
(1345 m)	[%]	7.6	8.7	12.2	13.1	12.1	7.3	61.0
Sudelfeld (Polizeiheim)	[mm]	103.7	152.3	204.2	195.1	199.2	112.0	966.5
(1070 m)	[%]	6.8	10.0	13.4	12.8	13.1	7.4	63.5
Tatzelwurm	[mm]	115.8	149.4	194.8	224.9	185.6	121.9	992.4
(795 m)	[%]	7.0	9.0	11.7	13.5	11.2	7.3	59.8

(2) In the Figure 1b the font of text is not clear enough. I suggest using different font.

> **Implemented (Line 1-2, Page 5):** *We make the font of text more clear by adjusting the size of Figure 1.*

(3) According to the monitored data from winter season the presence of frozen soil greatly affects the amount of runoff produced from snowmelt. From the site description one is unable to find information about frozen soils. If there is significant relationship between frozen soils, infiltration and PWP then you include effect of frozen soils to the tank models.

> *We added this explanation at the end of Section 3.4.*

>**Implemented (Line 4-14, Page 12 and Line 1-5, Page 13):** *“We ignore surface runoff flow resulting from snowmelt and heavy rainfall as (1) the slope angle is less than 15°, (2) the cumulative snowpack is no more than 70 cm during monitoring days and (3) the infiltration rate of slope in Quaternary deposits and on carbonates is relatively high. We ignore freezing effects on infiltration as (1) ground sealing by freezing is presumably not an issue since the bottom temperature of snow (BTS) is next to 0°C underlain by a warmer subsoil in addition to high permeable subsoil. (2) Snow accumulation during winters and winter rainfall precipitation prevent effective cooling of ground.”*

(4) Line 9: you did not explain why your data of PWP, temperature and humidity averaged over a 24-hour period, why you use this time frame? - Line 15: how you performed validation of tank model?

>We explain the choice of 24-hour periods in Section 3.2

>**Implemented (Line 7-8, Page 8):** “Since the whole monitoring period lasts for almost 3 years and time lags are in the range of days, days were considered to be the most robust and appropriate time unit.”

>We explain the validation of tank model in Section 3.2

>**Implemented (Line 12-14, Page 8):** “To validate the parametrized model, 55 days of rainfall (July 2009 to August 2009) and 44 days of snowmelt (March 2009 to April 2009) are used to compare model-calculated pore water pressure with real pore water pressure readings.”

(5) Authors have produced an interesting dataset but more needs to be done in the “Highlights of the modified model” before publication where major drawbacks and critical overview of the using methods must be included.

>The major drawbacks and critical overview of using methods have been added in section 5.4

>**Implemented (Line 1-7, Page 21):** “The naturally inevitable drawback for any “probabilistic model” is that it is physically not explicit. The presented model would need further adjustments for permafrost regions, with heavily frozen soils, for very steep slopes, with significant surface runoff and for very heterogeneous slopes, with complex fractured rock masses. However, it seems well suited for large mountain landslides on moderately inclined slopes in alpine conditions with significant snow accumulations.”

(6) Linguistic alterations: some paragraphs have to be rewritten (Discussions and Conclusions)

> We then have spend considerable effort in editing the revised paper and in making it even more concise. As this applied to the complete paper we have not marked all language changes and grammar changes; however, the main changes made in response to the reviewers’ comments, as listed here, are marked in red in the revised paper.

Review2:

(1) The approach is not novel. Tank models have been used widely in landslide research as cited by the authors. Often, their use is justified by the absence of more detailed knowledge about the hydro-mechanical processes and driven by direct practical concerns. Other model approaches have been used and often include better a conceptualization of the hydrological processes and the mechanical response. The hydrological approaches are lumped here under a non-descript mention

in lines 12-16 of page 3. A fairer evaluation of the consensus and state-of-the-art on the modeling of the hydrological response of rainfall-driven landslide is needed. The qualification that “many of these parameters cannot be measured easily” is too little to discard this evidence completely and the underlying problems are not given sufficient thought in the formulation of the research objective of the manuscript. The tank model may be used to describe the hydrology of the Aggenalm landslide but with what purpose and what the required accuracy are is not specified; therefore, the choice to use this type of model is insufficiently justified.

>We have taken this concern very serious and we have implemented a complete new section to refer our model to the previous state of the art and to demonstrate what is novel about our equivalent infiltration method including snowmelt and infiltration time lags.

>Implemented (Line 20-25, Page 4): *“The innovation of our approach is to calculate equivalent infiltration before it enters the tank. The equivalent infiltration deals with the infiltration time lag including snow accumulation and snowmelt in deep-seated landslides based on a simple tank model structure. We hypothesize that, compared to a simple tank model, our modified model has a higher accuracy and physical meaning by controlling equivalent infiltration including snow accumulation and snowmelt; compared to multi-tank model our modified model is more robust and reliable.”*

>In addition, we implemented a new section in the Introduction to thoroughly describe the state-of-the-art on the modeling of the hydrological response.

>Implemented (Line 20-32, Page 3): *“Traditional deterministic models have advantages due to their explicit physical and mechanical approaches, but they require accurate knowledge, testing and monitoring of soil physical parameters which are often not available with sufficient accuracy. For example, the widely used Fredlund and Xing method needs soil suction tests under variable moisture content which is difficult to achieve for complex landslides with multiple reworked materials. (2) Empirical-statistical models employ optimization or fitting parameters in their model structure. Tank and other models need historical monitoring data to train parameters (Faris and Fathani, 2013; Abebe et al. 2010). Such probabilistic models, because of their simple conceptualized structure, do rely to a smaller degree on explicit physical and mechanical approaches. However, they can avoid the problems induced by the uncertainty of material parameterisation and its spatial arrangement in the landslide mass. They can, therefore, be applied to a wide range of different landslide settings and we estimate that for more than 90% of all landslides no explicit parameters on soil suction etc. are available.”*

(2) The fact that a simple model is used is contradictory with the wish to study deep seated, complicated landslides. In principle, adding a time lag is not different from adding a multi-tank model (Eq. 4) like a Nash cascade. This limitation is severe as the addition of these model components is done without an a priori conceptualization of the pertinent hydrological processes or subject to a rigorous assessment of the added parameterization costs and uncertainty. No attempt is made to quantify the parameters of Eq. 7 in terms of processes (e.g., evapotranspiration,

interception and groundwater recharge). This sits ill-at-ease with the fact that for example snow melt itself is made land cover dependent by use of the forest fraction (Eq. 12). By doing so, again, the manuscript fails to innovate as similar work has explored the added benefit of process based approaches earlier (e.g., Bogaard & Van Asch, doi:10.1002/esp.419).

>Thanks for this comment - we added a section to explain the drawback of using multi-tank for adding a time lag.

>Implemented (Line 7-18, Page 4): *“Multi-tank models can deal with infiltration time lags to some extent by adding tanks but even then they (i) require data from several monitoring boreholes to track groundwater flow supplies in complicated geological structures and (ii) they are presently not designed to replicate time lags of increased infiltration, e.g., following snowmelt (Iverson, 2000; Sidle, 2006; Nishii and Matsuoka, 2010). Applying multi-tank models to compensate for time lags is questionable as especially deep-seated landslides would need several tanks to replicate time lags and every added new tank in vertical direction introduces 3 new parameters at least. This would reduce robustness and reliability of system especially if we just use the monitored groundwater table for the parameter training of whole system.”*

>A rigorous assessment of the added parameterization costs and uncertainty is a good suggestion. However, there is no effective application of multi-tank model in deep-seated landslide. In addition, no standard procedure can valuate the parameterization costs and uncertainty. Thus, comparison of parameterization costs and uncertainty cannot be operated easily.

> We add a section 3.4 to explain this effect of evapotranspiration on groundwater table

>Implemented (Line 3-5, Page 13): *“Due to the all-year humid climate, the rapid drainage of water in the permeable underground and the deep-seated nature of the slope movement, we did not explicitly consider evapotranspiration.”*

>For the snow accumulation/snow melt, we introduces well-operating empirical equations into the tank model and we do not aim at improving the estimation ability of snow model itself. This is a rough estimation since the precise calculation is very complex, but on the other hand we aggregate changes in pore pressure over years and daily uncertainties in snowmelt will be smoothed out after a few days.

(3) The fact that hydrological input is directly transferred into pore pressure is an assumption not proven to any conceivable standard in the paper and one that is highly tenuous in case of deep-seated, complicated landslides (e.g., the effect of undrained loading). This is particular the case as any natural variability in what supposedly is a highly heterogeneous sub-surface (Figure 2) is left out of consideration completely by analyzing only one well that is located relatively deep into the incompetent marl layers. Accumulation of groundwater in the more pervious and fractured materials higher on the slope (dolomite and debris) and any subsequent loading is left completely out of the equation. In this light, a formulation of an objective in terms of movement (hazard; see

also point 1) and a separate validation of the pore pressure levels in terms of acceleration of the entire landslide body are definitely missing. Similarly, an evaluation of the tank model in relation to other observed pore pressures / groundwater levels (e.g., the second well indicated in Figure 2, B2) would certainly add rigour to the assessment and may help to prove its actual worth. In terms of the mathematical formulation, the method is already fraught as changes in groundwater height are equated to the input in terms of water slice, thus neglecting the effect of the available pore space in which the water table is formed. Hence groundwater fluctuations and related pore pressure variations under the assumption of a freely draining aquifer are underestimated. Re (3), it also means that dynamics in the available pore space over drier and wetter periods are also ignored. Furthermore, the authors neglect seepage forces (p. 8, line 11) but using hydrostatic forces is questionable as it is not proven how water flows through the landslide complex and if the simulated groundwater level can be simply extrapolated to an effective pore pressure at the potential slip plane.

>We add the explanation about the “pore water pressure” in section 3.3.

>Implemented (Line 2-3, Page 11): “Hereby, “pore water pressure” is positive pressure induced by groundwater table height. It does not refer to perched water or negative pore water pressures.”

>The prediction model of groundwater usually extracts the transformed pore water pressure data then make a water table prediction. After that, predicted water table is transformed into pore water pressure for validation. What we did is coupling the pore water pressure directly into the model.

>We add section 3.4 to explain model assumptions to simplify slope hydrology.

>Implemented (Line 5-10, Page 12): “We assume that Quaternary deposits control the hydraulic properties of the tank model (tank interior with soil/rock in Fig. 3). The fractured limestone and dolomite control the water flow from higher to lower elevations (groundwater inflow and drainage in Fig. 3). The marly Kössen Beds are treated as impermeable layers (thin, low porosity and high normal stress above). As this is a regional groundwater table estimation, we can use the modified tank model to simulate the groundwater table changes induced by precipitation.”

>Our aim is only to estimate the local ground water table in deep-seated landslides. The relation between landslide movement and the groundwater table is not the focus of this manuscript. Firstly, the groundwater table is a regional estimation. Secondly, the landslide movement is complex and time-dependend and material strength is also very important besides groundwater table.

>An evaluation of the tank model in relation to other observed pore pressures/ groundwater levels would certainly add rigor to the assessment. Unfortunately, we have only one persistently functioning pore water pressure sensor in another well broke down.

>We add more details about explanation and calculation of pore water pressure by our tank model in Eq.(5) (6) and Eq.(7).

>Implemented (from Line 14, Page10 to Line 16, Page11):

$$\Delta h_i = h_{i+1} - h_i = \frac{\alpha}{n} (ER_i + ES_i) - (q_i - g_i) \quad (5)$$

where α is a proportional coefficient (only for ideal tank model, α is one) and n is the average porosity of slope mass. Hereby, “pore water pressure” is positive pressure induced by groundwater table height. It does not refer to perched water or negative pore water pressures.

Thus, PWP can be linearly correlated to groundwater levels as Eq. (6).

$$\Delta PWP_i = \frac{\alpha g'}{n} (ER_i + ES_i) - \Delta PWP_{(g+q)i} \quad (6)$$

where, g' is acceleration of gravity, $\Delta PWP_{(g+q)i}$ is the PWP changed by subsurface inflows and outflows on the i^{th} day. This allows us to evaluate changes in PWP resulting from infiltration, drainage, and groundwater supply. The major part of pore water pressure is static pressure induced by water table height. Minor components are seepage force and the difference of pressures in the available pore space over drier and wetter periods. Since the tank model is a “grey box model”, we do not know the exact proportions of static pressure, seepage pressure, and pressure dynamics in pore space, which are all three included in our equivalent pore water pressure.

$$\Delta PWP_i = \alpha' (ER_i + ES_i) - \Delta PWP_{(g+q)i} \quad (7)$$

In Eq. (7), α' replaces $\frac{\alpha g'}{n}$ to simplify the model.”

(4) In a similar vein to the above, the authors, whilst drawing from hydrology and using a water balance approach in their tank model, do not observe its physical foundation of conservation of mass. Equations 8 through 9 are fitted empirically and independently and closure of the water balance is not attested. From a hydrological perspective, it is strange to put the pore water pressure in the exponent of Equation 9 as it assumes that the recession of groundwater storage always starts at 13.4kPa, which violates directly the above principle. A linear reservoir of the form $Q = aS^{**}b$ would be more valid and more flexible to apply. In terms of hydrological functioning, the fact that only one point is considered and the physiographic context of the landslide is completely ignored is inexcusable. It cannot be accepted without evidence that the groundwater variations at point B4 halfway the slope are only governed by the local precipitation input and that the resulting groundwater levels are representative for the landslide as a whole. Furthermore, a regional water balance should be conducted to exclude any effects of lateral inflow from the higher elevations of the Kössen formation (Figure 2) or any spatial distribution in precipitation due to orography and exposure. At present, the model is merely a black box and any semblance to the observed signal at the point too much circumstantial. In terms of the analysis, performance is explored but only

partly explained. In addition to the RMSE, model performance should be explored using Nash-Sutcliffe model efficiency or Kling-Gupta as is standard in hydrology. Improvements should be evaluated in terms of the added information versus the added uncertainty and the importance thereof clearly follow from the research objective. Rather than calibrating model components, a corrected model with a stronger physical base should be used and calibrated using a clear objective function and issues of equifinality and the resulting parameter space be clearly evaluated. In terms of the snow model, I fail to see why the partitioning of precipitation into snow and rainfall cannot be based more reliably on the temperature (Eq. 10, which now can give snow in summer as it depends on the relationship between temperature and relative humidity only) and why forest fraction is of influence (and how) on the melt index of the snow melt model. I assume it is a constant value but this is not clear and overall methods are not fully transparent. In terms of text, the manuscript is readable with minor mistakes (e.g., page 3, line 28: mode= model) but the sentences are sometimes convoluted. However, the nomenclature is put in poor English throughout. References are mostly relevant (but see (1)) and correct although the order in the reference list is not purely alphabetical.

>As we said in Section1, this is a “probabilistic model” based on a modified water balance equation. Its physical foundation of conservation of mass is: input is subsurface water flow and infiltration while the output is the drainage, although the mass of water can not be measured directly.

>Almost every landslide has a basic water table or minimum water table (here starts at 13.4kPa). It means the “drainage position” is higher than the “bedrock”.

>According to the reviewer’s opinion, we used a linear reservoir of the form $Q = aS^b$ to describe the drainage and groundwater table as Eq.(9) and Eq.(10).

>Implemented (Line 11-14, Page 14):

$$“PWP_{i+1} = aPWP_i + b.” \quad (9)$$

where a and b are fitted coefficients.

Thus, ΔPWP_i calculation could be rewritten as:

$$\Delta PWP_i = \alpha' (ER_i + ES_i) - (aPWP_i - b). \quad (10)”$$

>We focus on the regional groundwater table and we do not claim it is representative for the landslide as a whole same as the precipitation distribution. Thus, for the research object-regional groundwater, it is affected by vertical infiltration, lateral inflow, and lateral drainage.

>We have taken a considerable effort in calculating the Nash-Sutcliffe model efficiency for all described model runs of the original simple and modified tank models in Section 5.

>Implemented (Line 2-7, Page 19): “In order to evaluate the performance of the modified

tank model with respect to heavy rainfall and snowmelt, we introduce the standard Nash–Sutcliffe (1970) efficiency (NSE) which is the most widely used criterion for calibration and evaluation of hydrological models with observed data. NSE is dimensionless and is scaled onto the interval [inf. to 1.0]. NSE is taken to be the ‘mean of the observations’ (Murphy, 1988) and if NSE is smaller than 0, the model is no better than using the observed mean as a predictor. ”

(Line 1-5, Page 20): *“The NSEs of the original tank model and our modified tank model during the heavy rainfall season are -0.09 and 0.63 respectively. It means the standard original tank model is no better than the ‘mean of the observations’ while our modified tank model has a significantly higher explanatory power.”*

(Line 14-16, Page 20): *“The NSEs of the original tank model and modified tank model during the snowmelt season are -5.95 and 0.75 respectively which emphasizes the performance of the modified tank model.”*

>We reduce the uncertainty by introducing equivalent infiltration and snow accumulation/melt equations based on simple tank model. This does not increase the numbers of parameters compared to multi-tank model which has a higher uncertainty/degree of freedom because of added parameters. We now calculate RMSE and Nash-Sutcliffe model efficiency of our modified and the simple tank model. Since there is no effective multi-tank model application in deep-seated landslides, we cannot evaluate the model efficiency between our modified and a default multi-tank model.

>Temperature and humidity are the main factors for estimation of precipitation. Accuracy temperature of snowline is the key to judge the type of precipitation, however, it is difficult to obtain even armed with the advanced device considering the variation of air temperature effect. Our snowfall/melt model is a state of the art statistical model not a physical model but we are only judging change in pore water pressure and in case of minor accuracies of daily snowmelt rates this smooths out over time.

>Forest fraction influencing on the melt index is still an empirical formula. In field, precise information is not easy to obtain. We think that the best strategy is the usage of the empirical formula.

>We have edited the language of the whole paper carefully and in two complete revisions made the paper significantly more concise after all revisions have been achieved.