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Discussions

Interactive comment on "A modified tank model including snowmelt and infiltration time lags for deep-seated landslides in Alpine Environments (Aggenalm, Germany)" *by* W. Nie et al.

Anonymous Referee #2

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With interest I have read the manuscript by Nie et al. submitted to NHESS-D (nhess-2015-341). I believe we need parsimonious and robust models to predict hazards or process-based models to serve as a diagnostic tool to test hypotheses about the functioning of natural systems, including landslides. In my opinion Nie et al. do not succeed in applying their model for either of these purposes. As such, I'd qualify their manuscript as a case study with insufficient depth and of little interest to the audience of NHESS in general. Four fundamental weaknesses undermine the quality of the underlying manuscript:

(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 modelling 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.

(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).

(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.

(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 13.4 kPa, 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.

All in all, this leaves me ambivalent about the manuscript. Potentially, it is the basis of interesting work but at present I find it lacking in consistency and quality.

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