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Hydrology and Earth System Sciences Discussions

Interactive comment on "Characterization of groundwater dynamics in landslides in varved clays" *by* J. E. van der Spek et al.

Anonymous Referee #2

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Overview

The study describes the development and the application of a hydrological conceptual model for the simulation of the groundwater dynamics in varved clays in the landslide-prone Trieves area in the French Alps. The model is tested by using groundwater observations collected at four landslides sites. Moreover, the model is run for the period 1959-2004 and the relationship between the simulated groundwater and the landslide activity in the region is investigated.

General Comments

The paper is well written, well structured and clear; the language is fluent and precise.

The content of the paper is surely of interest for the readers of HESS as the understanding of deep-seated landslide activity by using groundwater information is receiving significant attention in the recent scientific literature. In particular, recent studies employed observed (even through remote sensing) and simulated groundwater data in order to predict the triggering and/or the movement of landslides. As hydrologist, I am particularly interested to the conceptual hydrological model developed in the study and how it can be used to predict groundwater dynamics and, eventually, the landslide activity in the Trieves area. I really appreciate the interesting attempt of the study to conceptualize the system and I am aware of the difficulties of the problem. However, I found some issues that, in my opinion, should be addressed before the publication.

1) The proposed conceptual model simulates the groundwater dynamics in the colluvium, the varved clays and the fissures by subdividing the whole system in two subsystems (colluvium and varved clays) connected by the fissures. For each subsystem, and for the fissure, different equations are used and the corresponding parameters (theoretically also variable in space and with depth) should be defined. Therefore, the number of model parameters is quite high and its estimation does not represent an easy task, likely causing indeterminateness (or equifinality). Moreover, this issue is emphasized by the relative scarcity of field data that can be used to constraint the model. In these conditions, it is expected that the model predictions are affected by significant uncertainties.

Therefore, I am not sure that we can really understand if the proposed model is able to describe the real system. For instance, is a much simpler model (e.g. a bucket model) able to simulated the observed groundwater dynamics? Which are the differences with the simulated data obtained by the conceptual model proposed in the study? If significant differences are observed, then the use of the more complex model can be justified. In the discussion section, the theoretical comparison with the model of Van Asch (1996) is described but no clear evidences are given (at least for me) to conclude that the proposed model better simulates the hydrological behaviour of the area.

I suggest the authors adding further analyses to better justify the hypothesis behind employed model.

2) The method used for the calibration of the conceptual model is not clear. At page 306 it reads: "Because of the calibration data quality, a qualitative, expert-driven calibration was performed instead of a formal best-fit approach." However, looking at the simulations reported in Figure 6, the model fitting does not look good to me, even for the colluvium. Specifically, a constant shift between observed and simulated heads is always present. Can the authors add more information about the parameters calibration? Can the shift be removed?

Moreover, the results for only one landslide, out of the four for which the model was applied, are shown in the paper. For the other three landslides, no information is given. I believe that, if mentioned in the paper (line 4 at page 307), the results for the other landslides, at least briefly, should be given.

3) I believe that more details should be given for the analysis between landslide activity and groundwater dynamics. The authors say that a qualitative and quantitative analysis is carried out. I believe this analysis might be, potentially, very interesting. However, I found it quite weak, only stating that some landslides occurred or reactivated during periods with high simulated heads. I believe that the simulated head represent the initial condition and that the landslide triggering and/or movement should be also related to rainfall observations. In fact, high simulated heads occurred also in periods without landslide activity (see page 310, lines 1-3). Can the authors elaborate more on that?

4) In the discussion section, the crucial role that can be played by the fissures geometry (and their time variability) and the air entrapment in silt layers is described. However, I found the discussion too much "theoretical" only underlining possible problems due to the not knowledge of these characteristics. I believe that some quantitative information about the effects of these issues should be given, for instance in terms of simulated heads. Otherwise, these parts could be removed.

On this basis, I feel that the paper might deserve to be published after a moderate revision.

Specific Comments/ Technical Corrections (P: page, L: line or lines)

P296, L15: "the lack of important physical parameters". Reading only the abstract, the meaning of this sentence is not clear, please revise.

P307, L6-7: The results obtained by van der Spek (2011) should be reported in the paper.

Figure 2: The gray area should represent the presence of water (saturated area). However, it should be specified in the caption.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 295, 2013.