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Hydrology and Earth System Sciences Discussions

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Interactive Comment

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

J. E. van der Spek et al.

t.a.bogaard@tudelft.nl

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Overview / General comments

We thank the reviewer for his constructive review and the compliments. Here we will address the specific points mentioned by the reviewer.

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.

In a previous version of the manuscript we did list the parameters but for reasons of conciseness we decided in a later stage to exclude them. We used 10 subsurface hydraulic parameters of which we calibrated 4. The 6 others were derived from literature values. We will include the table of soil hydraulic parameters, there values and calibration values in the revised version of the manuscript..

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.

The reviewer is correct that we did not show that the conceptual model is correct. In fact, it can never be shown that a conceptual model is correct. The conceptual model is based on the physics of the problem. We implemented the conceptual model in a computer program and used it to simulate the head variations and obtained a reasonable match, especially when considering the quality of the data and the lack of knowledge of certain important parameters, such as the distance between the observation point and the fissure, and the distance between fissures. The reasonable match between the simulated and observed heads is evidence that our conceptual model is plausible. We elaborated on the uncertainty in the model results in the revised paper.

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The suggestion of a comparison with a bucket model or, more generally, a time series analysis where rainfall is the input series and head measurements in the varved clays is the output series is interesting, but beyond the scope of this paper. We believe the physically based modeling approach is needed in this case, because it allows us to simulate the pressure variations everywhere in the varved clays. A bucket model will only be able to simulate pressure variations at the location and depth of the observation well, which is not necessarily representative of the head variation at the slip surface. Therefore, the reviewer is correct that we do not show we have a 'better' model then van Asch, just that with our different conceptualization (so different hydrological behaviour). Of course we discuss our reasons for this conceptualization throughout the manuscript. We will add this clearly in the discussion.

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.

The reviewer's points show that we did not get our intention across and that we need to elaborate on the exact calibration procedure. We had no geometric exact description of the landslides for fissure location, fissure densities and especially of the position of the observation points for pore water pressure and the distance to/from a fissure. Therefore, we first of all decide to work with two 'standard' systems. Secondly, because of the same reasons, we could not do a standard calibration. We therefore adopted the

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expert-driven calibration in which we looked at maximum pore water pressure fluctuations at the interface of the fissure and the varved clays and in the varved clays at the maximum distance from the fissures. This also explain why we cannot get the shift totally away. We were already quite happy to get such reasonable results with such scattered and incomplete data set. We will extend our discussion on this aspect of the calibration.

Furthermore, the reviewer asks for the other landslides of which we had some data. We look at three more landslide: la Mure, St. Guillaume and Monestier du Percy. For reasons of conciseness we decided to work with Avignonet, as we had three observation periods. The La Mure and Monestier du Percy could be modeled and compared as well although there seems an issue with snow melt. For St. Guillaume we were not sure about the quality of the ground water data and decided to omit these from our hydrological analysis. We agree it could be interesting to add these analyses as well, but it also increases the complexity of the manuscript and could distract from our main objective. However, based on the suggestion of the reviewer, we will add a summary or extra example of the results of the other case studies as well to show the more general applicability of the approach.

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?

What we did was modeling the pore water pressure in the varved clay system for a period of 45 years using the meteorological input data. No additional calibration has been done (that was done using 1958 'standard' year). In that respect rainfall and pore

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pressure are both included in our analysis. Therefore, we do not see the simulated groundwater as only the initial condition on top of which the rainfall is the triggering factor. Moreover, the problem relating this analysis, is that the observed landslides was hindered by the temporal quality of the registered landslides. But we did get good correlation with the reported reactivations as we described. We will add this clarification / discussion in the revised version of the paper.

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.

In the Discussion section we discuss a few important processes that we have not included in our conceptual model. We believe that it is important to point out that these processes were left out of our model, but could have a significant effect on the groundwater dynamics. The reader is referred to the cited references for more quantitative information about these effects.

Technical corrections will be done.

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