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Hydrology and Earth System Sciences Discussions

Interactive comment on "A conceptual model of the hydrological influence of fissures on landslide activity" *by* D. M. Krzeminska et al.

Anonymous Referee #1

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This paper deals with modelling of preferential flow in fissures at the hillslope scale to assess the impact on landslide activity. It investigates a conceptual approach to represent fissure flow and the dynamical connectivity of fissures within a distributed hydrological model. This is highly relevant for landslide research, and might also be of potential interest for a wider range of applications in vadose zone hydrology. Especially the approach of using a process-based model to jointly investigate preferential flow and slope stability is laudable and represents a substantial contribution to understanding the relevance of hydrological processes for mass movements. Nevertheless, there are a couple of open questions regarding details of the methodology and the results. I think also the presentation and discussion of the results could be strengthened, and the writing style has to be improved. The manuscript needs to be carefully revised in

order to be acceptable, according to the general and specific comments given below.

General comments

Abstract

The authors study a conceptual model for dynamic connectivity of fissures based on relative saturation of the soil column. However, in more than half of the abstract they claim to investigate dynamic opening and closing of fissures due to landslide motion, which would indeed be highly interesting, but which is not investigated in their study. The authors only mention that they established a relationship between potential movement of the landslide and density of the fissure network (page 11046, lines 17-18), but this relationship neither is presented somewhere in the manuscript nor is it investigated in the modelling study. Under these circumstances, they might at most consider to discuss this potential feedback between landslide movement and fissure characteristics as an interesting aspect of future work. For modelling dynamic opening and closing of fissures, it could be interesting to consult the literature on modelling of soils with shrinking-swelling behaviour. For the time being, I suggest rewriting the abstract to reflect the actual contents of the paper, including the main results.

Methodology

A central focus of the study is on connectivity of fissures. The authors consider a factor which depends on matrix saturation above field capacity, and implement this into the spatially distributed model STARWARS. A similar threshold relationship for the activity of preferential flow paths and matrix water content has for example been presented by Zehe and Blöschl [2004]. Their approach has been motivated, among others, by the consideration that macropore flow will start above a threshold saturation of the matrix when the capillary forces become too small to attract water from the macropores. The authors of the present paper seem to view “connectivity” similarly in this “functional” way, although this is not yet explicitly stated in the manuscript. In my opinion, the word “connectivity” in conjunction with flow paths also has the connotation of paths being

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physically connected or not. Difference should then be made between a set of uniformly distributed and randomly orientated flow paths, and fissures due to landslide motion that are orientated more or less parallel and perpendicular to the direction of slope movement. This issue of fissure geometry touches the concept of fissure representation presented in section 2. The latter case of parallel fissures would imply that there is primarily no connectivity of fissures in downslope direction, and in the model, fissure flow has to be treated differently for the x- and y-directions. Despite explicitly dealing with landslides, the study seems to be concerned with randomly orientated fissures, if this can be concluded from the introductory part (page 11043, lines 11-13: "macropores [. . .] are not considered to be continuous throughout the soil profile or the hillslope but more likely [. . .] separated by matrix blocks"). I think this issue should be resolved by giving a clear definition of the geometry and orientation of the fissures that are considered in the study, even more as continuous preferential flow paths very well might exist [e.g., Jones and Connelly, 2002]. Secondly, it touches the approach of the authors to relate "connectivity" to water saturation of the matrix (section 3.2). In this point, the authors contradict themselves. If the fissures were separated by matrix blocks, this apparently would already be represented in the model (as stated on page 11050, lines 22-23), and there would thus be no need to additionally introduce a connectivity factor depending on effective saturation. This needs to be clarified by the authors. One possible alternative would be to focus on the aspect of saturation-dependent "activity/functioning" of preferential flow path, which would then result in a functional connectivity throughout the hillslope.

The title suggests that one objective of the study was to investigate the importance of preferential fissure flow for slope stability. This issue is only marginally presented in the results and discussion section. The authors should consider presenting and discussing their results on temporal and spatial differences in slope stability in greater detail.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 11039, 2011.