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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A coupled distributed hydrological-stability analysis on a terraced slope of Valtellina (northern Italy)" *by* C. Camera et al.

Anonymous Referee #3

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The authors present the application of a coupled slope-hydrology-slope stability model for the analysis of the triggering conditions of shallow soil slips on a terraced area in the Valtellina region. The case study is interesting because the location of the landslides is controlled by the presence of dry-stone walls constructed to use the natural slopes. Ideally, the manuscript has the objective of evaluating the hydrological (changes in material hydrodynamical properties and flow conditions at the vicinity of the walls) and mechanical (changes in material strength properties and strain distribution) effects of these walls on the stability conditions.

The proposed model is not new; the hydrological model consists in the already well established Starwars model used for many landslide studies, and the development consists in proposing a General Equilibrium Analysis for the slope stability component.

The efforts in developping and applying the slope stability model are very vaulable and could certainly be a promising approach, but at the moment the manuscript clearly misses: - a detailled description of this model (What are the constitutive equations? What are the parameters? What are the domain applications?), - a detailled sensitivity analysis of the influence of some parameters or resolution of equation, and; - a quantitative evaluation of its performance, for instance by comparing the results to well established geotechnical models (Seep/Slope, Abaqus, Comsol, Flac or any other couple hydro-mechanical model) even only on 2D cross-sections. - the authors might also consider to evaluate carefully the slope stability model on analytical solutions.

There are also some concerns that the numerical scheme of PcRaster may not be suitable for the problem at hand (e.g. resolving complex interelationships at the vicinity of the walls where changes of material properties and flow conditions may be drastic over a short spatial scale). To my point it should be made clear to the reader that the proposed results are robust.

Though the material is very clearly relevant to the journal, this lack of information and quantification of possible errors of the slope stability model is a critical issue that must be addressed before possible publication. In summary: (1) There are several potential shortcomings of both the model and the numerical implementation of the model that have not been sufficiently explored in the manuscript to engender confidence that the model and implementation represent dominant physics of the problem. (2) Information on the input data is sometimes weak. For example, once you are mentionning a DEM at 1m resolution, once at 2m resolution. Possible validation of the soil depth map with geoelectric data should be more emphasized (provide a geoelectric profile? Estimate its accuracy?, etc). The authors use alternatively the terms "hydrogeological" and "hydrological" which are not synonyms, etc. (3) The English usage, the lack of focus of the manuscript, and many other errors and omissions are sufficiently problematic to make the manuscript sometimes difficult to follow

My opinion is that this manuscript is, at this stage of development, not ready for publi-

cation.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 2287, 2013.