Hydrol. Earth Syst. Sci. Discuss., 10, C1707–C1712, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C1707/2013/

Hydrology and Earth System Sciences
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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 #2

Received and published: 14 May 2013

General Comment

The research presented in the paper collects valuable data on terraced environments, uses a model produced by third parties (STARWARS by Van Beek, 2002) to simulate the hydrological fluxes in this environments, and implements a stability model upon which is derived the stability of the hillslopes. The first topic was already covered, at least partially, in a paper appeared on the "Landslide" journal, the second is a novelty, as well the third. However, the stability analysis, at the single terrace-wall scale, was already covered in Camera et al., Landslides, 2012 using a setup of models which is different from the one used in this paper.

To free this reviewer from the idea that this paper is just a "remix" of topics already

discussed, the Authors should clarify better the differences with the other papers based on the same data and location, and, at the same time, summarise which issues of those papers push the writing of the present one. In any case, this new paper need to be justified on on the basis of answers to new research questions, which are a little missing here. In fact, the paper in subject includes, at present, a lot of detailed field analysis, but nor new insights for science and neither general statements about the hydrological-geomechanical behavior of the terraced hillslope in comparison to normal hillslopes.

Regarding the hydrology studied, I venture to say that I suffer cause the lack of equations, a fact that makes difficult to understand what the model STARWARS really does, how boundary and initial conditions are setup, and which is the consistency of the numerical method used for integrating the groundwater flow. Actually I went also directly to the dissertation from which STARWARS originates, and I frankly have doubts about some choices made in that model, which actually propagate on the validity of the results of this paper. In my opinion, Authors should not just assume that a model works: the appropriate verifications remain on their shoulders, and they are kind of missing.

Therefore, encouraging the Authors to extrapolate the good from the bad, I think that the paper needs to go through major revisions before being published.

Detailed comments

page 2290 - lines 9-10 - With all the respect I have for the cited colleagues, I would cite different papers for the merits of the infinite slope stability (like, for instance Skempton and DeLory, 1957)

page 2292 - lines 24 - 26 - Citing all of these models together and in this way, does not make justice of them. SINMAP and SHALSTAB, do a lot of simplifications (stationarity of the fluxes, coincidence of terrain and bedrock slope, do not solve partial differential equations, etc) that SHETRAN and GEOtop do not (they solve the three-dimensional Richards equation). TRIGRS is something in between.

page 2293 - line 18 and subsequents - The description of the STARWARS code is too generic to let the reader know what it does. Which equations does it solve ? Does it solve Richards equation or not (what does it means "calculated on the basis of Richards equation")? 1-D or 3-D ? If it solves the Richards equation, with which numerical method ? How does it set initial and boundary conditions ? How the solver deals with the transition between unsaturated to saturated conditions, where the "normal" Richards equation is invalid ?

page 2293 - line 25 - How can the Authors rely on this statement of Van Beek ? The relative velocity of water flow in the unsaturated and saturated parts certainly depends on the type of soil (and on the variation of its hydraulic conductivity with soil suction - or water content). How the Farrel and Larson, and Millington and Quirk parameters were identified ? (One of problems of using unusual parameterisations is the lack of references for setting the parameters).

page 2294 - line 1 - The Farrel and Larson (1972) characterisation of SWRC used has an exponential form which is difficult to support against the power laws which are currently used. Historically these exponential curves where used especially because they bring to analytical solution of the 1-D Richards equation, but are, in my opinion, completely unrealistic. Similar considerations apply to the Millington and Quirk (1959-1961) scheme for hydraulic conductivity. I do not doubt that, after a proper calibration, even these parameterisations can work. However the transportability of results to cases where the calibration is not performed, is to be verified. Since Richards equation is mass conservation plus continuum hypothesis plus parameterisations of SWRC and hydraulic conductivity, if we do a bad job with the latest, we probably do a bad job all over.

page 2294 - line 22 - What the Authors say about infiltration under the asphalt could be acceptable for lateral infiltration. However, how they deal with vertical infiltration ? Do they assume that water infiltrate through the asphalt ? If it does not infiltrate, how do they treat overland flow ?

page 2295 - line 20 - I do not have anything against the Sarma (1973) method. I wonder, however, if it can account for the apparent cohesion that can derive from a soil being unsaturated (e.g. Lu and Likos, 2004). Since in the Results, the Authors find some terraces being "unstable" even in completely unsaturated conditions, the case could be that those terraces are, in fact, stable because of the apparent cohesion generated by capillary forces. Also the studies by Lu and Godt support this (e.g. Lu and Godt, 2012). In particular the Authors could use, in this case, instead of the Sarma's theory the more recent scale field of factor of safety introduced by Lu and Godt in chapter 10 of their book. So, is really the Sarma method appropriate to the case study under scrutiny ?

page 2297 - The section of the soil depth is interesting, and, in reality could deserve a paper by itself, after the clarification of some issues.

page 2298 - line 19 - Using Rˆ2 as an indicator of good fitting is not very significative, even with very high regression coefficients (of the order of 0.9), and, therefore, various index of fitting where developed, and probably could be used also here giving some more quantitative insight.

page 2298- line 23 - It is not clear to me what the map of soil depth classes is. Can the training point be indicated in the figure ? How many are they ?

page 2299 - line 24 - Why put the part about evapotranspiration here ? It should pertain to the setup of the experiment more than to a discussion.

page 2300 - line 13 - Hourly and Daily time-step. Running a Richards equation solver for daily-time steps is obviously possible, loosing accuracy (if we are talking about of a solver of non-linear partial differential equations) but which kind of event is driving the simulation ? Was it used a real rainstorm, a sequence of rainstorms, or was used a virtual event ? In any case, with which characteristics ?

page 2300 - line 21 - The volumetric water content is universally denoted in literature with the greek letter θ, why using VWC instead ?

page 2301 - line 1 and subsequents. The arguments used here are not completely clear to me. First is said that a well maintained wall "drains" well, and, therefore the cell draining into it remain unsaturated. Then it is said that (with a virtual experiment, I suppose) changing the status of the wall does not change the evolution of the VWC. Finally it is said that the VWC of cells with poorly maintained is lower than normal. These three statements add confusion instead of clarifying the issues. Please rephrase.

page 2302 - line 12. Granted, but not accepted, that the stability model is "under construction", are therefore these preliminary results" ? Moreover, I would really like to see some equations about this new model, and I cannot accept its results if I do not see them explained.

page 2302 - line 14. How can the Authors say that there is an over-estimate of unstable areas ? How they assessed the instability independently from the modeling presented here? Where it is written in the paper how many are the unstable cells, before this point ?

page 2302 - line 26 - The fact that some cells are unstable under dry conditions could simply mean that suction plays a role there. Does the stability model implemented accounts for it ?

page 2303 - line 5 - Is the tendency of cells with higher wall to be more unstable a property of the geometry that affects the relative stability also in dry conditions (the higher the walls the closer to instability the cells) or is a property of the hydrology of these terraced hillslopes (higher wall favour more water storage, and therefore instability) ?

Figure 3 - What is the meaning of Figure 3a ? Why it is necessary to do the elaboration that brings to Figure 3b, if we have Figure 3a ? The control points can be visualised on the maps ?

Figure 4 - At least visually, the differences between geostatistics and geophysics seem

very high: there are points were these differences are more than 3 m ! How can these differences be explained ? The scatterplot among geophysics and geostatistics shows almost no correlation between the two data sets. These differences cannot be left unexplained, and constitute actually a problem that can invalidate any further simulation and subsequent conclusion.

Figure 5 - The large under-estimation of water levels at hourly time steps invalidate any further consideration about instability. On the other hand, the (relative) agreement between daily simulations and measurement is a surprise. Does the hourly time-step prediction means that the parameters of the model needs to be re-calibrated ? Why this different predictive capability of the hydrological model at the two different time steps ?

Figure 6 - If this is the pressure at the walls identified by the given numbers, probably a map for the identification of the walls referred should help. I would discourage the use of the yellow colour which is barely visible.

Figure 9 - Why in this Figure 9a the US class is missing ?

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