

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

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Received and published: 8 May 2013

The article focuses on a very well known area in Northern Italy, Valtellina, which has been the focus of many landslide studies. The authors analyze the stability of terraced slopes using a numerical modelling approach based on a coupled hydrological / slope stability model. This model is in part new (the stability part) and contains elements that are relatively innovative. The idea of developing a slope stability model based on slope slices and not on a cell-by-cell basis is interesting, generating a certain level of expectations in the reader.

The introduction section and the study area are well written, and the interest of the reader is kept. But after that the manuscript misses the point somehow. The objective

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of the paper is to describe a coupled hidro / stab model, but then in the results section we are told that the stability model is still under development; that is puzzling, and the reader gets lost: then what is the point of the article? Also, where are the equations? It is an article based mostly on modeling, but then there is a lack of detail in the description of those models. STARWARS we already know it, but the stability model, which is a new development, should be described with much more detail (show the fundamental equations, please! how is the critical acceleration,  $K_{acc}$ , calculated? provide some details about the numerical scheme, how is the timestep duration calculated to ensure numerical stability, etc). Model coupling, that is a fundamental issue of this article according to its title, is completely missing since no details are given at all: is it a hard or a soft coupling (i.e., do the two models communicate each time step and their outputs influence each other, or are the two models run sequentially and the output of one model is an input to the second?), what variables are exchanged between the models, do they use the same timestep or a different one, in which way does one model outputs influence the other, etc.

Then, the authors apply the coupled model to a case study in Valtellina, Italy. In section 3 the authors describe with detail (occasionally excessive, since they describe processes that led to no satisfactory results) the development of the DTM and a soil depth map, but then they provide no details about the rest of the model parameters: what parameters enter in the models, how were they determined, how were they spatially distributed, etc. Referring to past papers as it is done in the results section is not enough, the readers merit some further explanation.

The results section should be separated from discussion (section 4). Description of the model results gets confusing at some points, and several paragraphs would benefit from a deep re-shaping. Some results are described with very little criticism. For example, it is evident that the soil depth interpolation did not produce good results when compared with geo-electrical survey. Also, the map of  $K_{acc}$  in Figure 9, which is key for determining the safety factor, shows very unrealistic spatial patterns which

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are likely related to the routing algorithm used. These issues need to be critically addressed by the authors.

I was surprised that no calibration at all was performed. Do the authors believe that there are no uncertainties in their input parameters? It could be the case that by calibrating some parameters the results would result in a more precise prediction map, although care should be taken not to overfit the model as it sometimes happens.

I was also surprised that the model results were not compared to an inventory of past landslides, as it is usually done. That is, there is no validation of the model. This is a very critical point in my opinion. Also, how do the results compare with other susceptibility or hazard maps by other authors in the same area (if there are any, as it is suggested in the introduction).

Discussion is very weak. The authors should elaborate more on the validity of their model (the fundamental assumptions they did), their potential pitfalls, and compare it to other slope stability models out there that could produce similar analyses, if they are.

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/10/C1537/2013/hessd-10-C1537-2013-supplement.pdf>

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

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