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Interactive comment on "Regional physically based landslide early warning modelling: soil parameterisation and validation of the results" *by* Teresa Salvatici et al.

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Received and published: 22 February 2018

RC: The topic of the work meets the scope of the journal well. However, it is difficult for readers to recognize its contributions to the science community from its title, abstract and even the introduction part. The Introduction, Methodology and Discussion sections are not well-structured and pose difficult for readers to understand. AC: We would like to thank the referee for his/her careful revision and fruitful comments. We agree with the referee that the manuscript needs an in-depth revision, especially concerning the structure and organization of the sessions. We are currently working in this direction and we are completely reorganization the contents of the Introduction, methodology

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and discussion.

RC: My specific concerns are listed below: The introduction part fails to convey the current research gap and readers have difficulty to assess its scientific significance. It is unable to convince the readers why the authors carry out this work. It seems that authors want to share with the community some improvements by considering soil and vegetation parameters by using the existing model HIRESSS. I recommend the authors first detail the research question clearly, and then briefly describe their way to solve the problem. AC: We thank he referee for the comment. We are rewriting the Introduction, trying to highlight better our key research questions and which are the main objectives of the research work. Our aim is to test the application of an already developed, physically based model to forecast the occurrence of shallow landslides in a selected case study in Italy. Furthermore the work wants to highlight some model improvements related to the soil parameters characterization and contribution of vegetation to slope stability. In order to be consistent between title and contents of the manuscript we propose to change the title From: Regional physically based landslide early warning modelling: soil parameterisation and validation of the results. To: Application of physically based model to forecast shallow landslides at regional scale.

RC: The Methodology part is mixed with Results. For example, lines 123-135 were measured results. AC: We agree with the referee and we are currently restructuring the text in order to separate methodology and results.

RC: The structure of the Methodology is not logical. I suggest the authors put an outline paragraph at the beginning of this section, in which they brief the logics of this section. "3.3 HIRESSS description" and "3.4 HIRESSS input data" should be placed in the beginning of the Methodology. AC: Again we agree with the referee. The methodological part has being revised in order to be more readable and clear.

RC: Although physically based landslide model is desirable, the input data is enormous and rigorous. The data of root cohesion and some of the soil values seem to be

derived from existing literature review. Is it really proper to directly use these data in your study? You should justify this problem. AC: The physically based models require many hydrological and geotechnical parameters as input data. In many cases, for each geotechnical parameter, a constant value is used for the whole study area as averaged from in situ measurements or derived from literature data. In some studies, a limited degree of spatial variability is ensured using a certain value for distinct geological, lithological, or engineering geological units, as derived from direct measurements or from existing databases and published data. In this work we have tried to characterize as much as possible the soil covers from a hydrological and geotechnical point of view, through several direct in-situ and laboratory measurements. In particular the measured parameters are: effective cohesion, friction angle, dry unit weight, hydraulic conductivity effective porosity. Some other parameters have not been measured, in particular we have not defined the soil characteristic curve experimentally but the soil characteristic curves parameters were derived from literature values based on the soil types measured through laboratory analysis. At the same time the experimental evaluation of root cohesion is quite complicated and time demanding and we have chosen to define this value based on relevant literature for the different types of vegetation cover. We will explain better this issue in the text and we will critically examine it in the discussion.

RC: Please detail the acquired time, spatial resolution and other characteristics of the DEM used in the model. AC: We will add this information in the text.

RC: The discussion part is poorly written. Authors should explain the results, compare with other's work, provide implications, acknowledge its limitations and echo the introduction part. I think this part should be significantly improved. AC: As already said before we are completely reorganizing the discussion session.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2017-425, 2018.