

Interactive comment on “Operative and reliable landslide forecasting and influence of geology to predictability” by E. Intrieri and G. Gigli

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General comments Reviewer: The method presented is innovative and interesting, but it seems that too much conclusions are from this research. First the title, is probably to pretentious, I do not see that this method is more operative than others, despite the fact it is interesting and deserves to be published. It is the same for the term used geology, I do not see how it is possible to extract the impact on forecasts. Authors: The title has been changed as suggested by all the reviewers. It is now “Of reliable landslide forecasting and factors influencing predictability”.

R: It is also unclear to know to understand in the paper, what is an a priori or an a posteriori information. The way the variability is presented appears to be estimated a posteriori knowing Tf. Maybe I am wrong, but then it means that it is not well explained

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in the text. A: All the case studies are from past landslides that have already failed. Therefore the time of failures are all a posteriori known. In fact, as explained in the method section, the real a posteriori know time of failure is indicated with T_f , while the prediction with t_f .

R: My proposal it to remove the interpretation part and the argument stating that the geomechanics is not the main controlling parameter. But this is obvious from the usual confusion made about creep which is related to a materials, and the landslide failure which is related to a complex body that is controlled by several variables. The creeping does not apply to landslide except in particular cases, this is a general mistake. That is why you can say something about geomechanics, it does not comes from your results, and it can be criticized on fundamental aspects. Then, if you would keep this point, you need to expand the discussion. A: As stated concerning a similar comment of Reviewer 1, the authors did not mean to diminish the obvious importance of geomechanics to predictability. However, since this point has been unclear for all the reviewers, it is evident that we failed in our explanation. What we mean is that even though geomechanics is unquestionably a key factor, it is sometimes difficult to have a deep knowledge of the geomechanical features of a landslide, especially in the field and in emergency situations, although some safe assumptions can always been done by observation and a broad knowledge of the area. What it may be known about them is in part thanks to what is derived from displacement data. Like in a black box model, even if the real properties of a phenomenon are not known, we can draw conclusions from the output of those properties (i.e. the kinematics). In this case, importance has been done to kinematics because what is generally measured by monitoring are displacement data and because many other unknown factors (rainfall, ground saturation, earthquakes, anthropic disturbance) are included in the black box together with the geomechanics; this makes it virtually impossible to know in advance what may be the degree of influence of geomechanics alone with respect to other factors, thus leading to focusing on kinematics instead. Moreover, even though geomechanics is a key element, landslide prediction can be carried out with a variety of different geomechanical

settings. This explanation can be added in the conclusions, while in the rest of the text every misleading comment that may have reduced the importance of geomechanics will be changed or removed.

R: The oscillation of the values are interesting, but how do you know that you converge to T_f . In the probability index in the criterion include T_f , which you do not know a priori. Please clarify. You need also to discuss the limitations of the method. Your work deserves to be published because it is an interesting study, but please clarify the points above and avoid over interpretations. I propose that you present a figure that explain synthetically your process. A: the predictability index in fact can only be estimated after the collapse. It has been introduced here as a means to evaluate the performance of the different forecasting methods with different case studies and to allow us to draw conclusions. This can be better explained in the text. Thank you for your suggestion of adding a figure to show the process.

Speciîl comments R: Line 21: define what you means by geomechanics? In the text also. A: we mean the study of the behaviour of a landslide concerning its deformation with relation to the applied stress, with particular reference to its post-rupture conditions. We are interested in geomechanics especially concerning the issue relative to ductility and brittleness explained in the text. R: Line 46: instead of “is usually” use “can be” Line 48: you can add reference to the work of Blikra on Aknes rock slide A: all these will be corrected in the text. Line 49: what do you mean appropriately monitored. In fact, displacements are usually points that often do not represent the global landslide behaviour. : : A: Exactly. Moreover monitoring may be carried out for short periods not encompassing the final pre-failure stages, or may have been carried out with too low temporal frequency that do not allow to follow the displacement trend. R: Line 56: 1994 and not 19940 Lines 67-83: references to the works of Dick et al., 2014 (Can Geotech. J., 52, 515–529) and Crosta and Agliardi Can. Geotech. J. 40: 176–191 (2003) and Manconi and Giordan 2015 NHESS. A: these will be changed in the text. R: Line 108: I do not see any probabilistic approach in the paper: : : There is

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only stdev of the forecast in Figure 3. A: the standard deviation would not be possible with a deterministic approach which is the standard way of applying these forecasting methods, that is every method gives a single prediction. At most more predictions can be made in the future but usually only one (the most recent) is used. With our approach we show not only that the most recent prediction is not necessarily the most accurate, but also that the iteration of the forecasting methods (that is the probabilistic approach) enables to have a standard deviation, that is basically a confidence and a probability distribution. R: Line 111-113: this is the heart of the paper. I think you need to develop this and make a small flow chart with graphs to explain your procedure. A: thank you for the suggestion. R: Line 124- 133: you need to give more information about the assumption of these three equations, which will be helpful for the discussion. A: the assumptions of these equations are the presence of the tertiary creep and the absence of external influencing factors such as rainfall (as stated by Voight 1988, 1989) although we showed that even in the presence of external factors reliable predictions may still be made. R: Table 1: for the mechanisms, you must probably refer to a classification Hungr et al., 2015 or Varnes and Cruden (1996). Figure 2: improve the quality of graphs not simply from excel: : : Figure 3: improve quality remove the second box. A: these will be changed in the text. R: Lines 190-197: unclear if Tf must be known? A: Yes. See one of our previous comments. R: Line 199: use PI for predictable Index instead of Pi which give the impression of a probability. A: Agreed. R: Lines 249-251: this is not an argument because with an oscillating process it will always have something very close to the Tf which can be better before collapse. A: this conclusion seems obvious only after that we have demonstrated that predictions often oscillate around the actual time of failure. On the other hand, Rose and Hungr state that only more recent forecasts should be considered, without acknowledging the whole trend. This is one of the main differences between a probabilistic and a deterministic approach. R: Line 262-263: as it is presented the predictability index need the knowledge of Tf (see lines 190-197) A: Yes, as explained above.

Sincerely, Emanuele Intriari

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