

Interactive comment on “Analysis of slope processes in the Vallcebre landslide (Eastern Pyrenees, Spain) by means of Cross Correlation Function applied to high frequency monitoring data” by Marco Mulas et al.

Anonymous Referee #1

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General comments

The paper deals with the statistical analysis of time series collected in three boreholes of the lower part of a deep-seated slow-moving landslide located in Spain (Vallcebre landslide). The authors investigate the cross-correlation among the series of rainfall, piezometric depth and displacements, in various combinations and comparing measurements of the same variable in the different boreholes. The paper is generally well written, though there is a mistake in the organization of the results section (see details “specific comments”). It certainly fits within the scope of NHSS. The degree

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of novelty of the presented material is not so significant, since monitoring of the Vallcebre landslide has been presented in other papers (Corominas et al., 2005) and the cross-correlation technique is standard statistical exploratory tool. The novelty of the paper may be represented by: a) the presentation of data for other periods that those presented in other papers b) the use of cross-correlation function on these series to investigate the interdependence between the measured variables. Nevertheless, one main issue in applying (linear) cross-correlation techniques is the underlying assumption of linear correlation, which is not always valid in the case of the paper. I conclude that the paper may be submitted only after mayor revisions, provided substantial modifications following the specific comments given below.

Specific comments

Introduction: Literature cited in the paper may be enriched (for instance, for the early warning and the modeling part – first paragraph of introduction, P1 L15-19) P1 L20: Cross-correlation is a quite standard statistical tool (see Handbook of hydrology, Salas, 1993 - this must be cited)

P4 L20: Please provide more details on how the confidence lines have been determined (statistical significance threshold), i.e. the formula used

P4 L23: Why a 5% threshold has been chosen, and not another one? The authors should possibly investigate if statistical tests aimed at verifying the significance of cross-correlation variation do exist. Though this issue may significantly change the time-lag interval around the maximum CCF value, the 5% threshold may be however accepted. What I just ask to the authors is to possibly justify this value and to verify the existence of above-mentioned statistical tests

P5 L15-20: “it may be noticed that in some time series combinations the maximum CCF values are quite low”. This is probably due to the presence of non-linear (instead of linear) correlation among some of the variables. In fact, if one thinks to the Richards’ infiltration equation linking rainfall to piezometric height the relationship between this

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pair of variables is non-linear (e.g Iverson, 2000; D'Odorico et al., 2005; Baum et al., 2010; Peres and Cancelliere, 2014, Bogaard and Greco, 2015, and references therein). On the other hand, thinking at the infinite slope factor of safety FS formula (see again references above) the relationship between piezometric depth and displacement may be expected to be linear (this however assumes a linear relationship between FS and displacement). So what I expect is: non-linear correlation for rainfall-piezometric depth, linear for piezometric depth-displacement. In fact, this is reflected on the values of max CCF reported in tab 1 and tab 3 respectively. The authors should make scatter plots of one variable against the other at various time lags, in order to assess whether or not the statistical dependence between variables is LINEAR or NON-LINEAR. In the second case the authors should apply: or a transformation of the variables, or a non-linear (cross) correlation analysis. This is a crucial issue that the authors need to address for publishing the paper

Fig. 2: Significance of statistical analysis may be improved by adding other data. The data presented in the paper cover the years 1999-2002. From papers by the same authors it seems that other data do exist (e.g. Corominas et al, 2005). If this is the case, why not add these data to the analysis?

Sect. 5: Paper organization mistake in the Results section: subsections report results relative to a variables combination that is different from that declared in the subsection title. The discussion of rainfall vs piezometric depth is missing. In detail: title of 5.1. should be rainfall vs displacement, 5.2 piezometric depth vs displacement, 5.3. displacement vs displacement, 5.4. piezometric depth vs piezometric depth. 5.5 is a repetition of 5.4

Discussion (Section 6): Some of the conclusions seem to be not directly supported by the paper results, and are a rather subjective interpretation of the authors. Please better link discussion to results, and explicitly declare what should be assumed as a subjective/reasonable interpretation of the authors

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P8 L16-19: please explain better the "second mechanism"

Fig. 2b: is the plot of piezometric depth for S4 correct?

Technical corrections

P2 L6 perhaps replace "monitoring data" with "landslide-related variables" P2 L11: first 950 m then 1250 m; it should also be specified that elevation is measured a.s.l. (above sea level)

Is there a specific reason why borehole numbering has to be S4, S2 and S9? If not, why not renumber as SL1 SL2 and SL3 (where L indicates "Lower Unit")?

Tables 1-5 replace "lap" with "lag"

P10 L13 remove "350"

References (of review):

Baum, R. L., Godt, J. W., and Savage, W. Z.: Estimating the timing and location of shallow rainfall-induced landslides using a model for transient, unsaturated infiltration, *J. Geophys. Res.*, 115, F03013, doi:10.1029/2009JF001321, 2010.

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D'Odorico, P., Fagherazzi, S., and Rigon, R.: Potential for landsliding: Dependence on hyetograph characteristics, *J. Geophys. Res.-Earth Surf.*, 110, F01007, doi:10.1029/2004JF000127, 2005.

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