

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 #2

Received and published: 23 November 2016

The paper presents a statistical analysis of monitoring observations at the Vallcebre landslide for a time period not already published in (the many) other manuscripts documenting the behaviour of this landslide.

The novelty of the approach and findings is not significant for several reasons: - linear correlation is applied which is extremely questionable for geological processes highly influenced by non-linear relationships and transients? - the authors filtered the data applying signal-to-noise methods to remove instrumental errors – what is the effect of this SNR ? how many data were removed and what is the influence on the correlations?

[Printer-friendly version](#)

[Discussion paper](#)



- seasonal patterns are observed in the time series, and seasonal detrending should be applied before applying CCF – it is not very clear how the authors pre-processed their data. - the observation time series are also questionable. For instance, it has been demonstrated by several authors that effective rainfall is better correlated to piezometric variations, than net cumulative rainfall. What is the argument of using net rainfall for the analysis? Further, snow might impact the water budget. Did the authors consider the possible additional input of waters on the slope? Further, the piezometric depths should be transformed in hydraulic heads or better in pore pressures above the slip surface for a consistent analysis.

Further, the authors should discuss the characteristics of the studied period regarding the long-term evolution of the slope. Is the period 1999-2001 representative of a low/high geomorphological activity of the slope, or a period of interest because many data/sensors were available? Some justification is needed especially because the approach could be tested on the complete monitoring dataset available for the landslide (at least for some combinations of parameters such as rain and displacement). This would possibly give more significance to the work and reveal some changes in the behaviour in time.

The discussion section is weak. I would like to have a discussion on the significance of the time lag statistically calculated by the authors with regard to the many hydrological models that were applied on this slope.

I conclude that the manuscript has to be rejected for NHESD.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., doi:10.5194/nhess-2016-253, 2016.

[Printer-friendly version](#)[Discussion paper](#)