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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Analysis of sea cliff slope stability integrating traditional geomechanical surveys and remote sensing" *by* S. Martino and P. Mazzanti

Anonymous Referee #1

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The manuscript aims to study the stability of a coastal rocky cliff by merging information from topographical and monitoring measurements. Therefore, the subject of the manuscript is potentially interesting for the journal. After reading the abstract and the manuscript the reader expects something very different from what is presented, both in terms of data analyses, novelty and application of techniques.

Introduction: the intro is quite general suggesting a potential interest for tsunami but there is really no need to discuss, present and mention this because the size of the proposed failure mechanisms does not seem to have anything to do with tsunami generation. The aim of the study is to provide guidelines for designing a monitoring network

but it seems to me that the authors fall really far from it. This is even more true when they talk about failure precursors and strain effects (??) The adopted methods are now very commonly used (so they cannot be defined as non conventional) at many sites and the authors seems almost to suggest that this type of approach is new or of extremely recent application. The sentences about decreasing risk with cliff retrogression are not supported by any observation The wave data presented at the beginning are completely neglected in the following. If temperature oscillations are so important why no description of the variation? Only 17° are cited at the end of the manuscript and this does not seem to be a really large value Why and how they can be so important?

Landslide susceptibility: there is no clear assessment of susceptibility in this work. When someone talk about the geomechanical characterization it is enough to say according to ISRM standards without going into too much detail

Terrestrial laser scanning, ground based radar interferometry (here called TInSAR for what specific reason it is difficult to understand) and use of infrared thermal cameras are well established. The same can be said for all the geomechanical standard analyses for which standard approaches and codes are used.

Geomechanical details relative to spacing, persistence etc are not described or better discussed and it is difficult to understand the geometrical characteristics introduced in the analyses (e.g. size of wedges). It is unclear how the orthogonal joint sets should develop because of some supposed and not suppoorted deformation within the rock mass.

Weathering ...more intense or intensively... seem to suggest that it occurs more rapidly. I do not think this is true and anyway the authors do not present any data or real sound explanation about this and the depth of the weathering within the cliff or behind the cliff face. Weathering: everything seems related to eq. 5 and its use. At the same time almost nothing is said about the real weathering, its variability etc.

The description of the toppling and wedge type of failure is confused, and seems also erroneous for the toppling under pseudostatic conditions. I tried to derive this equation from the moments equilibrium but it doesn't seem to work the way it is presented. Water pressure and its contribution to instability is not clearly introduced and described. The block size is unclear and no real zonation is presented, just some data in tables but they even do not show big changes in size.

Description of the Barton empirical equation and of wedge stability eqs. is superfluous.

The use of TLS techniques is poor and unclear. Different codes have been used but only a poor description of the results obtain through one of them is presented. AGain this type of approach is quite standard.

GB-InSAR technique is well established, very good results are presented in the literature and in practical works. No references are presented about this. The position for the survey is clearly unfavourable looking at the cliff laterally, so being able to see only a very minor component of movements. 2 days of campaign with a resolution as the declared one could not show any real displacement, because in general toppling and similar failures in rock needs extremely small displacements probably not measurable in 2 days and in the worst geometrical condition. The lateral view and the presence of rocky spurs is also making more problematic the measurements at the spur limits. So something really new and a sound data discussion should be presented.

Fig. 9 c) the red point shows a displacement of about 1 mm every 2 days: if this is a rock block going to topple or to slide it seems really too large. Fig. 9b) what does it mean to plot data using a -10 - +10 mm ? everything is green -2 - +2 this is really not useful, does not add any information and does not support any conclusion about instability, its evolution and the validity of the monitoring approach What is the accuracy and precision of the measurements? How does they compare with measured displacements? The authors talk about a cyclic component. Did they carry out an atmospheric correction? And if yes, how?

Anyway, at the very end of the manuscript, it is unclear how TLS, GB-InSAR and In-

frared images have been used or resulted useful to improve standard slope stability analyses.

The authors start then a discussion and conclusion in which they make general remarks about stability and monitoring without really novel suggestions and very weak connection with the presented work.

References: references are incomplete about the topics: a lot has been done on rocky cliff surveying by TLS, GB-InSar, etc. As well as for geomechanical characterization. In general, too much self citations especially regarding these topics.

English language need some important corrections using suitable terms.

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