

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

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The Authors wish to thank the reviewer for his useful suggestion clearly devoted to improve the submitted manuscript and the related figures. The Authors' idea is to generally accept the proposed revisions. In the following we annotate some consideration/replies on more specific suggestions by the Reviewer #2:

- 1) we will reduce the abstract length
- 2) we accept the Reviewer#2 suggestion
- 3) we accept the Reviewer#2 suggestion

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4) since a similar suggestion was also proposed by the Reviewer #1 we will avoid the use of the term “unconventional” referred to the SAR and the TLS approaches in the revised version of the manuscript

5) we accept the Reviewer#2 suggestion

6) we accept the Reviewer#2 suggestion

7) in the revised version of the manuscript we will discuss in more detail the peculiarities of a sea-cliff monitoring with respect to mountain area. This is one of the topic this paper would be focused on. For this reason we accept the Reviewer#2 suggestion.

8) we accept the Reviewer#2 suggestion

9) we accept the Reviewer#2 suggestion

10) we accept the Reviewer#2 suggestion

11) we accept the Reviewer#2 suggestion

12) we accept the Reviewer#2 suggestion

13) we accept the Reviewer#2 suggestion

14) the range resolution value of 0,5 m was provided in the manuscript. As it was not immediately identified by the Reviewer we will improved the sentence structure in order to make it more clear in the revised version of the paper.

15) we accept the Reviewer#2 suggestion

16) on the basis of other experiences on this type of application we will discuss in more detail the processing methods we used for detecting such small movements. Furthermore, we will suggest some reasonable hypothesis for both the a permanent and cyclic displacements higher than 1 mm in a jointed rock mass, in view of the presented case study. Furthermore, we will also try to add some comments on the locally expected cyclic joint deformations on the basis of measured thermal variations

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both at daily and seasonal level.

17) we accept the Reviewer#2 suggestion by improving the part of the paper regarding the IRT technique

18) we agree with the Reviewer#2, in effect our intention was to discuss on criticism and possible improvements in case of sea cliff slope monitoring. In the revised version of the manuscript we will avoid to dedicate a section to the perspective of our project and we will focus more on a discussion of the criticisms and the improvements that we think are necessary for a sea-cliff integrated monitoring system. In this regard we also proposed in the reply letter to Reviewer#1 to change the present title of the paper in the form "An example of integrated use of field and remote surveys for a stability analysis of a sea cliff slope".

19) we accept the Reviewer#2 suggestion

20) we will add some considerations to justify the use of a pseudostatic approach to such a case-study. Anyway the main reason of this choice are: 1) no local accelerograms are available for correctly using a pseudodynamic (i.e. Newmark) approach ; 2) since a jointed rock mass represent a discontinuous system it is possible to identify characteristic volumes of blocks isolated by joint sets and it is reasonable to apply to the barycenter of this unstable mass a destabilizing force to evaluate the stability conditions of the considered rock block, i.e. to assume a limit equilibrium assumption and to consider an elastic-perfectly plastic rheological behavior for the rock block.

21) in the revised version of the manuscript we will improve the discussion on both SAR and IRT techniques, especially focusing on opportunities offered by their use and limitations encountered.

22) we accept the Reviewer#2 suggestion. In the revised version of the paper we will add some comments in the conclusions about landslide induced tsunami risk on the investigated area based on the results inferred from the slope stability analysis.

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23) we will edit the caption as suggested by the Reviewer#2

24) we accept the Reviewer#2 suggestion

25) we accept the Reviewer#2 suggestion

26) our present idea is to not delete IRT as it represents a possible integrative technique for sea cliff monitoring. Hence, in the revised version of the manuscript we will provide a more extended discussion on achieved results. However, it is worth noting that In the specific case of Mt. Pucci sea cliff we did not perform a long term continuous IRT monitoring suitable for achieving reliable time series of measured data. Anyway, we will provide more exhaustive images (with both absolute and differential temperatures) that have been used mainly for a spatial assessment of the rock cliff thermal behavior.

Regards The Authors

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