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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

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Response to Anonymous Reviewer #1

The Authors would thanks the reviewer for his interest to the paper, testified also by his so rapid revision (less than 1 day after the publication on the on-line discussion). Taking into account all his comments, first of all we would clarify that probably the spirit of the work was not be completely understood by the Reviewer, or not enough well explained in the paper. As a matter of fact the reviewer asks to us to give solutions and responses to problems and technical performances which are just the topic of our

criticism. In this regard, we underline that the aim of this paper is just to point out both "critical" features and "perspective" (as declared in the specific paragraph of the paper 7.2) for future improvement and refinements of the presented applications, specifically devoted to the sea cliff slope stability analysis. Nevertheless, instead of these original misunderstanding, some of the comments by the Reviewer can be useful to improve the overall quality of the paper and make it more clear. Therefore, as soon as we will receive all the comments by other reviewers (i.e. when the editor will communicate to us the final evaluation end of the revision process in the on-line discussion session) we will submit a revised version of the manuscript which will try to answer the questions and comments of the reviewer.

Coming into detail, first of all, based on our knowledge, integration of several ground based remote technqieus and direct surveys for studying sea cliff slope stability are not so "common" and/or "conventional". Anyway, we will be grateful to the reviewer if he can provide some suggestion to fill our gap. Furthermore, also the concept of "standard approach" should be better defined: what we intend for "standard" in case of experimental (i.e. not already completely tested) approaches? Coming to the specific comments of the Reviewer we list in the following our replies: - if the title provides an "unsatisfactory perspective" to the reader we could re-title the paper as follow: "An example of integrated use of field and remote surveys for a stability analysis of a sea cliff slope"

- in the abstract we declare the performed measurements and slope stability analyses, and we refer to the results that are reported in the text and in the tables. Moreover, we declare that "the integrated techniques allowed us to achieve a comprehensive and affordable characterization of the main joint sets on the sea cliff slope". . ..this is actually demonstrated in the paper! At the end of the abstract we list specific results deriving from the different remote survey techniques applied to our case study; these results exemplifies the performance of such techniques applied to a sea cliff slope (which is a quite different and more complex environment to be assessed than traditional landslide slope), hence, from our view we don't write anything that was not reported/discussed in our paper.

- In the Introduction paragraph we cited the tsunamis as it is generally intended that a tsunami wave could be considered also for a rock block falling in water, i.e. it is not necessary to expect for giant sea waves but also a sea wave due to the block impact which involve a tourist or a boat visiting the coast (especially in the case of a natural park such as in the proposed case study) can be regarded as dangerous. In this regard, it is worth stressing that the commission for this study was just given by the public manager of the Gargano natural park exactly for possible falling of rock blocks in the sea in front of the cliff slope which is usually visited by tourists during the summer season.

- Ee agree with the Reviewer that the sentence in which we propose to provide technical guidelines for designing monitoring systems devoted to alert could seem too ambitious . . .to avoid misunderstanding by the readers in the revised version of the paper we will change this sentence in the form "encouraging guideline to. . ."

- We could discuss on the term "conventional" and make philosophy on the "conventional methods" or on "conventional approaches which use not conventional methods" or "conventional techniques used in a non conventional way" or more "conventional techniques used for a non conventional application" . As stated above, we don't know so many papers presenting integration of several ground based remote sensing techniques for analyzing coastal cliffs, therefore, we would be grateful to the Reviewer if he can kindly provide citations/references of scientific works so that we could consider them for enriching our discussion in this manuscript.

- the hazard distribution is linked to a conceptual model of cliff slope evolution. . .we expressly use this term in our manuscript. Actually it seems to us that, due the worldwide experience reported in literature on sea cliff slope evolution (see references reported in our paper), the conceptual spatial zoning of the hazard related to gravitational instabili-

ties and connected to a sea cliff evolution represents an objective deduction. Based on this objective deduction it is possible to discuss/propose/plan monitoring and investigation systems more adapt and more adequate to manage the natural risk associated to the different evolutionary stages. . ..as to say, a doctor knows (based on the field experience) the possible evolution of a pathology and plan analyses and therapies based on this evolution process. So the here focused problem is not the evolution of the sea cliff slope, but the perspective to associate it to a monitoring or investigation strategy (see paragraph 7.2).. We intended that the aim of a journal like Natural Hazards and Earth Systems Sciences is just to propose methodological approaches and to reinforce the connection between the geological knowhow (i.e. evolution of the natural processes) and the available techniques for stimulating applications and experiments for natural risk mitigation. Once again, we think that the spirit of the paper was misunderstood by the reviewer in this regard.

- on the basis on our experience in this field (that we're carrying on by many others laboratory and field tests) 17° can be a not negligible value (especially if this stree is applied for a long time). Anyway we really appreciate if the Reviewer can provide us papers of works stating his sentence

- The concept of landslide susceptibility was split by the scientific community in two different features (see among the others the paper by Frattini et al., 2006 on Geomorphology http://dx.doi.org/10.1016/j.geomorph.2006.10.037): 1) spatial susceptibility which is intended to be a mapping of area which could be affected in future by some natural process (in case of landslide processes this concept is properly adapt to both first time and re-activation landslides); 2) physically-based susceptibility which is intended to be a quantitative evaluation of the degree of stability depending on boundary conditions and/or changing in physical parameters, i.e. a sensitivity analysis. Anyway, in order to avoid misunderstanding, in the revised paper we could substitute the term "susceptibility analysis" with the term "stability analysis" and the term "susceptibility" with "proneness to".

- TinSAR (e.g. Terrestrial SAR Interferometry) in some cases (like the one of our paper) is used as an alternative to GBSAR in the in the scientific community. Furthermore, we think that this term is more appropriate in a case like this one where other remote sensing techniques are discussed. For example Terrestrial Laser Scanner (TLS) is more common than Ground Based Laser Scanner! In order to avoid conclusion we decided to use the term "T" for Terrestrial for all the other techniques "SAR Interferometry", "Laser Scanner" and "IR Thermography". Anyway, in the revised paper we will add ad the beginning a clarification using the following sentence "TInSAR (also known as GBInSAR)". - Spacing is reported in Table 1 and Table 3, all the considered joints are highly persistent in the sea cliff slope as it results by the remote survey coupled with the direct one. The cited software "Wedge Failure Analysis" automatically computes the dimensions of the wedges by introducing joint attitude and spacing, as it results from geometrical constructions which can be analytically solved . Anyway we will specify this feature in the reviewed version of the manuscript.

- We don't discuss in this paper the origin of the joint systems. . ..we only report the information on the strata attitude. Anyway the origin of the joint sets in a tectonized area (see paragraph 3) should be regarded with respect to the local fault pattern (i.e. not only in terms of a gravitational genesis).

- In effect we don't intend the weathering to occur more sharply but we only considered different degree of joint weathering; anyway, in the revised version of the manuscript we will re-write the sentence cited by the Reviewer.

- As declared in the manuscript, we performed a sensitivity analysis to destabilizing actions/effects. . ..so we have no direct data on joint weathering but we only tested the "physically-based susceptibility" to failure in case of low-middle-highly weathered joints adopting the reduction ratio (i.e. eq. 5 cited by the reviewer) for weathering as proposed in literature. To avoid misunderstanding we could better specify that this study does not propose a deterministic evaluations or a probabilistic scenario for analyzing the sea cliff stability.

- about eq.2 it is derived based on the Goodman and Bray (1977) assumption by simply taking into account a horizontal pseudostatic action (see the chart in Fig. 8). As, under static condition, the topple on a inclined plane occur if: tan(beta)<tan(psi)ïĂă(if tan(fi)ïĂă> or < tan(psi)) to reports the equilibrium in critical conditions for a horizontal pseudostatic action we have to assume that: (tan(beta)+tan(delta_alfa))= tan(psi)ïĂăwhereïĂătan(delta_alfa)=ky. So, it is possible to derive that tan(delta_alfa)ïĂă= abs(tan(psi)-tan(beta))=ky. Anyway, we can report more explicitly these mathematic steps in the reviewed version of the manuscript.

- It seems to the Authors that the role of water as a destabilizing action is clearly declared in the following sentences : "The static action due to water filling of the joints was taken into account by assuming distributions of isotropic stresses all around the block; such distributions were integrated along the joint surfaces to compute the incremental lateral forces exerted by the water. By increasing the water level within the joint, the critical conditions for the block equilibrium were determined, indicating a critical value of the water-height (Zwcr) that must be assumed for each wedge geometry [. . ...]."

- In the reviewed version we will avoid to report the Barton and Bandis empirical equation and the Hoek and Bray equations as suggested by the Reviewer.

- For the "concept" of standard (here referred to the TSL techniques), please see the sentences reported above.

- About the Reviewer's comment : "GB-InSAR technique is well established, very good results are presented in the literature and in practical works. No references are presented about this.", we stress the concept that here we propose an application of the cited technique for a sea cliff slope. We are very conscious that GBInSAR (say also TInSAR) is now used in several applications for landslides and that several references are present in literature (as some of them comes from the authors of the paper!) but very few we know about GBInSAR (say also TInSAR) applications to investigation of rockfall issues from see coastal cliffs (that is the specific focus of the paper). Anyway, we will be grateful to the Reviewer if he can provide some citations at this specific regard (coastal rock cliffs). Please consider that we do not cited also our paper about GBInSAR for landslides as we considered them not relevant for this paper!

- We agree with the Reviewer that 2 days are not sufficient to highlight permanent displacements on rock masses, nevertheless here we search for cyclic deformations (i.e. not necessarily inelastic ones) referred to daily changing weather parameters by using a non conventional and dedicated processing approach. Anyway, we will better introduce this feature in the revised version of the manuscript.

- We can obviously provide more information on the accuracy and precision of the measurements and, of course, of the performed corrections for the atmospheric noise. We will try to detail these features in the revised version of the manuscript, even if probably a long dissertation on this topic could not fit with the paper structure and could be tedious for a generic reader of this journal.

- It does not seem to the Authors that they derive so general results in their Discussion and Conclusion paragraphs as they reported a summary of the experienced application in the Mt. Pucci case study, providing ideas/proposals for future applications in the same site. A discussion on more general topics is reported in the Future Perspective paragraph which is, for this reason, distinguished from the Discussion and Conclusion ones. In this paragraph we describe a methodological path which starts from a conceptual model of sea cliff evolution, select a kind of approach for managing the risk and, consequently, adopt a monitoring solution for providing security information. We applied this discussion approach to our case study (i.e. sea cliff slope) but is our opinion that these considerations should be more diffused and debuted in scientific contests; due to the so numerous technical solutions existing for managing the problem nowadays the problem is “how manage the solutions”! Please, would you consider the relevance of this criticism! We are of the idea that this journal, due to its so focused topic, offer a very good opportunity for proposing these features and also for discussing them: actually we’re just using this opportunity thanks to your comments in the on-line

discussion session.

- We will check the references to reduce self-citations and to provide some more citation. Anyway, it is worth stressing that in our knowledge application of TLS, GBInSAR for sea cliff slope are not so diffused, and other applications to rocky cliffs and landslides are not probably so relevant for this paper as they deal with very different environment and, therefore, different issues.

- Before the submission to NHESS the manuscript was reviewed by the American Journal Expert team for a careful revision of our original text. This is testified by a certificate of language revision dated on 3 May 2013 and it results at the AJE verification key: 33C8-4355-9A30-0983-13DA (the certificate is attached as supplement .pdf file)

Regards.

06 August 2013 the Authors

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/1/C782/2013/nhessd-1-C782-2013-supplement.pdf

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