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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 #4

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OVERVIEW The proposed research demonstrates an application of the combined usage of field and remote-sensing based techniques to investigate the stability of a sea cliff. This is a fairly new approach for investigating sea cliffs and offers the possibility to collect data at locations that may be difficult or hazardous to access in the field. The paper is interesting and focuses on Coastal erosions where publications on state-of-the-art-monitoring are scarce. It therefore fits well within the scope of NHESS.

However, as pointed out by reviewers 1 and 2, a substantial effort is necessary before the study can be published. Along with this review, the authors may have a look at the annotated PDF of the manuscript. All main concerns shall be briefly addressed below.

This is meant as constructive criticisms and aims to improve the quality of the paper, rather than criticizing the scientific work of the authors.

SUMMARY As pointed out be other reviewers, the main objectives of the manuscript are not clearly stated. From the authors response to reviewer comments it is apparent that the intent is to demonstrate the integrated use of field and remote-sensing based investigations on a sea cliff. The manuscript does not lead towards clear technical criteria suitable for failure forecast or early warning systems. With that in mind, section 7.2 is neither based on presented data/analysis nor does it contribute to the overall scientific value of the manuscript. It should be shortened if not deleted as it reads like the introduction to a publication on the design of failure forecasting strategies.

The authors show a significant effort towards an integrated monitoring of an exposed rock face but fail to sufficiently discuss the data. In any scientific publication it is important that the process that leads an author to draw conclusions is coherent and reproducible. It would be beneficial to show how TLS based data corroborate with field measurements, rather the datasets are merged and presented in a figure. Tables 1 and 3 show the results are similar but with a few sentences it could be elaborated beyond what is delivered on page 3700/16-22. For example, "The comparison between Tables 1 and 3 yields an agreement between remotely-sensed and field based data within +/- 10%. Deviation is apparent in joint set ... because". Furthermore, these sentences should go towards a discussion, since is to demonstrate the benefits of integrating different investigative techniques.

Given the objective of an integrative approach, section 7.1 should be tied in as well. Both IRT and InSAR investigations are not well discussed. As reviewer 1 pointed out, the technical details (set-up, software analysis, processing standards) are not described in detail. What is the accuracy for these methods? How reliable are the data? Section 7.1. is vague, speculative and only based on the authors personal interpretations. The authors should demonstrate how the data can be interpreted based on evidence a reader can follow. In other words, a discussion of the values and figures is

highly recommended. Also, the paper would benefit if the techniques were combined. For example, do the IRT measurements show anomalies at the same locations where continuous/diurnal displacements were measured by InSAR (see Figure 9)? Joints sets measured in previous sections could be pointed out on Figure 10.

In essence, if the authors aim to present an integrated approach, the entire process of integration must be presented in a coherent and reproducible way. This will help readers facing similar challenges in the field to apply an analogous methodology as proposed in the manuscript, i.e. if the site is inaccessible but remote-sensing is feasible.

Overall the discussion is a weak point. A good discussion aims at presenting and disseminating the data to the reader. What was measured, what does it mean, how can it be applied to other case studies? The issue of self-citations was previously raised. In the discussion, a publication must address what other researchers did to study difficult terrains, such as exposed rock faces. There, is a plethora of studies that shows integrated monitoring using the same techniques. In the proposed manuscript this would not require lengthy elaborations but pointing out similarities and differences, i.e. in the case of monitoring an alpine rock cliff. Such an approach greatly helps when conclusions/interpretations must be drawn.

Here is an example how this can be done: “The results of our IRT measurements can be used to identify main joint sets observed in the field by using temperature anomalies, as demonstrated by XYZ (2011). Our field and TLS measurements confirmed this, as the main joint set (J1/J2) appear as negative temperature anomalies. (comp. Figs. XYZ1 and XYZ2)”.

Again, the data analysis and most of the work are done. In fact, at several points throughout the manuscript the reader finds paragraphs that aim to discuss the results. It just needs to be consolidated and presented under the consideration presented above.

ABSTRACT The abstract is too long and could be simplified.

LANGUAGE AND GRAMMAR The language is overall acceptable, though a review by a native speaker is recommended before publication. Some sentences were not clear and are highlighted in the annotated PDF.

FIGURES The figures could be improved a little (Refer to annotated PDF). This mainly concerns readability, are helping the reader to interpret the figures by better referencing locations across multiple figures, and by marking crucial features, i.e. recent rockfalls. IRT and InSAR would benefit if a few crucial joint sets were marked. The stability analysis points out that some joint configurations maybe more prone to failure than others. Could such a configuration be identified in the in InSAR/IRT figures, i.e. where you measured continuous creep?

TERMINOLOGY A lot of confusion is introduced by the usage of terminology. This was already criticized by other reviewers but a few more comments should be made to illustrate the case. If terms such as classic, traditional, unconventional are used they must be defined or cited as such. It may be better just to use field-based geomechanical surveys (on-site measurements, field investigations etc.) vs. remote-sensing based investigations (TLS, InSAR, IRT). In the manuscript the term scan lines finds usage most likely referring to field measurements. However, this term can easily be confused with TLS investigations which are sometimes called Laser scanning in the literature. “Remote sensing point” is often used in the manuscript as a standalone term. It would be better to be more specific, i.e. by referring to this points as “the location of TLS measurements or TLS survey point etc.”. Regarding the issue of TInsar vs. GB-InSAR, the former seems to be used predominantly in publications originating from Italian universities. It is possible that the term was coined there but it seems that the internationally more used form in GB-InSAR. This can easily be confirmed by using both acronyms in an internet search. GB-InSAR yields publications from Canada, USGS, Norway, Italy while T-InSAR is mostly used in publications by the authors themselves. This is not to say that it is wrong, rather if a more regionally

used acronym is used it should be clearly defined in publication. The same is valid for PGA and SF/FS . Surely, a reader with geotechnical background is familiar with these terms, but it is good practice to define acronyms.

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/1/C1343/2013/nhessd-1-C1343-2013-supplement.pdf

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