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Comment on nhess-2021-16

Alan Trenhaile (Referee)

Referee comment on "Evolution of the Tazones Lighthouse slope (Cantabrian coast, N Spain). Multidisciplinary monitoring between 2018 and 2020" by María José Domínguez-Cuesta et al., Nat. Hazards Earth Syst. Sci. Discuss.,
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This is an interesting paper which is most notable perhaps for the frequent (monthly) monitoring of slope movement, and associated opening of fissures, on top of a cliff in northern Spain. It will make a useful contribution to the coastal literature once some changes have been made, the most important of which is to the English, which at present is rather poor. I have used the abstract in an attached copy of the manuscript to provide an example of the amount of work that needs to be undertaken on this aspect of the manuscript, but given the problem is throughout the paper I have limited most grammatical comments to only this section. I am also opposed to the results and discussion being combined in a paper. The results simply provide data, statistical analysis, and other qualitative or quantitative findings from a research project, whereas the discussion is used to discuss interpretations, implications, models, etc derived from the results. I find the Results and Discussion section to be poorly organised. Some of it belongs in the study area section and others in a Discussion rather than results section. I would begin this section with the displacement data from which everything else follows (interpretations, relationships with geology etc).

I also wonder about the assumption that mass movements in this area are generally fairly small. Surely, despite the lack of historical records at this particular site, the rapid opening up of some of the fissures must lead to a large slope failure. Depending on the depth of the failure surface this may only involve shearing of material from the upper part of the cliff but even this, given minimum depths and the distance of the fissures from the cliff edge, would involve a huge amount of material. I would like to see more discussion of this possibility and also an explanation of why there has been such a dramatic increase in the width of some fissures in the last few years.

There is also an implication in places in this paper that mass movement is the main mechanism rather than marine processes. Steep sea cliffs occur along coasts for one reason, which is oversteeping and resulting instability due to marine (usually waves in temperate regions). Without marine processes there would be no steep cliff and no mass movement. Precipitation may trigger mass movements (that is determine when they occur) but the conditions for their occurrence is determined by the sea. Incidentally, the

present dominance of blocks on the beach (relative to pebbles) is not evidence of mass movement dominance. If mass movement blocks were dominant here it would prevent marine erosion of an increasingly buried cliff foot. The cliff would then be essentially abandoned by the sea and would gradually become less steeply sloping and more vegetated. This is not happening - this is an active sea cliff with a steepness and internal geology that promotes a certain suite of mass movements.

I suspect you are quite right to relate mass movements to precipitation etc but there is also a possibility that wave activity may at least play a role, given that winter storms are also times when waves are highest. This can lead to enhanced undercutting and instability as well as generating vibrations in the cliff materials (eg. Thompson, C.F., Young, A.P., Dickson, M.E., 2019. Wave impacts on coastal cliffs: Do bigger waves drive greater ground motion? *Earth Surface Processes and Landforms*, 44, 2849-2860. and Earlie, C., Masselink, G., Russell, P., 2018. The role of beach morphology on coastal cliff erosion under extreme waves. *Earth Surface Processes and Landforms*, 43, pp.1213-1228).

The last point in the introduction claims that this research will "provide a solid knowledge base to perform predictive models of coastal retreat in future scenarios". Will it? In truth while I consider this paper to be a useful addition to the literature, there really isn't anything fundamentally new about the results (no new theory or model, etc). How then do you think it can help us to develop predictive models? Surely much of what is presented in this and many similar papers is site specific - strongly related to conditions at the study area. Please justify your statement in the conclusions.

Other comments and suggestions are listed in the attached annotated version.

Please also note the supplement to this comment:

<https://nhess.copernicus.org/preprints/nhess-2021-16/nhess-2021-16-RC1-supplement.pdf>