

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC2
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Comment on nhess-2021-16

Anonymous Referee #2

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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I was greatly looking forward to reviewing this paper being a subject of interest and some experience to me. However, the paper is very limited in scope and has a number of failings which fall short of the standard to be published in the journal:

- Limited scientific data is presented, basically several years of surface movement data, some rainfall analysis, and three joint measurements. Consequently there is insufficient site investigation to support the narrative which is somewhat speculative and confused in places. Even some of this limited data is poorly presented e.g. the joint data plotted on a stereo net is not provided.
- The site is compared to Holderness UK which is unfortunate because this does not provide a comparative analogue to support the observations at Tazones. The two sites are formed in very different geologies, the former Quaternary soft sediments subject to high rates of cliff toe erosion and episodic rotational failures. The latter being formed in Jurassic rocks that appear controlled by bedding and structural discontinuities.
- The authors present a short snap shot in time in the development of the landslide for which there is evidence of pre-failure as far back as 1984. The evolutionary setting in space and time is fundamental to understanding the causes, mechanisms and behaviour of the landslide; the authors have not done this. They could have produced a detailed geomorphological map of the coastal slopes and foreshore to set the landslide in context with the surroundings. They make no mention of the foreshore and the potential changes and contribution of erosion in the landslide development. What influence has the nearby groyne played in the exposure of the cliffs to high energy waves I wonder.
- Some considerable assumptions are made without site validation i.e. no ground investigation or testing of soils has been carried out or presented. This would be an expectation for a landslide investigation.
- The authors are confused about the landslide mechanism and reference to Holderness. It may have helped if they had referenced international landslides classifications such as Varnes 1988; Dikau and Brunsden 1996. Also, reference to other literature on Jurassic coast landslides e.g. Lyme Regis, UK. The dominant control is the sub-horizontal bedding which forms basal shear surfaces. The vertical cracks and joints form detachment surfaces. The discussion and narrative is rather speculative and not

supported by evidence with the exception of the movement and rainfall records.

- The conclusion that the landslide will evolve "without involving large volumes of material in a single episode" is simply wrong from the photo evidence, which clearly indicates potential for deep-seated landslides, which are episodic in time.
- The strength of the paper is the relationship between rainfall and ground movement. What is presented is consistent with other well investigated deep landslide sites e.g. Moore 2020 Undercliff, Isle of Wight. But the issue is this is a short snapshot in time and in no way can be used to corroborate the evolution of the site over longer time scales, in this case up to 3 decades.
- Finally, the standard of English would need to be improved for publication. The structure should also be improved, better introduction and reference to appropriate literature op.cit., expand the approach to set the site in context (geomorphology!); presentation of results section, discussion and conclusions. As it stands, the paper falls short of achieving this.

Please also note the supplement to this comment:

<https://nhess.copernicus.org/preprints/nhess-2021-16/nhess-2021-16-RC2-supplement.pdf>