EGUsphere, referee comment RC1
https://doi.org/10.5194/egusphere-2022-643-RC1, 2022

Comment on egusphere-2022-643

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Referee comment on "Earthquake contributions to coastal cliff retreat" by Colin K. Bloom et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-643-RC1, 2022

This is an interesting paper that specifically investigates the contribution of earthquakes to coastal cliff retreat. The research is welcome as there has been little written on this topic. Overall I find the methods to be well executed, the results are well illustrated and the paper is well written. My main challenge is in respect to whether the feedbacks between cliff failure and coastal erosion have been sufficiently considered.

Important framing for the paper is stated in section 5.3: "We hypothesize that, in the long-term, large landslide triggering events, for example earthquakes or storms, contribute disproportionately to cliff-top retreat while coastal erosion dominates background retreat at Conway Flat." This can be thought about a little differently from a coastal processes perspective. Over decadal timescales and longer, coastal erosion continually steepens the cliff toe preconditioning the cliff face for failure - some driver is needed for landsliding-driven retreat of the cliff top and this could be a heavy rainfall event, or an earthquake, or in some cases just gradual weathering and gravity. I wonder whether the paper is weighing up the relative importance of different failure event drivers, rather than earthquakes versus coastal erosion? I think this nuance is quite important. For instance, what do you think would be the effect of several large earthquakes in quick succession? Presumably the cliffs would fail toward a stable angle and further erosion would not be unlikely without basal debris removal and oversteepening? In this case, can you be sure that the long-term cliff retreat rate would be increased?

I became a little unclear on what exactly is meant by background retreat rates and long-term retreat rates. For instance, I agree with the point on lines 330-332, that in the long term the retreat rate is ultimately governed by the rate at which marine processes over-steepen the cliff, and I think this point could perhaps be strengthened by referring to some of the relevant modelling (e.g. Wolters and Müller, 2008). But later in the discussion (line 395) it is stated that "These values represent our best estimate of the long-term clifftop retreat rate at Conway Flat over the historical record and are, on average, c. 45% greater than the estimated background retreat rate excluding earthquakes." What exactly is meant by 'background retreat rate' and how does this differ from the long-term retreat rate. I wonder how it is possible to be confident that, had the earthquakes not occurred, that other mechanisms (e.g. rainfall) would not have stimulated failure (if the cliff were over-steepened by coastal erosion)? Eventually a cliff will become steep enough that it will fail, and while this may sound provocative, does it really matter, in respect to the long-term erosion rate, what the triggering event is? In other words, over a given time scale, is the fundamental pace of cliff retreat actually governed by the coastal erosion rate (i.e. the speed at which debris is removed and at which basal steepening occurs)? Clarifying this point would help in respect to several subequent statements. For instance, line 395, "Given the relatively low ground motion at Conway Flat during both the 2016 KaikÅ□ura earthquake and the 1951 Cheviot earthquake, it remains possible that stronger ground motion could result in greater single event retreat. In this case, strong ground motion could have an even larger influence on long-term cliff-top retreat." I'm not so sure about this. If stronger shaking resulted in bigger landslides, then the cliff would fail to a more stable state and more debris would be produced. These feedbacks regulate the long-term retreat rate (e.g. see Dickson et al 2007). And line 400 "excluding earthquake contributions will result in underestimates of long-term coastal cliff retreat over multiple earthquakes" - is this necessarily the case, given the points above?

Useful attempts to extend the work are provided in Equation 1 and the model illustrated in Fig 8. Is equation 1 assumed to be independent of the pre-existing shape of the cliff? If the cliff was already over-steepened, then the magnitude of shaking required to cause failure would be lower, correct? I found the model in Fig 8 interesting and I can see how the combination of high coastal erosion and low earthquake frequency means that earthquakes have limited influence on the retreat rate, but I'm not clear on how low coastal erosion and high earthquake retreat combines in the way you allude to, because I guess I am unconvinced that feedbacks have been accounted for (e.g. high sed supply to the coast and failure toward a more stable slope).

Other points to consider:

The introduction section is quite short. I think the potential implications of the research presented would be enhanced if the authors were to broaden the framing a little (e.g. some possible papers to consider below). However, the essential point re tectonic contributions to cliff retreat is well made in the introduction.

70 - label Conway Flat on Fig 1

box label the position of Fig 2 on Fig 1

108 - wording "have had limited to no anthropogenic modification"

The background section is lacking detail on uplift (if any) within the study site? This is important in relation to the level of wave action in relation to the cliff toe. Can some detail be added in this regard.

3.1 and Appendix A - can you provide a little more detail in regard to the appropriateness of using the 2017 DSM for historic photographs? As you demonstrate in the paper, there is significant erosion and landsliding between epochs, but it's not possible to have a DSM for each time period in the historic photograph record - to what extent is this accounted for in the uncertainty estimation?

175 - does the presence of dense vegetation preclude the possibility of cliff retreat - in other words, how were you confident there was no cliff retreat if you couldn't see the cliff because of dense vegetation? "where dense vegetation was present across all epochs of imagery and we were confident that no cliff retreat had occurred"

245 - can you be a little clearer with your wording regarding failure/no failure of the Greta Formation? Can you provide some numeric stats on how much of this section failed? This would be useful as a lot of the literature on soft-rock coasts deals with consolidated fine-grained rock as distinct to the (unconsolidated?) overlying delta deposits.

260 - the Medina fan deposit is 'more consolidated' - more than the Dawn fan delta deposits? Is there a way to characterise the degree of consolidation of these different deposits? It would be useful to put these erosion rates into context of other cliff erosion rates reported iternationally. You state that these more consolidated cliffs have eroded less - do you ascribe this to their degree of consolidation?

325 - I find this statement confusing: "it does appear that more indurated material (with assumed higher shear strength) in the lower cliff face may buffer the upper cliff face from wave action effectively reducing the background rate of cliff-top retreat (Emery and Kuhn 1982)". Are you trying to say that the presence of more resistant prevents over-steepening of the cliff face, and that this then manifests in slower cliff-top erosion rates? Can you provide some cliff-profile sections? These will presumably show that where the mudstone occurs the lower cliff is steeper (because of harder rock), in comparison to the sections where the fan deposits extend to the cliff toe. I think these profiles would help support the argument you are developing here.

5.2 - the section on sediment loss from the cliff-toe alludes to storms, and these may well be important, but variability in longshore sediment transport processes could drive much of the natural variability in beach volume - there is an extensive literature related to this. Is there any evidence to suggest that the 1960 tsunami runup resulted in significant sediment transport on beaches in NZ - not that I am aware of? On line 381 you more directly suggest that tsunami debris removal has occurred, but I don't think there is evidence of this?

360 - "Prior to 2016, most of the cliff face at Conway Flat was near vertical in many places (Figure 5), an indication of a combination of subaerial and marine erosion (Emery and Kuhn 1982)." Here Emery and Kuhn are implying a dominance of marine over subaerial processes.

379 - "Assuming little change in the background rate of coastal erosion between time windows" - here I think you are referring to the removal of basal debris, rather than cliff-toe steepening? Would it be useful for you to refer to beach erosion and cliff-toe erosion to distinguish what you are referring to when you say "coastal erosion"?

Possible refs to consider:

Hapke, C., & Plant, N. (2010). Predicting coastal cliff erosion using a Bayesian probabilistic model. Marine geology, 278(1-4), 140-149.

Hall, J. W., Meadowcroft, I. C., Lee, E. M., & van Gelder, P. H. (2002). Stochastic simulation of episodic soft coastal cliff recession. Coastal Engineering, 46(3), 159-174.

Walkden, M. J. A., & Hall, J. W. (2005). A predictive mesoscale model of the erosion and profile development of soft rock shores. Coastal Engineering, 52(6), 535-563.

Walkden, M., & Dickson, M. (2008). Equilibrium erosion of soft rock shores with a shallow or absent beach under increased sea level rise. Marine Geology, 251(1-2), 75-84.

Ashton, A. D., Walkden, M. J., & Dickson, M. E. (2011). Equilibrium responses of cliffed coasts to changes in the rate of sea level rise. Marine Geology, 284(1-4), 217-229.

Wolters, G., & Müller, G. (2008). Effect of cliff shape on internal stresses and rock slope stability. Journal of Coastal Research, 24(1), 43-50.

Dickson, M. E., Walkden, M. J., & Hall, J. W. (2007). Systemic impacts of climate change on an eroding coastal region over the twenty-first century. Climatic change, 84(2), 141-166.