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Comment on esurf-2021-44

Vincent Regard (Referee)

Referee comment on "Multi-objective optimisation of a rock coast evolution model with cosmogenic ^{10}Be analysis for the quantification of long-term cliff retreat rates" by Jennifer R. Shadrick et al., Earth Surf. Dynam. Discuss.,
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The paper entitled "Multi-objective optimisation of a rock coast evolution model with cosmogenic ^{10}Be analysis for the quantification of long-term cliff retreat rates" by Jennifer Shadrick and colleagues, reports on work aimed at understanding how the joint recording of rock platform topography and the cosmogenic isotopes (^{10}Be in this case) of the rocks that constitute it can provide a good description of the history of coastal cliff retreat during the Holocene. The work is based on a model of the evolution of the rock platform, associated with a module describing the enrichment in cosmogenic isotopes. This direct model requires knowledge of a number of variables, in particular the erodibility of the rocks (F_R), the wave dissipation coefficient on the platform (γ) and the weathering of the rocks in the intertidal zone (K). This model is run a number of times via an optimisation procedure based on the RMSE of the difference between model and data. This optimisation is proposed for two different English sites: Scalby (NE) and Bideford (SW), for which a rate of recession over time is produced, which shows a very clear correlation with the sea level rise rate. The discussion then turns to the combined effects of the 3 variable parameters.

I strongly support the publication of this work, although I have some formal reservations, which I express below.

Strong points

- Innovative inversion work, which advances knowledge of the problem. The tools developed will be available to the community, I hope.
- Good data
- The result in Figure 7 is excellent, although there is probably room for discussion.
- In the evolution of rock platforms, two erosive drivers are compared here that have never been compared before: erosion by waves vs. weathering in the intertidal domain. Indeed, this weathering has only been documented on the basis of laboratory experiments (Kanyaya and Trenhaile, 2005, Porter et al. 2010). This work is to be commended for advancing the comparison between wave erosion and weathering.

Weaknesses

- The description of the numerical process is difficult, not always understandable by an outsider (especially a non-numerician such as myself). There is also a mix of details (e.g. line 185-186) and very general considerations (e.g. description of the model of Matsumoto et al. 2016 without resolution, time step). I think the presentation of the methodology needs some work. The parameters a, b, c are not understandable by the text alone (Table 2 is needed). Some very long sentences are a bit complex to understand for a non-native English speaker.
- Two other parameters would have deserved to be considered as variable (i.e. not perfectly known): the sea level history, or the incident waves. I think it is a bit late to integrate them into this work, but it would be interesting to mention them, if only qualitatively.
- I still have a second order question: how to explain the group of points >180m from the cliff at Bideford: is there an expression on the platform explaining these points that stand out from the others?

Conclusion

I am very supportive of the publication of this work. I hope that my comments will help to improve it through moderate modifications, as well as open up perspectives for further investigations.

Other remarks

- Line 36-37. Premeillon et al. Esurf 2018 could be cited here.
- Field location. The authors should present the sites a little better. I suggest a photo of each site, especially so that the reader understands the influence of the geological structure on the morphology of the platform/cliff system
- Figures: the uncertainties shown by the shaded areas are unreadable. The colours should, for example, be reinforced.
- Line 198. More details on the model would be welcome: time step, spatial resolution. Which tide range did you use: the spring one, an average one?
- Part 3.3 is very technical, sometimes hard to understand.
- Line 357. Presentation of a, b, c rather obscure.
- Paragraph 362-375. Here the authors only consider the Holocene history. Is a reoccupation of an older platform possible? Is it possible to test this hypothesis?
- Line 386. I would add that this value of 20mm/yr is unrealistic.
- Figure 4: There is an error in the unit of the Cliff retreat rates. Change the grey (50%-50%) to another colour. Recall why there is cyclicity in the modelled ^{10}Be concentrations. At Bideford, the cosmo/topo data disagree from 180m away from the cliff. Do you have any idea why?

- Figure 5. The topo profiles need explanation: this is the current profile, the age corresponds to when the cliff foot was there, but not at the same elevation since there is a downwearing effect.
- Paragraph 495. Your conclusions in lines 499-501 are tremendous.
- Paragraph 4.3.1. Very important paragraph in my opinion. The results are that waves > weathering. The difference between the two processes is that weathering can sustain cliff recession for a longer period of time while the waves, which dissipate over the rock platform, eventually fade away. This has important consequences: anthropogenic sea level rise will necessarily be accompanied by an acceleration of cliff recession rates.
- Figures 10 and 11 are good, but why not provide the equivalent for Scalby?
- Figure 11: perhaps change the colour so that the two types of triangles are distinct?
- Part 5.2. Wave decay/Material resistance comparison. This is interesting, but the fact that a lower wave erosive capacity has to be compensated by an increased erodibility to achieve the same result is a bit trivial. On the other hand, I think we can go further. The dissipation of wave energy should decrease exponentially across the width of the platform. I imagine that the effect of a faster decay is not exactly compensated by an increased erodibility. For example, high dissipation with low resistance should favour the erosion of the outer part of the platform while low dissipation and high resistance should erode the inner part more strongly. It might be possible to discriminate between the two components. Furthermore I suspect that the best fit in figure 11a is not a straight line but a curved one.
- Line 799 "provided wave height decay rate (a) has adjusted accordingly" I am not so certain about that, refer to my previous remark.
- Lines 814-817. I fully agree with the authors.