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Earth Surface Dynamics Discussions

Interactive comment on "Controls on the distribution of cosmogenic ^{10}Be across shore platforms" *by* Martin D. Hurst et al.

Martin D. Hurst et al.

martin.hurst@glasgow.ac.uk

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Response to review

We are grateful to Dr. Mark Dickson for his positive and constructive comments, and extend our thanks also to Hironori Matsumoto for his contributions to the review. We welcome the inclusion of aspiring PhD students in the peer review process. Below we detail our responses to all of these comments, highlighting any changes we have made to the manuscript guided by the reviewer's suggestions. We have numbered the reviewer comments and they are coloured black, while our responses are coloured blue, and italicised where we are quoting directly from the revised manuscript.

1. This is a welcome addition to a rather slowly emerging literature on the application of cosmogenic dating to inform rocky shore evolution studies. The proposed model usefully extends the work described by Regard et al. (2012) and includes I think the majority of the factors required. In doing so, the paper highlights that the use of this method to determine erosion rates on rocky shores, while conceptually simple, in practice is rather more complex. Nevertheless, the lack of quantitative measurements of rates of change has been a long-standing limitation in our field, so efforts in the direction of this paper are welcome.

   We thank the reviewer for these positive comments and hope this work can be useful for guiding future applications of cosmogenic radionuclides in rock coast settings.

2. Below are some comments that the authors might wish to consider during any revision. Please note that these comments integrate thoughts from Hironori Matsumoto who is currently making further developments to a rocky shore profile evolution model (Matsumoto, H., Dickson, M. E., and Kench, P. S.: An exploratory numerical model of rocky shore profile evolution, Geomorphology, 268, 98–109, doi:10.1016/j.geomorph.2016.05.017, 2016.)

   Thank you for bringing this manuscript to our attention, we had already cited it in the manuscript. We thank Hironori Matsumoto for his contributions to the review, and more generally we welcome the consultation of PhD students in an open review process.

3. Can you clarify how the model considers platform slope? Does it calculate the cliff toe position with a fixed platform geometry, similar to previous tide-less models (Sunamura, 1975)? Perhaps not, because later, in section 5.5, there is reference to the gradient of the shore platform decreasing through time, as the platform widens. There is also reference to the emergence of a 'stepped' platform - what is the reason for this? Ultimately it is a little unclear how exactly the geometry

is handled. The text states that the model is similar to the existing models of Trenhaile (2000), Walkden and hall (2005), and Matsumoto et al (2016), which all consider tide and vertical components. It would be useful to expand and clarify similarities and differences in that regard.

We apologise that the details of the morphological development were unclear and are pleased to have the opportunity to clarify these points. There are two morphological models used in this study. The first is a steady-state model in which a constant shore profile characterised by a fixed platform gradient is translated landward through time, tracking the elevation of mean sea level. Therefore, in these steady-state scenarios, tides do not influence the morphological evolution of the platform. However tides still modify the production of ^{10}Be on the platform due to water shielding. In the second modelling approach, a dynamic morphological model is used, and it is this model that is broadly similar to Trenhaile (2000) and Matsumoto et al. (2016). The dynamic model calculates the point of wave breaking and therefore the width of the surf zone, which are dependent on the instantaneous mean water level which is set by the tides. Horizontal erosion at the water level is proportional to the delivery of wave energy (equation 1), parameterised as the height of the breaking wave once it has crossed the surf zone (equation 4). The platform is also eroded at depth below the water line, with the amount of erosion declining exponentially with depth. Stepped platforms can develop because the total amount of erosion integrated over a tidal cycle has two maxima as a function of elevation, one just below the upper tidal limit, and one just above the lower tidal limit (similar to Figure 1 in Trenhaile, 2000).

We have restructured the beginning of section 2 to be more explicit about the different approaches taken to morphological modelling:

"Cliffed, rocky coasts, are commonly fronted by shore platforms that have previously been classified into two types (Sunamura,1992). Type-A platforms are characterised by a gently sloping erosional platform surface extending offshore

beyond maximum low water. Type-B platforms are shallow gradient to sub-horizontal and terminate at their seaward edge at maximum low water through a scarp (Figure 2a). Numerical models of shore platform evolution have successfully recreated both of these endmember morphologies, but have revealed that there are a range of other possible morphologies in between, for example sloping platforms terminating in a scarp (e.g. Trenhaile, 2000; Walkden and Hall, 2005; Matsumoto et al., 2016)."

"Numerical models of platform evolution demonstrate that shore platforms and adjacent sea cliffs tend towards a morphological steady state, Under such conditions the morphology may reflect the combination of RSL change, tides and wave energy availability. However, the assumption of steady state retreat may not always be applicable (Dickson et al., 2013). We expected the style of platform evolution to be important for the distribution of ^{10}Be across a shore platform. Moreover, micro-erosion meter measurements of platform downwear suggest that downwear is not uniform across the shore profile, but tends to be faster in the upper inter-tidal zone and decline with depth (Porter et al., 2010), and to explore the influence of such a distribution on ^{10}Be concentrations, a dynamic morphological model was required."

"Therefore, two different morphological models are used in this study. The first assumes steady state evolution of the shore profile, such that coastal morphology does not change its form through time, and a constant cliff-platform geometry is translated landward through time. The second model is a dynamic shore platform evolution model similar to that of Trenhaile (2000) that can reproduce a range of platform geometries. These two approaches are described in more detail in the subsequent sections."

4. Did the authors consider prospects for using the model to test/discuss factors that affect ^{10}Be concentrations on other types of shore platform geometry beyond the sloping (type-A) platform investigated? The paper refers to type-B platforms,

raising the question as to whether the model adds any new knowledge of likely differences in concentrations across different possible platform geometries.

Please see also our response to Alan Trenhaile's similar comment (response #7 in the response to Alan Trenhaile's review). We are not exclusively considering type-A and type-B platforms, these are end-member cases, and the dynamic morphological model used in this study produces alternative platform geometries such as stepped platforms. We now write:

"Cliffed, rocky coasts, are commonly fronted by shore platforms that have previously been classified into two types (Sunamura, 1992). Type-A platforms are characterised by a gently sloping erosional platform surface extending offshore beyond maximum low water. Type-B platforms are shallow gradient to sub-horizontal and terminate at their seaward edge at maximum low water through a scarp (Figure 2a). Numerical models of shore platform evolution have successfully recreated both of these end-member morphologies, but have revealed that there are a range of other possible morphologies in between, for example sloping platforms terminating in a scarp (e.g. Trenhaile, 2000; Walkden and Hall, 2005; Matsumoto et al., 2016)."

"Therefore, two different morphological models are used in this study. The first assumes steady state evolution of the shore profile, such that coastal morphology does not change its form through time, and a constant cliff-platform geometry is translated landward through time. The second model is a dynamic shore platform evolution model similar to that of Trenhaile (2000) that can reproduce a range of platform geometries. These two approaches are described in more detail in the subsequent sections."

5. The model considers topographic shielding in the case of a constant cliff height. A further exploration of interest would be to consider a slowly increasing cliff height. What comes to mind is the example of progressive cliffing into hillslopes rounded over long glacial periods. This could introduce further complications for ^{10}Be con-

centrations, because erosion into cliffs of increasing height might progressively increase beach thickness (5.4.1)... assuming no gradient in alongshore sediment flux. The number of potential model scenarios can quickly increase, but it would be interesting to have a somewhat expanded discussion of this factor.

On the first part of your comment, we agree that it is unlikely on many coastlines that cliff heights have been constant during platform development but will depend on the morphology of the landscape that is being eroded into. Generally, topographic shielding only has a minor influence on CRN concentrations, since the shielding factors increase rapidly and non-linearly toward unity moving away from the cliff (Figure 4). To illustrate this we have attached a plot showing ^{10}Be concentrations for a simple case of steady-state platform retreat at 10 cm yr^{-1} for a 1/100 gradient platform for cliff heights of 0, 25 and 50 m. The difference in peak concentrations between the 0 and 50m cliff are < 10%. A gradual changing of cliff height would therefore not make a significant difference to the predicted concentrations.

With the second part of your comment we acknowledge that we offered little discussion of the feedbacks between cliffs and beaches. It was intentional at this stage not to perform experiments exploring these feedbacks because as yet they are poorly represented in existing morphological models, particularly when considering alongshore transport.

6. There is some interesting discussion on the implications of platform downwear, but I agree with the other reviewer that this aspect of the modelling could be improved - he makes a number of points to consider in that regard which I have not expanded on here.

We have made a number of changes to the manuscript to address Alan Trenhaile's comments and point the reviewer to these for details (see responses to Alan Trenhaile's review: #3, #5, #6, #8 and #13). We have not changed our modelling approach to include more specific processes related to downwear as we think this would distract from our intention to provide a heuristic exploration of the controls on shore platform ^{10}Be concentrations.

7. In concluding, is it possible to tabulate or summarise somehow the relative sensitivities of the different factors?

   Thank you for this suggestion, we agree that such a summary would be helpful and are considering different option for how best to achieve this in redrafting the manuscript.

8. Please double check that you have the exponent written correctly in equation 5 (i.e. '+' hw(t))?

   This should have been negative, thank you for noticing. We have removed the brackets so that the signs are now correct.

9. Please double check the wording of the last sentence on p7 (upper / lower?)

   These were correct but we see how it might have been confusing. These changes are relative to a scenario with no tides at all. The upper platform experiences periodic submergence and emergence during a tidal cycle, but the net effect is a reduction in the overall ^{10}Be production, whilst on the lower platform the net effect is an increase in the overall ^{10}Be production. We have rewritten this section to clarify:

   "Tides modify production in the platform by varying water depth hw and intermittently submerging and exposing the platform sub-aerially. Relative to a scenario with no tidal variation, Regard et al. (2012) demonstrated that tides have a net effect to reduce ^{10}Be production in the upper inter-tidal platform due to periodic platform submergence that reduces the net cosmic ray flux received, while ^{10}Be production in the lower platform increases due to periodic exposure."

10. two "and"s p4 line 28 - Clarify what is meant by significant (para 8, line 16)

    Corrected

11. Mis-spelling of fund[e]mentally

    Corrected

12. Grammar problem para 9 line 24

    We were unable to find this error. We contacted the reviewer directly who was not able to clarify an error. If this mistake exists and has not been corrected by our redrafting and proof reading, we hope it will be caught during typesetting.

13. no full stop before Failure p15 l12

    Corrected

14. Choi2012 p16

    Corrected

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