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Interactive comment

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

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Cosmogenic dating was initially greeted by rock coast workers, as a quasi-panacea, a long awaited solution to the question, that had arisen over more than a century, regarding the age of shore platforms, and especially whether they are primarily contemporary (Holocene) or inherited (from previous interglacials when sea level was similar to today's) landforms. Related to this question and of more immediate practical concern was its apparent ability to provide reliable data on rates of cliff recession over lengthy periods during the Holocene, and to test predictive models and better predict the effects of climate change, and especially rising sea level. There have been a few useful 10Be dating applications. They include the work of Choi et al. (2012), who suggested that the seaward portions of shore platforms in Korea are up to about 150 thousand years old, and that the modern platform is cutting into its interglacial predecessor. Conversely,

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Discussion paper

Regard et al., (2012) opined that in northern France, the mean rate of cliff retreat since the mid-Holocene has been 11–13 cm yr-1, which would have been sufficient to create 'contemporary' intertidal shore platforms hundreds of metres in width. Despite these valuable contributions, I would contend that cosmogenic nuclide analysis, whether for dating landforms or quantifying their rates of erosion has failed, as yet, to revolutionize the study of rock coasts. Given this, the present paper, which discusses and models the effect of several of the factors and assumptions that constrain the use of cosmogenic analysis on rock coasts, represents a welcome addition to the literature and one which will, no doubt, improve our ability to understand the longterm development of rock coasts. Nevertheless, this paper once again points out the inherent limitations of cosmogenic analysis on rocky coasts for which we have little longterm data on such factors as downwearing and backwearing rates (erosion in the vertical and horizontal planes, respectively) and historical and short-term (storm and calm condition) variations in sediment thickness, type, and extent.

My main criticism of this paper is the almost complete neglect of downwearing by wearing processes, including by tidal wetting and drying and salt weathering. The paper notes that models have indicated that shore platforms and cliffs trend towards a morphological steady state. There are certainly many examples of steady states in model predictions with constant sea level, although when one considers the effect of changing relative sea level, simulated longterm platform development is more complex (see for example: Trenhaile, A. S. 2001: Modeling the Quaternary evolution of shore platforms and erosional continental shelves . Earth Surface Processes and Landforms 26, pp. 1103-28, and Trenhaile, A. S. 2014. Modelling the effect of Pliocene-Quaternary changes in sea level on stable and tectonically active land masses. Earth Surface Processes and Landforms 39, 1221-35). The paper quotes our work (Porter et al., 2010) in eastern Canada (on page 15 of the manuscript), but essentially ignores the relevant conclusions that relate to spatial variations across the intertidal zone. This paper showed, based on laboratory experiments lasting several years and about 2000 rock samples, together with about 200 transverse micro-erosion metre stations in the field, that, while weathering downwearing rates (isolated in the laboratory experiments) tend to be a maximum in the upper intertidal zone, there is no clear pattern in the field, possibly because of the effect of other erosional mechanisms with different elevational efficiencies. Whether there are weathering patterns or not, mean rates of downwearing recorded by numerous workers in different environments and on different types of rock, which generally range from almost 0 up to a few millimeters per year (and hence significant lowering per millennium), surely have an important effect on predicted erosion rates. The reliance on models of platform development (whether parallel or declining slope retreat) to represent rates of surface lowering (as opposed to field measurements and micro-erosion metre data) remains an inherent weakness in attempts to apply cosmogenic techniques to rocky coasts.

Minor points include:

The traditional distinction (classification) between sloping and subhorizontal (or to use Sunamura's terminology, type A and B platforms) belies the fact that there is a continuous spectrum of forms with gradients that reflect tidal range and rock hardness (resistance), as well as possibly sediment grain size and the effect of Holocene sea levels higher that today .

It is not clear to me why, on line 12 or page 4, abrasion is assumed to be dominant with a steady state that assumes platform downwearing is gradual and constant (especially since weathering is not considered). Sediment on shore platforms accumulates preferentially at the cliff foot and in structural depressions on the platform. In the latter case, abrasion rates with vary enormously across the intertidal zone in an essentially random manner. Abrasion towards the rear of the platform is spread over the intertidal surface as the cliff retreats, but constant abrasion assumes constant wave and sea level conditions as well as constant beach types and amounts.

Page 4, line 20, many models do/did not emphasize erosion at the water surface but rather erosion due to wave generated bottom currents (which is only effective in clays and other 'soft' rocks.

An alternative to the Bruun Rule, which has been criticized by several workers and in any case is not designed for beaches with rigid (shore platform) foundations, is given by: Trenhaile, A. S. 2004. Modeling the accumulation and dynamics of beaches on shore platforms. Marine Geology 206, 55-72.

The paper needs some proof-reading. There are other examples that could be given but note 'ditributed' on line 14 of page 10 and on line 15 it should be 'adjacent to the cliff'.

On lines 12 to 13 on page 11 it is claimed that there is no downwearing on top of flat-topped steps. This is wrong - weathering certainly lowers the surface until (and in some cases before) it is eliminated by step retreat.

Lines 10 to 13 on page 13 emphasize errors due to differences in predicted downwearing rates according to the two evolutionary models. The reality is likely to be worse owing to weathering induced downwearing (with spatial patterns, if any, that we do not yet understand).

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