Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., referee comment RC1
https://doi.org/10.5194/esurf-2022-28-RC1, 2022

Comment on esurf-2022-28

Anonymous Referee #1

Referee comment on "Constraints on long-term cliff retreat and intertidal weathering at weak rock coasts using cosmogenic 10Be, nearshore topography and numerical modelling" by Jennifer R. Shadrick et al., Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2022-28-RC1, 2022

1 General comments (following Esurf structure requirements).

Scientific significance: excellent.

The article comprises field measurements and numerical modelling of Earth surface processes, here the effect of waves and weathering on chalk cliffs (i.e. essentially interactions between the hydrosphere and the lithosphere). For these reasons, this research matches undoubtfully the scope of ESurf.

The substantial contribution of the article to scientific progress within the scope of ESurf is achieved by new methods (dynamic model of cliff retreats with optimized parameters based on observed ^{10}Be concentrations) and by new data (long-term chalk rock cliff retreat rates data).

Scientific quality: excellent.

The historical v.s. long term retreat of chalk rock cliffs is here calculated, analyzed and compared with. appropriate approaches. The potentialities and limitations of the process-based modelling approach used to constrain long-term chalk cliff retreat are thoroughly discussed, as well as perspectives of improvement for the future.

Substantial conclusions are reached and are: 1) A confirmation of recent acceleration of cliff retreat in the south of England based on the investigated dataset, 2) The decoupling of the relation RSL rise – cliff retreat during the acceleration period of chalk cliff retreat, 3) the differential control of weathering processes on sandstone vs chalk rock cliff retreat, and 4) some advances in the "wave versus weathering" debate that tries to find the main controller of material removal from cliff over time.

It should be mentioned that this paper is not 100 % a stand-alone paper, and follows/re-use some methods and conclusions developed in Hurst et al., 2016, and mainly Shadrick et al. (2021), where a similar modelling approach was applied to rock coasts. A summary of the latter is provided in the present paper, but many references are made to it throughout the text. It is thus necessary to read Shadrick et al. (2021), to fully understand the whole paper.

The article is perhaps a bit unbalanced between the approaches for constraining historical cliff retreat and long-term cliff retreat, but this is fully justified as constraining the long-term cliff retreat demands a modelling exercise whose strengths, limitations and uncertainties are fully addressed.

Presentation quality: excellent.

Figures are well supporting the text and are easily understandable even at first sight. The text is written with a logical progression of ideas and with accessible but high-quality English language.

I find the discussion and conclusion a bit too long to read a perfectly balanced article, as the discussion represents 9.5 pages out of 31 pages of main text, so more than 30 % of the total article. The point on the cliff debris is very interesting to read, but is perhaps too long, as it was not mentioned in the objectives at the end of the intro or in the abstract that the influence of beach' pavements on erosion would be studied. It leaves me the sensation that you put a strong emphasis on a discussion point that was not expected when reading your abstract or your objectives.

2 Major concerns

L309-311: What you are interested in is the point to point difference in CRN concentration, and therefore is the inheritance background to the biggest issue. But as you mention the way you treat inheritance, could you justify the reasonability of your assumption that the last exhumated sample contains almost only inherited ^{10}Be atoms? If your sample remains buried under the cliff for a substantially long time, then only Muon produced CRNs accumulate at an extremely low production rate, until a possible secular equilibrium, from where no net CRN accumulation occurs. How would this inherited CRN quantity actually

compare with the ones produced via the surface production rate in your sample once it is exhumed?

Moreover, in your results (L360-365), you get an order of magnitude for the Cliff retreat of, say, 10 cm/yr. Your sample SS09 that is used for inheritance background is located 41

m away from the current Cliff position (Table 2). Correct me if I am wrong, but in such circumstances you would get a CRN accumulation at the surface of your sample for (41 m/ 10 cm/yr =) 410 yrs. Should your surface production ratio be around 4-5 atoms/g/yr,, you would end up with 1500-2000 at/g produced after the exhumation of your sample, that are finally not attributable to inheritance. This is a substantial quantity if you compare it to the measured CRN concentration in your sample SS09 (2770 atoms/g). The partial shielding for water probably decreases the CRN production rate, but to what extent?

If this example would be exact, then your CRN data points on Figure 5 for the Seven Sisters' Panel should be diverted 1500-2000 at/g above their current value. From the naked eye, you would get 4 more observed data into your shaded area, and likely a better fit between simulated and observed CRN concentrations.

This is a point that may deserve some attention in the discussion.

L633-635: again (see comment on L309-311), if there is any overestimation of the observed inherited ^{10}Be concentration (From Hurst et al., 2016, when I look at Fig. 3, I see that the closest CRN observed conc. is more than 50 m from the current cliff position), then you would have more of the ^{10}Be conc. attributable to post-exhumation production, your ^{10}Be conc in Fig. 5 would increase and thereby better match your shaded zone for the Beachy Head site. Is it a possibility?

3 Specific comments

L82 : perhaps give cite the studies you are talking about.

L187-189: Just a detail but i needed to read the paragraph L187-189 several times to make a clear difference between your 2 study sites + the two you reprocessed from Hurst et al. (2016) vs their associated GIA reference site. I think "the three sites on the Sussex coast, including Seven Sisters, Hope Gap and Beachy Head" is an ambiguous fomrulation as it leaves the impression that named sites are included in other sites. Perhaps rephrase slightly ?

L202-203: First time that "multi-objective optimisation" appears in the text. The sentence is formulated as if this concept has been described or at least mentionned above in the text. Perhaps around L105 would be a right spot to refer to it a first time?

L228-229: Could you precise how you account for resp. spallogenic and muogenic contribtuion to ^{10}Be production ? Currently, it sounds a bit like a 1D model with only 2 levels : the surface level where CRN production is only controlled by spallation (very reasonable approx.) and a depth level where CRN production is controlled only by muones (reasonability of approximation depends on what depth we talk about? Is is it a few meters or tens of meters? This would probably change the assumptions). Could you clarify this ? Or alternatively, show the formulas you have used ?

L233: You refer here to the CRN concentration analyzed at the surface across the progressively exhumed platform I guess? Perhaps just mention it to avoid people getting confused between your horizontal profile and the depth profile you referred to on L229.

L235 : Same comment as for L228-229 : Which production pathways' distribution governs your simulated CRN depth profile?

L335: Table 2: If i get it well, inheritance was subtracted to the background corrected ^{10}Be conc. (e.g., 5.06 - 2.77 = 2.29 for SS01)? Then some column calculation are not ok (e.g., SS05 is higher after the correction for inhertiance which is not possible - i think the true value is rather 0.922 and not 9.22). Same for SS07 and SS08. Please check carefully each column.

L350: Table 3: Do you use SM05 to correct for the inheritance ? you have a value of 1.97 x 10^3 at/g but in the column where inheritance is subtracted, it seems only 1.29 x 10^3 atoms/g were subtracted. Is there a reason for such a difference?

L417-431: Perhaps try to be consistent with the units you use throughout the paper. In Table 2/Table 3, your CRN conc. is written scientifically (x 10^3 atoms/g), whereas its is in atoms/g in the paragraph that starts on L417, and in k atoms/g in Fig. 5. Same with yrs BP, that are sometimes written in K yrs BP (Fig. 6, L460; Fig. 7, L571). Could you homogenize the way units are written?

L539: Table 6: it is a detail but in some cells you cite the rate values in a decreasing way (e.g. "7 to 3") and in some other in an increasing way (2.6 to 30.4). Is it because some rates tend to decrease with time, while some others increase with time? Is there a specific reason for this? If not, it would be better to cite everything in an increasing way. Also consider to put the same number of significant digits everywhere.

L504-505: when you say "an order of magnitude increase" of your short-term retreat rate compared to your long-term one, I agree with it if you take the lower values for Beachy

Head (22 v.s. 2.6), but not the highest one (22 v.s. 30.4). Perhaps give some nuance to your statement.

L581: in the title of subsection 5.4, you announce that you are going to talk about erosion processes, but the following paragraphs treat about weathering. At some places in your text, e.g. L87 or L137-138, you use formulation like "weathering and erosion". In your model, a free parameter is the "maximum intertidal weathering rate" and in L217, you use the formulation weathering-driven erosion. In L50-55, you clearly state the difference between weathering and wave erosion, thereby implying that any physical erosion performed by another agent than waves is encompassed in the notion of weathering. This makes the first reading a bit confusing as i always needed to check when you talk about erosion if it is a contraction of "wave erosion" or if it is physical erosion "belonging" to weathering.

In many (soil) studies, weathering is only used for chemical processes, by opposition to physical erosion. The sum of (chemical) weathering and physical erosion corresponds to the denudation.

I am not sure how those notions are transposed in coastal geomorphology, but I think you would gain in being fully systematic with the term you use.

4 Technical corrections

L53: This sentence seems to want to oppose chemical weathering and physical erosion. Shoudn't the "and" from L53 be replaced by a "," ?

L89: typo: missing dot at the end of the paragraph

L335: Table 2: A word seems to be missing in the header of your last column (1 sigma or so).

L840: Robinson, D.a. => “a” should be upper case.