REVIEWER 2

Peer review on: Evidence of slow millennial cliff retreat rates using cosmogenic nuclides in coastal colluvium Remi Bossis et al.

The manuscript presents a novel method of measuring millennial cliff retreat rates for rocky cost from coastal colluvium. The proposed method expands on earlier methods of sampling the coastal plaŋorm or the cliff face and allows studies focusing on different coastal seţngs where the earlier methods are not applicable. As such the manuscript is in principle a welcome contribuΘon to the community. However, it is not ready for publicaΘon in the current state but requires a major revision.

Currently the most notable weakness are the errors in the data reducΘon that will have an impact on all the figures and conclusions and need to be corrected before further assessment of the manuscript is undertaken. The manuscript presents a new method of sampling and measuring coast cliff retreat rates and future readers will need a sound dataset and in-depth discussions to assess the usefulness of the method. In the current form the manuscript lacks this.

While checking the presented 10Be concentraΘon data I came across some significant discrepancies to the presented data. I have highlighted these in red in the aΣached table. These needs to be corrected and subsequent figures and text corrected for the next revision of the manuscript.

The first obvious issue is the uncertainΘes. It appears that the uncertainty in the blank measurement was not included in the error esΘmaΘon of the final 10Be concentraΘon. This elevates the error bars significantly and the presented ~(5-7)% errors are now typically between (15-50)% with only 3 samples below 8% and 2 are over 75%. My arithmeΘc is based on simple assumpΘon of using batch specific blank correcΘon without any further consideraΘon on the validity of this, which is another topic as will be discussed later.

The second major error is the apparent lack of blank correcΘon to the second batch of samples. The blank corrected raΘos are idenΘcal to the measured 10/9 raΘos in Table S1. AŌer patch specific blank correcΘon the difference to the presented 10Be concentraΘons is between (10-90)%. As above these samples suffer from the underesΘmaΘon of the errors as well.

Then more philosophically given the blank correcΘon for the presented samples is between (7-90)% one has to consider what is the most appropriate way to do the blank correcΘon and what is the source of the addiΘonal 10Be atoms. Two blank samples were measured as part of this work and they have a difference of factor of 2. In this case using the analyΘcal uncertainty of a single blank sample as the error in the blank correcΘon is probably too opΘmisΘc and strong case could be made to use standard deviaΘon between the blank samples as the uncertainty to use in blank correcΘon. This elevates the errors even further and 4 out of 11 samples will have higher than 100% errors, as shown in the above table.

Manuscript would strongly benefit from expanded discussion on used analyΘcal processes and jusΘficaΘon of the choices made. Recommended reading for treatment of errors is “Data reducΘon and error analysis for the physical sciences” by Philip R. Bevington, D. Keith Robinson.

We are very grateful to the reviewer for reading the details, as some errors had unexpectedly entered the tables. For the two series of samples, we had 'lost' the error on the blank. For the second series, we had also failed to correct for the blank. We apologize for this and have corrected all the mistakes.

By the way, we'd like to thank the reviewer for his question about correcting from the chemical blank. We rechecked our spreadsheet and the propagation of errors, asking around for advice. We came up with a slightly different treatment to the one recommended by the reviewer, although the resulting uncertainties are similar to the ones given by the reviewer; the main difference being that we're working on the number of atoms instead of the ratio ^{10}Be/^{9}Be. Here is our error processing:
(i) We calculate the number of ^{10}Be atoms by multiplying the measured ratio ^{10}Be/^{9}Be with the number of ^{9}Be atoms from the spike. We propagate errors.
(ii)Then we subtract the number of ^{10}Be atoms for the blank from that in the sample. We propagate the error E = sqrt(Emeas^2 + Eblk ^2) where E denotes the error (or uncertainty), Emeas the error for the measurement and Eblk the error for the blank.
(iii) Finally we calculate the concentration by dividing the number of atoms by the mass of dissolved quartz, and we propagate errors.

Whilst I did not go into great depths with the manuscript after discovering the above I found the discussion to be very light on processes that might undermine some of the assumpΘons of the method and would encourage authors to elaborate on some of the following points:

1. Given this is a new method it would benefit from comparison data. Authors claim that there is no other method for this, but they could have easily sampled the planform if present, or the exposed bedrock from the cliff face as an additional line of enquiry.

This remark points to the very reason for this study. Sampling on the platform as has been done in the past (e.g. Regard et al. 2012, Hurst et al. 2016, Swirad et al. 2021...) requires a simple (fairly flat) platform, with little stochasticity. Everything is smoothed when recession rates are fast. For slow recession rates, on the contrary, the signal is much more complex (cf. Choi et al. 2010, we also collected a dataset that is difficult to publish on the Basque Coast, where the recession rate is less than 1 cm/a). Regarding the cliff, this is also a problem of stochasticity: what indicates that the cliff at a given point has a recession rate that is representative of the entire cliff escarpment? This is why we use colluvium where we guess Nature does the average, as has been proven for river sediments at the outlet of a catchment. We also sampled at different points about 10 m apart along a 50 m longitudinal transect along the cliff foot.

2. Expanding on the complexities of cliff erosion, that is how much of it happens as sand and what fraction is as rocks and larger cobbles or episodic landslides and how these impact any

possible signal from the sampled colluvium. It might all average out but clear in-depth discussion on this would be an advantage and added strength to the paper.

This comment is indeed relevant. It is the strength of cosmogenic isotopes that they provide a robust measurement of denudation rates, even if the processes are complex. We feel that a discussion of these processes and how they affect the measurement has no place in this paper, as they have already been the subject of numerous scientific papers on the measurement of denudation rates in river sediments. Furthermore, the statistically similar 10Be concentrations obtained for different sites along the Mediterranean coast with similar geomorphic contexts plaid for a low effect of stochasticity, what we indicated in the Discussion.

3. In this application the catchment is very small, and steep compared to the traditional use of the method to calculate basin wide erosion rates. I expect this to add a level of sensitivity to the method and wonder how easy it is to define the catchment size? E.g. does a landslide have the potential to significantly change the size of the catchment and thus the resulting retreat rate? It appears from the sampling photos that the authors tried to address this by sampling wider sand deposit. Elaborate on this.

As mentioned above, we use colluvium whose source area can be delimited by a simple geomorphological analysis, in a similar way to the delimitation of catchments when measuring denudation in river sediments. The catchment can be accurately delineated and we have used this delineation to calculate average slope and production rate. This delineation appears to be relatively insensitive to landslides. We do not believe that this aspect of our method is a significant source of uncertainty. Note that we built this approach on our experience acquired in a former study (Zavala et al., 2021), for which we sampled colluvium at the base of valley flanks along canyons to quantify valley-flank averaged erosion rates. The geomorphic situation along the studied coasts resembles the one along the valley flanks.

4. Is the sampled colluvium from the above cliff catchment or is there a potenΘal for waves to bring it in?

We sampled high enough to prevent any contamination by sea waves. It was stated in the original manuscript as: “To do this, we systematically sampled debris wedges that covered any slope break at the toe of the escarpment (located usually a few metres above the sea), so that the sediment sampled necessarily came from higher up.” It is apparently not clear enough and thank the reviewer for that. We added: “(i.e., no contamination by sand brought in by waves)”.

In summary the paper presents a novel and interesΘng method to evaluate cliff retreat rates and has the potenΘal to complement earlier methodologies and contribute to the topical quesΘons on the coastal erosion. However, in the current state the manuscript does not allow the reader to evaluate the usefulness of the method without addressing the errors in the data reducΘon and elaboraΘng on the assumpΘons of the method. I encourage the authors to resubmit the manuscript.

Best regards, Klaus

Thank you again for your careful check or our data.