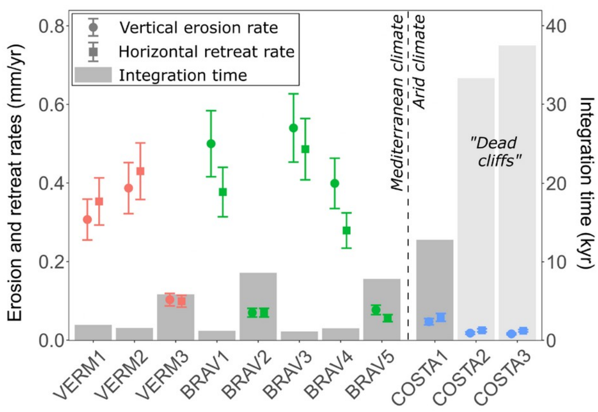
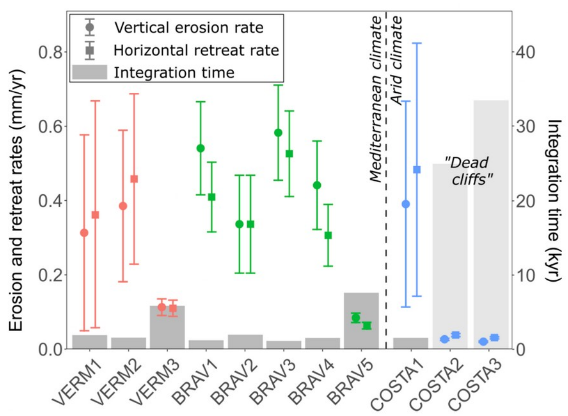
Dear Editor,

We submit our revised manuscript after taking into account all the comments of the two reviewers. Reviewer 2 Klaus Wilcken has identified errors in our calculations of 10Be concentration and associated uncertainties. We are deeply sorry for these errors and thank this reviewer for identifying them. We have revised our calculations to correct these errors. The new values of 10Be concentration and associated uncertainties have modified the erosion rates and their uncertainties. Here we present the main figure summarising these new values and compare it with the previous version of this figure. Overall, the uncertainties are greater than initially calculated and two erosion rate values have increased significantly, BRAV2 on the Mediterranean coast and COSTA1 on the Peruvian coast. Interestingly, the new erosion rate for BRAV2 is now similar to values for adjacent sites on the Mediterranean coast, and the erosion rate on the Peruvian coast actively subject to wave action, COSTA 1, is now also similar to values for the Mediterranean coast, despite the more arid climate. Apart from the absence of a climatic effect on the rate of coastal recession in Peru, our main conclusions remain unchanged.

previous version



after correction



Best regards,

The authors.

REVIEWER 1

Dear Editor, dear Authors,

In this short and clear contribution, Bossis and colleagues introduce a new method to quantify rates of coastal cliff retreat on a previously elusive timescale of kyr. To fill this important gap in coastal studies, the authors collect fresh colluvium at the base of quartz bearing cliffs and scarps and measure their 10Be concentration to back calculate a rate of denudation. This method is adapted from a similar approach that helps constrain the rate of lateral erosion in rivers. Three sites are used to test the method and the results conform to reasoned expectations. There are no independent confirmation of the rates but that reflects the novelty of the approach. I had reviewed an earlier version of this text submitted to another journal and my initial comments are all addressed. The current manuscript focuses on the method and its potential, and does so with clarity. I have minor comments that mostly concern readability. I believe the manuscript to be very close to publication.

Response: We would like to thank Luca Malatesta for his constructive comments and suggestions.

Note that I am not a specialist in cosmogenic nuclides and cannot comment on specific details of the methodology.

I address the authors directly for my comments.

I think that the authors should cite and comment the interesting of Swirad and Young (2022, Geomorphology https://doi.org/10.1016/j.geomorph.2022.108318) where the relative contribution of waves and precipitation for cliff erosion is discussed based on repeat lidar surveys. It is valuable as an example of Lidar constraints but more importantly helps understand where and why erosion can occur on coastal scarps.

Thank you, we added this citation.

Formulations and rigor of language.

Throughout the text, there are a few elements of language that veer on the poetic and figurative and do not provide clear-cut information. Below you will find examples and it would be good to screen the manuscript for more.

l. 28: “does little” What is the issue? If “little” what is the little amount that is done?

We reworded as “does not integrate for all the stochastic nature”.

l. 134: “budding vegetation” it is my understanding that when referring to vegetation, budding keeps its literal sense (developing buds) and not the figurative one that the authors were presumably targeting. “budding vegetation composed of” can be removed.

OK.

l. 148-149: to rise rapidly at a decreasing rate is an odd phrasing.

We reworded as “ sea level rose rapidly just after the last glacial maximum, and then at a decreasing rate”.

Discussion of the low erosion rates

I was confused reading the last four paragraphs of the discussion. It should be explaining the reason behind “surprisingly low erosion rates” (l. 198). But I don’t see which sites you refer to. The previous paragraph was about particularly high rates of erosion. And the before that, the slower rates in Peru do not seem surprising as they fit your expectations and you explain them already. Is it just a theoretical discussion or am I missing the link with a field site?

It's true that we were a little elliptical. We wanted to emphasize that all the cliff recession values known to date are in the order of millimeters to meters per year. These are values that correspond to modern speeds and to some average speeds over the last few thousand years. We therefore expected to find faster retreat rates, or even no measurable concentration of 10Be. In these 4 paragraphs we therefore consider all the hypotheses that could lead to an underestimation of the retreating rates. We have added this sentence at the beginning of these 4 paragraphs:

“As we have obtained coastal erosion values that are slower than those documented worldwide (Premaillon et al., 2018), we discuss in the following several biases that could lead to an underestimation of retreat rates in our study.”

Terminology: recession, retreat, erosion

You are using the term “recession” throughout the manuscript. Is that different from the more commonly used “retreat”? I am more familiar with retreat, or simply erosion. Is recession specific to coastal cliffs? Would it be simpler to stick to retreat for all cliffs?

OK we changed for retreat all along the text.

Line by line comments:

l. 7 “[…] erosion rates, AND the geomorphic and climatic […]”

Done.

l. 9 I think that “colluvium” is not used as a countable object: “from vegetated colluvium”

Done.

l. 16, 38 “wave action” not “waves action”

Done.

l. 28 Do you mean “account for” instead of “integrate for”? Alternatively drop the “for”

Done.

l. 38 “rocks resistant to wave erosion”. All rocks are “resistant” to erosion. It is a matter of how much.

OK we added “more resistant”.

l. 46 “less dissected” I can't see any river on your pictures. "less" or “not"?

OK “not”.

l. 49-50 “With this method, we obtain slow cliff recession rates, between 0.05 and 0.5 mm/a.” This suggests that this method can only capture this one order of magnitude. Is that the case? I think you are actually talking about the three sites you target here. Worth clarifying.

You are right, we discuss later in the discussion section that this method is suitable for retreat rate lower than 1cm/y (higher erosion would result in no measurable 10Be).

l. 50 mm/yr not mm/a, distance per time, not per age.

According to “HOLDEN N. E., BONARDI M. L., DE BIÈVRE P., RENNE P.R. & VILLA I.M., 2011 - IUPAC-IUGS common definition and convention on the use of the year as a derived unit of time (IUPAC Recommendations 2011). Pure and Applied Chemistry, 83 (5), 1159-1162.”, it is suggested to use /a.

l. 50 “reverse” do you mean inverse?

Yes!

l. 55 The sentence suggests that it is expected that there’s a hillslope above the cliff but that it will be ignored.

OK; we removed the reference to the hillslope. Actually, there is almost always a hillslope but we explain then that we sampled carefully at sites where the contribution from the hillslope is either null because the top of the cliff is a drainage divide (no hillslope above the cliff) or necessarily small because we are below a ridge. There is only one site that is more ambiguous (BRAVE2) and that we discuss in the discussion and in the Supplementary material: we demonstrate from mass balance arguments that the hillslope contribution is unable to significantly bias our result.

l. 56 ongoing erosion?

Done, thank you for the suggestion..

l. 66 coasts don't have a summit, or do they? I think it's clearer to directly mention the object you target, which is the top of the coastal cliff.

OK.

l. 70-71 the escarpment surface is regolith? or the top surface in which the cliff is cut?

We reworded it as “cliff surface”.

l. 93 small note: subscripts that are not variables themselves are usually not italicized.

We keep that as it is as we are consistent with the notation throughout the text.

l. 134-135 Is it just an educated guess (fine by me), or is there a method behind this estimate?

No, there is no specific method.

l. 148-149: So, when did the platform emerge? when did waves stop reaching the base of the cliff?

As the uplift rate is about 0.5 mm/a and the paleo-cliff foot is at about 100 m above modern sea level, an age of emersion of about 200 ka can be estimated. We have added a comment to indicate this typical duration.

l. 229: “when the sea level was largely below the current one, implying that the sea and waves” can be replaced by “when the sea level was lower and the waves”

OK thank you.

Figure 1 caption: “rather progressive” what does that mean?

Replaced by “ongoing”.

Figure 2 caption: “vertical equidistance” I don't know what vertical equidistance is. I guess it means contour line spacing. In the case I'm not the only person unfamiliar with the term, I'd suggest "contour lines are traced every 20 m." Feel free to ignore.

Done.

Thank you very much!

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 plaƞorm or the cliff face and allows studies focusing on different coastal seƫngs 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.