Peer review on:

Evidence of slow millennial cliff retreat rates using cosmogenic

nuclides in coastal colluvium

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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 platform or the cliff face and allows studies focusing on different coastal settings where the earlier methods are not applicable. As such the manuscript is in principle a welcome contribution to the community. However, it is not ready for publication in the current state but requires a major revision.

Currently the most notable weakness are the errors in the data reduction 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 concentration data I came across some significant discrepancies to the presented data. I have highlighted these in red in the attached table. These needs to be corrected and subsequent figures and text corrected for the next revision of the manuscript.

Sample	Qrtz mass [g]	9Be added [mg]	9Be added [atoms]	meas 10/9	error		10/9 blank corrected	error		alternative error using stdev between blanks		10Be [atoms/g qrtz]	error		Blank correction
VERM1	2.94	0.5209	3.48E+19	7.88E-15	5.97E-16	7.6%	9.03E-16	8.18E-16	90.6%	2.72E-15	301.5%	10686	9685	90.6%	88.5%
VERM2	7	0.5195	3.47E+19	8.69E-15	6.43E-16	7.4%	1.71E-15	8.53E-16	49.8%	2.73E-15	159.7%	8484	4227	49.8%	80.3%
VERM3	6.36	0.5198	3.47E+19	1.28E-14	6.72E-16	5.3%	5.79E-15	8.75E-16	15.1%	2.74E-15	47.3%	31617	4777	15.1%	54.7%
VERM4	1.71	0.5181	3.46E+19	5.82E-15	4.55E-16	7.8%	-1.16E-15	7.22E-16	-62.4%	2.70E-15	-233.1%	-23399	14603	-62.4%	119.9%
VERM5	2.27	0.5205	3.48E+19	6.07E-15	6.02E-16	9.9%	-9.12E-16	8.22E-16	-90.1%	2.72E-15	-298.5%	-13976	12597	-90.1%	115.0%
BRAV1	32.22	0.5205	3.48E+19	1.33E-14	1.01E-15	7.6%	6.28E-15	1.16E-15	18.4%	2.84E-15	45.3%	6779	1250	18.4%	52.6%
BRAV3	30.04	0.5213	3.48E+19	1.23E-14	6.93E-16	5.6%	5.36E-15	8.91E-16	16.6%	2.75E-15	51.3%	6209	1033	16.6%	56.6%
BRAV4	15.05	0.515	3.44E+19	1.07E-14	6.32E-16	5.9%	3.68E-15	8.44E-16	22.9%	2.73E-15	74.2%	8411	1930	22.9%	65.5%
BRAV5	13.26	0.5217	3.48E+19	2.35E-14	1.01E-15	4.3%	1.65E-14	1.15E-15	7.0%	2.84E-15	17.2%	43384	3026	7.0%	29.7%
BLANK-run1		0.5159	3.45E+19	6.98E-15	5.60E-16	8.0%									
BRAV2	2.98	0.5191	3.47E+19	4.14E-15	2.61E-16	6.3%	9.21E-16	3.45E-16	37.5%	2.67E-15	289.8%	10719	4015	37.5%	77.8%
COSTA1	2.84	0.5215	3.48E+19	3.66E-15	2.42E-16	6.6%	4.38E-16	3.31E-16	75.5%	2.67E-15	609.2%	5371	4055	75.5%	88.0%
COSTA2	4.27	0.5201	3.47E+19	1.44E-14	6.21E-16	4.3%	1.12E-14	6.61E-16	5.9%	2.73E-15	24.3%	91334	5378	5.9%	22.3%
COSTA3	10.38	0.5193	3.47E+19	4.27E-14	1.54E-15	3.6%	3.95E-14	1.55E-15	3.9%	3.07E-15	7.8%	131880	5190	3.9%	7.5%
BLANK-run2		0.5161	3.45E+19	3.22E-15	2.26E-16	7.0%									
				st.dev between blanks	2.66E-15										

The first obvious issue is the uncertainties. It appears that the uncertainty in the blank measurement was not included in the error estimation of the final 10Be concentration. 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 arithmetic is based on simple assumption of using batch specific blank correction without any further consideration on the validity of this, which is another topic as will be discussed later.

The second major error is the apparent lack of blank correction to the second batch of samples. The blank corrected ratios are identical to the measured 10/9 ratios in Table S1. After patch specific blank

correction the difference to the presented 10Be concentrations is between (10-90)%. As above these samples suffer from the underestimation of the errors as well.

Then more philosophically given the blank correction for the presented samples is between (7-90)% one has to consider what is the most appropriate way to do the blank correction and what is the source of the additional 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 analytical uncertainty of a single blank sample as the error in the blank correction is probably too optimistic and strong case could be made to use standard deviation between the blank samples as the uncertainty to use in blank correction. 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 analytical processes and justification of the choices made. Recommended reading for treatment of errors is "*Data reduction and error analysis for the physical sciences*" by Philip R. Bevington, D. Keith Robinson.

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 assumptions 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 platform if present, or the exposed bedrock from the cliff face as an additional line of enquiry.
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.
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.
4. Is the sampled colluvium from the above cliff catchment or is there a potential for waves to bring it in?

In summary the paper presents a novel and interesting method to evaluate cliff retreat rates and has the potential to complement earlier methodologies and contribute to the topical questions 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 reduction and elaborating on the assumptions of the method. I encourage the authors to resubmit the manuscript.

Best regards,

Klaus