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Interactive comment on "OSL rock surface exposure dating as a novel approach for reconstructing transport histories of coastal boulders over decadal to centennial timescales" *by* Dominik Brill et al.

Anonymous Referee #2

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This study attempts to determine the exposure ages of some large wave-transported boulders at the coast of Rabat, Morocco, using OSL rock surface exposure dating (OSL-RSED). The final exposure ages are however deemed as unreliable (i.e. imprecise and inaccurate) because of large data scatter, resulting in significant fitting uncertainties, and underestimated due to the erosion of boulder surfaces. This is altogether not very surprising, given that neither the selected lithology nor the chosen geomorphic settings are suitable for OSL-RSED technique.

OSL-RSED requires sensitive quartz and feldspar minerals, while the target boulders

in this study are calcarenite, a type of limestone that is predominantly composed of carbonate, which does not have the required luminescent properties for OSL dating. OSL-RSED is also based on the sunlight-driven evolution of mm- to cm-scale luminescence-depth profiles beneath rock surfaces, and is thus very susceptible to the effect of erosion, down to sub-mm scales. Such erosion-sensitive profiles cannot be used to derive reliable surface exposure ages from boulders undergoing wave and bio-erosion at rates of $\sim$1 mm.a-1, as is the case in this study.

While I appreciate the amount of effort the authors have put to overcome the challenges arising from this adverse combination of poor luminescence properties and erosion, I am afraid their manuscript, at its present form, is not rigorous enough to be considered for publication in Esurf. I could consider this study as a useful methodological contribution to the rapidly growing literature on OSL-RSED if the OSL methods were sound and the data were treated properly. But in my view, this is unfortunately not the case here. In the following, I give an account of both conceptual and methodological issues, which particularly seem problematic to me and try to explain how they could be dealt with differently, where possible. In my opinion, the manuscript may only be considered for publication after addressing these issues properly in a new submission.

Geomorphology and process/hazard information:

The application of OSL-RSED to coastal boulders as is shown in Fig. 1 is oversimplified, as it does not take the effect of reworking into account. If storm surges have enough energy to detach fresh boulders from bedrock, it is very likely that they can rework (slide and overturn) the previously detached boulders sitting loose on the beach as well. It is thus quite conceivable to imagine that some of the surfaces have undergone multiple burial and exposure events, and not only a single continuous exposure event after detachment, as is conceptualised in Fig. 1. In this environment however, the dose rates are low and the burial events are too short (because storm events have high frequency and occur on decadal timescales) to leave a record in the shape of the OSL-depth profiles. Thus, an observed OSL-depth profile measures the cumulative exposure time since the detachment event, and has no record of the subsequent storm events that might have reworked the surface.

Consequently, even in the absence of complications due to e.g. erosion and poor luminescence characteristics, such profiles are not particularly useful for deriving process information in similar geomorphic settings. They cannot be used for reconstructing boulder transport histories (as the title suggests), because they do not have a memory of the burial events, and they are not good proxy for storm events either, because they only record the single event that detached them from the cliff and not any of the subsequent storm events. One could argue that subsequent events of similar or higher energy are expected to pluck fresh blocks that could also be dated in a similar manner to give a chronology for the storm events. In that scenario, one would expect to see an overall trend of longer exposure events (the so-called "transport ages" here) and thus deeper OSL profiles as one moves farther from the coast, because the storms should gradually push the older boulders inland with time. But this does not seem to be the case; at least not here. For example, according to the age control, sample VAL 6 at a distance of ~80 m from the cliff seems to be younger than sample VAL 4, which is located only ~25 m from the cliff. This presumably implies that boulder detachment is not merely driven by wave power, but is also controlled by other factors such as joint formation and orientation. This inherent geomorphic character can limit the use of OSL-RSED to derive process/hazard information from coastal boulders.

OSL-RSED data presentation:

I find the presentation of profile data in Figs. 4, A14-16 cluttered and obscure. The mean data points with standard errors include all the information one needs to evaluate the reliability of individual data points and the overall progress of the bleaching front in a given surface. These are also the data points that are fitted to derive either the exposure age or erosion rate. So, in my view, the presentation of individual aliquots and cores in the way it is done in Figs. 4, A14-16 does not provide any useful information and impedes a proper assessment of the quality of the data.

The fits to the profile data that are used to derive the parameter values in Table 2 are not shown. Without the fits, one cannot evaluate their goodness and the reliability of the resulting parameter values.

In order to enable a clear evaluation of the data, my suggestion is to only present the mean data points with standard errors and the fits to the mean data.

OSL-RSED calibration

The data from calibration sample RAB 5-1 CAL in Fig. 5 seem to reach a plateau at ~0.8 and not 1. This makes me wonder i) why this sample was normalised differently and ii) how this apparently different normalisation must have affected the calibration values derived from this sample, and hence the mean calibrated parameter values used to derive the exposure ages/erosion rates. I note that the same (mean) data presented in Fig. A18 seem to have been normalised correctly. This needs to be revised, in case the authors choose the keep this sample in a new analysis of calibration data. Please see my comment below.

The data from calibration samples VAL 4-1 CAL 2 and RAB 5-1 CAL seem to be much more scattered than those from the other samples. Given the goodness (badness?) of the fits to such poor-quality data, I do not think that the parameter values derived from these samples can be deemed as reliable. It is also intriguing that although the data from these samples are much more scattered than those from e.g. sample TEM 3-1 CAL, the relative uncertainties on sample-specific \overline{\sigma\phi_0} values derived from these samples are smaller than the uncertainty on the corresponding value obtained for sample TEM 3-1 CAL.

It is argued that the sample-specific μ values have "huge uncertainties", and therefore site-specific values of μ have been derived instead as "a reasonable and necessary compromise". This argument is not supported by the presented data, and is not in accordance with our understanding of μ as a physical parameter. Firstly, the relative standard deviation (RSD) of sample-specific μ values derived from the calibration samples in Fig. 5 is ~34%, while the RSD of the corresponding \overline{\sigma\phi_0} values is ~210%. So, if sample-specific μ values can be dismissed because of large uncertainties and overdispersion, how can sample-specific \overline{\sigma\phi_0} values, which have even greater uncertainties and are more dispersed, be acceptable and taken as a shared parameter between the calibration samples? Secondly, if μ is dependent on lithology and all samples come from the same calcarenite bedrock, why not sharing μ between all the samples from all the sites? There is no evidence (or at least not presented here) that bedrock lithology varies from one site to another, so I cannot really see the logic behind sharing μ between samples from individual sites, but not between all the samples.

The issues mentioned above make me wonder about the robustness of the calibration approach undertaken here and the reliability of the resulting parameter values. To address these issues, I would reanalyse the calibration data by i) excluding the inferior data of samples VAL 4-1 CAL 2 and RAB 5-1 CAL, and ii) sharing μ between all samples or leaving it as a free sample-specific parameter in fitting.

Erosion rate modelling

The authors have followed a numerical approach (not "analytical" as is mentioned in line 333) to model the OSL erosion rates. But, the OSL erosion rate equation has an exact analytical solution that is already published (see Sohbati et al., 2018). So, there is no need and no scientific justification for making guesses at the solution numerically as is done here. The parameter values derived from the calibration samples can simply be inserted in the erosion rate equation and fitted to the profiles to give erosion rates.

Minor comments:

Line 17: I suggest "wave-driven" instead of "wave-emplaced". The boulders cannot be "emplaced" by waves and "transported" at the same time.

Lines 48-49: "...these approaches are restricted to certain boulder lithologies and time

scales.". So is OSL RSED; it is largely restricted to lithologies that "contain quartz and/or feldspar" and to timescales of "decades, centuries up to a few millennia" as is mentioned later in lines 61-62.

Line 63: Does the statement "...to reconstruct...tsunami frequency patterns..." imply that the tsunami events are expected to follow some sort of temporal/spatial patterns?

Lines 71-72: Consider to change "...erosion of post-transport exposed boulder surfaces..." to "erosion of boulder surfaces exposed after transportation" or something like that.

Line 79: Add "buried" before "sediment".

Line 94: What Fig. 1 is actually showing is a boulder that is detached from a wave-cut platform and overturned by waves. There is no "transportation" involved in the depicted scenario.

Line 147: I cannot see how 2-3 m-high spring tides can reach and exceed the 5-m high first ridge (as is mentioned in line 154) to flood Oulja. Lines 189-196: The preheat temperature should also be mentioned somewhere in these lines as Table A2 is in the Appendix.

Line 191: The stimulation time in Table A2 is 150 s and not 160 s.

Lines 197-208: I suppose the dose recovery and preheat plateau tests described in this paragraph were carried out to guide decision on the most suitable measurement protocol. In that case, this paragraph must precede the previous paragraph in which the actual measurement protocol is explained.

Line 207: The "burial ages" suddenly appear here. So far, only OSL RSED is discussed. It is also mentioned (in lines 104-105) that the buried sides of the boulders are inaccessible and "not tried in this study". So, speaking of burial ages here is confusing to me. In fact, it is first 60 lines further down in the text (line 267) that a careful reader may find out that what here is referred to as burial age, is actually the rock formation age, calculated by dating quartz extracts from deep layers within the boulders that have never seen light after rock formation. These should not be confused by boulder surface burial ages.

Line 212: I find the use of the term “background level” inappropriate here. Background level in OSL dating is commonly referred to while discussing the stimulation curves. I suggest “plateau” instead.

Lines 214-215: This sounds to be a subjective and qualitative approach towards removing the outliers, while there are various quantitative methods to identify them. One common approach that could also be used here is to remove those data points that are different than the mean by three standard deviations.

Line 229: Not sure what is meant by “comparable preconditions for sunlight exposure”. If the scenario is as simple as shown in Fig. 1, then all the boulders must have experienced comparable conditions (i.e. detachment and overturn). But if they are likely to have been reworked (i.e. moved and turned over multiple times) then it is very difficult to imagine how they could have had comparable exposure conditions.

Line 252: How about “target” instead of “dated”?

Lines 253-256: It is difficult for me to judge this inference by the way the data are presented in Fig. A12. The pure quartz BSL, K-rich feldspar IRSL and polymineral post-IRSL-BSL signals must be normalised and shown on the same graph to enable a direct comparison.

Lines 274-275: I assume that calibration was carried out before fitting the actual data? Please present the steps in data analysis in the logical order.

Line 279: Sohbati et al. (2011) is the correct reference.

Lines 293-295: This is an interesting observation that the calibration sample TEM 3-1 CAL that is collected from an inclined surface yields a \overline{\sigma\phi_0} value that is $\sim$3 orders of magnitude larger than the corresponding values estimated for the

horizontal surfaces. If this conclusion still stands after data reanalysis (see my comments above), it would be useful to report the tilt angle of the surface. At the moment, there is no data on the dependence of \overline{\sigma\phi_0} on the incident angle of solar radiation in the literature.

Line 307: What is meant by “inadequate” here?

Line 333: The approach of Lehmann et al. (2019) is numerical not analytical.

Line 356: “observed” instead of “achieved”?

Line 365: It seems unlikely to me that “mineralogy-induced does rate differences” can result in the observed scatter in data from such samples. Hot minerals such as zircon and K-rich feldspars are rare, if not non-existent, in calcarenite. Meyer et al. (2018) have attributed similar scatters in their data to the presence of opaque minerals and iron hydroxides, which strongly impede the penetration of light with depth. In the absence of any independent evidence, this seems more reasonable to me as en explanation here.

Lines 366-368: I am not sure I follow. How can the aliquot-to-aliquot variation in feldspar content can give rise to additional scatter in profile data? Does it mean that test dose is not adequately correcting for this possible variation? Why not? What is the evidence?

Lines 375-378: While the interpretation that age underestimation could have been caused by unreliable \overline{\sigma\phi_0} values and erosion of the boulder surfaces may be right, it would nevertheless be interesting to see what erosion rates one would get by applying the erosion rate model to samples that do not seem to suffer from age underestimation. The erosion rate of such samples must be negligible compared to the erosion rates of the samples showing age underestimation. This should provide a good basis for your interpretation.

Line 377: Does “inadequate” mean “unreliable” here?

Line 388: What is meant by “environmental factors beyond the exposure time”?

Lines 418-419: It may be worth mentioning here that, in retrospect, IRSL signals were likely to work better than the post-IRSL-BSL signals for these samples.

Line 422: What is considered as “insufficiently bright signals”? If the post-IRSL-BSL signals shown in Fig. A12 are typical for these samples, they are all well above background by more than 3.

Line 431-434: 1) The ages obtained from eroding surfaces are “apparent” surface exposure ages. The fact that they underestimate the expected ages, does not mean that they are inaccurate. They may be accurate, but they simply do not reflect the age of the event of interest. 2) There is no scientific basis to support this general statement that the ages from inclined surfaces are inaccurate. Surfaces can be dated regardless of their orientation provided that suitable calibration samples are available.

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