Reply to interactive reviewer comment by reviewer 1 (Pedro Costa)

We appreciate the constructive comments and suggestions on our manuscript. Below, we will reply to each of them separately.

Your manuscript is very well prepared. It is nicely written and fits perfectly within the scope of the journal. The figures serve their purposes very well. In fact they illustrate with high-quality the reasoning forwarded and facilitates the reader's job because they are very informative. Nevertheless, their number seems a bit excessive and a couple of them couple be merged (e.g.A14-A15-A16).

We decided to use a large number of figures to document our findings to the reader as comprehensive as possible. However, we agree that an excessive use of figures may be rather distracting from the main aspects of the paper. We will therefore merge figures of the appendix where possible in a potential revised version. Specifically, Figs A14 to A16 can be simplified by only presenting average values and omitting individual measurement data (as also recommended by reviewer 2). This would allow the IRSL data presented in Fig. A16 to be merged with the associated plots in Figs A14 and A15. The same applies to figures A18 and A19.

The text flows well and, with the exception of very few misspelling words, it is impeccable to read. References seem to be updated and formulas used are properly formatted.

Regarding science, this manuscript focus on one key issue on storm and marine deposits, namely in boulder deposits. It is a known problem to accurately date the transport of these boulders in coastal settings and it is a theme that have constrained the accurate establishment of return periods and hazard assessments in many locations worldwide. The authors used a well-controlled setting within a short-time window of observation which allowed comparison with aerial/satellite imagery. Thus, narrowing time-interval of transport being studied. The concept and the example selected is interesting and very sound. However, several question still remain to be answered. I will raise a few below but first would like to stress that I feel this manuscript clearly addresses a relevant topic and, with the results presented, moves science forward.

The "new" OSL methodology presented is robust and should/needs to be further tested in other locations. A shame we do not have this methodology compared with other dates from other previously studied locations. The fact that is from specific locations clearly puts forward its potential but still leaves some doubts regarding its reliability. It would be interesting to have further direct age comparisons.

We absolutely agree that independent age control is required to better evaluate the reliability of the dating approach. Unfortunately, most alternative dating techniques that have been used for determining boulder chronologies so far (i.e. mainly radiocarbon and U/Th dating of coral boulders or attached organisms) are associated with pure limestone lithologies, which cannot be used for OSL dating. Cosmogenic nuclide dating that would work on the same rocks, is not sensitive enough to provide useful age control due to low production rates at sea level and the comparatively short time scales of a few centuries or less. There are currently plans to try to establish a lichen chronometry for the study site, an approach that showed large potential for the time scales we are talking about in a recently published study (Oliveira et al., 2020, Progress in Physical Geography). But even if this attempt should be successful, it will take years to work robustly.

Similar constraints apply to most other boulder deposits. So when we selected the site for this study, we chose boulders with potentially adequate properties for OSL-RSED (which excludes pure limestone boulders due to the lack of quartz and feldspar, and magmatic boulders due to problems with clearly identifying overturning), for which at least age control in form of satellite data and observations for the last decades was available. We realized that the reasoning for site selection may not be clear enough in the original manuscript and will add 1-2 explaining sentences to the introduction of a potential revised version. Since this age control is undoubtedly limited, the presented study is of course only a first attempt to better understand the potential and the challenges associated with the dating approach. More case studies are definitely required to further evaluate the reliability of the dating approach, and we think that the selection of future sites will significantly benefit from the conclusions drawn from our data.

One aspect that concerns me is the obvious dependence on mineralogy. Limestone coastal areas will still be a challenge and one that needs to be addressed. Nevertheless, this manuscript clearly points very interesting future research directions.

Indeed OSL-RSED cannot be applied to pure limestone boulders, which unfortunately excludes a large portion of all boulder deposits, particularly in tropical regions. However, the approach promises to provide chronological information for boulder sites with quartz and/or feldspar bearing lithologies, such as sandstones, calcarenites and igneous boulders, which also account for a significant number of boulder sites. In other words, we do not pretend to present a dating solution that is applicable to all boulder deposits, but a technique that might provide chronological information for some of them. It is, however, important to highlight, that OSL-RSED can address boulders which are specifically hard to date with alternative approaches so far. Most existing chronologies for Holocene boulders are restricted to limestone boulders that are composed of or associated with calcareous organisms datable by radiocarbon or U/Th. We will try to document these lithology-related limitations and chances of OSL-RSED more explicitly in the introduction and conclusions of a revised version.

The mineralogy-dependence is an obvious constrain to this methodology. This is also evident when we have weathering or erosion. There are micro-erosion meters and they should have been used. I am aware erosion meters have slow rates and require a larger time-window of observation, nevertheless the modelled erosion rates represent for me a huge degree of uncertainty that might have been avoided with empirical data. Furthermore, these rates are highly controlled by lithology, mineralogy and texture. So, this section of the manuscript is valuable but would benefit from a larger discussion on its shortcomings. Furthermore, this is a key issue in the new OSL methodology: before dating the surface, one must very accurately establish the erosion since deposition.

We appreciate the suggestions to improve the discussion of erosion as a key factor for reliable OSL-RSED ages. Micro-erosion meters are a very good idea that we unfortunately did not consider when starting the study, but which should be included in systematic future studies on OSL-RSED as a possible means of better evaluating modelled erosion rates inferred from the OSL data. We already discuss the uncertainties introduced by dating unstable (eroding) surfaces and consider the influence of texture and mineralogy on erosion rates, since these are inherent factors controlling the model output of individual samples. We, however, agree that the paper would also benefit from a critical discussion of the approach we used to determine erosion rates and about benefits of potential alternative approaches (such as erosion meters). This will be implemented in an extended discussion of erosion rates in a revised version of the manuscript.

Regarding the study case, it has been widely established that in many coasts along the North Atlantic from Iceland (Etienne and Paris, 2010), Ireland (Cox et al., 2019) to Portugal (Oliveira et al., 2020) boulder deposits are essentially associated with storm events. There are occasional cases where tsunami origin has been discussed but many times with caution. In that sense, the authors should be less bold on lines 470-475 in particular when comparing case studies with multiple dating methodologies with others with a single methodology or even with just a single measurement. So, the dominance of short-lived and frequent storms on the creation and shaping of boulder deposits is natural *in particular in areas not so prone to tsunami events like the North Atlantic. This raises the issue of poor and difficult recognition of tsunami boulder deposits except when very specific dates are obtained (which is very difficult) or when size of boulders and its heights allows to disregard storm origin...but even then, there is the possibility of being palaeo-storm signatures of past higher sea-levels. So, to conclude the data provided from the study case reinforces the reasoning above and I recommend the authors to stress this aspects by adding a couple of sentences on this.*

Thank you for this comment. We agree that the aspect of discriminating between storm and tsunami origin might need a bit longer discussion. In essence, our data support your opinion that in most regions the majority of coastal boulders are associated with storms and that a tsunami origin at such locations is usually hard to verify with the chronological data available. As such, our data also support that boulders identified along the Atlantic coasts of Morocco and Iberia have to be treated with caution when it comes to discussing their tsunami origin, since the associated chronologies usually do not allow to precisely differentiate specific events. It is, however, right that most of the associated studies already acknowledge storms as an alternative transport mechanism. We apologize, if our formulation has implied something else, and will use a more cautious wording in a revised version of the manuscript. There, we will also discuss the difficulties of tsunami boulder recognition with more detail.