

Earth Surf. Dynam. Discuss., author comment AC2
<https://doi.org/10.5194/esurf-2022-18-AC2>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Reply on RC2

Rémi Bossis et al.

Author comment on "Initial shape reconstruction of a volcanic island as a tool for quantifying long-term coastal erosion: the case of Corvo Island (Azores)" by Rémi Bossis et al., Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2022-18-AC2>, 2022

Bossis and colleagues present a methodology for reconstructing the initial shape of volcanic islands to quantify coastal erosion. They apply this method to Corvo Island in the Azores and find that variations in eroded volumes in the coastal zone between different sectors of the island correlate more strongly with differences in the frequency of smaller, more common waves than with differences in the frequency of large, infrequent storm waves.

The manuscript is generally well organized, and I appreciated the introduction with nicely articulated broader context, motivation, and review of the literature. However, in present form, I believe the manuscript requires some significant revisions to make a meaningful contribution to coastal erosion and volcanic ocean island research, namely: (1) removal of all quantification and discussion of "cliff retreat distances" and "retreat rates" inferred from the distance between the presumed initial and modern coastline/cliff top; (2) more careful discussion and consideration of the influence of island vertical motion and relative sea level change on the results; (3) restructuring and further analyses to make the comparison between the sector eroded volumes versus wave conditions the main focus and result of the paper.

We thank the reviewer for his strong support and his constructive and detailed comments. This second reviewer questioned the use of platform width to estimate an average cliff recession rate, because the platform can also re-erode vertically and horizontally during eustatic variations. Reviewer 2 is right. We have therefore focused our paper on calculating the total eroded volume by ocean processes. We do mention the measure of net cliff position change (terminology inspired by what reviewer 2 suggested) in the discussion because this distance has been used previously in the literature and is currently the reference measure for some communities such as researchers measuring recession on a 1-100 year scale (see the following papers cited in the manuscript for example: Costa et al. 2019; Dewez et al. 2013; Dornbusch et al. 2008; Hapke et al. 2009; Marques et al. 2013...). To accompany this refocusing on the eroded volume we modified the introduction by referring to the scientific challenges that require such a quantification. In this way, we have also included an important new result which is an estimate of the rate of volume eroded per year for Corvo Island. For the secondary points, we invite the reviewer to consult our responses to all his comments below.

Regarding (1), as the authors themselves acknowledge, and as Mackey et al. 2014, Huppert et al. 2020, and others have shown, constraints on relative sea level history are required (and essential) to calculate an average coastal erosion rate over the long-term (you get significantly different rates when dividing the distance from the shoreline to the coast by age than when modeling the transient evolution of the profile under relative sea level change). Yet, even though these horizontal distances/rates inferred directly from the distance between the paleo- and modern coast/cliff are most likely not meaningful in any directly interpretable way to the consideration of coastal erosion, the authors throughout the manuscript present and report them as such and, later, compare them with wave conditions. Why? Seems to me that the analyses and interpretations would be significantly strengthened (and bad practice thwarted) by removing this and focusing only on the eroded volume measurements. This will not change the main results of the study. At very least, if these distances/rates are reported, I think it's critical that they be referred to as something more like "net shoreline position change" since they are NOT a proxy for seacliff retreat distances/rates.

We have taken this into account. Consequently, we have reworked the text by focusing our work on the evaluation of eroded volumes, which are not very sensitive to the location and modalities of erosion. We briefly discuss cliff recession under the name "Net cliff position change".

In the same vein, and regarding (2), I think throughout the manuscript, more care needs to be taken in clarifying the limitations of not accounting for island vertical motion and relative sea level change. Certainly removing the "cliff retreat" distances and rates will help, since the eroded volume calculations should be robust to not considering relative sea level history (so long as sea level occupations have remained between the bedrock elevation datums of the initial and modern shorelines (the coastal zone considered)— and the possibility that they may not have should be discussed). I also suggest making this limitation clearer (esp. by stating it earlier — see my line comments), and also directly accounting for a different possible eustatic sea level at the time of initial shoreline formation (e.g., by using the range of eustatic sea levels within the full possible age bounds of the instance of maximum extension) when reporting apparent net island vertical motion from the modern elevation of the IE. I suspect this will add to the already large uncertainty on the net vertical motion, but I think better to report this than to omit it. Are there any paleoshorelines or other constraints on Corvo's relative sea level history? It would be useful to discuss this if so.

As mentioned in the general remarks, we have fully followed the reviewer's recommendation by relegating the notion of cliff retreat to discussion and by refocusing the paper on eroded volumes. We are also much more cautious about the vertical motion of the island.

To our knowledge, no marine terrace or paleoshoreline observations have been published in the case of Corvo Island.

Regarding (3), I suggest the authors restructure and refocus the manuscript to highlight the comparisons between the coastal eroded volumes in the different sectors of the island versus wave conditions. The authors currently emphasize that the primary new contribution of this work is presenting a methodology for reconstructing initial volcanic topography that also takes into account bathymetry, but e.g. Huppert et al. 2020 already

did more or less exactly what is done here, extending submarine and subaerial portions of the profile to reconstruct onshore and offshore topo-bathymetry. Slightly different, but Zhao et al. 2020 also examined bathymetry to estimate the extent of coastal erosion on lava deltas — so I do not think this idea is the new or important advancement of this work.

The reviewer is right, there have been different strategies to reconstruct volcano shape considering both the aerial and the (relatively shallow) submarine topographies. Here we try to account for deeper morphology. We think there is a need for a simple but as much as possible standardized method to quantify the eroded volumes, which requires taking the deeper part of the volcano's morphology into account. We think we are successful on that point and report here our method, whose main innovations are a 3D exponential (not linear) fit and a double fit, for both the aerial and submarine parts. In turn we are happy that the reviewer finds our conclusions interesting, because that's the second goal of our project: using the method on a wide variety of edifices in order to better understand the rocky coast erosion of volcano islands.

However, I believe the trends between sector eroded volumes and wave conditions presented in figure 7 are compelling and provide a more important advancement in coastal erosion research, given the rather scant (but growing) evidence for an influence of wave climate on long-term coastal erosion. The authors nicely articulate this motivation in the introduction and go part of the way to making a complete story here. It'd be nice to see this fleshed out further (e.g., by computing various metrics of wave power — the quantity most often presumed to affect coastal erosion rates — to the different sectors and seeing how these correlate with eroded volumes, reporting the recurrence intervals of the most geomorphically significant waves,...). I believe that focusing on this scientific question, rather than on the methodology for volcanic island reconstruction, will also significantly broaden the audience and impact of this work, since relationships between wave climate and bedrock coastal erosion, all else equal, should apply equally to island and continental coastlines — taking full advantage of volcanic islands as natural laboratories to understand general controls on coastal erosion.

We thank the reviewer for this valuable comment. In the present case, the data we dispose of (Rusu and Guedes Soares, 2012), do not allow the calculation of the wave power. We keep this remark in mind and will try to apply it in our future work.

LINE COMMENTS:

ABSTRACT

Lines 8-9: Suggest rephrasing "to determine the timing of start-up" to clarify what this means. Perhaps "to correctly date the onset of coastal erosion"

We rephrased as "where and when erosion started".

Line 12: Nitpicky but suggest removing split infinite: "to spatially quantify" □ "to quantify spatial patterns in coastal erosion..." or "to quantify coastal erosion continuously along coastlines"

We think it would make the text heavier, so we prefer not to make this change.

Lines 13-14: Suggest removing sentence with initial island dimensions from abstract, since these do not seem to be a critical finding/result or necessary to understanding the context/scope/key advancement of this paper

The sentence about the initial size of the island has been removed from the abstract.

Lines 17-18: "These values are consistent with the orders of magnitude of" □ "These values are the same order of magnitude as"

This paragraph was deleted when the text was reworked to meet the requirements of the reviewers.

Lines 18-19: Can you clarify what exactly this means? Coastal erosion rates or volumes? Wave height, power,...?

This paragraph was deleted when the text was reworked to meet the requirements of the reviewers.

Line 19: "highlight a stronger erosion control" is a bit oddly worded □ "a stronger control on erosion by"

We changed "a stronger erosion control" to "a stronger control on erosion".

Line 19: suggest changing "moderate and usual" to "smaller and more frequent"

We changed "moderate and usual" to "smaller and more frequent".

Line 20: What does "consolidate the method" mean? Perhaps change "strengthen and consolidate" to "streamline and improve"?

We changed "strengthen and consolidate" to "streamline and improve".

INTRODUCTION

Lines 23-24: Perhaps this sentence should be qualified a bit..."In many coastal environments..." to acknowledge that weathering/wetting-drying of tidal platforms could be the dominant geomorphic process on some rocky coastlines

This paragraph was deleted when the text was reworked to meet the requirements of the reviewers.

Lines 27: Suggest adding historic maps and surveys, e.g. Dornbusch et al. 2008

This paragraph was deleted.

Line 41: Delete "over the short term" or perhaps change to "...and are thus unlikely to be recorded over historic timescales" (since they occur very rarely over long timescales as well)

This paragraph was deleted when the text was reworked to meet the requirements of the reviewers.

Line 41-42: In a similar vein, suggest changing to e.g. "Long-term measurements are more likely to include these rare events..."

This paragraph was deleted when the text was reworked to meet the requirements of the reviewers.

Line 42-43: I'm not sure "quantify a delay in the short-term versus long-term cliff retreat" is quite right and suspect you mean to say something more like "understand discrepancies between short-term versus long-term cliff retreat rates..."

This paragraph was deleted when the text was reworked to meet the requirements of the reviewers.

Line 42: Again, suggest more careful wording (and perhaps easiest to add this point as a clause on the last sentence" e.g. "for instance arising from the inclusion of rare catastrophic events in long term records"...then pointing out the implications for better understanding natural hazards.

This paragraph was deleted when the text was reworked to meet the requirements of the reviewers.

Lines 45-47: You might add some citations of other studies that have attempted to quantify contributions from long-term sea cliff retreat to deep sea deposition outside of Europe e.g. Couvaut et al. 2011, Sharman et al. 2021

Unfortunately, we did not cite these interesting works because the corresponding paragraph has been deleted in the current version.

Line 50: Citation for previous estimates?

The corresponding paragraph has been deleted when the text was reworked to meet the requirements of the reviewers.

Line 51-52: Sentence "Lastly,..." needs some rewording, maybe "Lastly, studies of historic coastal erosion have shown first-order controls of rock strength on erosion rates (e.g., Premaillon et al. 2018), rather than dominantly climatic or tectonic controls." Also suggest adding at least "e.g." to the citation, since e.g. Benumof et al. 2000 have also documented an overriding influence of lithology on historic seacliff retreat rates

The corresponding paragraph has been deleted when the text was reworked to meet the requirements of the reviewers.

Line 54: Seems like this statement could be a bit more specific e.g. "with regards to the dominant drivers of coastal erosion..."

The corresponding paragraph has been deleted when the text was reworked to meet the requirements of the reviewers.

Line 56: Coastal erosion rates?

The corresponding paragraph has been deleted when the text was reworked to meet the requirements of the reviewers.

Lines 64-65: Suggest deleting or changing "conversely" to e.g. "In addition" since I don't think this is a contrary point? This sentence could probably, in fact, be deleted all together since this point does not seem relevant to quantifying coastal erosion rates

We made the change.

Line 68: "fitted" □ "fit" Also "function curve" seems redundant...suggest using just "function" or "curve" or suggest changing to/clarifying (if this is correct?): "...can be reconstructed by fitting a function to remnant surfaces between the peak and shoreline, then rotating this function around the center of the edifice."

This paragraph was deleted when the text was reworked to meet the requirements of the reviewers.

Lines 72-79: Suggest adding the additional point that some volcanic ocean islands are predominantly constructed during a relatively short but voluminous period of shield-stage volcanism, accounting for the vast majority of their constructional growth (e.g. the Hawaiian Islands, Moore & Clague 1992)

This paragraph was deleted when the text was reworked to meet the requirements of the reviewers.

Line 73: Not sure I fully understand what "and the maximum extension cannot have the same age all around the island" or more specifically how that point differs from the first half of the sentence. Suggest clarifying or deleting if redundant

This paragraph was deleted when the text was reworked to meet the requirements of the reviewers.

Line 76: I'd suggest qualifying this statement a bit "indicates" □ "can indicate" since there are ~ radially symmetric islands (e.g. Gran Canaria, Kaua'i) with multiphase/asynchronous constructional histories

This paragraph was deleted when the text was reworked to meet the requirements of the reviewers.

Lines 80: What exactly is meant by maximum extension? Does this mean maximum subaerial extent? Specify if so. In this vein, in the following lines, probably more relevant than degradation to a decrease in maximum subaerial island extent early in an island's history is subsidence (esp. rapid isostatic subsidence following construction), at least in many volcanic island chains

We thank the reviewer for this good remark. We changed "maximum extension" to "maximum subaerial extent".

Line 90-91: Huppert et al. 2020 showed that constraints on relative sea level history are also necessary to calculate an average coastal erosion rate over the long-term (you get significantly different rates when dividing the distance from the shoreline to the coast by age then when modeling the transient evolution of the profile under a possible relative sea level history). Hopefully this is addressed later, but these statements should be corrected here too (and in the sentences in lines 92-94, to which you could add that volcanic islands also have the benefit of often being good locations to preserve paleo sea level indicators, so you may have more constraints on RSL history — another essential ingredient — in these locations too)

As mentioned in the general remarks, we have fully followed the reviewer's recommendation by relegating the notion of cliff retreat to discussion and by refocusing the paper on eroded volumes. Hence, even if the present paragraph has disappeared, we improved the text in this sense.

BACKGROUND & HYPOTHESES

Lines 97-99: Numerous other studies have done this too (e.g., Seidl et al. 1994, Menéndez et al. 2008, Ferrier et al. 2013, Murphy et al. 2016) so I'd suggest at very least adding "and others" (and potentially also shortening this list, since it's not exhaustive anyways)

The mention "Numerous authors (e.g. [...])" has been added to the series of citations.

Line 111: Suggest changing "results" □ "can result" or adding "In many settings," since coastal erosion also occurs on e.g. shorelines where coral growth may be more dominant in setting the overall coastal morphology, so that these coastlines do not necessary have an erosional shelf and coastal cliff.

We thank the reviewer for this good remark. We added "In many settings" to the beginning of the sentence.

Line 111-112: Suggest deleting or changing "in the nearshore zone" since this is usually associated with the shallow offshore (plus is redundant with saying a coastal cliff)

We deleted "in the nearshore zone".

Lines 114-115: Seems like this statement needs to be qualified a bit, since e.g. in a rapidly subsiding environment like Ka Lae, Big Island of Hawai'i the SLA is ≥ 6 m deep

Indeed, the depth of the SLA is context dependent. That is why we specified "does not depart from the current sea level by more than a few meters".

Lines 115-118: I'm not sure I understand the basis for, nor agree with, many of the statements in this passage... During wave shoaling and transformation before wave breaking, wave energy is conserved (according to Airy wave theory.) Guessing the first sentence here means to say that orbital velocity decreases with water depth? Waves enter shallow water and start to transform because they "feel the bottom" when they enter depths one half their wavelength, which goes as the square of their wave period, which can vary by at least a factor of two e.g. even for different wave regimes impacting the different coasts in the Hawaiian Islands...meaning shallow water depths in Hawaii alone can vary by a factor of at least 4...and I'm not sure where the 10 m depth cited here as a universal standard is coming from. What is the evidence/reference for significant seabed erosion at depths within 10 m below sea level? Seems like this would be difficult to the determine on a subsiding coastline where erosion occurring at the shore face inevitably passes through the shallow water or surf zone. The (old) reference I'm aware of presenting evidence for measurable sea bed erosion on rocky coasts is using etching of pyroxene grains on the Santa Cruz, California coastline (Bradley 1958) — and these suggest that significant sea bed erosion only occurs within the surf zone (after waves break), since sediment transport at greater depths is not sufficient to cause abrasion. If this is indeed the case, the depth of wave breaking also depends on wave period and wave height (which can vary worldwide and even at local scale on island coastlines facing different wave regimes by orders of magnitude) — so I am even more concerned about the suggestion that there might be some (even ballpark/ back-of-the-envelope) standard depth at which sea bed erosion might be occurring. The Treinhale papers cited use a model that assumes erosion occurs within the shallow water zone and demonstrate that various factors (wave period, tidal range,..) affect the depth at which erosion by waves occur in the nearshore.

We agree this part needed clarification. Now we cite the paper by Dietz and Menard (1951), the reader can refer to it. In this paper, Dietz and Menard nicely relate the shape of the shelf break to waves and show that significant wave abrasion is unrealistic below 10 m under the water level. So, we changed the wording to: "As they approach the coast, the waves conserve their energy until they break. From this point on, the energy of the swell

is dissipated: one aspect of this dissipation is erosion of the bedrock until about 10 m below sea level (Dietz and Menard, 1951; Trenhaile, 2000, 2001; Sunamura, 2021). The erosional feature formed during the present-day sea level by wave action therefore has a theoretical depth ranging from around 0 m at the coast to about 10 m at the edge and it is called a shore platform."

What is the basis for the statement "The maximum of erosion takes place where water depth is similar to the mean wave height"? I could not find this in the Trenhaile papers, nor is it consistent with the model used therein (seabed erosion decays exponentially with depth to account for the decrease in orbital velocity with depth and limited evidence of seabed erosion - so the maximum seabed erosion should occur in the shallowest water?)

We agree that this sentence needs more details. In the end, we felt that it did not add anything important, so we deleted it.

Line 120: suggest deleting "a" before stochastic

We deleted "a" before stochastic.

Line 121: change comma to period

We changed the comma to a period.

Line 122-123: Seems like there are more relevant references to cite here than a general geomorphology textbook and two modeling papers that make this assumption. Suggest citing e.g. Huppert et al. 2020, Zhao et al. 2020, Young et al. 2021,... instead

We added these references to the other citations here.

Lines 128-143: I'm concerned that this does not mention the slope break formed on ocean islands at the transition from submarine to subaerially cooled lava, referred to in this manuscript as the initial extension (IE) and first mentioned in line 148. This also can generate a submarine shelf + shelf break on net subsiding islands (likely the majority of island coastlines), particularly on basaltic shields where the subaerially cooled portion of the edifice is typically quite gentle. Can you mention this slope break here first to help clarify how it differs (and how it can be distinguished) from a slope break formed by erosion?

The reviewer asks for an earlier introduction of IE and its significance. To comply with this recommendation, we have added the sentence to line 68: *"The initial silhouette is marked by a break in slope at sea level, at the transition between the aerial and submarine areas (e.g. Ramalho et al., 2013), which we will later refer to as IE."*

Line 132: "lowest eustatic level" should be changed to "lowest relative sea level" since

few, if any, volcanic ocean islands are stable over their evolution - most experience subsidence (and sometimes uplift) that frequently exceeds the amplitude of eustatic sea level variations...so it's the lowest relative sea level, not just the lowest eustatic sea level, that potentially bounds the portion of the volcanic edifice that experiences coastal erosion

We thank the reviewer for this good remark. We changed "lowest eustatic level" to "lowest relative sea level" here.

Line 149: Not sure what the sentence "The IE is the reference..." means here. Necessary or can it be deleted?

As we refocused the study on eroded volumes, this sentence has been deleted.

Line 151: "total vertical motion" □ "total relative sea level change"

We changed "total vertical motion" to "net relative sea level change".

Line152: suggest changing "total" to "net" and adding "assuming that glacioeustatic sea level has not been higher than present sea level since the island formed." Delete "corresponding to the elevation of the IE" since this is not correct as stated (one must account for the difference in glacioeustatic sea level at formation and present, too, in order to infer the net vertical motion from the elevation of the IE).

In order to rework this part following the reviews, these sentences have been deleted. But we change the remaining text accordingly.

Lines 153: Same point here — this sentence is not correct, since even an uplifting island whose IE formed at an instance of a eustatic SL lowstand could have a submarine IE

In order to rework this part following the reviews, this paragraph has been deleted.

Line 153: Ah ok, good to see this point but I still don't think it makes sense to refer to vertical motion relative to a time-varying datum...suggest mentioning eustatic SL change earlier in this paragraph, so that the preceding (currently incorrect) statements can be avoided

In this improved version, we have written things in a subtler way, and we hope it is now clear that one can eventually analyses vertical movements but with a lot of care about relative sea level.

Lines 159-160: I would suggest qualifying this sentence, since there's scant direct evidence of nearshore sea bed erosion (see earlier comments and references) "may occur vertically." I also find it confusing, after the discussion of relative sea level change, the notion that the depth of the ESB should coincide with the greatest depth at which sea bed

occurs. This would be highly coincidental (and unlikely) on a subsiding or uplifting coastline.

We understood this paragraph was confusing. We entirely reworked it for easier understanding.

Lines 167-168: The citations in this sentence are confusing, since they seem to imply that Huppert et al. 2020 used the model of Ramalho et al. 2013. "eustatic" □ "relative sea"

We changed "eustatic level" to "relative sea level". The corresponding sentences have been modified to clarify what is owed to the individual previous works.

Line 171: Necessary to define/denote this as downwearing here (esp. in this atypical context where progressive horizontal retreat by receding sea level erodes down bedrock, not vertical incision)?

We removed this and now we don't mention downwearing in the manuscript anymore.

Lines 175-178: I like the approach of using a volumetric measure of erosion to sidestep the issue of the large discrepancy in apparent horizontal retreat rates from a distance between markers divided by age versus a rate taking into account relative sea level change and the effect of distributing coastal erosion across range of bedrock datums occupied by sea level. However, I think this warrants a statement here — similar to previous statements made earlier in the manuscript — that the volumetric rates lump together erosion occurring by various processes: sea cliff erosion, possibly some minor sea bed erosion in the surf or shallow water zone, and most importantly, mass loss occurring because of undercutting and rockfalls/landslides/gravitational collapses

This specific part disappeared when the manuscript was improved. However, we use this good advice and add the recommended statement.

Lines 176-178: Echoing my comments at lines 90-91, and as you state in the preceding lines 167-173, horizontal rates calculated in this way are most likely not meaningful. Why do you calculate them then?

This specific part disappeared when the manuscript was improved. However, as mentioned earlier, we followed the reviewer's recommendation.

Lines 179-183: Not all previous work has relied upon only onshore topography to reconstruct initial topobathymetry e.g. Huppert et al. 2020 did more or less exactly what is done here, extending submarine and subaerial sectors of the profile to reconstruct onshore and offshore topobathymetry. Slightly different, but Zhao et al. 2020 also examined bathymetry to estimate the extent of coastal erosion on lava deltas. This should be acknowledged and the claims of a new method removed.

It is true that the use of offshore bathymetry to infer coastal erosion is not new and we

better acknowledge them in the revised version, but we partly disagree with this comment concerning the novelty of our approach. Quartau et al. (2010) used multibeam sonar, chirp and boomer seismic reflection to map the morphology of the Faial Island's shelf in the Azores to determine the shelf. Zhao et al. (2020) used bathymetric LiDAR and historical sounding data to estimate shoreline retreat between the submarine platform edge and the modern coastlines of Azores, Hawaiian islands and Ascension Island. Huppert et al. (2020) used submarine bathymetry to identify the slope break and reconstruct a paleo-topographic profile above it assuming that the paleo-profile corresponds to the steepest line extending from the modern sea-cliff top to the slope break. Nevertheless, These studies have focused on the platform topography at relatively shallow depth (<120 m) and were mostly focusing on the determination of the ESB, not the deep submarine topography. Mitchell et al. (2003) used deep submarine topography of Canary Islands to quantify erosion on the submarine flanks but they did not try to fit the topographic profiles with a geometrical model. Our approach here takes advantage of all the available offshore topographic data to better constrain the paleo topographic profiles and the shape of the Islands. In particular, we show that the submarine profiles of the Corvo Island are consistent with an Exponential model, which, to our knowledge, is a novelty of our contribution. This is this reconstruction that allows the eroded volume to be better constrained.

Following the reviewer's comment, we add the following text at the beginning of the discussion (line 362): *"The use of offshore bathymetry to infer coastal erosion is not new but it has been limited to a relatively shallow depth (<120 m) (Quartau et al., 2010; Huppert et al., 2020; Zhao et al., 2020). Mitchell et al. (2003) used the deep submarine topography of the Canary Islands to quantify erosion on the submarine flanks but they did not try to fit the topographic profiles with a geometrical model. Our approach here takes advantage of all the available offshore topographic data to better constrain the paleo-topographic profiles including the deep part of the island flanks. In particular, we show that the submarine profiles of Corvo Island are consistent with an exponential model, which, to our knowledge, is a novelty of our contribution."*

Lines 193-195: I don't think this is correct as stated. The eroded volume measurements most certainly include mass loss due to gravity collapses but themselves do not provide evidence of this.

We thank the reviewer for this remark. In fact, the evidence of gravity collapse is given by the position of the IE, not by the measurement of the eroded volume. This measure of eroded volume over the platform is theoretically distinct from the volumes involved in the shelf edge collapses. In order to clarify this point, we added "by the comparison of the horizontal position of the IE and the ESB." at the end of the sentence.

METHOD

Line 222: What does "fairly marked" mean? Can you quantify this?

We removed "fairly" because it is not really relevant, and we can't really quantify this.

Line 227: Can you clarify what you mean by "slope threshold of 15 deg"? Is that the magnitude of slope difference between the shelf and deeper bathymetry or the absolute maximum slope of the shelf?

We changed "slope threshold of 15°" to "absolute maximum slope threshold of the shelf of 15°".

Lines 231-233: This seems incorrect as written...the area between the CCT and ESB, some of which may be submarine (no?) is part of the initial subaerial volcanic edifice whereas anything offshore of the ESB (but not necessarily the entire submarine portion) is the initial submarine part of the edifice

Yes, that is correct. However, in this sentence we focus on the areas inside the CCT and outside the ESB, not between CCT and ESB.

Line 247: What is the "top of the watershed"? Its highest point? The point furthest from its outlet? Please clarify.

We changed "top" to "highest point", to clarify.

Line 248: The direction is defined as the line? Suggest renaming "watershed direction" to "watershed centerline" perhaps since you utilize the intersection of these lines, not their directions/azimuths.

No, we use their directions/azimuths, so we keep "watershed direction".

Line 254: "allows to" □ "allows us to"

We changed "allows to" to "allows us to".

Lines 269-270: Suggest adding "and others" since this is quite a common approach also utilized in Seidl et al. 1994, Ferrier et al. 2013, Mackey et al. 2014,...

We added "and others" here and moved this paragraph in the discussion (section 6.1).

Lines 283-284: This needs a qualifier, "provided that island has not experienced sea levels occupying elevations above the CCT or below the ESB." or something along these lines, since e.g. the modern coastal cliff top may not be the highest topography eroded by coastal erosion if the island has uplifted and abandoned former sea cliffs onshore (e.g., as has been suggested on Kaua'i, Mackey et al. 2014)

We added "provided that island has not experienced sea levels occupying elevations above the CCT or below the ESB." at the end of the sentence; thanks to the reviewer!

Lines 287-290: Repeating myself, and as you have stated in this manuscript, this is not necessarily the total distance of cliff retreat, since changes in relative sea level can cause

sea cliffs to form on what is now the insular shelf, retreat, and subsequently be eroded away during relative sea level fall and coastal erosion — so this is not a meaningful measurement of total cliff retreat

The sentence about the total cliff retreat has been deleted, following the reviewer's advice.

Lines 291-294: Repeating my comment from line 152, this is not the total subsidence or uplift relative to the current sea level, since it does not account for the difference in eustatic sea level at the time of initial shoreline formation. This needs to be clarified.

In order to clarify, we changed "relative to the current sea level" to "relative to the past sea level at the time of the initial shoreline formation".

CORVO ISLAND

Line 295: "settings" □ "setting"

We changed "settings" to "setting".

Line 296: Suggest changing to just "Corvo Island"

We removed "Presentation of" in the title of this section.

Line 302: Suggest quantifying what is meant by "rather humid and with mild temperatures"

"Temperate oceanic climate" is maybe clearer and sufficient. So we just deleted "rather humid and with mild temperatures".

Line 304: "largely" □ "frequently" ?

We changed "largely" to "frequently".

Line 307: Seems this could be stated more clearly "Because of their modest elevations, the Azores do not generate considerable orographic rainfall, so the relics of their initial aerial volcanic morphology remain relatively well preserved." or something along these lines

We modified the sentence as proposed.

Line 321: "infrastructures" □ "infrastructure"

We changed "infrastructures" to "infrastructure".

Lines 324-325: "shaded map" □ "shaded relief map"

We added "relief" here.

Line 343: Suggest deleting "which seems sufficient for Corvo" since it's not clear what this means

We deleted "which seems sufficient for Corvo".

Lines 344-345: This sentence is clunky. Can you just say "The global GEBCO data is too coarse in resolution to identify the contours of the insular shelf"?

We changed the sentence as proposed.

Lines 350-351: Not clear what "It appears that the SRTM1 data are more accurate than the ASTER data." means. Clarify or delete. Ditto line 353 re: SRTM1 data...what does "quite precise, but lack accuracy" mean? In general, it seems this entire subsection can be significantly streamlined and shortened.

In order to rework this part following the reviews, this paragraph has been deleted.

RESULTS

Line 361: "more" □ "further"

We changed "more" to "further".

Lines 384-386: not sure these sentences are necessary but "representative data" □ "elevation grid points", "of the initial aerial shape" □ "in the initial aerial domain" (ditto for submarine). Why do you call it the "effective coastal erosion area" and not just "coastal erosion area"?

We changed "representative data of" to "elevation grid points in" and "shape" to "domain". We also removed "effective".

Line 391-392: Not sure I understand this sentence. "The grey area shows the southern sector, in which volcanic progradation has occurred. We excluded this sector from the analyses."?

We changed this sentence to "The grey area figures the southern sector where volcanic

progradation is located and which has been subsequently discarded for the analysis”.

Line 394: Suggest deleting “radially”

We deleted “radially” here.

Lines 395-397: To repeat earlier comments, I think more caution needs to be taken in calling this “total coastal erosion” or “cliff retreat” in figure 6A. Maybe “net shoreline position change” would be more accurate. Ditto for “coastal cliff retreat: in Table 2

The rose diagram for “total cliff retreat” has been removed in Fig. 6, and “Retreat” in the head of Tab. 2 has been changed to “Net Cliff position change”. We also removed “cliff retreat” in the text.

Line 412: Suggest adding “and relative sea level change” since some of this area loss (or perhaps gain, detracting from the net 80%, if the island has uplifted as potentially implied by the elevation of the IE) could be due to sea level inundation or retreat alone

We added “and relative sea level change” at the end of the sentence.

Line 412: “provide an insight” □ “provides insight”

We changed “provide an” to “provides”.

DISCUSSION

Line 420: “allows to” □ “allows us to”

In the revision of the paper following the reviewers' comments, this section has been removed.

Line 421: “the coastal cliff retreat” □ “net shoreline position change” Ditto in the next sentence

In the revision of the paper following the reviewers' comments, this section has been removed.

Line 432: dials? Not sure what this means...sectors?

We changed “dials” to “sectors”.

Line 434: Suggest paragraph break at "Moreover"

In order to rework this part following the reviews, this paragraph has been removed.

Line 449: "hardly" □ "not"

In order to rework this part following the reviews, this paragraph has been removed.

Line 450-451: You might mention (more explicitly) coral growth in many island settings too

In order to rework this part following the reviews, this paragraph has been removed.

Line 458: "total costal cliff retreat" □ "net shoreline position change" (both occurrences)

In order to rework this part following the reviews, this paragraph has been removed.

Line 459: "stand for the sectors to" □ "occur in"

In order to rework this part following the reviews, this paragraph has been removed.

Lines 461-469: As mentioned, I feel strongly that the calculation of a rate from the distance of shoreline position change divided by age is not a meaningful calculation, and I think this should be removed. Without accounting for island vertical motion (is there any evidence that Corvo has been stable since its formation?), it is not possible to state even the approximate fraction of time sea level may have been close to the present one

As stated before, we followed this recommendation and focus now on eroded volumes. We mention in discussion the net cliff position change, just for information.

Lines 472-475: What does "the theoretical depth of -130 m in the absence of vertical movements of the ESB" mean? Is this the estimated glacio-eustatic sea level (relative to present) at the end of volcanic construction/time of IE? Please clarify. Suggest changing "mean" to "net" in line 475

This part has been removed, but we put the ideas long before. It reads: *"The depth of the shelf break theoretically corresponds to the limit of wave action during the lowest relative sea level the island has experienced (Quartau et al., 2010; Ramalho et al., 2013). If the island is older than the last glacial maximum and its vertical displacement is negligible, the depth of the shelf break is theoretically around 130 m, i.e. LGM level (around 120 m) + wave action limit (around 10 m) (Shepard, 1973; Yokoyama et al., 2000; Trenhaile, 2001; Quartau et al., 2010)."*

Line 476-477: Didn't you just do this anyways? Or perhaps I'm confused about what mean uplift versus total uplift means here (are they the same?)? I presume for at least one of the quantities you refer to (mean or total uplift - though maybe these are the same?) you may want to rename this "net vertical motion."

We followed the reviewers and did not try any more to calculate vertical displacements.

I think it's also necessary to state earlier in this paragraph (i.e. than at lines 479-481) that differences in glacio-eustatic sea level at the time of initial shoreline (IE) formation versus present need to be taken in account too to estimate the net island vertical motion. Can you not at least report the range of eustatic sea levels within the possible age bounds of the IE and account for this in the net vertical motion estimate + uncertainty? It seems more appropriate to me to do this than report the value not taking this into account at all.

As stated in the previous remark, we do not try any more to calculate vertical displacements. However, we mentioned the effect of eustatic sea level in the introduction (see our response to the comment about lines 472-475).

Line 500: Would it be possible to show this data in rose diagrams instead of in tabulated form? It'd make it easier to compare visually to Figure 6 (and evaluate the interpretation at line 501). I'm not familiar with wave frequency reported as a percentage...what does this mean? Is this related to differences in wave period in the different sectors and/or longer term (hourly, seasonal,...?) differences in wave occurrence?

The data presented in Tab. 3 are already shown in rose diagrams in Fig. 6.

The wave data are outputs from a model used by Rusu and Guedes Soares (2012, Tab. 2). The model generated waves and returned their significant height (Hs) and their direction. These data do not contain temporal data. In Table 3, we show the repartition of these waves according to their significant height and their direction, in percentage terms. So the wave frequency for each sector represents the percentage of waves occurring in each sector, with a total of 100%.

To clarify, we changed "Wave frequency" in the head of Table 3 to "Wave occurrence".

Lines 504-505: Can you also plot eroded volume versus wave energy flux (i.e. wave power)? I guess I'm not sure how the reported wave frequency relates to wave period, but plotting wave power would account for differences in both the frequency and magnitude of waves simultaneously — and this is the metric often assumed to drive variations in sea cliff retreat rates in coastal erosion models (see e.g. Anderson & Anderson 2010). I'm guessing this was the motivation of plotting the product of frequency and height in the right-most panels of the top portion figure 7 (subplots should be lettered)?

As said in the previous point, the wave data used for our study do not contain temporal data. So, we can't have access to the wave period and thus we can't calculate wave power. This is why we have tried to approximate it by calculating the product of the significant height and the frequency (or occurrence) of the waves.

Following earlier comments, I do not see the value in plotting the "cliff retreat" (net shoreline position change).

We deleted "cliff retreat" here.

Line 506: "inception" □ "inspection"

We changed "inception" to "inspection".

Line 507: I'm not sure distribution is synonymous with frequency here. Suggest changing to frequency

We changed "distribution" to "frequency".

Line 508: "those" □ "that"

We changed "those" to "that".

Line 510: "more usual" □ "more frequent"

We changed "usual" to "frequent".

Line 525: "allows to" □ "allows us to"

We added "us" here.

Line 530: add "due to coastal erosion and relative sea level change"

We added "and relative sea level change" at the end of the sentence.

Lines 530-533: Suggest reporting eroded volumes here instead (or at least deleting sentences about "retreat distances" and "rates")

We deleted the two sentences about cliff retreat and we added a sentence on the rates of volume erosion.

Line 537-538: "consolidate and enrich" □ "streamline and improve", "improve the correlations..." □ maybe something more like "provide additional evidence to the influence of wave climate on long-term coastal erosion" ?

We changed “consolidate and enrich” to “streamline and improve”, and “improve the correlations observed in this study” to “provide additional evidence to the influence of wave climate on long-term coastal erosion”.

REFERENCES

Benumof, B.T., Storlazzi, C.D., Seymour, R.J., and Griggs, G.B., 2000, *The relationship between incident wave energy and seacliff erosion rates: San Diego County, California: Journal of Coastal Research*, v. 16, p. 1162–1178.

Bradley, W. C. (1958). *Submarine abrasion and wave-cut platforms. Geological Society of America Bulletin*, 69(8), 967-974.

Covault, J.A., Romans, B.W., Graham, S.A., Fildani, A., and Hilley, G.E., 2011, *Terrestrial source to deep-sea sink sediment budgets at high and low sea levels: Insights from tectonically active Southern California: Geology*, v. 39, p. 619–622, <https://doi.org/10.1130/G31801.1>.

Ferrier, K. L., Huppert, K. L., & Perron, J. T. (2013). *Climatic control of bedrock river incision. Nature*, 496(7444), 206-209.

Mackey, B. H., Scheingross, J. S., Lamb, M. P., & Farley, K. A. (2014). *Knickpoint formation, rapid propagation, and landscape response following coastal cliff retreat at the last interglacial sea-level highstand: Kaua 'i, Hawai 'i. Bulletin*, 126(7-8), 925-942.

Menéndez, I., Silva, P. G., Martín-Betancor, M., Pérez-Torrado, F. J., Guillou, H., & Scaillet, S. (2008). *Fluvial dissection, isostatic uplift, and geomorphological evolution of volcanic islands (Gran Canaria, Canary Islands, Spain). Geomorphology*, 102(1), 189-203.

Moore, J. G., & Clague, D. A. (1992). *Volcano growth and evolution of the island of Hawaii. Geological Society of America Bulletin*, 104(11), 1471-1484.

Murphy, B. P., Johnson, J. P., Gasparini, N. M., & Sklar, L. S. (2016). *Chemical weathering as a mechanism for the climatic control of bedrock river incision. Nature*, 532(7598), 223-227.

Seidl, M. A., Dietrich, W. E., & Kirchner, J. W. (1994). *Longitudinal profile development into bedrock: An analysis of Hawaiian channels. The Journal of Geology*, 102(4), 457-474.

Sharman, G. R., Covault, J. A., Stockli, D. F., Sickmann, Z. T., Malkowski, M. A., & Johnstone, S. A. (2021). *Detrital signals of coastal erosion and fluvial sediment supply during glacio-eustatic sea-level rise, Southern California, USA. Geology*, 49(12), 1501-1505.

Zhao, Z., Mitchell, N. C., Quartau, R., Ramalho, R. S., & Rusu, L. (2020). *Coastal erosion rates of lava deltas around oceanic islands. Geomorphology*, 370, 107410.