Cover Letter

Dear Simon Mudd,

We submit a revised version of our manuscript "Initial shape reconstruction of a volcanic island as a tool for quantifying long-term coastal erosion: the case of Corvo Island (Azores)". We received three detailed evaluations, two from referees and one from a community researcher. These reviews made three main points in our view:

- Neil Mitchell (RC1) supports this paper, but asks for a more in-depth treatment of the effect of sediment deposition on the flanks of the volcano that could artificially widen the marine abrasion platform, and thus overestimate the net cliff retreat. We evaluated this source of error by considering that all the sediment eroded by the sea is deposited as an extension of the erosion platform on the submarine flanks of the volcano. We thus estimate that the increase in the apparent width of the platform represents 20% in this extreme case. We have added a paragraph to the discussion concerning this source of uncertainty.

- Reviewer 2 (RC2), another supporter of our manuscript, also 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.

- Rui Quartau (CC1), despite his strong support of the manuscript, criticized our measurement of the width of the erosion platform from the IE point, at the intersection between the aerial and submarine topographic profiles, rather than the ESB. Following the change made, refocused on total eroded volume following the previous reviews, this became a very minor point in our paper. The platform width measurement is now only mentioned in the discussion.

We have taken into account all the comments of the reviewers, for which we thank all the suggestions and proposals for improvement. We will answer them point by point in the following.

We hope that our improved manuscript fulfills your expectation and is now publishable in Esurf.

Rémi Bossis, Vincent Regard and Sébastien Carretier

Response to RC1

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 Bossis et al. (in review).

Neil C. Mitchell

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12 July 2022

We thank the reviewer for his constructive comments and his strong support. The reviewer asks mainly for a more in-depth treatment of the effect of sediment deposition on the flanks of the volcano that could artificially widen the marine abrasion platform, and thus overestimate the net cliff retreat. We evaluated this source of error by considering that all the sediment eroded by the sea is deposited as an extension of the erosion platform on the submarine flanks of the volcano. We thus estimate that the increase in the apparent width of the platform represents 20% in this extreme case. In terms of volume, the increase is no more than 13%. We have added a paragraph to the discussion concerning this source of uncertainty. For the secondary points, we invite the reviewer to consult our responses to all his comments below.

The proposed method described in this manuscript is attractive in potentially providing a straightforward way to quantify the spatial variation in long-term erosion rate around volcanic islands. This effectively continues observations of apparent erosional asymmetry observed at islands with persistent wind and wave directions that appeared as nautical charts became more accurate. Menard may be one of the originators and certainly wrote about this in his book (some historical background could be interesting in the introduction). By building on the work of Karátson et al. (2010), it benefits from their study of many stratovolcanoes. It uses trends in both subaerial and submarine slope elevations to estimate the original coastline position. The results presented suggest erosion has been greatest on the side of the island where waves dominate at the present day.

I had some questions about the assumed geometry. There is some evidence that sediments released by erosion are not fully exported into the deep basins around volcanic ocean islands, as assumed by Bossis et al., but instead accumulate on their uppermost flanks. Examples include:

1. Short term development of the Capelinhos Surtseyan cone since its formation by a volcanic eruption in 1957/58 (Zhao et al., 2019). In this case, the original position of the coastline is known from aerial photographs taken during and immediately after the eruption. The platform slope break located using swath sonar is seaward of the

position expected from the starting coastline position by 100 to several 100 m, even allowing for dip of material between IE and platform edge. (These distances may not seem important but the cone was small so they actually represent a large proportion of the original cone diameter.)

2. Work carried out by Yu-Chun Chang and co-workers on the central Azores has involved comparing volcaniclastic turbidites containing evidence of shelf involvement (e.g., bioclastic particles) in sediment cores from basins near the islands (Chang et al., 2021a) with the volumes of landslide valleys in the uppermost submarine slope (Chang et al., 2021b). Between the landslides and the core sites, there are abundant sedimentary waves produced by sedimentary gravity flows (Chang et al., 2022). Chang and co-workers have found that only the largest landslide volumes correspond with the turbidite volumes, so the smaller landslides produce sedimentary gravity flows that deposit on the submarine slope without reaching the basin floor. This is also corroborated by their study of sedimentary fluxes. They used estimates of Quartau et al. (2012) of sediment released by island erosion (coastal and fluvial) and biogenic production on the shelf of Faial Island (Azores). Those estimates scaled to the portion of the island facing north have been compared with the depositional fluxes in the basin to the north of the island constrained using ^{14}C dates and volume modeling. They found that <10% of the sediment produced at the island has reached the basin over >ky timescales.
3. Modeling of seismic refraction data collected around volcanic islands typically reveals low seismic velocities beneath the submarine slopes. While this could be caused by volcanic processes (e.g., lower bulk rigidity of lavas compared with intrusive rocks), they could also be due to widespread clastic deposits. Watts et al. (1997) show a seismic reflection image collected from data perpendicular to Tenerife, which shows some seabed-parallel reflections within the lower slope (you may need to see the original paper copy as the scan is poor).

I therefore recommend that Bossis et al. consider another reconstruction in which the ESB in their Figure 1 is moved landward by such an amount that the eroded volume equals a deposited volume on the submarine slope. This would assume that the particles released by erosion have prograded the uppermost slope. The result would be only one end-member of possible geometries because, if the upper slope were depositional, we might expect to see evidence of a landward ESB on eroded shelves of other islands but the evidence is unclear. For example, Santa Maria Island of the eastern Azores has many hardgrounds but it is difficult to see an ESB within them, although there is a break in slope beyond hardgounds to the north (Ricchi et al., 2020; Zhao et al., 2022). Mitchell et al. (2003) presented morphologic evidence that the upper submarine slope of the Anaga massif was eroded and not depositional, although that was based on lower resolution multibeam sonar data than is available elsewhere. In my opinion, the original slope position remains uncertain so I would recommend using the above adjustment to present alternative results that illustrate the effect of this uncertainty.

We understand the reviewer's concern. Although we believe that the sediment thickness is reduced (cf. Ricchi et al. 2020), we propose a maximum evaluation of the error generated: for this we assume that all the submarine eroded volume is deposited as a prism between the shelf and the abyssal plain. We find an error of 20% on the platform width in the Corvo example, which is overestimated because we do not take into account the radial symmetry of the edifice. Corvo is probably a rather extreme case because the cliffs are high and the abyssal plain not very deep: for islands with lower cliffs and a deeper abyssal plain the error should be even smaller.

This argument is now in the discussion and indicates that the error is minor.

Here we detail the calculation. The error is evaluated as an horizontal distance (Δx) between our modeled shelf break and its position if all the eroded material is used to build a prism between the shelf and the abyssal plain (in 2D, we do not consider the 3D circular shape for simplicity).

1) It is assumed that sediment is deposited to a depth of the basin of about zb=2000 m. The amount of sediment decreases linearly so the cross-section of the sediment has an area (A2) of A2 = (zb * Δx)/2.

2) This area of sediment must be equivalent to what is eroded. Again, a triangular section of height the height of the cliffs (hc), width xIE - Δx - xSLA can be assumed. This defines the area of volume lost by erosion A1=hc * (xIE - Δx - xSLA)/2

3) Assuming A1 = A2 which is false (see below) we find Δx ~ hc * (xIE - xSLA) / (zb + hc). Where, hc/(zb + hc) is our error proportion (~20%). In terms of volume, Δv/v is evaluated similarly Δv/v ~ hc*Δx/A1, and gives an error on volumes of no more than 13%.

In fact, this calculation overestimates this error because it does not take into account radial symmetry: there is much more room to put sediments on the submarine slopes.

Another remark: this error increases with the height of the cliffs but decreases with the depth considered. It would also be smaller if an isopach deposit were considered.

Volcanic ocean islands tend to be permeable structures so that a large proportion of rainfall penetrates the edifice, whereas runoff becomes focused within deep valleys. This leads to classical structures such as planezes (areas of the original volcano that are poorly eroded) with intervening deep valleys. This is acknowledged by the map in Figure 2 and mentioned in the text. Some of the introduction or other sections could explore this further, e.g., see articles and book by Ollier. I would think we would want only to use profiles over planezes that appear weakly eroded, to have the best chance of reconstructing the original geometry, rather than stack profiles as suggested on line 189. It would also be useful to have local geological knowledge to confirm the planezes are formed of laterally continuous volcanic units.

We fully agree with the reviewer. On line 145 (previously 189), we expose a summary of the workflow, and the reconstruction method is developed in part 3.5. In this latter, we explain that our choice was dictated for simplicity (we aim at applying the method to various cases), while it does certainly not overestimate the cliff retreat value. Moreover, in this case, most of the DEM points in Corvo can be considered as planeze (see the discussion on this point).

The main volcano ("Pico") of Pico Island (Azores) has a steep upper flank but low gradient lower flanks (Mitchell et al., 2008). It is effectively a hybrid - stratavolcano upper with some shield-like or at least lower gradient lower parts. Many of the Galapagos volcanoes also have steep upper flanks (Mouginis-Mark et al., 1996). Other volcanic ocean islands can be found with other shapes. There have been various ideas for the different gradients and profiles of oceanic volcanoes published over the years, though erupted lava viscosity and the thermal insulating effect of lava tubes has been invoked to explain low gradients of Hawaiian volcanoes (Greeley, 1987). This worries me also about the current analysis, as it poses the question of whether the geological process could have been systematically different for the lower subaerial flanks (now not accessible to inspection as they have been removed by erosion) compared with the upper flanks that remain. This implies uncertainty in the original subaerial profile.

We are also concerned about this issue. After consulting some experts in volcanology, in particular of the shape of volcanoes, it appears to us that the processes shaping them are not well constrained (beyond the obvious combination of ballistic ejection, erosion and deposition of lava flows and lahars). So, we used the results of Karatson, who provided strong evidence for an exponential model of the topographic profile. In addition, we tested conical and parabolic shapes that are clearly too far from reality to be useful. In the case of Corvo, the conical fit cuts the platform in its middle while the parabolic fit rises before crossing the underwater fit.

The method assumes that the submarine parts of the island have exponential forms. Exponential forms were originally noted by Gee et al. (2001). However, there is no theoretical explanation for this form. Lee et al. (1994) talked of earthquake shaking as leading to curved-upwards profiles. Cassalbore et al. (2020) reviewed the work on sediment waves commonly found around volcanic islands, suggesting that sedimentary gravity flows have created them. Without a theoretical basis for the exponential form, it is difficult to know if we should expect the original form to have been exponential. Indeed, "constructional" flanks tend to have a change in slope near their base (Gee et al., 2001; Mitchell et al., 2002).

The reviewer is right. As the reviewer says, there is no theoretical basis, and there are also few attempts in the litterature to characterize this submarine shape. Assuming that the submarine processes are not so different than the aerial ones, we hypothesized an exponential model for the submarine profile too. A posteriori, the good fits support this choice. We added the following text (line 221):

"*The exponential profile was originally noted by Gee et al. (2001), but without a theorical explanation. Here we assume that, similarly to the aerial profile, the submarine topographic profiles follow an exponential function. This assumption has been validated a posteriori by a good fit (section 5.2). In particular, we will see in the results that this assumption is supported for Corvo Island.*"

The abstract mentions that the derived coastal erosion rates are consistent with short-term rates. This would not be expected, because rates measured over different timescales are affected by episodicity of erosion (Gardner et al., 1987). There is some admission of this effect, though it would be nice to explore how the rates could fit in with schemes developed to address episodicity of process. Erosion rates tend to decline with increasing timescale over which it is measured logarithmically (Sadler & Jerolmack, 2014) - it would be nice to see how the inferred erosion rates here fit in with such a scheme, e.g., by comparing with modern rates over known timescales.

This comment echoes some of the points raised by the other reviewers. We agree that our text should have better discussed the temporal relevance of estimated erosion rates. As this disturbed the other reviewers, we now focus on eroded volumes, which are well defined, in contrast to cliff recession rates, as the object designated as cliff is not the same during a glacial cycle. We have integrated this in the text, especially in lines 135-142: "*However, over the long term, coastal erosion will not affect the same area of the coast depending on the relative sea level (Huppert et al., 2020). For example, during a sea level highstand, i.e. during an interglacial period, coastal erosion occurs mostly horizontally via coastal cliff retreat, whereas during sea level fall or lowstand, i.e. during a glacial period, coastal erosion mainly affects the erosional shelf, in such a way that its surface appears to move downward (Ramalho et al. (2013, Fig.8). As a result, when the eustatic level is intermediate, the already-carved shelf is newly eroded, possibly forming marine terraces, without retreating the coastal cliff (Fig. 1). Therefore, the total retreat of the coastal cliff, i.e. the shelf width, cannot be a proxy for the total amount of coastal erosion (e.g., Huppert et al, 2020) and consequently, we cannot use the horizontal measurement to accurately quantify the long-term coastal erosion rate.*"

As for the comparison with long-term retreat rates, this goes a bit beyond the objectives of this paper. We mean that in this paper we illustrate the method on Corvo because the geometry is clean there; on the other hand, we do not dispose of ad hoc retreat rates; we can only compare with generic values (Prémaillon et al. 2018).

The text could be substantially shortened, which would allow the authors to incorporate more information on the geology of Corvo, a geological map and various constraints, e.g., better description of the dates and their significance. In my opinion, the study would be better updated by extending the step of terrain analysis, taking account of geological and geomorphological structure. The Karátson et al. (2016) study of Gran Canaria has some good ideas for this.

We have significantly revised the text and in particular the introduction has been completely reworked and shortened. We hope it fits better.

Detailed suggestions:

Line #

8 ... to determine when erosion started.

We changed the end of the sentence to "... to determine where and when erosion started."

15 surface area or surface volume?

Surface area, as said.

23 It seems strange to open the article's introduction with so many publications on modern erosion rates when the subject of the article is really long-term rates.

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

35 I would change the emphasis in this paragraph and others to erosion and sediment transfer for volcanic islands specifically. It is not clear how the results of this study will affect those broader global issues, so why mention them? It is better to use the introduction effectively to raise issues that can be returned to in the discussion in the light of the new results.

The emphasis of the introduction has been changed to focus on the eroded volume rather than retreat rate. Nevertheless, we still think that there is a lack of rocky coast data preventing from establishing erosion laws. This limitation is a problem to tackle global issues, and this is clearly one of our motivations (see our recent articles Premaillon et al. Esurf, 2018; Regard et al., EPSL, 2022) to develop a method that could be applied later to many islands worldwide.

70 In local instances, pyroclastic deposits are important.

It's true. This part has been deleted.

78 The logic here is not correct. If the ages are unknown, we cannot say that coastal erosion began at the same time.

It's true. In order to rework this part following the reviews, this paragraph has been deleted.

97 There have been some other reconstructions of volcanic islands that may be cited also. For example, Urgeles et al. (1998) reconstructed La Palma prior to its large landslides, quantifying their volume. Mitchell et al. (2003) attempted to reconstruct the pre-erosion structure of the Anaga massif of Tenerife. As noted below, it shows similarities to the erosion of Corvo.

Urgeles et al. (1998) and Mitchell et al. (2003) have been added to the cited references here.

101 extension -> extent

We changed "extension" to "extent".

116 Please cite Sunamura (2021) here.

Sunamura (2021) and other papers have been added to the cited references here.

134 These are quite old articles to cite for the LGM level and some more recent articles have suggested that a deeper level was reached (e.g., see Yokoyama et al., 2000).

Indeed. We added "*around*" before the depth values to be less assertive and we added Yokoyama et al. (2000) in the cited references here.

217 This is not the resolution of the data, rather it is the spacing of grid nodes. For much of the Earth, there are no bathymetry soundings to constrain depths.In the case of Corvo, there may be only old hydrographic soundings (single-beam) from widely spaced survey lines that contributed to the grids used.

We changed "... a resolution of approximately 500 m…" to "... a 500 m horizontal spacing of grid nodes…".

218 This seems an important assumption, which ought to be explored more earlier.

We do not see where to put this assumption earlier; we think that it is in the right place here.

272-278. In my opinion, Karátson et al. took a better approach. Ignoring the geometry of subaerial erosion as done here could prevent the method from being widely accepted.

We do not ignore the geometry of subaerial erosion here. Karátson et al. used the planèzes on Gran Canaria because it is the less eroded part of the paleo-surface of the volcano, compared to the incision by deep incised valleys elsewhere on the island. In the case of Corvo Island, there is no deep incision of the island, so all the surface above cliffs, with the exception of the southern part and the caldera, can be considered as planèzes. In sum, we used the planèzes as Karátson et al. did, with the difference that we can't select the best part of these planèzes because, given the small size of the edifice, it is described by a much smaller amount of elevation data.

Note, that we have moved this paragraph in the discussion (section 6.1) and enlarged it to better explain that.

315-319. Please outline the constraints on these ages, e.g., the radiometric method (Ar-Ar of K-Ar). Also locate dated samples on the map and discuss their significance (e.g., how well they are likely to constrain a particular unit and show its extent).

We added that the method used by Dias (2001) for dating the last stage of Corvo volcano-stratigraphy is the K-Ar datation on one sample. However, due to the poor definition of the map used by Dias, it is not possible to accurately locate where it has been sampled.

We are aware that this is problematic for correctly constraining the age of the Corvo palaeo-surface, but it is the only age source to our knowledge for this unit of Corvo Island.

350 Please provide a map showing survey lines of data contributing to the EMODnet grid, or at least consider that the surveying was not continuous around the island.

We are sorry, we cannot do that, as the survey lines cannot be found on the EMODnet site through which we have accessed the DEM.

360 It is interesting that the centre in Figure 3 also appears to be roughly centred within the ESB. Such a structure was found for the Anaga massif (Mitchell et al., 2003).

That's true, thanks for this remark.

361 The apparent minor discrepancy in Figure 5 (right panel) between the proposed IE and the ESB could be explained if the ESB is not the erosional shelf break to the NW, rather the uppermost slope has prograded due to sediment deposition there.

It could be an explanation, but the ESB has been determined by the threshold of 15° on the shelf break, and this slope does not theoretically allow the retention of sediments of the shelf break (Quartau et al., 2010). Consequently, given the absence of published seismic data, we assume this is the ESB.

362 Presenting uncertainties in this way gives the impression they are random uncertainties, but a large part of them is likely due to using profiles over planezes and eroded areas together, i.e., it includes systematic errors.

It seems the line reference is not exact. We assume the reference is to the first sentence of the paragraph presenting uncertainties. The reviewer is right, thus we added "*(including systematic errors in the profiles due to the slight erosion of the aerial part)*".

Table 3 - are these wave data or model outputs?

These data are model outputs. This has been clarified in the text.

510 How representative are modern wave predictions for the long period of erosion of Corvo?

We thank the reviewer for this very good question. So good we cannot answer it! We added "*... if we assume that modern wave data are representative for the long period of erosion of Corvo Island ...*" here.

511 Perhaps instead: We have adapted the Karátson et al. method ....

The line reference is wrong, maybe 416? The text has been changed and this sentence has been removed.

512 I would not put this in the conclusions or abstract given that we don't have "paleo" wave direction data.

The line reference is wrong. The reviewer is right, we have no idea about the data on the direction of the paleo-waves. On the contrary, the dominant direction of the waves, coming from the trade winds and the position of the nearby islands, must not have changed much. Our results must hold up over the long term.

Bossis, R., Regard, V., and Carretier, S., in review, Initial shape reconstruction of a volcanic island as a tool for quantifying long-term coastal erosion: the case of Corvo Island (Azores): Earth Surf. Dyn. Disc., doi:10.5194/esurf-2022-5118.

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Response to RC2

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. Couvault 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 mean 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 "and others" 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 theorical 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 sea bed erosion - so the maximum sea bed 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 downdearing 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 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 min 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”.

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Response to CC1

Dear Associate Editor,

It was with pleasure that I read the submitted version of this paper about the quantification of long-term coastal erosion on Corvo Island, Azores.

This is a subject very dear to me, because I have dedicated most of my research time to the study of insular shelves surrounding reefless volcanic islands (Mitchell et al., 2008; Quartau et al., 2010; Mitchell et al., 2012; Quartau et al., 2012; Meireles et al., 2013; Mitchell et al., 2013; Quartau and Mitchell, 2013; Ramalho et al., 2013; Quartau et al., 2014; Casalbore et al., 2015; Quartau et al., 2015a; Quartau et al., 2015b; 2016; Melo et al., 2018; Quartau et al., 2018a; Quartau et al., 2018b; Ricchi et al., 2018; Romagnoli et al., 2018; Lucchi et al., 2019; Santos et al., 2019; Zhao et al., 2019; Ramalho et al., 2020; Ricchi et al., 2020; Zhao et al., 2020; Innocentini et al., 2022; Zhao et al., 2022). And shelves formed on these types of islands that do not suffer considerable uplift/subsidence over time are mostly formed by coastal erosion.

I think the work is pertinent and the authors have read most of the pertinent references and have made a good job in integrating all that information.

I have made most of my work at the Azores studying their insular shelves and I am working on Corvo right now so I believe that knowledge and my comments can make the paper and the discussion a bit solider and improve the impact of this study:

We warmly thank Rui Quartau for participating in this discussion and supporting our work so much. Despite his support, he does not completely agree with us on our measurement of the width of the erosion platform from the IE point, at the intersection between the aerial and submarine topographic profiles, rather than the ESB. Thanks to his and the other reviewer's comments we refocused our work on total eroded volume. Thus the choice of IE or ESB becomes a minor point in the revised manuscript, we only discuss the platform width measurement in the discussion. For the secondary points, we invite the reviewer to consult our responses to all his comments below.

Lines 39-41: "Given that coastal erosion is a discontinuous phenomenon over time, short-term measurements may omit the existence of catastrophic events, such as cliff collapse, which occur only very rarely over the short term (Lim et al., 2010; e.g. Dewez et al., 2013; Rohmer and Dewez, 2013)."

Comment: I think you should also cite here Gardner et al. (1987) and Scott Snow (1992) because they showed that this tendency for erosion to appear faster over shorter measurement periods reflects the episodic nature of cliff erosion, related to individual cliff failures and varied cliff resistance. They have showed that geomorphic/sedimentary process rates generally decrease with the measured time interval because of the episodic nature of these processes.

This section has been deeply changed to respond to reviewers' comments, this sentence has been removed.

Lines 86-87: "This moment likely follows the setting of the flows constituting the top of the sea-cliffs, and thus the age of these flows indicates the age of the "initial" shape."

Comment: This is not exactly true and as discussed at Ramalho et al. (2013) edifice lateral growth is characterized by rapid coastal progradation, sustained by the successive generation of coastal lava deltas as flows enter the sea. We have seen this at La Palma eruption last year and the moment the lava flows enter the sea there is no cliff and only a change of slope between the subaerial flows and the submarine flows of the lava delta (see figure 4 of Ramalho et al., 2013). Off course that lava deltas are very prone to erosion and a cliff starts forming almost immediately, but only a very small cliff. It is better to use the erosional shelf edge as this marker of the start of erosion.

We understand the point here; we did two things to clarify our message and to improve our manuscript. First, we added the precision that the cliffs are the modern ones (not the ones existing when the lavas were emplaced). Second, we added a paragraph in the discussion in order to discuss this specific point (line 394): "*The method is based on a late surfacing of the edifice. We consider that the flows that caused this surfacing occurred during a relatively short period of time. The good quality of the fits that we present supports this view. This does not preclude later lava flows which may have created deltas as flows enter the sea. If there are some left, most of them must have been eroded. The bias introduced is therefore an underestimation of the eroded volume. We expect that the volumes of deltas eroded in this way are relatively small compared to our estimates of the total eroded volume: if this were not the case, there would be alterations to the circular or elliptical shape of the building. These alterations are detectable as shown in the southern part of Corvo Island; we do not detect them on the other sides. This shows that the lavas younger than the surface of the edifice only introduce a minor bias in our estimates of the total eroded volume.*"

Lines 108-110: "As a result, the methods that reconstruct the volcano morphology ignore the submarine geomorphology. Yet, the submarine realm of volcanoes offers other constraints to better reconstruct the initial edifice geometry and to quantify coastal erosion, as seen in the next section"

Comment: This is absolutely true and I have published several works that show exactly this, so citations are missing here to support these claim such as Quartau et al. (2010, 2013, 2014, 2015a).

It is good to know that the reviewer agrees with us. We have now cited the request of Quartau et al. (2010, 2014) to make more use of underwater constraints.

Lines 111-112: "Coastal erosion results in the formation of an erosional shelf below sea level and a coastal cliff above sea level in the nearshore zone (Trenhaile and Bryne, 1986; Sunamura, 1992; Anderson and Anderson, 2010; Ramalho et al., 2013)."

Comment: This is exactly what Quartau et al. (2010, 2018) have shown with topographic data and modelling, so I think citations are missing here

We have added "e.g." because the cited references are not exhaustive, but we have added Quartau et al. (2010, 2018) at the reviewer's request.

Lines 118-119: "The erosional shelf therefore has a theorical depth ranging from around 0 m at the coast to about 10 m at the shelf edge (Fig. 1)."

Comment: As it is written it may confuse readers, because we know that with water level variations during glacial-interglacial periods, the sea level changes ranges the 0-120 m depths and therefore shelves have similar ranges. What the authors are describing with the 0-10 m range are shore platforms, not shelves. Although the authors acknowledge that a few lines ahead I would rephrase the sentence to become clearer as "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 (Fig. 1) and it is called shore platform."

We acknowledge the reviewer for his comment. To clarify, we modified this passage to (line 92): "*The erosional feature formed during the present-day sea level by wave action therefore has a theorical depth ranging from around 0 m at the coast to about 10 m at the edge and it is called a shore platform. The variations in sea level can cause the formation of a series of platforms which can be called a shelf (Fig. 1). In this case, each platform is called a marine terrace.*"

Lines 135-138: "If the shelf edge has been covered by sediments or by volcanic progradation, the apparent depth of the shelf break is reduced; in this case, the shelf break is called a depositional shelf break (DSB) (Quartau et al., 2010). On the contrary, if it has not been covered by any material, it is called an erosional shelf break (ESB) (Quartau et al., 2010)."

Comment: I would add a reference here to the work of Quartau et al. (2015b) where the study of Pico Island showed that most of this shelf is a "rejuvenated shelf", i.e., where a previous eroded shelf was partially filled by recent volcanic progradation and made the previous ESB shallower (see sketches at figure 7 of Quartau et al., 2015b).

In this section we present a deliberately simple reconstruction. The work of Quartau et al (2015b) is very interesting but shows a polyphase history that goes beyond our (simple) statement. We therefore left the text as it was.

Lines 139-140: "In summary, the range of coastal erosion is spatially limited by the ESB on its ocean side and by the cliff up to its top (Coastal Cliff Top or CCT) on its land side (Fig. 1)."

Comment: Yes, I believe it is much better to consider the area between the CCR and the ESB, than to consider the IE which is an extrapolation of the subaerial profile and as I explained in comment to lines 86-87, it does not exist the moment that volcanism stops and the shelf starts to form. But what you are measuring on bathymetry is not ESB but a depositional shelf break (DSB), which should not be much different in terms of distance to the coast, but in terms of depth can make a difference, especially if there is a great sediment thickness on the shelf.

We have followed the reviewer's advice. We now introduce the distinction between ESB and DSB. See also our response above to his comment on lines 86-87.

Lines 141-143: "…whereas the submarine part of the edifice below the ESB (Fig. 1) is not subject to any erosive process except for gravitational collapses at the shelf edge which may form an embayment (Ramalho et al., 2013; Chang et al., 2021)."

Comment: The ESB is also subject of erosion by headwall retreat of canyons that develop on slopes of the submarine volcanic edifice (see Krastel et al., 2001; Casalbore et al., 2017; Quartau et al., 2018a).

We thank the reviewer for pointing out this shortcoming. We have now introduced this alternative underwater process.

Lines 145-146: "……Following the models of Peterson and Moore (1987), DePaolo and Stopler (1996) and Ramalho et al. (2013), we can estimate the volcano aspect before coastal erosion via two extended radial profiles."

Comment: You are missing the works of Quartau et al. (2010; 2014) who have done this to calculate the original extension of the volcanic edifices at lowstand sea levels.

It is true that Quartau et al. (2010, 2014) have reconstructed the pre-erosion extents of several volcanoes on the islands of Faial and Terceira. However, here in our paper we discuss the use of two radial profiles, which we do not find in Quartau et al. (2010, 2014).

Lines 150-152: “Theoretically, the IE must be located near the sea level at the time of volcanic island-building. Thus, the IE could also serve as a proxy for estimating the total vertical motion experienced by the island since its formation.”

Comment: As I explained previously at comments to lines 86-87 and to lines 139-140 the IE is an extrapolation of the subaerial profile and does not exist the moment that volcanism stops and the shelf starts to form. Therefore the use of the ESB is much more reliable to estimate vertical motion of a volcanic island. In addition, as we know from the literature, the sea level has changed frequently around 130 m in the Quaternary. So, you would need to know the age of the island with uncertainty of a few thousands of years to do that. If the last volcanism was at a lowstand then IE would be much deeper than current sea level.

R. Quartau is right and we acknowledge this. We have improved the manuscript to be more cautious about the meaning of IE in terms of vertical movement, see our responses to previous comments.

Lines 150-152: “For example, during a sea level highstand, i.e. during an interglacial period, coastal erosion occurs mostly horizontally via coastal cliff retreat, whereas during sea level fall or lowstand, i.e. during a glacial period, coastal erosion affects only the erosional shelf, in such a way that its surface appears to move downward, which is sometimes called downwearing. In the latter case, the ESB not only moves deeper but also farther from the shoreline, increasing the width of the insular shelf, as well as the apparent amount of coastal erosion.”

Comment: It is a bit more complex than that. Although intuitively we tend to think that shelf widening occurs mostly during highstands because sea level is attacking the cliffs, downwearing and horizontal erosion occurs in the entire cycle of sea level and you can see that on figure 2 of Trenhaile et al. (2001). On this figure we see the several profiles after each sea level cycle, and it is clear from one cycle to the other (respectively one profile to the other) that shelf widening occurs not only when sea level is at the high stand but along the entire cycle.

This comment echoes some of the points made by the other reviewers. We agree that our text should better discuss the erosion that takes place in other places than the main cliff. We have integrated this notion in the text, especially in lines 135-142: “*However, over the long term, coastal erosion will not affect the same area of the coast depending on the relative sea level (Huppert et al., 2020). For example, during a sea level highstand, i.e. during an interglacial period, coastal erosion occurs mostly horizontally via coastal cliff retreat, whereas during sea level fall or lowstand, i.e. during a glacial period, coastal erosion mainly affects the erosional shelf, in such a way that its surface appears to move downward (Ramalho et al. (2013, Fig.8). As a result, when the eustatic level is intermediate, the already-carved shelf is newly eroded, possibly forming marine terraces, without retreating the coastal cliff (Fig. 1). Therefore, the total retreat of the coastal cliff, i.e. the shelf width, cannot be a proxy for the total amount of coastal erosion (e.g., Huppert et al, 2020) and consequently, we cannot use the horizontal measurement to accurately quantify the long-term coastal erosion rate.*”

Lines 176-178 “However, in order to compare long-term coastal erosion metrics with traditionally calculated short-term metrics, it seems necessary to use the position of the coastal cliff top as a benchmark, so that the IE-CCT distance quantifies the erosion.”

Comment: As I mentioned previously I would use the CCT-ESB because at the moment volcanism stops and erosion starts to first create a shore platform (with a sable sea level) and then a shelf (as sea level migrates up and down during glacial-interglacial periods) the IE and ESB are at the same spot.

Following the general comments, we have focused our measurement on volumes; the IE-CCT distance is no longer useful and this comment is no longer relevant.

Lines 181-182 “We therefore propose here a new method based on the analysis of aerial and submarine topographic data of volcanic islands.”

Comment: The analysis of aerial and submarine topographic data of volcanic islands to calculate erosion rates is not a new method since Quartau et al. (2010) has already done that for the first time in 2010 with high resolution topography and multibeam bathymetry. Even Menard (1983) has made it long time ago with very poor bathymetry. The new approach here is to consider the IE in the reconstruction (which I do not agree to be the best marker to use) and to use exponential equations to reconstruct the initial morphologies with uncertainties calculated.

The reviewer is right: the originality of our approach is not so much to use aerial and submarine profiles, but to model them. We have deleted this sentence.

Lines 193-195: “Compared to a simple measurement of shelf width, which quantifies the total retreat of coastal cliffs (e.g. Quartau et al., 2010), this method is used not only to calculate an eroded volume and its uncertainty, but also to provide evidence that portions of the shelf have possibly been lost due to gravity collapses.”

Comment: Quartau et al., (2010; 2014) did not simply measure the shelf width with profiles. They first started to fit a circular or elliptical shape of a volcanic edifice centred on the island (or in the several volcanic complexes if more than form the island) and with its edges coinciding most of the shelf edge (figure 15 of Quartau et al. (2010) and figure 8 of Quartau et al. (2014)). And off course in the places where this fitting lied offshore the shelf edge it meant that gravity collapses have happened that made retreat the original shelf edge.

We apologize for underestimating the significance of the nice results of Quartau et al (2010; 2014), this was definitely not our intention. We reworded the entire sentence which now reads (line 157): “*Compared to the measurement of the shelf width (e.g. Quartau et al., 2010), this method is used not only to calculate an eroded volume and its uncertainty, but also to provide evidence that portions of the shelf have possibly been lost due to gravity collapses by comparing the horizontal position of the IE and the ESB.*”

Lines 218-220: "High resolution bathymetric data (a horizontal resolution of at least 50 m) around the island are therefore necessary to clearly identify the boundary between the insular shelf and the non-eroded submarine slopes of the volcanic edifice."

Comment: Later (line 345) the authors mention that they use the EMODnet database to get the bathymetry around Corvo Island. EMODnet bathymetry has in the best-case scenarios 115 meters of resolution (https://www.emodnet-bathymetry.eu/internal_html/qaqc-and-dtm-production-details/9). Furthermore, as I explain in my comment to lines 345-347 the bathymetry they use appears to have around 150 to 250 meters of resolution. So, it appears that the authors cannot clearly identify the boundary between the insular shelf and the non-eroded submarine slopes of the volcanic edifice with this data.

We thank the reviewer for this good remark. We did not pay enough attention to the horizontal resolution of the EMODnet bathymetric data in this area, which is indeed more like 200 m. However, we succeed in identifying the ESB with this data, in particular by calculating the slope map and selecting slopes greater than 15 degrees. We therefore consider that a resolution of 200 m is sufficient to identify the ESB.

So, we changed "50 m" to "200 m" line 182.

Lines 299-300: "These islands are quite young (2 Ma to the present), modest in size….."

Comment: First, you mention the ages of the island without any citation to relevant literature which is abundant. In addition, there is one island which is much older than 2 Ma, which is Santa Maria that has around 6 Ma (Ramalho et al., 2017).

We changed "(2 Ma to the present)" to "(2 Ma to the present, except for Santa Maria Island which is 6 Ma old; see Feraud et al., 1980)".

Lines 291-294: "Moreover, the difference between the calculated IE elevation and the current sea level could be used to estimate the total subsidence or uplift, relative to the current sea level, that the island has experienced, with the uncertainty of the vertical error bar of the IE."

Comment: You cannot use the difference between the calculated IE elevation and the current sea level to estimate the total subsidence or uplift because you do not know with detail the age of the island and as we know from the literature, the sea level has changed frequently around 130 m in the Quaternary. So, you would need to know the age of the island with uncertainty of a few thousands of years to do that. Also the geomorphic marker IE is not the best to use because it results from the intersection of extrapolation of the subaerial profile and the submarine profile below the shelf edge. I have always used the ESB because is something that you can measure from the bathymetry if the seafloor is rocky and from seismic profiles if the seafloor is covered by sediments.

The reviewer is right. We have removed our attempts to quantify vertical movements. See the discussion, part 6.2 where we wrote the following paragraph (line 414): "*In addition to being able to quantify the total eroded volume by coastal erosion, the reconstruction of the initial shape of Corvo Island allows us to obtain the elevation of the junction relative to the current sea level between the aerial and submarine profiles, which is assumed to be the initial* *extension (IE) of the island. The IE is 23 ± 104 m above current sea level. The uncertainty of this value is too large and too close to the current sea level to interpret it as a marker of the vertical dynamic of the island*.” For the comment regarding the IE, cf below our response to R. Quartau’s comment about lines 376-378.

Lines 307-308: “The modest elevation of these islands moderates the phenomenon of orographic precipitation (Ramalho et al., 2013) which makes the relics of their initial aerial volcanic morphology relatively well preserved from aerial erosion.”

Comment: This is not true, the orographic precipitation exists and the great majority of the islands have more than 2000 mm of annual average precipitation with the exception of Graciosa and Santa Maria which are the lowest in altitude (another evidence of orographic precipitation). Check Agencia Estatal de Meteorología, Instituto de Meteorologia de Portugal (2012).

We agree with the reviewer. But we did not say that orographic precipitation does not exist on these islands. We simply said that this phenomenon was ‘moderate’ because of the modest elevation, which means that it still exists.

Lines 345-347: “As a result, we used the EMODnet database. This database covers the whole European territory, of which the Azores are part, and offers aerial and submarine topographic data around Corvo with a horizontal resolution of 50 m per pixel, which is sufficient for our analysis.”

Comments: The 2020 DTM version of EMODNET has a grid size of 1/16 * 1/16 arc minutes (circa 115 * 115 meters). See https://www.emodnet-bathymetry.eu/internal_html/qaqc-and-dtm-production-details/9. But that is true only if the source bathymetry was multibeam, but in Corvo the bathymetry available at the time that the 2020 DTM version of EMODNET was released, was single beam. The source of the data can be consulted in the Bathymetry Viewing and Download Service (see figures above) and it is single beam bathymetry collected in 2000. In conclusion the bathymetry used does not even reach 115 meters of resolution. By looking at the size of the features discernible at a RGB Geotiff downloaded from the EMODNT portal it looks to be 150 to 200 meters resolution.

We thank the reviewer for this good remark. As said before, we did not pay enough attention to the horizontal resolution of the EMODnet bathymetric data in this area, which is indeed more like 150-200 m here.

So, we changed “50 m” to “150 to 200 m” here.

Lines 376-378: “As discussed previously, the IE indicates the maximum (initial) extension of the island. The IE is shown in purple in Figure 5, it is obvious that the IE is slightly internal to the ESB contour.”

Comment: As I mentioned to my comment to lines 291-294, the IE is not a good geomorphic marker because it is an extrapolation rather than something that you can measure now with the current topography or bathymetry. In addition, if the island formed during a lowstand the IE would coincide with the ESB, so it would not be internal to the ESB contour.

As we are also field geologists, we understand that R. Quartau does not like us to use IE which seems a bit abstract. Let's say that we need field studies using ESB and data analysis using IE, because ESB alone does not allow eroded volumes to be calculated. However, we disagree with R. Quartau: even if the island formed during a lowstand the IE must be internal to ESB, because the latter forms below sea level, not near it, which is the case for IE.

Lines 457-458: “The reconstruction of this pre-erosion shape allows us to evaluate a total coastal cliff retreat ranging from 550 to 3300 m.”

Comment: From my knowledge of the island (and discounting the southern lava delta). The smaller retreat should be in the order of at least 1000 m. That discrepancy has to do with the authors adopting a circular shape for theedifice, when it seems more like an elliptical one (see further comments to lines 484-486)-.

See our response to R. Quatau’s comments about lines 484-486.

Lines 461-462: “Depending on the sector, erosion rates range 0.7-4.3 mm/yr and 6-37 mm/yr, respectively, for the maximum and minimum age bounds of 770 ka and 90 ka.”

Comment: These erosion values deserve a better discussion here. How do they compare with those measured on other volcanic islands? Other authors have already done that for other islands. Quartau et al. (2010) has made a good review but you can check recent works like Quartau et al (2015a). It is also different to compare younger islands with older islands and you have a big uncertainty on your age constraints. Erosion rates in the beginning start very fast but as shelves get wider erosion rates decrease because of mainly two factors: (1) Waves tend to loose energy as they cross wide shelves and (2) As cliffs get higher when they fail they deliver more material to the cliff base that protects the cliff from erosion during longer periods. There is also the influence of vertical movements of the island, subsidence tends to help increasing erosion rates (see Quartau et al., 2018b) and uplift apparently tends to decrease erosion rates.

While this is an interesting discussion, it is no longer relevant because following the comments of the other two reviewers, we are no longer analysing retreat rates but rather volumetric erosion rates.

Lines 472-477: “This value, despite its large uncertainty is comparable to the difference between the theoretical depth of -130 m in the absence of vertical movements of the ESB (Shepard, 1973; Trenhaile, 2001; Quartau et al., 2010) and its actual value of -107.25 m. This suggests that Corvo Island could have possibly experienced a mean uplift of approximately 20 m since the formation of Central Volcano. However, given the large vertical uncertainty, estimating a total uplift value would be too uncertain to be further discussed.”

Comment: I agree it is too uncertain to be discussed. You have to bear in mind that the depth of the ESB you are measuring is normally a depositional shelf break because of the sediment bodies covering the shelf (see Quartau et al., 2012, 2015b). So, it means that normally the ESB is deeper than that you measure in the bathymetry and you need seismic profiles to get the real ESB.

Cf. above (Lines 291-294): we agree with the reviewer and no more quantify vertical motion rates.

Lines 484-486: "This indicates a vertical and horizontal protrusion. Together with the protrusion caused by the younger volcanic edifice in the southern sector of the island, these protrusions give the total edifice a slight elongation along the north-south axis."

Comment: From figure 3C and from my knowledge of the island, the edifice is elliptical (even discounting the southern lava delta). Actually an ellipse fits better the subaerial edifice and the ESB and not a circle as suggested in figure 5.

We recognise that there is a slight N-S deformation of the island's shape. This is clearly not sufficient to invalidate our analysis, and we now indicate that it may lead to second order variations in the results. As for the specific history of Corvo, this is discussed in the second part of section 6.2.

Lines 521-522: "These results open up promising perspectives that must be confirmed by the application of this method to other volcanic islands."

Comment: I agree with the observation but this work needs to be based on better data (i.e., higher resolution bathymetry, better age constraints, better definition of the ESB with seismic reflection profiles, etc.).

We completely agree and are happy about it. We changed "...confirmed by the application…" to "...confirmed by new data On Corvo Island and the application…".

We are very grateful to Rui Quartau for his comments.

Rui Quartau

11th August 2022

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