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Reply on RC1

Rémi Bossis et al.

Author comment on "Initial shape reconstruction of a volcanic island as a tool for quantifying long-term coastal erosion: the case of Corvo Island (Azores)" by Rémi Bossis et al., Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2022-18-AC1, 2022

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).

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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:

- *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.)*
- *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.*
- *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, $\Delta v/v$ is evaluated similarly $\Delta v/v$ ~ 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.

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