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Reply on CC1

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-AC3>, 2022

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 solidier 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 theoretical 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 theoretical 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 Stople (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.

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 stable 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 the edifice, 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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