Answer to the reviewer on:

Report #1

Submitted on 09 Dec 2021

Referee #3: Philippe Lesage

Referee #3: Following the comments of the 2 reviewers, the manuscript was greatly improved. However, some methodological aspects remain unclear and poorly founded and some sections of the paper are still difficult to understand.

Answer: We thank the reviewer for the helpful review and for appreciating our efforts.

R3: In volcano seismology, many efforts have been done to develop methods for locating emergent events based on arrays (e.g. La Rocca et al., 2004; Metaxian et al., 2002; Saccorotti et al., 1998; 2000; all the papers by Almendros et al.). The authors of the manuscript did not use these methodological developments and instead they propose a much simpler, poorly rigorous and not well founded method. Although the locations obtained are probably not that bad, a comparative discussion on the advantages and drawbacks of all these methods would be welcome.

A.: We thank the reviewer for pointing out the references to Métaxian et al. (2002) and Saccorotti et al. (1998), that we missed in the previous version of the manuscript. However, our method is straightforward and based on the well-known and well-founded delay and sum approach in the time domain. In previous comparisons of locations obtained from network-based and array-based approaches the method performed very well (as the reviewer also points out). Furthermore, in our study, we focus on events with a well-defined signal onset (not emergent events), which partly lack, later arriving, clear phases such that the multi-array approach is appropriate.

The extension of the time-domain array analysis to a multi-array analysis makes the velocity model redundant.

We modified the text to better justify the method.

Changes in manuscript: lines 54-61, 64-65.

R3: During the beamforming process, the traces are shifted according to the tentative values of the slowness vector, then they are stacked and a value of 'energy' is obtained. This energy is not defined, which is an important lack in the description of the method. In a reply to R1, the authors explain that energy is an 'absolute amplitude'. In this case, the term energy is not appropriate. In other approaches, the mean correlation coefficient between the traces is calculated and is maximized in order to find the optimal slowness vector. Thus, the calculation of the 'energy' must be explained and justified, and the 2 approaches should be compared.

A.: We agree that the description of the beamforming process needed to be improved. For a better explanation of the procedure, we now have included the corresponding definition of the energy, which is estimated during the beamforming. We also experimented with correlation approaches but did not obtain better results. As the energy of a signal is proportional to the squared signal amplitude, it is eligible to either refer to the absolute squared amplitude or to refer to the energy (quotation of *reply to R1: "This is the contour plot of the energy, representing the absolute amplitude of the sum trace for each grid node").*

<u>Changes</u>: lines 117-122.

<u>R3:</u> The estimation of uncertainties is quite empirical. First a kind of Monte Carlo process is used to estimate the standard deviation of the maximum 'energy' by randomly perturbing the bounds of the stacking windows. The amplitude of these perturbations is determined by an ad hoc manner. The way the standard deviation and the distribution of baz are obtained from the distribution of energy displayed in Fig 3d needs to be specified.

<u>A</u>.: As suggested by the reviewer, we specified this point in lines 146-153.

<u>Changes</u>: lines 146-153.

<u>R3:</u> Although the authors demonstrated that similar perturbations on the frequency range used produce small standard deviation on the energy, there are other sources of error that should be taken into account or discussed at this stage in order to estimate the uncertainties on the slowness vectors (array response, ray bending, approximation of plane waves, position and elevation of the stations, trade-off between slowness and baz…). The section (lines 113-125) should be moved to §3.1 or to §3.3.

<u>A</u>.: Absolutely, the sources of error can be manifold. We extended the discussion of this in section 3.3 The choice of frequencies (lines 175-181) are also evaluated. We also tried to constrain possible influences of velocity heterogeneities along the ray path or beneath the arrays (lines 183-202). The influence of the station elevation difference is also discussed (lines 203-205). The array response function is shown in Figure S1 (lines 87-89).

The determination of the standard deviation of the maximum energy is one of the first steps taken in our multi-array analysis. Therefore, we believe that it should be described in the corresponding section dealing with the multi-array analysis.

<u>R3:</u> The discussion on multi-array analysis (lines 126-138) is very difficult to understand. It must be clarified. In order to find the source location, the energies as a function of back-azimuth estimated at each array are summed. How is this theoretically justified and how do you proceed (grid search?)? As stated by referee 1, a product of probabilities would have more sense.

<u>A</u>.: As suggested by the reviewer, we modified the text to improve the readability of the discussion of the multi-array analysis. However, a product of probabilities is not really different from the summation of energies, except probabilities provide a different scaling, exp(~E), and exhibit much stronger variations, which may give the impression of a better constrained result.

<u>Changes</u>: lines 146-153.

<u>R3:</u> From the reply to a question of R1, it seems that instrument responses are not corrected and the traces are only filtered. This is another source of uncertainty on the slowness vectors that should be considered.

<u>A</u>.: As described in section 2, our arrays consisted of both short-period as well as broad-band sensors. Even for the short-period instruments, the dominant frequencies of the signals we investigated are well within the flat part of the instrument response function. This is supported by the observation that, after filtering, the waveforms of the signals at stations with different sensor types are very similar.

<u>R3:</u> One of the main shortfall of the present work is the lack of source depth estimation. As pointed out by R1, it'd be possible to estimate these depths, at least for the VT events, using the available information on the slowness vectors and the velocity model of Vales. Of course, the precision of these estimations would be limited, but it could be evaluated and discussed taking into account the uncertainties on the slowness, on the velocity model and on the epicentre locations. Furthermore, these depths estimations could be compared with those obtained by the classical analysis based on travel times.

<u>A</u>.: The referee is absolutely right. Under consideration of a velocity model and the angle of incidence, it is possible to assess the depth of e.g. the hybrid events. We performed such an estimate (lines 305-318), however, as the reviewer points out, the analysis is very sensitive to the velocity model and, considering its uncertainties, we obtained highly variable depth estimates (between 5 and 14 km) and concluded that they are not reliable. This is also underlain by the observation of other indicators, which point towards a rather shallow source (lines 332-345).

<u>R3:</u> Detailed comments

Lines 91-92. Explain how you define the epicentre from the intersection of several beams.

<u>A</u>.: We have added a brief explanation at this passage in lines 101-104.

<u>Changes</u>: lines 101-104.

<u>R3:</u> L 100. What is the period length?

<u>A</u>.: We have rephrased this in lines 113-114.

<u>Changes</u>: lines 113-114.

<u>R3:</u> L 104. Define 'energy'.

<u>A</u>.: As mentioned above, we now have added the definition of the energy determined during the beamforming (lines 117-122).

<u>Changes</u>: lines 117-122.

<u>R3:</u> L 176 and elsewhere: the term 'aberration' could be replaced by 'bias'.

<u>A</u>.: We think that the term 'bias' does not fully match, however we replaced the term 'aberration' by deviation.

<u>Changes</u>: lines 155, 198, 202, 356, 358, 367, 368, 371, 379, 381, 383.

R3: L 211. The term 'earthquake' includes the seismic events of all types (VT, LP, Hyb…). You should specify here 'VT earthquakes'.

A.: The referee is absolutely right, we changed the heading of the chapter accordingly.

Changes: line 236.

R3: L 217. This point is not described in Sect. 3.3. What is a stable results?

A.: We discussed this in sect. 3.3. Criteria, which can lead to the exclusion of an event from further analysis are in particular the occurrence of strong sidelobes (lines 208-210) or parallel or reverse trending beams (lines 210-216). Nevertheless, we further clarified this in the passage addressed by the referee.

Changes: lines 242-243.

R3: L 243-244. How do you calculate probabilities here?

A.: Yes, strictly speaking, we do not calculate probabilities. Here, we superimpose all beams, which are determined for the hybrid events. As a result, we determine an area, where most beams point to, or in other words: where the energy values are the highest. We clarified this in the revised manuscript.

Changes: lines 269-271, 581-582.

R3: Fig 5. This figure compares the slowness and baz obtained from array analysis and from classical location method. However, the uncertainties estimated for both approaches (e.g. fig 4, S5 and S7) should be reported on this figure.

A.: We tried this but we think that the inclusion of uncertainties would overload this Figure and reduce its clarity.

R3: Fig 8. Indicate the location of the 2 seismic events (coordinates of epicentres).

A.: As suggested, we have added the coordinates of the epicenters to the description of the events.

Changes: lines 565, 567.

R3: Fig 9. Suppress the small circles that make the histograms unclear.

A.: We have modified the figure as suggested by the referee.

Answer to the reviewer on:

Report #2

Submitted on 08 Feb 2022

Anonymous Referee #4

<u>Referee # 4:</u> "Multi-array analysis of volcano-seismic signals at Fogo and Brava, Cape Verde" by Leva et al. : se-2021-52-version2

This manuscript deals with the application of a classical multi-array application to locate seismic events surrounding or on the volcanic islands of Fogo and Brava, respectively. The authors argue several times that the use of seismic arrays in a volcanic environment is superior over "classical" single station networks as many of the volcano related signals show weak and emergent phase onsets, are often highly scattered and in most cases a 3D velocity is missing. Statements that I mostly agree even if missing velocity information and topography might reduce the advantage of array analysis (besides the heavy load in processing the data). The authors followed the reviewers critics in the original version in exactly this point but still claim that they need no velocity to locate the (epi-)center of the different signal types. At various places in the manuscript, however, they refer rather to the hypocenter as to the epicenter which was estimated by using the pretty weak argument of amplitude ratios for justifying a preferred shallow location of so called hybrid events.

<u>Answer</u>: We thank the reviewer for appreciating our work and for the helpful comments.

However, there seems to be a misunderstanding regarding our use of amplitude ratios. Indeed, the epicenter of an event is determined from the intersecting beams. This is the basis of a multi-array analysis. Neither during the beamforming nor during the intersection a velocity model is necessary.

Nevertheless, for some purposes it may be useful to perform estimations under consideration of a velocity model. In the manuscript, this is done during the assessment of the depth of hybrid events (lines 305-318). To evaluate possible source depths we use a simple velocity model and trace back the ray path under consideration of the angle of incidence determined by the beamforming. However, we found that the uncertainties of the depth estimates are relatively large. The depth estimates for the same event at the two arrays can lead to significantly different results. This indicates that the ray paths to the arrays are affected by 3D structure and the resulting depth estimates are inconsistent. We, therefore, conclude that this approach is not suitable for a reliable determination of event depth, as our information on the velocity structure is too limited. Thus, the results of the multi-array analysis only yield the epicenter location of the event.

Apart from the strong influence of the velocity model on the depth assessment of the hybrid events, additional observations give rise to doubts on the depth estimated from the ray tracing approach. One such observation is the amplitude ratio at the different stations, indicating amplitudes twice as large at the station in the Chã das Caldeiras. It is worth noting, that we do not observe such a contrast of the amplitudes at this station for earthquakes recorded on Fogo or on Brava. We can thus exclude a bias due to a site effect at this particular station.

The observation of the large amplitude lets us conclude that the events must be located relatively close to it, which is in disagreement to the large depth estimate that we obtained from the ray tracing.

In view of the comment by the reviewer, we modified the text to better explain our approach.

Changes: lines 58-60, 101-104, 307-315, 342-344.

R4: Another weak point, at least in my opinion, in the presented analysis is the restricted use of delay-and-sum beam forming. The authors argue that using DLS a wider frequency band can be used in comparison to fk- analysis. The authors seem not to recognise that broadband fk-analysis was introduced already in the early 1990ies by e.g. Gupta et al. (1990). I admit that DLS beamforming is the method of choice if the beam itself is used for later analysis (e.g., phase picking or phase identification) as it is a completely linear operation and does not distorted the waveform or phase of the signal. However, the authors did not perform anything in this direction but simply back-project the first coherent arrival. In this respect the paper is a bit disappointing as it does not fully exploit the capabilities of a multiple array installation. I assume that the used seismometers are 3C so why not using it? 3C beamforming, which might shed more light into the source and path contribution of the different seismic signals would a natural thing to do.

A.: Waveform analysis was not intended or necessary for our analysis. Nevertheless, our approach is obviously correct and works well. We focus on the first arrival of the signal. Here, the wavefront experienced a minimum of disturbances, as generated by, e.g., scattering effects. For the analysis of the signal onset the analysis of the vertical component is best suited. As some of the events located lack clear S-phases, we apply a method which can be used to reliably locate the events using only the onset in the analysis and without the need of further phases. We further comment of the broad-band frequeny-domain analysis below.

R4: In addition to this more general critics I included several more technical points directly into the manuscript.

Comments from the appended manuscript se-2021-52-referee-report.pdf:

Line 55: Well, same is true for BB-FK

A.: The time domain approach, as used here, (automatically) implies incorporating a wide frequency band. Transformation into the frequency domain first and then using a wide frequency band is therefore not necessary. A narrow-band Fk-analysis would have the advantage to reduce the computational time of the analysis. If a broadband fk-analysis is applied, this advantage no longer holds. We, therefore, stay in the time domain.

R4: Line 56: Why not name it: delay and sum?!

A.: We have added this alternative description of the approach to the Introduction.

Changes: line 60.

R4: Line 68: 3C seismometers?

A.: *In the revised manuscript we point out, that we operated 3C seismometers.*

Changes: lines 80-81.

R4: Line 75: 5 Hz is quite close to the corner frequency of the short period instruments and thus group delays of the seismometer might influence the result - to what precision did you know the calibration parameters of your instruments? Did you test its influence?

A.: *We evaluated the instrument response during the implementation of the magnitude determination. For the magnitude determination we need to remove the instrument response. However, the dominant frequencies of the events and phases used are higher (e.g. about 10Hz for the first arrival of earthquakes) than 4.5Hz. Also, the comparisons of filtered waveforms from broad-band and short-period stations within the arrays shows no significant influence of the lower corner frequency.*

R4: Line 97: Triggered or how is the analysis window centred around the onset?

A.: *The choice of the analysis windows is done by the analyst. We have added the information in the revised manuscript.*

Changes: lines 110.

R4: Line 100: is 1 or 2 periods sufficient? dt*df >= 2*pi results in at least 6 periods (if you would use a gaussian bandpass) what is the dominant period? 1/7.5 ?

A.: *The dominant period depends on the event. Typically it is about 1/10 s. However, yes, one or two periods are sufficient, as we perform the analysis in the time domain and do not need to Fourier transform the waveforms. We have also performed several tests with different lengths of the stacking window.*

R4: Line 116: I think this is not sufficient to represent the true error. Why not using directly the beam width (say 65% level) for estimation? Another option would be jack-knifing through the array stations

A.: *Indeed, we have tested this carefully. At first, we have chosen the beam width as basis for the error estimate. However, it turned out, that this error was smaller than the error determined by choosing the standard deviation as described in the manuscript. Also, the choice of the error level is a bit arbitrary. We can avoid this by using the approach described in the manuscript.*

We have clarified this in the revised manuscript.

Changes: lines 132-133.

R4: Lines 116-119: Where does this come from? I thought you are using one or two periods (still the question of what)?

A.: *We use one or two periods of the signal as the stacking window during the determination of the backazimuth (and the energy contour plot). In lines 116-119 (previous version of the manuscript) we* *describe how we determine the standard deviation of the maximum energy. As the determination of the energy depends on the choice of the stacking window, we perform this estimation by varying the stacking window a sufficient amount of times (in our case 100 times). For each iteration, we randomly vary the start- and the end-time of the stacking window by ±2s.*

In the revised manuscript, we have clarified that we choose one or two periods of the signal for the stacking window.

<u>Changes</u>*: lines 113-114.*

<u>R4:</u> Line 119: Don't get this. I thought the analysis window was set to 2s? Where is the 0.6s coming from?

<u>A</u>*.: The analysis window typically has the length of 1 or 2 seconds, that is correct. However, here we are talking about the stacking window. This stacking window corresponds to the length of one or two periods of the signal length. This is better seen in Fig. 3. Here, the analysis window is shown in Fig. 3a and the stacking window is marked in red around the signal onset at the reference (central) station.*

<u>R4:</u> Line 146: I guess that synthetics would help to proof if e.g., topography of the area has some influence on the results… These synthetics are dearly missing in the whole paper

<u>A</u>*.: To what use? For the array analysis and to estimate the effects of topography, under the assumption of a uniform velocity beneath the array, synthetics are an overkill. We do not have a 3D velocity model to test for more intricate effects on the waveforms. We already provide detailed information on uncertainties of our approach in the current version of the paper.*

<u>R4:</u> Line 151: Again, this is only part of the story….,

<u>A</u>*.: We cannot comment on this.*

<u>R4:</u> Line 154: Did you then also change the stacking window length? This would be needed mainly because the changed bandwidth- time product

<u>A</u>*.: The reviewer is of course right, that a change in frequency has an impact on the stacking window. However, our tests show that it is not significant.*

<u>R4:</u> Line 158: So you did NOT change it! This is formally not entirely correct…

<u>A</u>*.: See above.*

<u>R4:</u> Line 159: exactly, but I do not understand the reference to figure 4?

<u>A</u>*.: We clarified this in the revised manuscript in line 182.*

<u>Changes</u>*: line 182.*

R4: Line 164: As far as I know hypocenter is strictly 1D and assuming flat earth with no topography - are these restrictions valid here? Why not using e.g. NonLinLoc which is able to use a 3D velocity structure (even if its a simple homogeneous one which only includes topography) and also the error estimate is far more realistic?

A.: We perform the analysis using SEISAN. This program is based on HYPOCENTER. However, in the SEISAN analysis the topography is incorporated. We clarified this statement in the revised manuscript.

Changes: line 187.

R4: Line 180: But you still have to deal with topography along the complete ray path - did you evaluate the influence of it?

A.: We focus on the first arrival of the signal. For the earthquakes occurring around Brava and analyzed with the stations on Fogo, we assume a ray which is first propagating downwards from the source. Thus, in this case the topography above the source and along most of the ray path obviously does not play a role.

For the analysis of events closer to the array (like it is typically the case for the array on Brava or for the hybrid events on Fogo) the topography may influence the result. For the first arrival of the signal, the depth of the source is a critical factor. If the event occurs in larger depth, the first arrival still is not perturbed much by the topography. This is typically the case for the earthquakes around Brava, which show mean depths of about 5 km (Faria and Fonseca, 2014). However, for the hybrid events on Fogo, the situation is (likely) different. Here we have a stronger impact of the topography and of the ray path. This is discussed in detail in lines 305-348.

R4: Lines 196-204: I'm confused! What do you want to say with it?

A.: We rephrased parts of this to improve its readability.

Changes: lines 220-225.

R4: Line 202: Did you ever check if there is really no S-phase present, e.g. by applying 3C beamforming?

A.: These events have been analyzed carefully. The first signal onsets on the horizontal components are rather weak (in comparison with the vertical component) and emergent. This makes the beamforming difficult, as the phase of interest is hard to determine. We decided not to focus on this aspect in the current paper.

R4: Lines 209-210: This should be mentioned at the beginning

A.: We have included it in sect. 3.1 of the revised manuscript.

Changes: lines 108-109.

R4: Line 248: Well, this is not really a new information? Have you ever cross validated the depth and ray paths by applying the same velocity model as during the hypocenter procedure?

A.: Indeed, we always use the same velocity model by Vales et al. (2014) for all estimations and assessments, where a velocity model is necessary, as already described in the text.

R4: Line 251: you are not referring to BBFK (e.g. Gupta et al., 1990) which gives you at least the same results (or better!).

A.: At this passage, we describe our own findings and the reference to Gupta et al, at this point, would not be helpful. As described above, the time-domain approach automatically implies the incorporation of a wide frequency band such that there is no clear advantage of transforming the signal to the frequency domain first.

R4: Lines 251-252: I do not see why a time domain beamforming is of any superior use than a broad band FK or even a high resolution FK (Capon). Time domain beamforming is very good to identify specific phases in the beam (e.g. through vespagram analysis) but you don't do it here. Capon or multiple Capon (IAS-Capon, Gal et al., 2014 GJI) would possible do a much better job here…

A.: See above.

R4: Line 282: Is this of any relevance here? I tend to say no… Different volcanoes with different local velocities

A.: Of course, different volcanoes exhibit different velocity structures. To test for the influence of the chosen velocity model, we apply different velocity models. As there are some similarities to volcanoes of the Canary Islands, it is justified to use these velocity models for comparison. Even choosing the velocity model of Etna gives additional information on the sensitivity of the results.

R4: Lines 331-332: Are your error estimates then still representative?

A.: Yes, as described in the text, we evaluated these errors carefully.