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Interactive comment on "Focal mechanism and depth of the 1956 Amorgos twin earthquakes from waveform matching of analogue seismograms" *by* A. Brüstle et al.

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Response to comments of Dr Konstantinou

- *Page 1914, lines 19-21: The authors find that the moment magnitude that corresponds to the Amorgos event is equal to 7.1 - this is actually 0.5 units lower than the magnitude estimated by Okal et al. (2009) who performed a similar study of the Amorgos event using historic seismograms recorded at teleseismic distances. How do the authors explain this large difference? Could it be due to the different methodologies used and/or processing of the historical seismograms?*

  Some more discussion about this point would be in order here.

  The magnitude estimation goes back to the ratio of average energy in the observed and synthetic traces. There are two major potential reasons for a discrepancy in magnitude estimation: a structural effect due to the narrow azimuth range covered by the available stations or a source directivity effect. Our estimate is below all others in the literature. But it should also be noted that Okal's estimate is the highest. In response to comments by another referee, we will look at modern events to assess a possible magnitude bias. We shall discuss this issue in the revised version.

- *Page 1916, lines 3-11: Konstantinou (2010) has argued, based on rheological modeling, that the hypocentral depth of the Amorgos event probably lies at about 30-33 km (depth of peak strength of the seismogenic layer) which actually supports the results of the authors for a depth 25+/-5 km. Strangely enough this study has not been cited in the manuscript. I attach the PDF of this article in case the authors are not aware of it.*

  Thanks for making us aware to this publication. We will take it into account in the revised version.

- *Continuing from my point 1 above, the authors find that the amplitude of the second event is "barely visible" on the seismograms despite the fact that they calculated a magnitude for this event similar to the Amorgos event (7.1). They further try to justify this difference using some complicated arguments, however, they do not consider a simpler explanation which is that the Amorgos earthquake was indeed much larger than the second event (i.e. 7.6 versus 6.9-7.1). If they cannot exclude this possibility, then I think they should explicitly state it as an alternative explanation.*

  Once again, our estimate is derived from the ratio of average energy of observed and synthetic traces which may suffer from the poor station coverage. In addition, the error of the magnitude estimate for the second event is certainly much larger as the waveform fit is just a desperate attempt to get a hint on the source depth. The magnitude estimate could be, for example, too high because of scattered surface wave signal of the first event included in the trace when calculating the energy of the observed traces. As proprosed by referee 2, considering only time windows around P or S-phases might help here.

- *I do not understand why the authors chose to use tsunami height values from studies that have been conducted in the 1960s, while Okal et al. (2009) offer a more recent and probably more accurate assessment of these values.*

  Okal's tsunami heights are derived from interviews with residents of the affected islands. We were not sure how reliable these values are and therefore used the values by Ambraseys and Galanopoulos. We will consider Okal's values in the revised version and also add a reference to Dominey-Howes (1998) on tsunami heights on Astypalea.

- *Page 1903, line 28: the authors probably mean "calculated" rather than "lived". Page 1906, line 7: the acronym MFT should be properly defined before it is used (i.e. Multiple Filter Technique (MFT)).*

  We will correct these issues in the revised version.

Response to comments of Referee 1

- *The introduction provides information about main faults and geological features and about focal mechanisms in the area (from instrumental seismicity). However, a clear figure is missing showing the main geological features discussed in the text, the distribution of seismicity and the reliable moment tensor solutions (those from well registered recent earthquakes).*

  We will add such a figure.

- *The introduction points out that some solutions are available for the first event, none for the second. For the first one, two possible general mechanisms are proposed: strike-slip (according to Papazachos & Delibasis, 1969, and Ritsema, 1974) or normal faulting (Shirokova, 1972, and Okal et al. 2009). Further information is needed on the method which were applied to infer a strike-slip mechanisms which are not given. If polarities are provided by Shirokova (1972) these can be used to rule out some of these solutions, and later, to discuss your own finding (e.g. Check if the obtained focal mechanism for the first event would fit polarities as in Shirokova, 1972). I feel it would be important to discuss these mechanisms in the light of focal mechanisms derived for more recent events in the area, which are certainly more reliable.*

  The paper by Shirokova is virtually inaccessable as it appeared as contribution to a monograph published by Nauka, Moscow in Russian language. It appears that only the focal mechanism has survived because it was reproduced in papers citing Shirokova. Regarding focal mechanisms of modern events in the Amorgos area, these are treated in a companion paper also currently available as SED discussion paper (Friederich et al., 2013. Focal mechanisms in the Southern Aegean from temporary seismic networks - implications for the regional stress field and ongoing deformation processes, www.solid-earth-discuss.net/5/1721/2013/) There, we indeed find a majority of earthquakes with typical normal faulting mechanisms that nicely fit to the one found for the 1956 event.

- *The station distribution, as shown in Figure 3, is obviously not optimal, and poses the question, whether a reliable focal mechanism can be obtained with such poor and asymmetric distribution. In my experience, a stable focal mechanism would be difficult to be obtained in such conditions, even using modern digital data from broadband seismic stations. This should be even more demanding with* *analogue data, given the required corrections and digitalization procedures, and possible source of errors depending on recompiling instrument parameters and information. Given these problems, the authors should try to convince the reader on the reliability of their solutions. I can suggest two options, but these authors may consider others: (1) repeat the inversion using a jack-knife method and verify the result keeps stable, when removing single traces or stations (2) check the polarity fit (according to the old study fitting polarities) with the assumption of your focal mechanism (this should easily works, as the solution from polarity is very similar to the one here obtained).*

We wonder whether jackknifing will yield a significant result given the few available traces. As already mentioned, the paper by Shirokova is rather inaccessible. We will look into the papers by Papazachos & Delibasis and Ritsema and, moreover, attempt to obtain first motions from the historic records not suitable for waveform inversion. In addition, we will look at modern Aegean events recorded in Central Europe to asses the relilability of the focal mechanisms. This was also proposed by referee 2.

- *Figure 6 should be improved by marking major phases of the two earthquakes. Honestly, I cannot recognise the second quake from this figure. Moreover, red and black lines seem to overlap for trace GTT-Z after minute 14. It is not clear if this part of the waveform could be used or not (if this is not used, may be more clear to plot only in red).*

  We will add marks to the seismograms indicating the seismic phases identified using the MFT. The overlapping traces are a plotting mistake we will correct in the revised version. The black trace is valid in the overlapping part.

- *Figure 8 is very interesting and could be used to support the estimate of a deeper source for the second event, as surface waves are not seen for the second event. However, the plot is not deeply discussed, e.g. S waves are also poorly resolved*

for the second event, while P waves are well seen, possibly pointing out a different mechanism for the second event. The discussion and support of given result would definitely benefit, by including similar plots for other stations. Therefore authors should include these, at least as electronic supplement.

We can provide similar plots for other stations that could not be used for waveform inversion because of incomplete response information. We will also add some discussion in the text referring to the figure.

- *Can the authors clearly state which instruments out of the long list and plot in Figure 1 were not used and why?*

  We will provide a table listing all stations from which data were digitized including reasons why they could not be used for waveform matching.

- *Why is the gridding on strike, dip and rake angle different (1 vs. 12)? This should be stated in the text.*

  Since strike and rake range over 360 degrees while dip only over 90 degrees, we chose 12 degree sampling for strike and rake and 1 degree sampling for dip.

- *Figure 10 could be given as a part of Figure 9. Results include some estimates of size and slip. Can some source information concerning the duration of the rupture process been inferred from the spectral analysis (spectrogram)? Would this fit to these paramters?*

  Estimates for source size and slip are derived from the seismic moment. We will check whether corner frequencies can be estimated from waveform spectra.

- *I understand that the inversion of the second event is very demanding, but some more results should be given. The first question concerns the depth: both the spectrogram and the focal mechanism inversion results point out a deep source. From Figure 11 it is not clear what is the found depth, nor whether it is reached at* *all (e.g. From the central plot, it seems the minimum misfit is found for the deepest depth). This suggests that further depths should be investigated. Then, once a minimum is found, a best focal mechanism solution can be given and waveform match shown, then stating why this should be or not considered reliable (e.g. looking at the misfit quality, should justify to ignore the focal mechanism, and limit the interpretation to the hypocentral depth which is confirmed by the previous analysis).*

There is no chance to achieve a waveform fit at all because of the scattered surface waves from the first event overlaying the second event. It may be more helpful to just fit envelopes or restrict the fit to the P- or S-phases. The waveform fitting for the second event was rather a desperate attempt to obtain additional constraints on source depth. It mainly shows that, regardless of focal mechanism, shallow sources produce much larger misfits than deep ones. We do not think that a minimum in Fig.11 can provide a significant hint at the true source depth because the misfits are much too high to be statistically significant. The depth range considered should be sufficient because there are no modern earthquakes in the Amorgos region at greater depths.

- *Minor comments: Maybe adding "southern Aegean" to the title*
  Figure 1: Please give intensity scale and units for the tsunami height.
  Since depths estimate are a major finding, some information should be given whether any estimate is given in the paper references in the introduction (those used for epicentral locations in Figure 1), when introducing them to discuss Fig. 1.
  Figure 4 should be better references as an example of observatory bulletin. It cannot provide detailed information on the instrumentation as station STU (as cited now).
  Finally, Figure 5 has a poor definition on my screen. I have no safe suggestion, how this could be improved. Perhaps the figure could only focus on a part of the

waveform in order to better appreciate the signal?

There is depth information for the second event in Papadopoulos & Pavlides, 1992, Galanopoulos, 1982 and Makropoulos et al., 1989. We will integrate them into Figure 1.
We do not understand the comment of the referee with respect to Fig. 4.
We will attempt to use image processing to enhance the contrast in Fig. 5.

Response to comments of Referee 2

- *Four questions posed by referee 2 referring to the first event:*
  1. Why are the remaining stations rejected? The author make a very vague reference to "various reasons" but this is clearly not enough. 2. How robust is to model such frequencies for fault plane solution determination using stations located such a small distance/azimuth range located at around 2000km? 3. What are the expected errors of their FPS estimates? What happens if we look at e.g. the best 100 FPS rather than the best 10? 4. What happens if the authors allow for larger depths, similar to what they did for the 2nd event? Are optimal solutions also found for larger depths?

1.) We shall extend Table 3 to include all stations shown in Figure 3 including reasons why they could not be used for waveform matching.
2. and 3.) We will do an analysis of a modern event using only few stations in Central Europe. Moreover, we will more closely analyse the various solutions offered by the grid search with respect to misfit. In addition, we will try to obtain first motions from stations not used in waveform matching to get additional constraints on the focal mechanism. We are not sure about the usefulness of jacknifing when applied to such few traces.
4.) We will extend the grid search to larger depths for the first event. When only

body waves are matched, small misfits may appear. But these solutions can be probably excluded because they will not produce the surface waves seen in the data records.

- *Event 2: Why do the authors present a waveform fit and not a simple envelope fit. Even simpler, their interpretation essentially is based on peak surface wave amplitudes, against the corresponding body-wave amplitudes. Why didn't the authors present a simple peak-amplitude vs distance analysis (using surface-to-body wave ratios), adopting a fixed fault plane solution (e.g. from regional tectonics for intermediate-depth events, e.g. see Benetatos et al., 2004)?*

  We appreciate the ideas formulated above and will follow them for the revised version (i.e. envelope fit, surface wave to body wave ratios for typical focal mechanisms). Since the scattered surface waves from the first event are rather large, they will probably disturb the envelope fit as well. Looking at peak-amplitude ratios appear to be more promising.

- *Event 2: Using AK135 for 0.03-0.07Hz for the Aegean area (wavelengths of 60-120km for S-waves and larger for P), as the authors did for the 1st event, seems OK, as these large wavelengths do not see the strong crustal variations in the study area, which has a very different structure (e.g. lower sub-Moho velocities with respect to Ak135). However, this is clearly not the case for frequencies around 0.15-0.3Hz, corresponding to wavelengths 10-20km, which are strongly affected by the local structure. I am sure that an attempt to fit waveforms for a recent event, especially regarding its surface waves, at distances of 1900-2300km would fail miserably, not only regarding the actual phase information but also the recorded amplitudes. If the authors could not fit the surface waves of the 1st event at much lower frequencies (0.03-0.07Hz), how can they draw conclusions even from approximate surface wave amplitudes at much smaller wavelengths, given the very strong structural effect and the very small distance and azimuth range of their stations? I think the authors need to demonstrate that their analysis*

  (summarized in figures 11 and 12) is reliable. This can be partly supported by the analysis for large depths performed for the 1st event, previously proposed. However, the best way that the authors need to follow is to demonstrate the feasibility of their analysis, by interpreting data for a recent, similar intermediate-depth event. The 2002-05-21/20:53:30, h=105km, M=5.8 Milos event (strike-dip-rake 352/89/4, information from Benetatos et al., 2004) is a very good candidate event for the area, which they can use for their stations (or 4 other stations close to their recordings) to demonstrate that the analysis shown in Fig.11 and 12 can be repeated and be employed to identify intermediate-depth events in the area for the specific frequency range.

  This is a tough comment and probably correct. Yes, it is definitely difficult to fit short period surfaces waves around 0.15-0.30 Hz at distances of a few 1000 km. Our waveform fitting approach was just a desperate effort to get an idea of the surface wave to body wave ratios for deep and shallow events. We will analyse a modern event to find out whether amplitude ratios obtained from waveform modelling are useful at these high frequencies and distances. We will check whether reliable amplitude ratios can be read from other records of the second event not considered in the manuscript.

- *It should be noted that the results presented in Figure 12 show that for all the recording stations the actual (observed-black line) S-wave amplitudes are smaller than the modeled S-wave amplitudes (red-line) for the deeper earthquake scenario (h=158km), hence the authors essentially only fit the P-wave amplitudes. This is expected, as all the 4 recording stations lie in the southern Aegean back-arc area, for which intermediate-depth event with $h > 100$ km show significant S-wave attenuation, even at low frequencies due to the attenuation of the mantle-wedge. This is known since the 70s but has been recently quantitatively demonstrated by Skarlatoudis et al. (2013), who proposed a reduction factor of 2 for frequencies of 0.25Hz between back-arc and along-arc stations (see coefficients* *c41 and c51 in Table 2 of this publication for the period of 4sec). In my opinion this is the main or at least a complementary explanation for the small amplitude of S-waves recorded in the examined stations, while the authors only consider the fault-plane solution issue, as is mentioned by the authors in lines 26-28 of page 16.*

We are aware of the generally low S-wave amplitudes observed in the backarc region. We did not use this argument because we are not sure whether an S-wave propagating from an intermediate depth earthquake towards Central Europe is greatly affected by attenuation in the shallow mantle as it rathers travels downwards through the deeper mantle.

- *c) I am somewhat skeptical about the magnitude proposed for both events, especially the second, intermediate-depth event, using the energy-ratio approach (equation 6). More specifically: 1. For the first event, the waveform comparison (figure 9) shows that both real and synthetic data have similar frequency and phase content, therefore the energy ratio of equation (6) seems quite robust. On the other hand, as all 3 stations lie within a very short distance and azimuthal range, it is clear that any structural difference (different geometrical spreading, anelastic attenuation structure, etc.) but mainly any special source effect (e.g. source directivity) may result in apparently lower or higher magnitudes. In the present case, the final magnitude (7.1) is significantly smaller than all published Ms values (7.2-7.5) and the Okal et al (2009) estimate (7.6-7.7), which could be simply a result of preferential (e.g. to SW-to-NE) rupture directivity. The authors need to discuss this, and possibly use the previously proposed analysis for a recent event, to assess the bias that could be introduced by such an effect, in an attempt the explain the origin of possible magnitude bias.*

  We were slightly puzzled by the low magnitude but accepted it as a result of the waveform matching. Certainly, the estimate may be biased by the reasons given above. We will discuss this issue in the revised version.

- *2. A critical result of the manuscript is that the second event is not only of intermediatedepth but of practically identical magnitude (Mw 7.1, Table 4) as the first event! Such a large intermediate-depth event has important implications (e.g. for geodynamics, seismic hazard assessment, etc.) and is not compatible with the traditional view that considers the deeper part of the Hellenic Benioff zone capable of producing only moderatemagnitude (up to M=6.7 for event depths larger than 100km, e.g. Papazachos et al., 2005). The possibility to have M>7 events at depths >100km is intriguing and will significantly alter our current perspective about the deeper section of the Hellenic Subduction area. However, what makes me skeptical about this estimate is the applicability of equation (6) using waveforms such as those shown in Figure (12), at such high frequencies that the simple AK135 model can not predict. As seen from all synthetics in Fig12b, the observed waveforms contain a large amount of non-modeled (e.g. scattered) wavetrains, especially before, in-between and after the P and S arrivals. If this energy has been accounted in the Ed estimation for equation (6), this may have resulted in significantly overestimating the Mw for the 2nd event. Furthermore, since the authors did not fix the event FPS, we cannot be sure about the effect of the source energy release of the real and the modeled FPS for the specific recording stations, which I again repeat concern a very small distance and magnitude range, therefore a very small solid angle of the source radiation pattern. I think that the authors need to constrain the FPS using a realistic solution and perform the analysis for a recent different intermediate-depth event, as previously proposed, so that they can at least calibrate the process and the magnitude assessment. In any case, they need to at least compute the energy-ration of equation (6) by considering the P- and S-time segments for which they can predict significant synthetic amplitudes, as it is clear that they lack the structural information to reliable asses additional seismic phases seen in the real data of Figure (12) at the investigated frequency range.*

  The estimated magnitude of the second event is certainly prone to large errors as the waveforms could essentially not be matched at all. Scattered surface waves from the first event may certainly be a reason for overestimating the seismic moment. We will follow the advise of the referee and analyse a modern event to calibrate the magnitude estimation. In addition, we will try to consider time windows around P and S-waves for computing energy ratios and discuss the results in the revised version.

- *Minor comment 3: The coincidence of shallow and deep tectonics along the southern Aegean volcanic arc (where Amorgos events also belong), and which supports the proposed interpretation by the authors, was also proposed by Papazachos and Panagiotopoulos (1993). I think it is worth that the authors discuss the implications of a double shallow/intermediate-depth event, perhaps looking at similar worldwide cases, as this kind of dynamic triggering is certainly extremely interesting.*

  We shall take up this issue in the revised version.

- *The Shirokova (1972) mechanism, based on long-period first motions, is a very close match for the proposed FPS. Why did the authors not combine their inversion with these long-period first motions, or even the teleseismic data used by Okal et al. (2009)?*

  The focus of the manuscript was originally on the European stations. In case we can get access to records of some of the worldwide stations, we will consider to include some of them to improve the azimuthal coverage. We will also check whether we can use first motions from European stations that could not be used for waveform matching.