Solid Earth Discuss., 5, C818–C827, 2014
www.solid-earth-discuss.net/5/C818/2014/

Solid Earth
Discussions

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

Anonymous Referee #2

Received and published: 2 January 2014

(Please see the review in the attached comment file)

Comments on the paper "Focal mechanism and depth of the 1956 Amorgos twin earthquakes from waveform matching of analogue seismograms" by Brustle et al. (2013)

This is an interesting paper concerning the Amorgos major event, the largest 20th century event in Europe, and its aftershock. The paper contains 2 main results: a) A re-assessment of the 1956 mainshock fault plane solution and, b) A completely new view of its aftershock, proposing that it is a different, intermediate-depth event. The authors have done a good job at attempting to recover the fault plane solution of the

main event, as well as investigating the aftershock hypocentral depth, using historical records. However, there are several technical and scientific open questions regarding the obtained results and especially their interpretation, which suggest that the paper can only be accepted after major revision and re-evaluation, as I explain in the following.

Language and organization of the paper

The paper is written in clear and understandable English, properly organized and presented. Minor issues are listed at the end of this review.

References

Generally OK, quite complete. Please correct "Papazachos B." to "Papazachos C." for Papazachos and Nolet (1997) in the reference list. Please consider adding the Galanopoulos (1957) and Papazachos et al (1985) publications, which are related to the Amorgos earthquake and its tsunami (see reference list at the end of this report in BibTex format). Some additional references related to the scientific interpretation of the results are mentioned later and also listed at the end of this report.

Figures and Tables

Tables are useful and informative. I think we need an additional Table that explains the data limitations, possibly replacing Table 3, as is later described in the SCIENCE section. Figures are also generally OK. Some minor comments follow:

1. In figure 1 the "Cyclades" and "Turkey" labels are hard to see, as their color is very similar to their backgrounds. Also the tsunami height numbers shown in Figure 1 clearly miss several observations, especially those reported by Galanopoulos (1957). Since the Galanopoulos original work was published in Greek, the authors can retrieve his observations from Table 1 of the Papazachos et al. (1985) paper. Also, please make the isoseismal lines more thick, as they are hard to see. Please notice that the first isoseismal lines for this event where produced by Shebalin (1974) in the corresponding

UNESCO macroseismic isoseismal atlas.

2. Fault Plane Solution (FPS hereinafter) beachballs in the upper figures of figure (9) are very hard to see, even when zooming in the electronic version. It would be helpful if the upper figures could be enlarged.

Notice that some comments related to the Science of the figures are later discussed.

SCIENCE

This is the main reason that I recommend major revision for the paper. My main reservations are explained in detail, following the manuscript page numbering and order. More specifically:

a) The authors present in Figure 3 a large number of stations for which data are available, covering a wide distance range and an acceptable azimuthal distribution. However, for the first Amorgos event, they finally fit:

1. Only S-waves between 0.03-0.07 Hz, as they (correctly) suggest that surface waves are more strongly affected by the model complexity 2. Only selected data from 3 stations located in NW Europe, which exhibit a very small distance and azimuthal range. Eventually, they present their 10 best results for 3 candidate epicenters, which show compatible results for depths $\sim$20-30km, although secondary maxima at larger depths are also seen. Four important questions immediately come to mind here: 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 $\sim$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?

I think the authors need to somehow answer the 4 previous questions. For Q1, I propose that they enlarge Table 3 with all stations/components and provide clear explanations of the limitations of each case for which they did not use data. For example, I am very curious why the ATH and the ISK seismogram could not be used for the FPS determination, as they are located at regional distances and can provide a good control of the FPS, especially its depth. In any case, a specific justification needs to be provided for each station.

For Q2, the authors need to demonstrate that they could perform this estimation for a couple of modern events, using only a few recordings from the same (or neighboring) stations. The neighboring 2006 Mw=6.8 Kythira event is a good choice, as it was well-recorded in Europe, although it has a larger depth.

For the error assessment, either a formal error analysis (using the $\chi 2$ value) or the variability of the solutions can be used, e.g. in a jack-knife approach, could be used to give some estimate to the errors of the final solution, or at least some confidence limits.

Finally, Q4 can be answered by extending the tests to larger depths or by presenting a figure similar to Figure (11), so that the depth determination can be considered as reliable. This will also allow assessing the significance of Figure (11), which is critical for the interpretation provided for the second (intermediate-depth) event, later discussed.

b) For the second event, the authors perform a "fit" at much higher frequencies (0.15-0.3Hz), extending the grid search down to the depth of 163km. Here surface waves are also included in the fit, in an attempt to use them to constrain the focal depth. As waveforms are not really fitted ($\chi 2$ values $\sim$26-66 are statistically meaningless), essentially the authors fit envelopes, rather that waveforms. This is most probably the reason that no FPS is shown in the paper for the second event. The main author argument, essentially summarized in Figures 11 and 12, is that the small amplitude of surface waves for the 2nd event supports the large hypocentral-depth interpretation.

The authors idea is very interesting and appealing. However, again some questions clearly arise:

1. 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)?

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-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.

It should be noted that the results presented in Figure 4 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>100km 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 ~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.

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.

2. A critical result of the manuscript is that the second event is not only of intermediate-depth but of practically identical magnitude (Mw$\sim$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 moderate-magnitude (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.

Minor comments

1. *Page 3, line 29*: "…Stress tensor inversions of fault plane solutions (Bohnhoff et al., 2006; Friederich et al., 2013) lived from available first P motion polarities of shallow high-quality hypocenter locations indicate…". Not clear. Perhaps change "lived" to "derived" ?

2. *Page 40, figure caption*: Change 33-35s to 33-35min

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.

4. 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)?

In order to summarize, I think the paper contains very interesting results. Despite the fact that the first event has already been studied and that the proposed solution is very similar to Shirokova (1972) or Okal et al. (2009), the manuscript information provides a complementary look, also suggesting a much more reasonable depth estimate. Even more important are the 2nd event results, which suggest a very different, intermediate-depth event, providing an interesting association between shallow and deep seismicity in the area, which has important geodynamic, tectonic and seismic hazard implications. However, no matter how much I would like to accept the results, I think that the authors need to do a better job in proving the reliability and robustness of their findings, especially regarding certain aspects (e.g. event magnitudes, especially for the 2nd event). In my opinion, the main weak point of the paper is the very small distance and azimuth range of the employed recordings. Perhaps the only way to prove the reliability (even partial) of the obtained results is to perform a feasibility study by similar processing for selected modern-era events, using similar recordings at the same or neighboring stations, especial for the 2nd deeper event.

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Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/5/C818/2014/sed-5-C818-2014-supplement.pdf

Interactive comment on Solid Earth Discuss., 5, 1901, 2013.