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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.

Anonymous Referee #1

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General comments

I find the paper very interesting and a nice example of how analogue seismograms can be carefully processed and used to an important seismological investigation, shading light on important past earthquakes. However, I feel there are some important aspects where the paper should be still improved and not enough discussion on the reliability of the inversion approach, given the very poor stations distribution. Therefore I recommend a major revision of the paper. I point out my major remarks in the next paragraph, and add at the end a list of minor comments.

Specific comments

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).

2. 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 & Delibasi, 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.

3. 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).

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4. 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).

5. 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.

6. Can the authors clearly state which instruments out of the long list and plot in Figure 1 where not used and why?

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

8. 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?

9. 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

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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).

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?

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