

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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The manuscript by Brüstle et al. "Focal mechanism and depth of the 1956 Amorgos twin earthquakes from waveform matching of analogue seismograms" deals with two topics that are both quite interesting, namely the use of historical seismograms to study past events and the source parameters of the 1956 Amorgos earthquake. After reading the manuscript I have a number of points that I would like the authors to consider when preparing the final version of their paper.

1. 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

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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.

2. 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 \pm 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.

3. Page 1916, lines 18-20: 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 (\sim 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.

4. Figure 1: 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.

5. Page 1903, line 28: the authors probably mean "calculated" rather than "lived".

6. Page 1906, line 7: the acronym MFT should be properly defined before it is used (i.e. Multiple Filter Technique (MFT)).

Please also note the supplement to this comment:

<http://www.solid-earth-discuss.net/5/C708/2013/sed-5-C708-2013-supplement.pdf>

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