Solid Earth Discuss., 6, C1410–C1419, 2014
www.solid-earth-discuss.net/6/C1410/2014/

Solid Earth
Discussions
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Interactive comment on "High-precision relocation of seismic sequences above a dipping Moho: the case of the January–February 2014 seismic sequence in Cephalonia Isl. (Greece)" *by* V. K. Karastathis et al.

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Received and published: 15 December 2014

We are grateful to Dr Ivan Koulakov for his efforts to improve the manuscript. His thorough review was important and his comments were all taken into account.

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Reviewer: The first sentence of the abstract does not sound to me. How geometry of a network is associated with boundaries of convergent plates?

Author: Usually plate boundaries (Hellenic arc, Japan, Chile, Western part of North

America, Aleutian trench, Indinesia etc.) fall outside the national seismic networks. We change the text in order to make it more clear.

The new text is: Detailed velocity structure and Moho mapping is of crucial importance for a high precision relocation of seismicity occurring out of, or marginally to, the geometry of seismological networks. Usually the seismographic networks do not cover the boundaries of converging plates such as Hellenic arc.

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Reviewer: P2702 l.7-10: I think two factors, such as misidentification of phases and picking errors, should be separated.

Author: We added the picking errors as a separate source of errors.

The new text is: More specifically, the main sources of errors for an accurate determination of the hypocentral parameters are: (a) picking errors, (b) the false identification of the seismic phases, (c) the insufficient number of phases, (d) the deficient azimuthal coverage of the seismographic network and finally (e) the use of non-effective seismic velocity models that are usually oversimplified (often one-dimensional) without adequate information for the velocity structure and the lateral velocity heterogeneities.

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Reviewer: P2704 l.13-14: It sounds paradoxical that weaker events with smaller number of recorded picks are better located than strong ones, and I find this statement not correct. It is obvious that if the number of stations is same, the location accuracy should be better with a stronger event which has clearer picks. Probably it should be said here that better aftershock locations is due to deployment of additional stations at short distances.

Author: The large number of phases limits the uncertainty relative to the calculated hypocenter. In a given network area the larger the number of stations the better the solution (denser network is better). However, if a large number of extra stations located at far distances where the velocity model used is apparently inadequate were to be added then the additional information does not improve the solution and may introduce biased errors (shift). In other words, the problem is caused by the problematic velocity model beneath the remote stations.

The new text is: Therefore, significant errors were involved and consequently the epicenter of the first strong earthquake was shifted substantially to the east. Its aftershocks, however, were not shifted significantly because of their small magnitudes prevented the use of many distant stations where the velocity model was inadequate. In a next paragraph it is explained more precisely how errors are introduced in the epicentral solution.

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Reviewer: P2705: Description of the active source experiments is unclear. There are many terms and abbreviations which are probably clear to specialists dealing with the experiment, but remains Chinese for a broader audience. What are "bearing N62E", "M/V Bin Hair 511", "36-airgun tuned array", "36-fold seismic profile"? I encourage describing these experiments using more simple terms. Do these previous active source experiments provide the S velocity distribution?

Author: We simplified the text: The profile ION-7, with an azimuth of N62°, was conducted offshore between Cephalonia and Zakynthos (Zante) islands having total length of 180 km, starting from the deep Ionian basin and reaching the western Gulf of Patras (see maps in Hirn et al., 1996). For the data acquisition a Motor Vessel (M/V) of Geco-Prakla was used with a 36-airgun array (for processing details see also in Kokinou et al., 2005). The seismic reflection profile acquired, provided useful information for the shallower structure.

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Reviewer: P2706. L4-6: It is unclear whether this is an active or passive experiment.

Author: We pointed out that the model was based on a passive experiment

The new text is: The velocity model of Haslinger et al. (1999) (Fig. 3) was built for the region at the east of Lefkada Isl., western Greece, which as regards to the Cephalonia 2014 sequence, concentrates a high percentage of the ray-paths between the earthquakes and the stations. This model was based on a passive experiment and was built as a "1-D minimum velocity model" for this region by VELEST algorithm (Kissling et al., 1994; Kissling, 1995) and used in a following stage as initial model in the local earthquake tomography method and SIMULPS code (Thurber, 1993; Eberhart-Phillips, 1990, 1993), implemented to calculate the 3-D crustal velocity structure. The SIMULPS code uses a linearized damped least-square inversion to solve the non-linear problem of the hypocentral location and velocity model.

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Reviewer: P2707: Description of the synthetic experiment. It is unclear where the profile in figure 4 is located. Please show it on the map. I do not understand why the contour lines of time differences in figure 5 have so complicated shape. I would expect that in the case of 2-D velocity model versus the 1-D model, the time difference patterns should be close to the elliptical shape without corner-shaped structure, as observed in NE of the plots. Why in this experiment, the crust in W part is thicker, whilst apparently it corresponds to the sea area? Is the Moho interface in this model represented by sharp transition or by a gradient layer? I do not almost see any significant difference between plots in figure 5. I would expect much stronger differences for events at different depths.

In my opinion, the description of synthetic modeling should be placed into a separate section to distinguish it from the analysis of observed data.

Author: We added in figure 5 the location of the figure 4 profile. The contour lines weren't of an absolutely smooth and ellipsoid shape since the calculations of the time differences between the models, were based on the actual stations and their synthetic travel-times (green triangles in figure 5). The software used for the contouring couldn't effectively handle the highly uneven distribution of the stations. We fixed the problem by using denser synthetic travel-times with hypothetical stations, evenly distributed in the study region. Thus we replaced figure 5 with a new one.

Regarding the Moho shape, in our tectonic consideration we are interested in crustal thickness to the east of the CTF (Cephalonia Transform Fault). This is because our seismic stations are located on the Aegean (i.e. Eurasia) plate. To the west of the CTF exists a continental margin type of crust, a transitional type of crust and the oceanic crust of the Africa (Nubia) plate. On average, crustal thickens to the west of CTF is 30 km. To the east of CTF (continental Greece) the crust is thicker in the area of the Ionian Sea and western Peloponnese (40 km) and becomes thinner towards central Greece and Attica (25 km). The 40 km thick crust is inherited form the last orogenic phase of the Hellenides and is developed in a NW-SE direction as has been established by many studies (geological and seismic). Towards the east, thinning is the well-established result of crustal extension since late Pliocene times. This on-going process results in a dipping-Moho configuration that we present in Fig. 4 of our paper.

The Moho interface is represented by a sharp transition. Its shape is based on the bibliography referenced. The traveltime differences although significant were not prominent since they were calculated for source depths of 5 and 15 km. These depths correspond to shallower and deeper events of the given sequence. If the comparison was between 5 and 30 km the difference would be a lot more prominent.

In order to distinguish the real data processing from the synthetic modeling we divided the specific text in three separate sub-sections.

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Reviewer: P2708 – Data description: Total number of events corresponding to the mentioned phases should be indicated here. Station corrections: were they computed by VELEST of estimated from a priori information?

Author: We added the total number of events in P 2708 line 15 and information about the estimation of the station corrections in P2708 L21-23.

L13: For the relocation of the Cephalonia 2014 aftershock sequence we used NOAGI phase dataset consisting of more than 44 000 P wave and 24 000 S wave arrivals for the time interval from 26 January 2014 to 15 May 2014 inclusive, corresponding to more than 3300 events.

L21: The station delays were calculated as an average value of observed travel time residual of the well located events (gap < 180o, RMS < 1s, erh and erz < 1km) and applied to the location procedure. Station corrections compensate for the effect of the station local geology, which could not be taken into account by the use of 1-D velocity model.

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Reviewer: P2709 – Presenting errors: What is the definition of the source location error? Is it the distance from the unknown true location, or it is just a measure of remaining time residuals? I guess the former is more valid in this case. I think that it is incorrect to call these values errors, because for most readers it means that the true source coordinates should be located within the error ellipse. However if you consider two velocity models you may obtain two source locations at a distance larger than errors of each event. The true source cannot be located simultaneously within both ellipses. I think, it should be stated here that one velocity model provides smaller rms of residuals that another one; therefore it is considered as better one.

Author: As per the reviewer's suggestion we replaced the term error, since it may be confusing and misleading to many readers. We instead use the terms “horizontal uncertainties” and “vertical uncertainties” when needed.

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Reviewer: For the same part of the text: what is the mean difference between source locations in different velocity models. It is not easy to see the difference between source locations in maps in Figure 7. In my opinion, it would be much more informative to present just one map showing the differences between locations in the best 1D model and the 2D model presented by dots for events in one velocity model and line connecting with the solution in another model. I think that the conclusion, which summarizes the main achievements of the study, is necessary at the end of the paper.

Author: We added a table showing the mean differences in source locations between the 2D and the other models. The only case where the location differences in the maps are small is this between the 1D and 2D version of Haslinger model as was actually expected. However, even in this case the differences in the three major events are noticeable. The figure also points out the effect of the velocity model on the final location. We added a conclusions section at the end of the text.

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Reviewer: Figure 1. I think here it would be useful to present the bathymetry (instead of Figure 2) which may give an idea about the transition from the oceanic to continental crust for the study area and surroundings. For the transform fault, the direction of the displacement should be shown.

Author: Added bathymetry in figure 1 and slip direction for the CTFZ.

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Reviewer: Figure 2, caption: Correct “26 January”. For the GFZ, the name is too long for the caption.

Author: It was fixed.

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Reviewer: Figure 3: Are the S-velocity distributions available for all studies? In my opinion, these graphs can be shown in one plot.

Author: The S velocity distributions is available for Haslinger et al. (1999) model. In the case of NOA's model a constant Vp/Vs=1.73 is provided. The model proposed by Hirn et al., (1996) is based on the results of a P-wave active seismic survey therefore no S-wave velocity model was provided. For this model we used the Vp/Vs value proposed for the area 1.80 (This value is compatible with the Wadati diagrams of Haslinger et al., 1999 and the results of prior studies (Hatzfeld et al., 1995; Le Meur et al., 1997). Sachpazi, et al. do not also provide an S-wave velocity model. We used the constant value of Vp/Vs=1.80 (suggested by the corresponding author by personal communication).

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Reviewer: Figure 4: Show the location of the profile in one of the maps. Do you use the same 2D model for synthetic and real data?

Author: The same 2d model was used for the synthetic and real data.

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Reviewer: Figures 6 and 7. I think all these figures can be combined in one plot showing the deviation of the main shocks and differences between locations of smaller events in two models (best 1D and 2D) indicated by vectors.

Author: We tried the suggestion but we finally decided to keep the present figures since the amount of the events and their close proximity, especially between those two models, resulted in cluttering the figure making it a bit confusing.

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Reviewer: Histograms in Figures 8 and 9 seem to me not informative because I do not understand what the error of source location is, regarding the fact that the true source locations are unknown. Instead, I would recommend presenting a table with standard deviations of residuals.

Author: Although Figures 8 and 9 show clearly that for the 2D and 1 D models of

Haslinger et al. the number of events with low ERH and ERZ values are a lot higher in comparison with the other models (the total number of events is the same for all models) we removed them and added the more quantitative measure of the sd of the residual as suggested by the reviewer.

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Reviewer: Figure 11. Indicate the time ranges in each of the plots

Author: Added time ranges below each map.

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Reviewer: Figure 12. I find coloring in vertical section non informative. I propose using different colors for different time periods. In map, yellow and red dots of events are not well seen in the yellow-red topography background.

Author: Following the reviewer's suggestion we used different colors for the time periods.

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Reviewer: Figure 13 seems to me not necessary: it relates to another story and appears to be confusing to me.

Author: We removed Figure 13.

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Reviewer: Figure 14. I don't understand the logics of this figure. A and C relate to the same time period, but different sections; b is another section and another period. Wouldn't it be better to present in Figure 12 two or three different cross sections with indications of different time periods by different colors?

Author: We incorporated all the cross-sections in Figure 10. We added color coding relative to the time period that corresponds to each cross-section.

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Interactive comment on Solid Earth Discuss., 6, 2699, 2014.