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Interactive comment on "Fault reactivation by gas injection at an underground gas storage off the east coast of Spain" *by* Antonio Villaseñor et al.

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First of all we would like to thank the helpful and constructive comments made by both reviewers on the manuscript. While the comments are generally favorable, the reviewers raise a number of issues that we would like to respond in this rebuttal letter.

A common criticism of both reviewers is the size of the figures and their labels and symbols. While the original figures had a reasonable symbol and font size, when combining them into multi-panel figures, the size was reduced. To compensate for this deficiency we have redone most of the figures to increase the visibility of symbols and labels, and also to include some of the suggestions by the reviewers.

Moreover, in order to address some of the comments, and to facilitate the re-

producibility of the results presented here, we have created a data repository where all the data used and modeling results are available. The link to the repository (https://digital.csic.es/handle/10261/192082) and the DOI of the dataset (10.20350/digitalCSIC/8966) have been included in the "Data availability" and References sections of the manuscript.

Now we provide detailed responses to the reviewers comments (in italics), followed by our responses (in normal font).

The paper provides a seismological discussion on an interesting case of triggered seismicity in Europe, occurring in 2013 offshore Spain. The sequence was studied by a number of previous publications and reports. However, beside a general agreement on the relatively shallow hypocenters and strike-slip dominated mechanisms, accurate depth and fault geometry remain to a certain extent debated. Given the interest of the sequence and its relevant in the field of induced seismicity, this study appears to be justified.

Target of the study are basically on one side dispersion curves and velocity models, to improve Green's function and data modeling up to higher frequencies, and on the other side a contribution to the estimate of focal depths and focal mechanisms (or moment tensors).

Again we are glad to see that both reviewers understood the main message we wanted to convey with this manuscript.

I think this is an interesting manuscript, but requires some moderate improvement. I provide below my major comments:

Main comments:

1. Uncertainties In order to provide new insights into a sequence which was discussed by previous papers, I think authors should not only provide a new result (depth, location or mechanism) but also some uncertainties. The estimation of uncertainties

is discussed indeed in the first sections, dedicated to the assessment of dispersion curves and velocity models, but they are not used to derive a uncertainties on derived parameters, such as the depth.

In our study we have considered that it is more valuable to demonstrate that the main results and interpretations are supported by the data than to provide a rigorous error analysis, which is both difficult and not well established for a complex nonlinear problem such as moment tensor inversion.

For this reason we have made available in a repository all the data used, together with detailed modeling results. For all the 14 events analyzed (listed in Table 3) we provide in the repository the distribution of stations used, results of the grid search for focal depth, and waveform data fits. Resolution/uncertainty in focal depth can then be assessed by the reader by looking at plots like those shown in new Figure 5. When analyzing these plots we observe that in some cases the uncertainty in depth can be large (e.g. greater that 5 km), but it is also clear that for all events focal depths smaller than 4 km are not supported by the data. Since we do not interpret the actual value of focal depth, but the fact that it is significantly deeper than the injection depth, the evidence presented in the manuscript and in the repository supports our claims.

2. Network asymmetry Both depth estimation, location and hypocenters suffer in this region by the asymmetric distribution of the stations. In this study, some new data have been taken into account (e.g. upon the Topoiberia project), but the azimuthal coverage remain strongly unbalanced. This may have a strong influence on the location accuracy, and some works suggested that the distribution plotted e.g. in Fig. 1b, may be partially attributed to the network geometry. The azimuthal coverage may also affect the depth, because of an inaccurate epicentral location. Has this been verified? Finally, it surely affects the focal mechanisms estimation. All these effects are not discusses.

The location of the earthquakes in this sequence was addressed in a previous publication of our group (Gaite et al., 2016), and this is why it is not discussed in detail here. In

that study we determined high precision traveltimes from waveform cross correlations, obtained a 3D velocity model for earthquake location, and located the earthquakes with a nonlinear method that produced realistic estimates of hypocentral uncertainties. After all this analysis the NW-SE orientation of the seismicity remained, so it is most likely a real feature and not only a result of the network geometry. This was discussed in lines 227-235 of the original manuscript.

3. Data used for MT inversion Furthermore, authors use the same velocity model for all stations. While this may be proper for onshore stations, I doubt this is accurate for stations on Balearic islands. It is unclear whether these stations have been used or not, as they appear in Fig. 1 but not in Fig. 4. Using them will surely improve the coverage, and improve the moment tensor inversion result, but possibly a different velocity model should be used. Fig.4c should show some waveform fit there.

Figure 1 shows all permanent stations in the region, while Figure 4a shows the stations that were used for the earthquake analyzed in that figure. The repository contains a map for each earthquake showing the stations that were used to obtain that particular moment tensor. We have modified the captions of Figures 1 and 4 to make this clear.

Stations in the Balearic Islands were not used because of the reasons described by the reviewer. The 1D model used would not be appropriate for paths to those stations, and the inversion method used (Herrmann et al., 2011) does not perform well with marine/oceanic paths. Although we do have a 3D velocity model for the region (the one obtained by Gaite et al., 2016), it is computationally expensive to generate synthetic seismograms in 3D models, and we are not aware of any regional moment tensor inversion method that uses 3D models.

The effect of lack of station coverage to the east of the earthquakes is partly compensated by the fact that we fit the 3 components of the displacement: Z, R, and T Surface waves have the largest amplitudes at the distances and frequencies considered here, so using Z, R and T components means that we are fitting both Rayleigh and Love waves. Since Rayleigh and Love waves have different radiation patterns, even with an unfavorable station distribution, it is possible to obtain well determined nodal planes.

4. Velocity models Since a lot of velocity models are discussed, they should be included in the document, as table or in the e-supplement. Having them available is need for the reproducibility of results.

The two models discussed in the manuscript are listed in Tables 1 and 2. This and other comments by reviewer 2 make us wonder whether he/she was not provided with a complete PDF or it was a low quality one (?). We agree with the reviewer that all the information necessary to reproduce the results should be available, and therefore we have created the aforementioned repository with all data used and modeling results.

5. High frequency waveform modeling The high-frequency waveform comparison is very interesting and in my opinion the most interesting and novel part of the work. However, too little is said on how data were processed. Please, provide accurate information on how you process and fit data.

The only processing done to the data was to filter it (0.2-2 Hz band pass). This information has been added to section 5. Data fit is measured using the cross-correlation coefficient, and that is already specified in the manuscript.

The velocity of the structure is so far poorly resolved, especially at shallow depths. This can strongly affect the high frequency synthetic waveforms and thus your inference. How sensitive is the method to such velocity model uncertainties? You only show the fit for the "best" depth, but a reader has no idea what are the uncertainties... Could you plot the fit for perturbed depths as well

The shallow velocity structure in the area of the CASTOR UGS is not poorly known because there is a lot of information from well data. We have used this information to create our model for forward modeling of crustal reverberations (section 3.2), so the model uncertainties should be small. And the fact that we are able to match well the

reverberations confirms that the model used is appropriate.

The value of the cross correlation coefficient as a function of depth shown in the right panels of Figure 7 provides information on the sensitivity to depth, particularly the low (poor fit) values obtained for depths lower than 4 km. However, showing the waveform comparison between synthetics and observed seismograms for different depths, as proposed by the reviewer, also provides very clear information about the sensitivity to depth. In fact we have produced these figures (such as Figure 9 in Frohlich et al., 2014) as intermediate results, but did no include them in the manuscript. To keep the balance between text and figures in the manuscript we have included one of those figures in the supplementary material. That figure clearly illustrates the poor fit to shallow depths, which is the main result of this manuscript.

Next question is why only one station was used, since there are two of them at local distances. The analysis should be shown with both.

The analysis was done with the closest station ALCN (25 km from CASTOR) because it was the only station in which the S wave reverberations were observed. The waveforms recorded at the second closest station, ALCX (40 km from CASTOR) did not exhibit clear reverberations (this could be caused by differences in seismic structure, attenuation of high frequencies, or other reasons) and therefore we did not try to match reverberations for that station.

6. Minor comments: L. 76: quantify "low frequencies"

We have modified the sentence to include the actual value of the frequency.

Fig. 1: figure misses axis labels

We do not know what the reviewer is exactly referring to. Both maps have latitude/longitude labels. In any case, we have increased the font size of the labels for better visibility.

Fig. 4: plots (or labels) should be enlarged, as labels are too small to be readable

To increase the visibility of the symbols and labels, and to address a comment by reviewer 1, this figure has been split in two, and the panels made larger.

Fig. 5: improve figure quality, it seems inadequate for the journal. There are no axes nor labels in plot c. If you add (too small) numbers in panel (a), they should refer to some events in the Figure or its caption.

We have increased the size of all panels and also have added axes with coordinates labeled in Figure 5c. Numbers in 5a above each beach ball corresponds to the entry of that event in Table 3. This is indicated in the caption, but since the caption is very long the reviewer might have overlooked it.

Fig. 6 should show ALCN and ALCX

We have previously explained why the crustal reverberation analysis was only done in ALCN.

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2019-113/se-2019-113-AC2-supplement.pdf

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