Earthquake static stress transfer in the 2013 Valencia Gulf (Spain) seismic sequence

L. Salo et al.

AR = Authors' Response
AC = Authors' changes being made in the manuscript

---

1 General comment

"I think that the problem raised in the manuscript is potentially of broad interest for SE readers and the scientific community, and should be considered for publication. However, reading this manuscript I am confused about that if it is at all possible in the form presented here. First of all the aim of the work is not clearly presented and the final conclusions are also not stressed and strong enough. Authors admit that future studies are needed when additional data will be available. But my main concerns is the proposed methodology to check the contribution of the mapped faults in the analyzed seismic sequence in the Valencia Gulf. I understand the first component of work to consider possible cause of interactions among seismic events as static stress transfer. Authors focus on the cumulative changes in stress due to the consecutive seismic events in the analyzed series. The cumulative stress changes are calculated after the occurrence of each event according to location and faulting type of the next event in the series. Although for this part I have some comments which I provide below, I think this part after improvements would be ready for publishing."
(AR) After both reviews, we as authors acknowledge the need to restyle the whole manuscript in order to present our method, assumptions and results more clearly. In addition, we will do our best to be concise and stress out our conclusions. The identification of the first goal of the study is correct (we aim at quantifying ΔCS after the occurrence of each event; in exception of the first event, cumulative stress changes are presented).
(AC) We are restyling the manuscript to both present a clearer approach to the methodology used and assumptions made, and also results and discussion. In the introduction, the goals of the analysis will be clearly indicated. We believe in our analysis to provide new information of interest regarding the 2013 Valencia Gulf seismic sequence, and thus are confident of its value; after an overall restructuring, clearer writing and improved approach to specific sections, we expect it to have the quality needed to be submitted for a final evaluation in this journal.

"The problem is with the two other goals, if I identified them properly: the contribution of mapped faults in the seismic sequence and the contribution of static stress changes in the seismic cycle of these faults. If the Authors assumed to consider their own Focal Mechanisms (FM) and depths of events (from 3 to 11 km) how is the sense to resolve the stresses of these events from the depth of these events on the mapped faults planes at the depth of these faults and at the same time hypothetically expecting that maybe these mapped faults contribute in the slip of the whole sequence. In my opinion if they could contribute they first should correlate with the parameters of the following seismic event in the sequence and second, if the range of the depths of events in the sequence is consistent and similar FM of events are as we see in Table B1, mapped faults had to experience the Coulomb Failure Function changes from the events at similar depths as they are. We know that the depth factor plays very important role in the CFF changes (DCFF) analyses. The consistency of the depth of events in the sequence is easy to be proven by the normalized signal cross-correlation (e.g. Schaff P. and Waldhauser F., 2005). Looking at the Table B1 one can notice that the FM of events are not so different to each other.

Based on the idea that signals of events with close hypocenters and similar FM recorded on the same station are very similar, the signals cross- correlation analysis may indicate the possible differences in recorded signals either due to events' different depths or focal mechanisms. Moreover, this analysis may reveal some highly correlated pairs within events group. Did the Authors perform such kind of analysis?"

(AR) The two other goals (second and third) are correct. There is one clarification which is worth noting, although it may be unnecessary here; previously mapped faults DO NOT slip in our modeling. Due to the fact that most of the seismicity seems to have taken place in between 5 and 8 km of depth, with both latest locations [1], and this study's best FM solutions for each event being consistent in this sense, we use FM results to place the sources. However, taking into account that one event is located at 3 km of depth and 2 others are quite close (resolved at 5 km), and also that owing to injections shallower locations should be expected, we investigate the cumulative stress changes along the whole sequence in the previously mapped faults as well to assess whether there is evidence that could support slip on these previously mapped structures. This does not mean we believe they did actually slip (we agree with conclusions by [1]), but rather, that this possibility should not be completely discarded yet. From our standpoint, if some FM solutions are close enough in depth and in geometry to cause relevant negative ΔCS onto one (or more) of the previously mapped structures, the simplest assumption is to think that the event could have occurred on the mapped fault. We take this position based on location uncertainty due to available seismic network distribution, and different depths at which FM solutions have been resolved (compare depths reported by [2] and the later analysis by [1]). We believe this assumption to be compatible with the usage of FM solutions to place the sources for our modeling, which we believe to be deeper than the previously mapped faults.

Regarding the Signals Cross Correlation (SCC) analysis, it was not performed here. FM solutions were chosen for each event after varying input depth and selected waveforms, as described in the draft, and the highest-confidence solution was chosen for each event. Hence, we believe the presented FM solutions to be reliable enough as they are. Moreover, SCC was performed by [1], who found solution depths very similar to this study (For example, they place two of the M $>$ 4 events at 6 km of depth and another one at about 10, which is almost the same we obtained in the presented FM solutions). SCC in their analysis allowed them to reduce scatter in hypocentral locations; their improved locations show almost all seismicity in between 3 and 10 km of depth, with most of it occurring at depth = 6 km. Thus, depths of the located events and previously mapped structures could not be linked.

(AC) Even if we do not rule out the possibility that some of the previously mapped faults could have slipped based on resolved ΔCS, the fact that the previously mapped faults experienced the Coulomb Failure Function changes from the events at similar depths (as they are placed from FM solutions) is also possible. We agree that this should be better clarified in the discussion. Moreover, it will be stressed that even though the remote possibility of slip on one of the previously mapped structures exists, our study globally supports deeper fault sources. A cross-section in which *FM* and *previously mapped* faults all appear will be included to highlight depth differences.

"The same problem I see with the cumulative CFF changes impact on the seismic cycle of the mapped faults. If the mapped faults experienced CFF changes due to events on shallower depths the values of CFF changes would be quite different."

(AR) We agree that the values of ΔCS would be quite different, should the mapped faults have experienced stress transfer from shallower events. But, as commented before, we consider our FM solutions to be reliable at the placed depth. We expect the 8 main events to be representative of, at least, the strongest phase of the sequence (which started after injections had been halted). These events occurred a week or two later than the injections halt and this second phase has been acknowledged to be different in nature than the first one (e.g. [2]). No FM solutions are available for events of smaller magnitudes at the moment. Thus, it should be reasonable to expect that stress perturbations due to smaller events at similar depths would not change computed values onto the previously mapped faults enough for the conclusions drawn from the performed analysis to be significantly different. Believing in this hypothesis, we performed the calculations to obtain the shortening of the seismic cycle onto the Main Fault plane.

(AC) We are ensuring the hypotheses and limitations of the performed approach are clearly written.

2 Detailed comments

"1. Part of the results presented in the manuscript is based on the assumption of characteristic earthquake phenomenon. Could the authors provide the justification of such approach?"
(AR) In our analysis, we investigate the stress state as resolved onto a particular fault plane, and the shortening in its seismic cycle as a result of computed ΔCS. The characteristic earthquake theory implies that earthquake occurrences on single faults and fault segments do not follow the Gutenberg-Richter relationship. This hypothesis was initially bolstered by paleoseismicity data; later, other works based on seismological and geological observations reinforced it (discussion is provided in [3, 4]). This hypothesis has been previously used in studies addressing the effect of ΔCS on earthquake recurrence times, both due to large earthquakes and also in anthropogenic seismicity assessment. In addition, previous works in the Valencia Gulf used this approach as well ([5, 6]; we apologize for these references being in Catalan and Spanish respectively).
We believe it to be the most logical hypothesis to be done in this case so as to be consistent with previous works conducted in this area and in this context.
(AC) We are ensuring that our approach is properly justified in the section dedicated to this part's methodology, which will be longer than this reply and supported by references.

"2. Paragraph 2.3.1. The provided description of the web-service is to detailed and unrequired in the comparison to the other sections of manuscript."
(AC) We will take it into account and modify it.

"3. Paragraph 2.3.2. I do not see the justification of the implemented approach to select the slipped nodal plane. As Authors stressed several times in the paper, the stress changes that are the cause of seismic events are also due to other factors than only DCFF. Seismicity accompanying technological activity results from changes in the stress field in the rock mass mainly due to this activity. If the rock mass are in highly pre-stressed conditions, even small stress perturbations can cause seismic events. Thus, it is not excluded that the plane in such sequence cannot experience the negative DCFF. Here the pore pressure changes modelling is not taken into account. The best would be to investigate all the possible plane scenarios and then to provide statistical based conclusions."
(AR) This is a critical issue and we are grateful for your comment. This is perhaps one of the most challenging parts in this study, and we have taken into consideration the following:

- **a.** The first option that is always explored is comparing both conjugate planes with previously mapped structures to see if one can be discarded. This is hardly feasible here due to 1) the fact that similar structures compatible with strikes of both Nodal Planes (NP) are present and 2) depth difference for most of the solutions.
- **b.** Another option that has been used in studies of anthropogenic seismicity [7], is using the hypocenter distribution of adjacent events to choose the causative nodal plane "by eye". Here, seismicity cloud density around each FM solution and location uncertainty make it inadvisable to use this approach.
- **c.** A possible alternative is using background stress to determine Optimally Oriented Fault Planes (OOFP) and then chose the NP, for each pair, that is closer to OOFPs. It was first discarded after a rough look of stress orientations based on which both conjugate planes for each FM were similarly oriented. However, we are currently quantifying it and this will be used in our revised selection of nodal planes.
- **d.** Under the assumption of events triggered by fluid overpressures, the Critical Pore Pressure (CPP) criterion may be used to select the causative nodal plane (e.g [8]). We, as authors, are not favorable to using this alternative on its own, due to the fact that almost no information regarding reached overpressures has been made public (using this method for

NP selection implies assuming that the events took place as a result of fluid overpressures). In spite of that, an evaluation of the selected NP using this criterion is likely to be included in our revised version.

e. Our hypothesis in the discussion manuscript involved considering earthquake static stress as a destabilizing trigger; thus, the selection of each NP was made after a test-and-error analysis in which both alternatives for each FM of a particular event at time x were explored, and the one with higher ΔCS as a result of all events with FM solution up to time x*-1* was selected.

(AC) We agree with your comment regarding the consideration of other present factors that may have played a role in the activation of fault planes. Because of that, alternative ***e.*** should be contrasted with other options so as to provide a broader range of plane scenarios. We are going to include information from at least ***c.*** in our revised version.

"4. Distinguish between the two Paragraphs 2.3.2 and 2.3.3 is misleading since receiver faults are also source faults. Moreover, the nomenclature used to determine faults is also misleading because we have source, receiver, mapped and finally hosting faults. Could the Authors think over this issue how to simplify the information for easier understanding the contents."
(AR) We chose "source" and "receiver" terms based just on whether the fault slipped or not in our modeling. Thus, selected NP from FM solutions were named "source" and the previously mapped structures in the area "receiver".
(AC) We agree that, even with the aforementioned terms being chosen with our best intentions, nomenclature should be rethought. This is specially due to the fact that both FM faults and previously mapped structures receive stress. We are going to name *FM-derived* or just *FM* faults the fault planes selected from FM information, and *previously mapped* or just *mapped* faults will be used instead of receiver. It will be noted that both receive stress, and that only *FM* faults slip in the performed modeling. We are considering to include a table to better note this distinction.

" 5. Paragraph 2.4. Authors consider the particular impact of the input parameter uncertainties on the results. But the most relevant is the combined approach which may be achieved using synthetic samples and then statistical inference. Even if we focus on the sole effect due to particular parameter uncertainties there is no information how the calculations were performed. How many synthetic values was considered, or maybe only the worse and the best scenario. Fig. 12 presenting the results of this step of analysis could have a scale of DCFF more adopted to the range of the experienced distribution of DCFF."
(AR) The final stress state on the previously mapped faults was first computed by using a *best estimation* of the parameters. This corresponds to geometry (strike, dip and rake) and depth as given by the selected NP out of each FM, and μ (effective friction coefficient) of 0.4. Afterwards, variations in the geometrical parameters and depth of each NP were carried out to complete the sensitivity analysis. The strike was varied up to ±15° (this should be corrected in table C1, our apologies) in 5° increments (e.g. NP strike, NP strike + 5°, NP strike + 10°, NP strike + 15°, NP strike -5° and so on), the dip up to ±10° in 10° increments, the rake up to ±20° in 10° increments, and final result with μ 0.2 and 0.6 was also computed.
Depth of all FM solutions was also switched 1, 2 and 3 km upwards, until the shallowest solution reached "surface depth". This part focused on the observed variations in the Main Fault. In spite of the fact that computed values onto particular patches augmented (distances were minor), the total area (total number of patches) of the Main Fault subject to positive ΔCS did not remarkably change.
Our analysis is focused on the "best estimate" of the parameters. The sensitivity analysis was performed as a complementary tool to investigate sole effects due to particular parameter uncertainties, as indicated in your comment. Each variation was introduced on its own and the final stress state was computed just for each variation (e.g. 7 different calculations were performed regarding the strike). After each calculation step, the maximum, minimum and average (computed from all patches of a particular mapped fault) values of ΔCS were obtained, and this is how Fig. 12 was plotted. In this case, the selected variations were chosen "by logic" and

not owing to any specific calculation or reference. We aimed at maintaining the nature of each solution. As indicated in our response to RC1, we varied the rake ±20° instead of ±10° because we believe its value to be less well constrained. We consider a variation of ±20° for the strike and dip to be too large (e.g. a fault that is reported to dip 50° would vary from 30°, which is a gentle dip, to 70°, not far from being sub-vertical).
(AC) We will revise our sensitivity analysis and probably include the same range of variation for all geometric parameters (in this case ±15° will be used for strike, dip and rake). We agree that a further study of FM depth and its specific effect onto each mapped fault would be interesting but believe it to be out of scope in this study (which considers a best estimation of the parameters), as would be a complete probabilistic analysis.
We are not sure about changing the scale of ΔCS in Fig. 12 due to the fact that it was already adopted to ease the comparison of the experienced distribution of values, but we will consider it.

"6. Information from the Paragraph 3 should be incorporated into other Paragraphs."
(AC) Proceeding as indicated.

"7. Paragraph 4.1. Line 20. Indeed an empirical threshold for triggered natural seismicity of 0.01 MPa is usually used (e.g. Reasenberg & Simpson 1992; King et al. 1994). While many studies suggest that triggering requires a minimum stress change, the variation in this threshold spans an order of magnitude or more. In mining induced seismicity the minimum Coulomb stress change that influences the occurrence of future seismicity was 0.005 MPa; this triggering threshold was confirmed to be statistically significant (Orlecka-Sikora, 2009). Another example from mining induced seismicity in Deep Gold Mine in South Africa suggests also that seismic activity was triggered by mainshock in the areas where static stress increased not more than 0.01 MPa (Kozlowska et al., 2015). Ziv and Rubin (2000) and Ogata (2005), however, point out that triggering is not a threshold process. Hardebeck et al. (1998) suggest that any small stress change is capable of triggering and the existence of an apparent minimum triggering stress is connected with the sufficient number of triggered events to be detectable with dataset used."
(AR) We certainly appreciate this comment, as well as the following one. We will revise the references provided to foster our discussion regarding this part.

"8. Paragraph 5. Line 10-13. The papers e.g. Orlecka-Sikora, 2009; Kozlowska et al., 2016, open ICHESE Report describing the May-June 2012 Emilia sequence case provide the results of analysis of impact of the cumulative DCFF on the following events in the considered sequences."
(AR) We will study the results obtained from the analysis of the impact of cumulative ΔCS in the events that follow in a sequence.
(AC) Comments regarding the references provided will be made in the discussion (section 5 in the manuscript).

"9. What about the distribution of smaller seismic events? How they are distributed according to the DCFF due to stronger events and particular FM and mapped faults. The smaller events distribution may provide additional insights into the location of stronger events and their slipped plans."
(AR) As indicated in the response to comment 3, it is true that the smaller events distribution may provide useful information regarding the selection of the NP. However, the problem in this case is that location uncertainty for smaller events, which are logically much more numerous, is too high to use for this purpose. The general cloud of events seems to be distributed NW-SE, but no particular alignment of seismicity that could be used for this purpose (high density of events in a constrained area around the FM placements) has been reported either (see [1]).
(AC) We are accounting for the distribution of smaller events when it comes to the general interpretation of the results, but not for NP selection.

"10. The quality of the Figures 8-10 is not satisfied. The DCFF scale is missed on Fig. 10."
(AC) Quality of the mentioned figures will be upgraded, and scale in Fig. 10 included for all subplots.

"11. Paragraph 2.1. Line 23: an homogenous -> a homogenous; Paragraph 2.3.1. Line 2: solution consists on inverting -> solution consists in inverting; Line 3: whose information is -> that are."

(AC) We will correct it and we will do our best to ensure that our English is easily readable and without mistakes.

References

[1] B Gaite et al. "Improving the location of induced earthquakes associated with an underground gas storage in the Gulf of Valencia (Spain)". In: *Physics of the Earth and Planetary Interiors* 254 (2016), pp. 46–59.

[2] S Cesca et al. "The 2013 September–October seismic sequence offshore Spain: a case of seismicity triggered by gas injection?" In: *Geophysical journal international* 198.2 (2014), pp. 941–953.

[3] SG Wesnousky et al. "Earthquake frequency distribution and the mechanics of faulting". In: *Journal of Geophysical Research: Solid Earth* 88.B11 (1983), pp. 9331–9340.

[4] DP Schwartz and KJ Coppersmith. "Fault behavior and characteristic earthquakes: Examples from the Wasatch and San Andreas fault zones". In: *Journal of Geophysical Research: Solid Earth* 89.B7 (1984), pp. 5681–5698.

[5] H Perea. "Falles actives i perillositat sísmica al marge nord-occidental del solc de Valencia". PhD thesis. Universitat de Barcelona, Barcelona: 382 p.(http://hdl. handle. net/10803/1919), 2006.

[6] JA Álvarez Gómez and F Martín González. "Una aproximación multidisciplinar al estudio de las fallas activas, los terremotos y el riesgo sísmico: 2ª Reunión Ibérica sobre Fallas Activas y Paleosismología: celebrada del 22 al 24 de octubre de 2014, en Lorca". In: (2014).

[7] Y Mukuhira et al. "Pore pressure behavior at the shut-in phase and causality of large induced seismicity at Basel, Switzerland". In: *Journal of Geophysical Research: Solid Earth* (2016).

[8] Y Mukuhira et al. "Characteristics of large-magnitude microseismic events recorded during and after stimulation of a geothermal reservoir at Basel, Switzerland". In: *Geothermics* 45 (2013), pp. 1–17.