Review of the paper "Earthquake static stress transfer in the 2013 Valencia Gulf (Spain) seismic sequence" by L. Salo et al. submitted to Solid Earth

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. 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? 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.
Now more 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?

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.
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.
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.
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.
6. Information from the Paragraph 3 should be incorporated into other Paragraphs.
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.
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.

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.
10. The quality of the Figures 8-10 is not satisfied. The DCFF scale is missed on Fig. 10.
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.

References:

Hardebeck, J.L., Nazareth, J.J.&Hauksson, E. 1998. The static stress change triggering model: constraints from two southern California aftershock sequences, *J. geophys. Res.,* **103,** B10, 24 427–24 437.

International Commission On Hydrocarbon Exploration And Seismicity In The Emilia Region (ICHESE), Report on the hydrocarbon exploration and seismicity in Emilia Region, (2014, http://unmig.sviluppoeconomico.gov.it/unmig/agenda/dettaglionotizia.asp?id=175).

King, G.C.P., Stein, R.S. & Lin, J., 1994. Static stress changes and the triggering of earthquakes, Bull. seism. Soc. Am., 84, 935–953.

Kozłowska, M., Orlecka-Sikora, B., Kwiatek, G., Boettcher, M. S., Dresen, G. (2015) Nano- and picoseismicity rate changes from static stress triggering caused by a MW2.2 earthquake in Mponeng gold mine, South Africa, *J. Geophys. Res. Solid Earth*, 120, doi:10.1002/2014JB011410.

Kozłowska, M., Orlecka-Sikora, B., Rudziński, Ł., Cielesta, S., Mutke, G. (2016) Atypical evolution of seismicity patterns resulting from the coupled natural, human-induced and coseismic stresses in a longwall coal mining environment, *International Journal of Rock Mechanics and Mining Sciences* 86, 5-15, doi: 10.1016/j.ijrmms.2016.03.024.

Ogata, Y. (2005), Detection of anomalous seismicity as a stress change sensor, J. Geophys. Res., 110, B05S06, doi:10.1029/2004JB003245.

Orlecka-Sikora, B. (2010) The role of static stress transfer in mining induced seismic events occurrence, a case study of the Rudna mine in the Legnica-Glogow Copper District in Poland, Geophys. J. Int., 182, 1087–1095

Reasenberg, P.A.&Simpson, R.W., 1992. Response of regional seismicity to the static stress change produced by the Loma Prieta earthquake, Science, 255, 1687–1690.

Schaff P. and Waldhauser F. (2005). Waveform Cross – Correlation – Based Differential Travel – Time Measurements at the Northern California Seismic Network, Bull. Seismol. Soc. Am. 95, no.6, pp 2446-2461, doi: 10.1785/0120040221.

Steacy, S., J. Gomberg, and M. Cocco (2005), Introduction to special section: Stress transfer, earthquake triggering, and time-dependent seismic hazard, J. Geophys. Res., 110, B05S01, doi:10.1029/2005JB003692

Ziv, A., and A. M. Rubin (2000), Static stress transfer and earthquake triggering; no lower threshold in sight?, J. Geophys. Res., 105, 13,631– 13,642.