Earthquake static stress transfer in the 2013 Valencia Gulf (Spain) seismic sequence
by L. Salo et al.

Submitted to Solid Earth

General comments

In its current form the draft reads not very well and its major points are not clear and convincing enough. It needs a major revision to be worth publication. First of all, I think that this manuscript could significatively benefit from an overall restyling. I recommend a general and thourough improvement of the English (see *Technical issues*) and a more organized, concise and focused presentation of the method and results. The authors should be clerer in presenting and justifying their initial assumptions and stress out their goals (which is the final message and why is it significant in this context?). In this study the authors should bring into focus with more enphasis the specific analysis and result interpretation, which represents the innovative part of the study. Their results should be significative and presented in a convincing way. The methodology and the numerical code used for the static stress estimatimation are indeed already quite well known in the literature and do not represent novel tools.
The authors are studying the role of static stress redistribution due to earthquake mutual interactions during a seismic sequence that is probably induced by the injection of cushion gas for gas storage. In their analysis they are ignoring any influence that could come (and would probably be significant) from the triggering effect of pressure changes caused by the injection. This is a limiting but possible assumption if the focus of the study is on the role of earthquake interactions only (e.g. *Baisch et al*. 2009; *Schoenball et al*. 2012; *Catalli et al*. 2013). However, the authors have to stress out more clearly their specific goal and strong assumption from the beginning and touch the general issue of induced seismicity to give an overview on all the possible triggering mechanisms involved in such a case. The feeling after reading the text is that they comment their assumption of ignoring any other triggering effect during the final sections Discussions and Conclusions and it sounds like the study is incomplete due to shortage of data more than having a different initial aim.

Specific comments

I think that in this study there is an important confusing point: the calculation of ΔCFS made both on the *source fault planes* (section 4.1) and on the *mapped fault planes* (section 4.2). Is this distinction worthy and why? I would avoid to say *source faults* when they are used thereafter also as receivers. Additionally, it is not clear which are the source planes used for estimating ΔCFS on the mapped faults; are also the mapped faults contributing to the cumulative ΔCFS as sources? The authors should be clearer in explaining which are the sources/receivers for each different case they present and list all relative information, as for example in Table B1. It is very difficult to interpret Figure 11 with current information; in particular, one does not easily know the time evolution of ruptures relative to the faults or fault's patches. Did for example the East 4 fault slipped at the beginning or in the middle of the sequence, or maybe at the end? Which are the sources contributing to the cumulative ΔCFS estimated on top of the East 4 fault? How does ΔCFS on this specific fault evolve with time? This would help undertsanding its negative ΔCFS.

Another crucial problem regards the interpretation of results in terms of ΔCFS: the authors conclude that the fault named East 4, which shows a negative cumulative ΔCFS at the end of the sequence (Figure 11), is therefore the most likely to have slipped. However, another possible interpretation could be that the influence in terms of cumulative ΔCFS of all the previous events on the fault East 4 is negative, i.e. the fault is not favored to slip by the cumulative stress redistribution. The stress drop on the fault might indeed have been caused also by its own slipping phase, but for a better understanding of this point one needs to know the fault's stress state before and after the time of its slipping in relation with the other events in the sequence (time evolution). In other words, ΔCFS on the East 4 fault is negative before or after its own rupture? This is also not clear from Figure 10. All these aspects should be clarified for a proper interpretation of results. Moreover, the fault is only partially visible in Figure 11.

Referring to Figure 12, how are the best estimations of the parameters caculated/assumed? Why do the authors limit the analysis to just four parameters (strike, dip, rake and apparent friction)? Why do the authors perform this analysis referring to the mapped fault and not also the the so called source faults? The table that describe free parameter variations (now Table C1, which describes all parameters involved in the methodology) should be a dedicated table for the sensitivity analysis. Why do the authors vary the strike of the sources and not of the receivers? I think both have to be randomly perturbed within 20 degrees for understanding the sensitivity to the strike angle (the same for dip and rake). They might reproduce this way a large number of realizations for a more probabilistic based sensitivity analysis. The same concept would apply also for the nodal plane, depth and friction coefficient.

Can the authors explain why *the fact that the mean value in the East 4 fault is near -0.1 bar, supports the idea of this structure to have slipped* (section 4.3.1 lines 22-24)?

The final message about static stress transfer as triggering mechanism is not enough convincing (section 5.1): what is the take home message? The fact that the static stress acted only as a partial triggering factor is not a strong finding itself. The conclusions are dispersive.

The three first lines of Conclusions are confusing: for the East 4 fault a negative ΔCFS meant possible slip (see before), while here a positive ΔCFS resolved onto 7 of the 8 FM means possible destabilization. It looks like the authors are changing their point of view of the same process. They should be clearer and more consequential.

Technical issues

Some examples of intricate, hard to understand sentences: p.2, lines 16-16; 22-23; 27-29 (here the authors seem to justify the fact that they use FM solutions as sources of Coulomb stress change. Why do they need it? This is a very common assumption in the literature); p.3, lines 18-19 (explain which error is minimized and how). P.4, lines 4-5: reduction of what? try to be clearer; lines 11-13: explain better this concept of conservative or non-conservative assumption and which of the two you are following. P. 5, lines 3-4.

Section 2 is probably too long and confused. I would suggest to respect the order resources/available data - methodology description – Coulomb model assumptions. I suggest to create a section on the methodology (Coulomb and seismic cycle) and one on the model assumptions (sources and receivers). The section about uncertainties should be independent and self-consistent. I think that the factors of uncertainty are several and regard both the methodologies involved in this study (the Coulomb stress estimation and the impact on the recurrence time). The authors should list them clearly and explain why do they analyse only some of them and how.

Section 2.1: the authors allude to *various criteria to determine failure conditions* and then they

do not mention any of them. In general, if one refers to an issue, then one should at least spend a few words on that or omit it at all. Equation 1: why do the authors use the primes for *c* and σ? The simpler notation, the better. Later in equation 3 the prime disappears for $\Delta\sigma$ and appears (correctly) for μ (but this causes confusion for the reader). ΔCFF is defined but ΔCS (line 7) is not. More generally on this section, it looks like that the authors are jumping from a formula to another for describing the concept of Coulomb failure criterion but they forget to give to the reader some simple elements for understanding a quite intuitive concept. If they want to start from the Mohr-Coulomb theory, they could then show a figure with a general description of the Mohr-Coulomb diagram. This would also be a good expedient for describing the possible effect (even though neglected in this study) of a pressure increase.
The authors should be more careful in explaining the precise conditions under which equation 4 is valid, i.e. the pressure effect is taken into account only under the undrained condition (no fluid flow is considered) and the solely contribution to the pressure is given by the compressional terms of the stress tensor. They should remind that the pressure change due to the injection is still completely neglected in equations 3 and 4 and in general in their study.
Why do the authors think that working with the software Coulomb 3.3 allows to deal with tridimensional complexity and not with other numerical models of the Coulomb stress redistribution? How do they justify their choice of calculating the stress change at 11 km depth (line 22, p.3)? From table B1 one can see that the 8 major events used as sources/receivers are all shallower than 5.9 km with an ecception of an event at 8.9 km. Additionally, Figure 5b does not help making a clear picture of the depths of the receiver faults. The depth of calculation of ΔCFS plays a significative role and should be discussed with greater attention.

Section 2.2: I would add some more explanations and comments referring to equations 5 and 6. Both equations imply the assumption of the concept of the characteristic earthquake. This is a strong assumption and it is worth some more discussion. Equation 5 implies that the average stress drop is released from a characterstic earthqake only, so that $\Delta\sigma/T_r$ corresponds with the regional stressing rate; all other possible phenomena of stress release are ignored. Equation 6 on the other hand implies the existence of a characteristic length for the source of a characteristic earthquake. I think that the authors should briefly comment on these hypotheses (why do they think these hypotheses are realistic in their context? What are the limitations?)
In general, I find that sometime in this draft the references to other studies for justifying or explaining some assumptions are ambigous. For example, equation 5, p. 4: neither in *Harris* (2000) nor in *Baysch* et al. (2009) I can explicitly find any reference to this equation but solely hints to the idea that a stress change can affect the recurrence time of large earthquakes. I would find a reference as for example to the study of *Parsons* (2005) and references therein much more focused on the problem.
The *Main Fault* cited for the first time in correspondance of equation 9 and then in Figure 9 should be clearly defined. Please also define *SR*.

Section 2.3.1 can be merged with Data and Resources.

Section 2.3.1: rather than giving information on details about the input/output data of the method FMNEAR used for FM estimations, I would focus on the approach itself and the reliability of the method. CFS estimations are indeed very sensitive to the FM solutions used for sources and receivers and their relative hypocentral distance. Why do the authors decided to use FM solutions obtained via FMNEAR? Why did they not use solutions already published in the cited literature (*Frontera et al*. (2013); *ING* (2013) and *Cesca et al*. (2014)? How sensisitive are their results to the different solutions?

Section 2.3.2: in the first paragraph of this section the English and wording needs a substantial improvement. The method for selecting the nodal plane is unclear. However, I do not agree/do not understand the idea that the nodal plane is selected referring to the highest ΔCFS. The authors need to justify their assumption. An alternative idea could be to quantify the difference when using a different nodal plane solution for each event or by using random extrapolated nodal plane solutions for a more probabilistic approach. P.5 from line 32: are the authors explaining how do they estimate the slip of each event? Does it come from the magnitude? It is unclear.

Section 2.3.3: what I would need to understand clearly here is on which receiver planes do the authors estimate ΔCFS and why. From this paragraph one thinks that the FM solutions are used as sources and the geological faults as receivers. However, already in the abstract the authors say that *:"…the evolution of static stress is quantified both on fault planes derived from focal mechanism solutions…and on the previously mapped structures…"*. The issue is touched then again in section 4. I think that the authors need to be more concise and precise on this assumption from the beginning and explain the reason of their assumptions.

Section 2.4: this section is not complete. A reader needs to understand which are the parameters at play; which of these parameters influence uncertainties the most; how do the authors perform a comprehensive sensitivity analysis and major findings. To be more concise, tha authors may merge section 2.4 directly into section 4.3.

Section 4.4: first line: the poor reader needs to check the two tables ahead and equation 5 behind to understand what you are discussing about. The tables can be merged together. The case *Best* should be better defined and justified. The tables are not easily comprehensible. Where are *the results for the best estimate* plotted? There is no reference to a Figure.

Section 5.2, the first lines are uncomprehensible.

In Figure 1 would be interesting to see also the injection rate.

Figure 4 can be merged into panel c of Figure 1.

Figures 5 and 11 can be merged together into an unique Figure.

Figure 7 is not clear and one cannot follow the relative comments reported in section 4.1. Projecting ΔCFS on the FMs can just give a rough idea of the stress change (positive/negative) and information on the spatial distribution is lost. Why do the authors decided for this kind of presentation? Which is the message they want to give through this figure?

Figure 8: which are the events causing a ΔCFS on the FM of the event occurred the 09/24?

Figure 11: report in the figure, close to the corresponding patch, the identifying letter+number already reported in Figure 10.

Tables B1 and C1 are fundamental for the interpretation of the methodology and results, why are there in the appendix?

References

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Catalli, F., M.-A. Meier, and S. Wiemer (2013), The role of Coulomb stress changes for injection-induced seismicity: The Basel enhanced geothermal system, Geophys. Res. Lett., 40, 72–77, doi:10.1029/2012GL054147.

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Schoenball, M., C. Baujard, T. Kohl, and L. Dorbath (2012), The role of triggering by static stress transfer during geothermal reservoir stimula- tion, J. Geophys. Res., 117, B09307, doi:10.1029/2012JB009304.