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Interactive comment on "Active tectonic field for CO_2 Storage management: Hontomín onshore study-case (SPAIN)" *by* Raúl Pérez-López et al.

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Letter to the Anonymous referee #2 related to the interactive comment on "Active tectonic field for CO2 storage management: Hontomín onshore study-case (SPAIN)" by Pérez-López et al.

The authors (3 February 2020)

First of all, thank you very much for your time and your effort, and for the constructive review of our work. We really feel that an open discussion improves the scientific results and we are willing to deal with it. Having said that, we answer all the points kindly provided by the Anonymous Referee #2 (hereafter AR2), in the same order he did.

We didn't analyze the past and present "stress" fields, we have analyzed the past and present "strain" fields. Although the orientations of both fields are similar, it is important to highlight the difference in terminology between stress and strain and that we have calculated strains. The Right Dihedral based on fault slip data yields "strain" and "paleostrain" fields. It is assumed that in the general case for the Anderson fault model, ey (the trajectory of maximum strain) is parallel to SHmax (maximum horizontal shortening) being different terms although (e.g. Giner-Robles et al. 2009).

Moreover, our results do not evaluate "the risk" for leakage of CO2 injection neither the possibility of triggering an M5 earthquake as AR2 suggests. RISK is a term that includes HAZARD plus VULNERABILITY plus EXPOSITION, and in our manuscript, we didn't calculate that. In our conclusions, we expose the fault sets and orientation according to the present-day stress orientation which is affecting the caprock and the vicinity. We also describe a low-cost methodology to obtain paleostrain data, fault sets and their relationship with the present-day stress field, and we suggest including this kind of low-cost analysis for long-term geological CO2 storage (GSC).

On the other hand, for estimating the potential seismic triggering of active faults, more analyses have to be carried out, for example, the study of active faulting by tectonic geomorphology and paleoseismology (see McCalpin 1996 and Papanikolaou et al. 2016, for instance). Therefore, the risk assessment for leakage of CO2 is out of the scope of our manuscript. Comment 1a. AR2 said that the analysis we made is common in monitoring strategies for GSC management. We don't know any work about "strain inversion techniques" applied in GSC management so far. All the references suggested by AR2 are devoted to induced seismicity and the tectonic role in GSC management. We do not deny that and we do not claim that we are pioneers in induced seismicity and tectonic studies on GSC. We simply present how the Structural Analysis of fault/slip data can improve the knowledge of the tectonic large-scale fault network for the potential seismic reactivation during fluid injection and time-depend scale for fluid stays. Perhaps we failed to explain more clear our results. We will revise the manuscript to check this point.

Comment 1b. In this point, we agree with AR2 regarding the terminology about Carbon Capture and Storage (CCS), geological storage even Carbon sequestration (discarded terminology that we have used in the conclusion section). Hence, we will use only one. We agree with the term of geological CO2 storage (GSC) and therefore, we have changed CCS (carbon capture and storage, and other some former term which was included in the manuscript and removed for the revised version) for the term GSC.

Comment 1c. Once again, the “risk assessment” was not performed in our work. This confusion is relevant to solve, it could lead to major mistakes for managing GSC. We insist that we have not carried out any kind of “risk analysis”.

On the other hand, the expression “extremely unlike” used by AR2 to describe the potential of triggering M5 earthquakes by active faults near the GSC is not relevant to determine the real seismic hazard. There is no formal reference to this sentence and therefore we have ignored it. We have applied a physic model by the estimation of the total volume injected (yes, we did it in room conditions), from the official data (referenced in the manuscript), and then we have applied the McGarr’s (2014) approximation. Taking into account the uncertainties from this analysis, our analysis “should not be regarded as an absolute physical limit”, paraphrasing McGarr (2014)’ words (page 1, ending sentence of the abstract section). According to McGarr (2014), the utility of the analysis that we have performed is “to predict in advance of a planned injection whether there will be induced seismicity”, and in the case of the Hontomín Pilot Plant, by the estimation of the “total injected volume” in a small-scale injection plant (yes, the utility of a small injection plant, as pointed out by Cook et al., 2014). McGarr (2014) applied his approach for three cases: (1) wastewater injection, (2) hydraulic fracturing, and (3) geothermal injection. We have gone one step beyond by including geological storage of CO2. We assume that the pore pressure increases from CO2 injection, in a similar way that wastewater does (originally defined by Frohlich, 2012).

Regarding Lines 673 and 674: We agree with AR2 that we have calculated the volume in "room conditions" for estimating the total injected volume, but we disagree with AR2 that it is a wrong estimation. We have read the work from McGarr (2014), and we did not found a reference that the injected value for the McGarr equation (eq 13) should be in “reservoir conditions”. That means that our volume estimation was correct, and simply we have included "room conditions".

Concerning the calculation of the maximum seismic moment by the total injected fluid we did it as follows: We have used the expression Mo(max) (Nm) = G ÂůïĄĎV (McGarr 2014, eq. 13) Where G for the upper limit is 3 exp10 Pa and ïĄĎV is 5.56 exp3 m3 for the total injected volume (room conditions). The result is 1.65 exp14 Nm (Joules), which corresponds to Mmax = 6.025 of the maximum Richter magnitude by applying the equation Log E = 11.8 + 1.5ÂůM; where Log is the logarithm to the base 10, E is the seismic released energy in Joules, and M the Richter magnitude. We agree with AR2 that we have to include this calculation in the revised manuscript.

Respect to the Ubierna Fault System (UFS), we simply claim that it is necessary to know the seismic cycle of their active segments to know the possibility that small induced seismicity could trigger a natural earthquake by changing the strain conditions. Today, that question is not well-known but as a matter of fact, the study of the strain fields by inversion techniques and the relationships with the active faults in the vicinity is a good approach to get a realistic answer. Up to now, there is not a known model or work analyzing the seismic cycle of the UFS. Our statement about the paleostrain and present-day strain analysis is a suggestion for best practice in the long term geological storage, not a fact. Regarding the “seismic cycle” and missing references, we have included Scholz (2019), one of the most general explanations of a wide-used description of active faults and related references.

Comment 2. AR2 asked a statistical analysis to the fault dataset but the Fault Population analysis in Structural Geology is a statistical analysis itself, so we did it. Perhaps AR2 means a calculation of uncertainties from age outcrops associated with quality parameters measured on the field. In this sense, we do not find any quality differences between the faults striations measured on fault planes. Any field data with doubt or potential misinterpretation from the field data was directly obviated to avoid biased results. Only high-quality striations and fibers were measured (see annexed figure 1). Moreover, geological ages mapped in the outcrops are enough constrained to be homogenous for the paleostrain reconstructions.

Comment 3. Thank you very much for your time and your effort to improve our English style, although we are not pretty sure that all of the suggestions are correct. Therefore, your suggestions will be revised and included if so. As a non-native English speaker, we always use professional aid. Bearing in mind that the manuscript was written by three different authors and finally homogenized by the first one, the English style could oscillate in different parts of the text. Thanks indeed.

Minor comments in the supplementary doc: (Major questions):

Q1 (abstract). AR2 asks about the "tectonic parameters": They are those parameters which characterize the tectonic field and framework: stress/strain parameters like SHmax, ey, k', R, natural heat-flow, Moho depth, crustal thickness, etc. In this case, the stress/strain ellipsoid and strain trajectories, master faults, tectonic slip rates, were shown in our work.

Q2 (abstract). Did In Salah (Algeria) case analyze the "tectonic strain field"? Please give us a reference.

Q3 (abstract). The reviewer confuses stress with strain again. They could be related in space, but are different concepts, as we said before.

Q4 (page3). Ancient literature does not mean outdated literature. Please, indicate more appropriate literature if so related to the first stages of GSC and why Pearce 2006 is obsolete.

Q5 (Line74). What do you mean by long-term monitoring? We are meaning about the

expected life of the reservoir in geological terms.

Q6 (Line92). Geomechanical models related to the active tectonic field is not the aim of the paper and is out of the scope.

Q7. The election of 20 km for the strain analysis from geological outcrops is well explained. 3. Method and Rationale, section 3.5.

Q8 (Line110). Ok, removed.

Q9 (Line113). Why should we indicate that Hontomín is a pilot plant before the geological framework? We don't know any reason to prioritize this. Geology is the key for underground storage.

Q10 (Line211). Geomorphic markers (misfit).

Q11 (Fig. 5). Some outcrops are close to 20 km but not exactly. Well, despite we use a GPS in the field, some stations were slightly out of 20 km but quite close to have influence in the geological map. We checked if outcrops were located about 20 km but not exactly. This is another reason to include the outcrops in a geological map and showing that they are related to the structures inside the 20 km circle.

Q12 (Line449). We strongly disagree. The tectonic field is relevant and their expressions are the master faults, they accommodate all of the deformations that it generates. The tectonic field and master faults are not independent concepts.

Line592. Reference misprint (Alcalde et al. 2014.). Line631-640. One of the tectonic parameter to be considered is the crustal heat flow (see Q1). The relevance of this paragraph is to highlight that within intraplate areas large earthquakes could appear. Line643. Both strain fields. Line691. The use of focal mechanism solutions is crucial for understanding the failure mechanism in seismic prone areas. This information is world-wide used for the study of seismogenic faulting, modeling, earthquake hazard, seismic wave propagation, design of the seismic network, etc. Line693-696. Well, one thing is the permeability and lateral diffusion due to a single injection, and another thing is the mechanical behavior of the caprock under episodic injections.

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Fig. 1. Example of high-quality fibers on a Cretaceous fault plane measured in the station HTM3 (see manuscript for the geographical location). You can observe as the fibers lineation change with th