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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.

Anonymous Referee #2

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In their manuscript "Active tectonic field for CO2 storage management: Hontomín onshore study-case (Spain), Pérez-López et al employ fault orientations sampled in the surroundings of the Hontomín site to analyse the past and present stress field. Their results provide an indication of the risk of leakage of the injected CO2 in the reservoir due to tectonic activity or fault reactivation during the storage operations. This seems a reasonable approach, although some aspects of this appraisal are hard to assess with the data presented in the manuscript. I have a number of issues that need to be significantly improved before this paper can be considered up for publication. I outline the major issues here, and attach a detailed list of minor comments that should be addressed by the authors.

General comments 1) The authors should revise several aspects of the manuscript

related to the geological storage of CO2 (GSC), which constitute the ultimate goal of this study.

a. First, it is claimed that this type of studies are not conventionally implemented in monitoring strategies. I would argue the opposite, as the tectonic activity is one of the main factors that need to be considered in the site screening and selection processes according to well-cited best practice guides (e.g. IPCC, 2005; Chadwick et al., 2008; IEAGHG 2009). Fault reactivation and induced seismicity are also major concerns in storage operations, which is reflected in the vast body of literature available (e.g. Nicol et al., 2011; Zoback & Goerlick, 2012; Juanes et al., 2012; Vilarrasa and Carrera 2015; White and Foxall, 2016). The authors fail to put their work in perspective, and I actually suggest them to switch their rationale: these issues are so important that their study is imperative.

b. The terms CCS and CO2 storage are used indistinctively throughout the manuscript (e.g, lines 57, 84, 667), but they are different processes (the former encompassing the latter), and thus should not be confused.

c. I have major concerns about the risk assessment that is proposed in this study. First, the scale of the Hontomín facility is in my opinion overlooked. Hontomín is a pilot storage plant, and as such it was never conceived for the storage of large volumes of CO2. It is extremely unlikely that the injection of 10k tonnes of CO2 would produce any effects in the faults bounding the reservoir, not to mention the reactivation of the regional-scale Ubierna Fault. Also, the volume calculation presented in lines 673-674 is wrong, as it assumes room conditions for the CO2 (i.e. 556.2mˆ3/ton) instead of reservoir conditions. Besides, the authors cite McGarr's approach to calculate the Mmax of potential fault ruptures, but do not show any of the calculations or the assumptions that they make to claim that M >5 earthquakes are possible as a result of the injection (line 676). This is a very audacious statement (with which I strongly disagree), and as such it must be well documented and presented in the text. In summary, the authors fail to prove how the injection in a small reservoir (which is likely to be compartimentalised

from the UFF splay anyways) could produce a distortion so great to re-active the entire 6km long segment.

2) I think that the authors should include some sort of analysis of the statistical significance of fault measurements in their calculations. For example, some ages count with several outcrops (as well as greater areal distribution) and samples than others; how does this affect your paleostress calculations?

3) In terms of the quality of the writing, several portions of the test require a profound rewriting to improve readability. There are remarkable differences in the quality and clarity on some of the sections (for example, sections 3.3 and 3.4 read very well compared to sections 3 and 3.1). I have made suggestions or marked the sentences that particularly require a thorough rephrasing in the attached file, but I encourage the authors to review and polish the entire manuscript or look for potential professional aid in this sense.

References: Chadwick, A., Arts, R., Bernstone, C., May, F., Thibeau, S., and Zweigel, P. 2008. Best practice for the storage of CO2 in saline aquifers. Keyworth, Nottingham: British Geological Survey Occasional Publication No. 14. ISBN: 978-0-85272-610-5 IEA Greenhouse Gas R&D Programme (IEA GHG), “CCS Site Characterisation Criteria”, 2009/10, July 2009. IPCC âĂŤ Intergovernmental Panel on Climate Change, 2005. IPCC Special Report on Carbon Dioxide Capture and Storage. Cambridge University Press, Cambridge, UK – Table 5.3 Juanes, R., Hager, B. H., & Herzog, H. J. (2012). No geologic evidence that seismicity causes fault leakage that would render large-scale carbon capture and storage unsuccessful. Proceedings of the National Academy of Sciences, 109(52), E3623-E3623. Nicol, A., Carne, R., Gerstenberger, M., & Christophersen, A. (2011). Induced seismicity and its implications for CO2 storage risk. Energy Procedia, 4, 3699-3706. Vilarrasa V, Carrera J (2015) Geologic carbon storage is unlikely to trigger large earthquakes and reactivate faults through which CO2 could leak. Proc Natl Acad Sci USA 112(19):5938–5943. White, J. A., & Foxall, W. (2016). Assessing induced seismicity risk at CO2 storage projects: Recent progress and remaining challenges. International Journal of Greenhouse Gas Control,

49, 413-424. Zoback, M. D., & Gorelick, S. M. (2012). Earthquake triggering and large-scale geologic storage of carbon dioxide. Proceedings of the National Academy of Sciences, 109(26), 10164-10168.

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2019-196/se-2019-196-RC2-supplement.pdf

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