Reponses to the reviewer's comment 2 on manuscript egusphere-2024-3882

Review by Mauro Cacace, 04 Apr 2025

Dear Mauro Cacace

Thank you very much for the thorough review and the many useful comments and suggestion. They are addressed as stated in the responses to you comment below and have led to an improvement of the manuscript on our study.

Overall comment: The study by Gischig and co-authors present a review on lessons learnt from hectometer-scale stimulations done in the Bedretto Underground Laboratory on the feasibility/merit/limitations/open challenges for probabilistic seismic hazard estimates during hydraulic stimulation. The authors present their workflow to what they referred to a PISHA, which includes data collected and available at different stages, as derived from other geothermal projects and those more specific to their underground laboratory during past projects. In their workflow, data are used to update at each time the computed (in a probabilistic sense) seismic hazard, which they cross-correlate mainly to operational parameters (injected fluid volume), and, relative in a weak manner to the local geology/tectonic. The authors consider an additional layer to better refine their PISHA by including GGMs and discuss in the final chapter of the study the benefits of their multi-component and "time-dependent" workflow in the light of existing (A)TLSs.

I personally found the manuscript scientifically sound and well organized, from the introduction to the problem, associated open question(s), proposed solution(s) --> data/modelling/results and implications/next steps. While the manuscript is in general well written, there are some parts where the authors could (and should) improve the level of details in order to ease the efforts from the readers to not only completely follow their procedure but also to properly judge the scientific merit of each step described. On similar lines, while I agree with the authors' choice on the final discussion points, I personally found all 3 sub-paragraph filled with too many generic statements and I would advise the authors to carefully reconsider those by adding concrete explanations to their sentencing.

I'm listing some (minor) open questions/suggestions to improve the readability/clarity and sometimes the scientific output of the manuscript (considering what has been already discussed in the previous post by the other reviewer), which I consider fits well with the topic of SE and would make a nice contribution to the journal.

Reply: *We appreciate the overall assessment of the manuscript and, in particular, agree that the discussion section is somewhat too extensive and contains statements that do not add much to the main messages and may be omitted. We went through the discussion and removed some statements that we feel made the text more difficult to read rather than help to general understanding. Together with the suggestions for improvement by Review 1, we feel that this has led to an improvement.*

* Abstract (line 40-41): While I agree that a different seismogenic response between deep reservoir studies and underground laboratory is likely to be related to specific differences in their settings (stress levels and fault area) as well as in the operations (injected volume), I have some difficulties in how this information can be used to properly (i.e. in a quantitative manner)

used to propose/advance safer exploitation concepts. After reading through the whole of the manuscript I was expecting a discussion point addressing this specific issue, but the authors failed to take it up later in the paper. This said, I would consider either to avoid such generic sentences or at least rephrase them to read less abstract and more scientifically enriched.

Reply: *The statement was meant as an outlook towards future research and not as an outcome of our study. We agree that such statements are more appropriate in the discussion and decided to omit it in the abstract.*

* Abstract (concluding sentence): A first-order control here stems from the local geology and geological knowledge that is orders of magnitude simpler/known/understood in underground laboratories than in the field. In addition, also controlled conditions of an underground laboratory are hard to achieve in the field. All these aspects contributes as the authors stated in a "safer seismic hazard", but also makes the "up-scaling" of the applications hard.

Reply: *We agree with this comment and added a statement onf the limitation regarding upscaling to the concluding sentence.*

* Introduction (line 58-60): while discussing real forecasting, a bit of caution here. To my knowledge there is no approach we can rely upon to forecast induced seismic hazard. What current approaches offer is to either statistically project in time previous knowledge (as in this study) or at best hindcast (with diverse success) induced seismic hazard.

Reply*: We fully agree that this should be written cautiously. We reworded the statement and added that no reliable forecast models exist.*

* Introduction (lines 70 onward): The authors should add that thresholds in (A)TLS are likely to be empirically derived (based on experts knowledge and/or previous experience) and should potentially also be considered as an additional source of (potentially epistemic) uncertainties in PSHA (which they are not).

Reply: *We added a sentence highlighting that TLS thresholds are indeed mostly determined by expert judgment.*

* Introduction (lines 75/76 onward): while discussing Mmax, please review the study by van der Elst and co-workers (https://doi.org/10.1002/2016JB012818), where the authors nicely showcased that whether it is true that the Mmax can scale with injected (net) volume (in reality they should rather scale with the previous earthquake population) there is only poor (if not at all) control their exact position in the seismicity population, that is, Mmax occurrence can be at best randomly picked within the statistics. This poses some questions on the feasibility of TLS thresholds, as demonstrated for real field applications by post-injection seismicity, which "hosts" preferentially the largest magnitude seismic event (lessons learnt from Pohang, Vendenheim, Soultz and many others).

Reply: *We added the statement referring to the work of van der Elst and on its implications on the seismicity trailing effect.*

* Introduction (line 95/97): This is an excellent question, I like it a lot. Caveat here: how to cast the governing physics (only partially known/understood) into a probabilistic approach? The same is true to a certain degree for underground laboratories, which target a specific fault of a

limited extent under controlled conditions that are really hard to achieve in any "real" field application.

***Reply**: Thank you for this comment. We generally agree with the comment. However, at the scale of the Bedretto underground laboratory several faults and fracture system could be targeted so that together with the overall scale, we are closer to the real field application.*

* Method (line 249/251): mean and median are not the same thing, and they provide different outcomes. In addition, stating that "Conservatism" comes from a conservative choice reads at least redundant. Please, consider rephrasing this sentence in order to clarify the message (also by considering that the choice of the traffic light system is empirical it not subjective to the experts' knowledge).

***Reply**: Rhe two sentences were reworded for more clarity.*

* Method - Magnitude rates (eq 1):

- V(t) should rather be V_dot(t) (during injection)

***Reply**: We corrected this error.*

- This is more about personal taste. I have some hard times to understand the main idea behind the post shut-in definition of the seismicty rate (from the original paper). As a matter of fact the equation shows (as it should given observation) the same traits of a typical exponential decaying (not too much dissimlar to an Omori law), but it has apriori parameters (e.g. V_dot(t_shut-in) and tau) that are way harder to constraint than more classical approaches based on a (modified) Omori Law. As an example I find it difficult to have it representing any tailing in time if not by correlating injection rate at shut-in to the corresponding overpressure computed/monitored (this also assumes linearity in the pressure reservoir response which is not always the case).

***Reply**: Clearly, there are different ways to express trailing seismicity. For the reasons mentioned by the reviewer, we chose a much simpler approach and represent trailing seismicity with the percentage of seismic events that occurred after shut-in. The simplicity and the fact, that we are not interested in the time-dependent seismicity after shut-in but the total seismicity, justifies in our view this approach.*

- Any explanation behind the reference (0.05) b-value?
- Same as above for the 10% of post shut-in seismicity?

***Reply**: These values were chosen heuristically and roughly match a median value of those cases studies for which these values are reported. We indicate this in the text.*

* Method - maximum moment magnitude (line 310) - how physical considerations come into play here?

***Reply**: This is described in the paragraphs that follow. We reworded the sentence to make this clear.*

* Method - maximum moment magnitude (line 312-314) - honestly speaking this sentence/remark is not true (or at least not always), see the recent seismicity at Vendenheim project.

***Reply**: This is a misconception of our statement. While the maximum expected and/or observed magnitude may indeed have an impact on a project in terms of the associated risk, the physical upper bound of induced earthquakes is a very rare event the would produce substantial damage to a project or the surrounding infrastructure, but occurs at such a low rate that the risk (probability of damage to occur) is very low. The reference in the text clarify this issue.*

* Method - maximum moment magnitude (line 322-325) - A bit of caution here, lessons learnt form Pohang entails a tectonic control on Mmax as per classical theory.

***Reply**: We fully agree – and also state it in our text – that cases like Pohang show that Mmax has been controlled by what is tectonically possible. However, this may not always be the case depending on depth, stress regime, and orientation and state of faults accessed by the high-pressure fluid injection as many authors argue. Our choices of Mmax tries to reflect these different views and outcomes.*

* Method - maximum moment magnitude (line 341-343) - Please refer also o the study by Galis et al (2017 - DOI: 10.1126/sciadv.aap7528) on exactly this topic.

***Reply**: We added the study by Galis et al to the discussion on Mmax and also included the limits in Figure 8 as also proposed by reviewer 1. We do not consider these limits in the non-tectonic volume-dependent choice of Mmax, because the McGarr limit already covers this option sufficiently well.*

* Method - maximum moment magnitude (line 350-351) - from where the 3 MPa stress drop comes?

***Reply**: The value is an average value that can be seen as representative across many magnitude levels. We added a statement with reference to Cocco et al (2016) in the manuscript.*

* Results - Magnitude rates - It's not clear, and I have my limitation to it, why the authors don't discuss normalized PDFs for the exceedance probability. I warmly advise the authors to add their own point of view/explanation, given that all their results read to a certain level "biased" by this choice.

***Reply:** The figure 11 presents the probabilitiy of exceeding a certain magnitude and at the same time presents the uncertainty in these estimates as grey shading. We do not fully understand what the reviewer means with normalized PDFs in this context.*

- While discussing GMMs, the "unreasonable" range might stem from the high frequency content (see the comment from the previous reviewer)

***Reply**: We highlight that the actual reason for this may be related to combining several GMMs leading to added epistemic and aleatory uncertainties.*

* Discussion - sensitivities and uncertainties

Introducing the discussion paragraph with a rather generic sentence of benefits from PISHA should be followed by a detailed discussion of what those benefits are. I missed this. In addition, sometimes the authors state the obvious as while discussing the median and percentile sensitivity (percentiles provide a view of the data distribution)

Reply: *We rewrote part of the section to bring across the main message of it (which is relative sensitivity of uncertainties to GMM, seismogenic properties and Mmax) more concisely.*

* Discussion - scale and depth dependent seismogenic response

Again here the authors discusses aspects that have been already discussed/proposed in previous study and that, to my own reading of their manuscript, are not completely related to what was presented. Their concluding sentence reads too generic. It is not clear how studies based on underground laboratories help in addressing the problems described above. Please note that I do agree that such studies are extremely important, and this is why I would advise the authors to discuss what in their opinions are opportunities from those studies as it would greatly advance the scientific merit of the discussion.

Reply: *We feel that the discussion prior to the concluding sentence explains how we reach this conclusion. If we understand the reasons why some geological, hydromechanical or operational aspects lead to weaker responses, we may find ways to reduce induced seismicity in full-scale operations. We reworded the sentence to be more specific on this.*