Reponses to the reviewer's comment 1 on manuscript egusphere-2024-3882

Review by Martin P. Mai, 15 Feb. 2025

Dear Martin Mai,

We'd like to thank you for your thorough review and the supportive comments. We agree that the paper has to cover a wide range of topics to explain the workflow and results of our probabilistic induced seismicity analysis. You comments helped to clarify many aspects of our analysis.

General Comments

Induced seismicity in context of enhanced oil & gas exploitation, wastewater injection, and geothermal-energy harvesting is a recurring problem that operators, regulators and nearby communities have to deal with. In case of geothermal energy, hydraulic simulations have led to induced seismicity at a level that caused substantial shaking such that operations were stopped because the associated seismic hazard was not tolerable any more. The question then arises if this (time-dependent) seismic hazard can be quantified and "controlled" during hydraulic stimulation.

The study by Gischig and colleagues examines this question, with a focus on hectometer-scale stimulations in the Bedretto Underground Laboratory for Geoenergies and Geoscience (BULGG) in Switzerland. Inspired from observations and lessons learned in several geothermal projects around the globe, the authors develop a work-flow to compute/update the seismic hazard at a location of interest given fluid-injection parameters and the overall boundary and initial conditions at the site in terms of geology, seismotectonics & regional stresses, whereby the seismic-hazard updates are based on the known injection history and measured seismicity parameters. Noting that hazard estimates may vary greatly depending on the state of information/data and can be better constrained with more data and refined seismicity parameters, the authors also stress that site-specific ground-motion data and a related ground-motion model (GMM) are critical to narrow down the hazard estimates to plausible ranges. The study concludes with proposing an adaptive traffic light system (aTLS) that capture the time-dependent seismic hazard changes in near-real time.

The manuscript is well written, with very accessible graphics and a well-composed structure that naturally navigates the reader through the rather comprehensive material in terms of previous studies, the site of interest, related observations, models developed in the past and applied for the chosen case study, hazard calculations and how these are eventually embedded into a traffic light system. That is, the paper is rich. It is dense. It contains a lot of information that the reader needs to digest. In my view, the authors did an excellent job in this regard, but, ***I do remark that for most 1st to 2nd year graduate students in this field and also the "general but interested reader", this paper may not be an easy read***.

From a technical point of view, I don't have any major comments and concerns. The science is solid. The methods are well known (but not all explained in detail, hence readers need sufficient background knowledge), the data are exquisite, and the overall goal of the study is of importance scientifically and from a socio-economic point of view. Nevertheless, I have several remarks and questions related to the presentation, **level of detail provided on certain aspects of the study (sometimes too much, and thus distracting from the "big picture"'; sometimes too sparse to be able to follow)**, and a few editorial remarks.

The one major point I would like to raise is the use of peak ground velocity (PGV) instead of peak ground acceleration (PGA) as ground-motion intensity measure. The earthquakes consider here are predominantly of small magnitudes and clearly dominated by high-frequency seismic radiation. The authors also state the most earthquakes studied radiate above 10 Hz. ***A rule-of-thumb is that PGV***

captures shaking intensities for waves around 1 Hz. Hence, the use of PGV is counter-intuitive and perhaps not physically justifiable. This aspects needs detailed consideration and explanation (see below for more on this).

Overall, I rate this paper as "publishable after minor revisions". New calculations/analyses or substantial re-organization/rewriting are not needed, but I ask for clarifications and editorial improvements that should be straightforward to implement.

In summary, this is a very interesting and well-written manuscript that I think will be quite impactful.

Below, I provide a few technical comments follow by minor editorial suggestions.

Technical Comments

+ Figure 2: It would be help to graphically show here the principal stress orientations (and magnitudes) discussed in Lines 139ff. That would help the readers to quickly grasp all tectonic details.

***Reply**: We added the stress orientations in the figure as recommended.*

+ Figure 3: For the laymen readers, these 3D plots are hard-to-impossible to put in context. In essence, a detailed map / 3D graphic is needed that shows the locations of these boreholes within the Bedretto Lab. I am not sure if these locations can be easily added to Figure 2, or if another zoomed-in close up near the underground lab is needed. Please consider.

***Reply:** We added a clearer 3D view of the borehole setup from two different angles and larger spatial context to Figure 2.*

+ Section 3 (Instrumentation and Experiments) can be and perhaps should be deleted. In my opinion, these details are not needed to understand this study in terms of science, methods, scope and final results. On the other hand, this section distracts from the main "story line" and the main goals of this paper. If deemed important for this completeness purposes, I suggest to move this section into an Esupplement/Appendix.

***Reply:** We agree that this section is too long and that it contains information that is irrelevant for the paper. However, deleting it completely is not possible, because its main goal is to describe the succession of the performed experiment that provided the data for the hazard study and also defines the moments, when the hazard study in updated. We believe that this is much clearer now that the section is shortened substantially.*

+ Figure 4: Which magnitude scale is used here, Ml or Mw? Please indicate. In general, since this topic comes up later again, I suggest to explain already early on how magnitudes are estimated, if Ml or Mw is routinely/automatically determined, and with which uncertainties.

***Reply:** It is indeed important to clarify early on that the magnitudes reported and used for the analysis are Mw. We added a sentence in Section 3 and added Mw to the axes labels.*

+ Line 275: The "simplifying assumption that the b-value remains constant during injection and after shut-in" is an interesting point to (re-)consider. First of all, is that assumption valid? Given the wealth of data and the experience of the team of author, this should be a very quick and easy point to check and verify. My suspicion is that this is not the case, looking at Figure 4. Perhaps time-dependent b-values, and the variations, over different time-window lengths could be computed to check if/when this assumption is correct. And if not, then we need to think about how this may be propagated into the later hazard calculation.

Reply: *Considering a variable b-value is not possible in our approach, because we do not use a time-dependent induced seismicity model. Thus, any potential variability in the b-value must be accounted for by the aleatory and epistemic uncertainty of the constant b-value used. We agree that the impact of time-dependent b-values on a priori hazard analysis should be investigated. However, this must be subject of future research.*

+ Figure 7: Panels a) and b) need some modifications. First, the y-axis range in both panels should be identical. Second, the grey-scale density plot in panel b) is too fuzzy and doesn't allow being able to see details. I suggest to use a distinct colorbar with 6-10 visually clearly separable colors (say at 0.1, 0.2, 0.3 …) so that details can be seen.

Reply: *We modified the figures as recommended.*

+ Lines 326 - 335: Here, reference should be made to Galis et al (2017) (already in the reference list) and perhaps to Gabriel et al (2024, in Science) and Palgunadi et al (2024, JGR) on arrested and run-away ruptures in complex-geometry fault systems.

Reply: *We agree the referring to the work by Galis et al is appropriate here and we added the reference. Although the work by Gabriel et al and Pagunadi et al do shoot into a similar direction in terms of finding different rupture propagation regimes, they are focussed on interaction between small earthquakes and larger rupture rather than induced earthquakes.*

+ Lines 347 - 352: Using a simple constant stress-drop assumption to translate an estimated fault dimension to a possible event magnitude seems too simplistic, too approximate, and does not include any uncertainty. I strongly recommend to apply modern source-scaling relations (i.e. Thingbaijam et al, 2017), possibly also considering different faulting styles, to estimate potential event magnitude and its range.

Reply*: Comparing our value for the local tectonic Mmax of 5.4 with the scaling relations by Thingbaijam et al, (2017), we find that our estimate is reasonably well in agreement. We added this to the text. We also emphasize stronger that the uncertainties in the scaling relations is generously covered by the standard deviation of 0.8, which not only includes these uncertainties but also those brought in by the estimate of the potential rupture area.*

+ Figure 8: I strongly recommend to plot the scaling relations van der Elst et al (2016) and Galis et al (2017) into this figure for completeness and reference. (This will also shorten the figure caption by two lines …).

Reply: *We added the scaling relations by van der Elst et al and Galis et al to give a more complete picture on the recent research on the Mmax topic.*

+ Section Ground motion models: As someone who has experience in PSHA and GMM's for "standard" regional/national seismic hazard assessment, I am puzzled that PGV is used as ground-motion intensity metric, instead of PGA. I realize this may be the engineering/operational practice in mining-seismicity studies, but this is very counter-intuitive, in particular because we are dealing with very small events that are dominated by high-frequency radiation, and hence PGV may not be an ideal shaking parameter to use. I suggest that the authors provide some clarification and rationale for their choice,.

Reply: *we realize that this is somewhat puzzling because PGA is the more commonly used metric to represent hazard. The main reasons for this decision are:*

- *We wanted to use metrics for TLS thresholds that are in agreement with what the Swiss norm indicate for comparable cases in terms of shaking frequencies. This is the Swiss Norm 640 312a,* *which deals with constant vibration and occasional vibrations. The thresholds therein are given in PGV.*

- *The damage scenarios deemed most relevant in our study are cracking of tunnel walls and ceiling, rock fall, rock burst etc.. Damage thresholds for these scenarios stem from mining literature and are given in PGV with comparable values to the recommendations of the Swiss Norm.*
- *Most GMMs in literature that deal with our magnitude level and distances come from mining literature and are given in PGV rather than PGA. An example is also for this is also the PSHA study for a mine by Wesseloo (2018).*

It is not entirely clear, why the relevant literature for our scale and magnitude level deals with PGV rather than PGA. A reason may lie in the fact that traditionally ground velocity is easier to detect with standard devices, because they are more sensitive to the ground motions at this magnitude level.

We feel that these aspects should be clarified better in the manuscript and summarize these points at the beginning of this section.

+ Figure 9: The Cai-Kaiser (2018) model seems to be an almost exact replicate of McGarr & Fletcher (2005), just shifted downwards by "-1 log bias unit". Is that the case? Perhaps an explanatory sentence.

Reply: *We found a small mistake in the script creating this figure. Although the difference between data and the model has decreased a little, there is still a shift between the Cai-Kaiser and the McGarr-Fletcher models. We do not have an explanation for this other than that the fitted parameters in both publications differ because they must have relied on different datasets. We added a sentence clarifying this.*

+ Line 411: The wording "site-specific information" confuses me here, since it is not clear what the "site" is. In BULGG, there are numerous seismic sensors that each could be considered a "recording site". On the other hand, the overall spatial foot-print of BULGG or any similar experimental facility is rather small and would be typically considered as a "single site" in any local/regional PSHA study. Please clarify.

Reply: *We reworded the term site-specific here to be clear that we mean information from the BULGG seismic network.*

+ Line 420: "induced earthquake … have frequencies higher than 10 Hz" —> this relates back to my comment above: Why is then PGV a useful ground-motion metric? And wouldn't PGA make much more sense?

Reply: *see our answer to the earlier comment.*

+ Figure 14: For the 10 panels shown on top, I suggest not to use a continuous colorbar-scale, but one with 8-12 clearly distinct color. Visually, the hues of red between, say 200 - 800 mm/s cannot be discriminated.

Reply: *we changed this according the suggestions by the reviewer.*

+ Line 537-539: The fact that PSHA estimate increase as more data are added is in fact a widely occurring but not well appreciated fact, in general; not only in the context of induced seismicity. I suggest to add corresponding references from the PSHA literature.

Reply: *We added a comment that PSHA suffers from the same "problem" and added the references by Bommer and Abrahamson, (2006) as well as the review paper by Gerstenberger et al (2020).*

+ Sub-section Scale dependent seismogenic response: I would have expected at least a short discussion on whether there are dependencies of the b-value on the faulting-style of the earthquake. Schorlemmer at al (2005?) found a very compelling dependence of the b-value given the faulting-style, which in turn can be explained by the dominant acting stress regime. I suggest to add a few sentences on this here.

Reply*: we added the reference by Schorlemmer et al (2005) as well as Petruccelli et al (2019) and also indicate that Scholz (2015) sees a stress-dependence of b-values.*

+ Line 606: somewhere close to the reference to Deichmann (2017) and in this section there should also be made reference to two papers by Bethmann et al (BSSA, 2011, and GJI, 2012) that examine Mw-Ml scaling relations and site/attenuation effects on Ml/Mw estimates in Switzerland.

Reply: *we added this reference as suggested.*

Editorial Suggestions

+ The authors refer to "hazard" and "risk" numerous times in the paper, and I do understand that they want to clearly distinguish the two. However, in several instances this distinction is not clear and then things become confusing. Because there are no risk calculations included here and risk is only referred to in a general sense, I suggest the authors add a specific "item at risk" in corresponding statements, for example "risk for tunnel collapse", or "risk to geothermal surface facilities" to better guide the readers what they in mind in each case.

Reply: *we went through the text and became more specific on the term risk or replaced it by hazard if more appropriate in the context.*

+ Please carefully check the punctuation. I noticed many missing periods (" .") to conclude sentences, but even more so I found incorrect setting of commas (" ,") that lead to confusion in terms of meaning of the respective sentences.

Reply: *we did go through the punctuation carefully and replaced a few incorrect commas.*

Other points:

+ Line 100: move "Sweden" after Aspo (in Line 99)

Done.

+ Line 106: "intense" is not a good word here, as it cannot be quantified. Use something more specific: real-time; high-resolution (in space, time and frequency frequency) or something like that …

Done.

+ Line 109: "seismic risk" … see above …

Reply: *Changed to hazard.*

+ Line 281: (and others) - the text refers to Table 1, but this does not exist; it is Table A1 in the Appendix. This may just be a formatting issue or problem with the latex-template, but please check such referencing carefully.

Reply: *Corrected.*

+ Line 314: The wording "moderate" seems unclear here. Moderate "magnitude"? But what magnitude would that be in the context of the event sizes shown here? Or perhaps better "more frequent events"?

***Reply**: we replaced "moderate" by "smaller magnitudes occurring more frequently".*

+ Line 390: The "Table" mentioned here is given in the Appendix. Please correct.

***Reply**: in fact we refer to the table 2 in Douglas et al., (2013). We changed this to be more clear.*

+ Line 463: Figure caption to Figure 11: The "hazard" here should be clearly specified as "Hazard to exceed a certain earthquake magnitude". Most readers associate "hazard" with "seismic" (i.e. "shaking hazard") …

***Reply**: changed for more clarity.*

+ Line 487: The title to this sub-section should be "seismic" or 'shaking' hazard …

***Reply**: changed as suggested.*

+ Line 505: See comment above to Line 463 / 483

***Reply**: changed as suggested.*

+ Lines 540 - 544: This sentence seems garbled up; I cannot understand it. Also, change "became" to "become".

***Reply**: Corrected and split in two sentences for more clarity.*

+ Line 548: remove or quantify "somewhat"

***Reply:** Removed*

+ Line 596: abbreviation "GSK" not defined$

***Reply:** We define GSK at first use.*