nature portfolio

Peer Review File

Transient evolution of the relative size distribution of earthquakes as a risk indicator for induced seismicity

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

8th Jun 22

Dear Ms Ritz,

Your manuscript titled "Transient evolution of the b-value as a risk indicator for induced seismicity" has now been seen by 3 reviewers, and I include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

In particular, please ensure that in the revised manuscript you meet the following editorial thresholds:

* Present a compelling case that your model-observation comparison is robust

* Provide additional evidence that the difference in the b-values in question is statistically significant

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:
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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Teng Wang, PhD
Editorial Board Member
Communications Earth & Environment
0000-0003-3729-0139

Joe Aslin
Senior Editor
Communications Earth & Environment

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Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The paper "Transient evolution of the b-value as a risk indicator for induced seismicity" presented by Ritz et al., demonstrates the b-value is the main driver for risk/hazard through simulating seismic activity of an EGS project in Basel with a loosely coupled hydro-mechanical-stochastic model. They proposed using transient b-values as a diagnostic tool to assess and mitigate seismic hazard. They show the change of b value during reservoir stimulation for different injection site conditions and injection strategies. The paper is well written and presented. There are two questions need to be addressed.

1.It is little hard to physically understand the low transient b value is related with high probability of a large earthquake. Based on the method part, the b value is determined by the differential stress which depends on both pore pressure and stress drop of activation of seeds. Mathematically, it is right that the b value is large for high pore pressure and small for small pore pressure. However, it is hard to understand when the pore pressure is small, it would have a relative high probability to induce a large event.

2.For earthquake hazard assessment and forecasting using the b-value, it seems the transient b is important during the revisor stimulation, but b value would return to the long-term b value after the stimulation. Is there a time limitation for using transient b value for the forecasting?

Reviewer #2 (Remarks to the Author):

The paper aims at showing how the b-value can be used in induced seismicity to assess the risk during
injections.
The paper is well written and clear. The model is rooted in a solid well-known software package, TOUGH2, which is successfully used in many fields.
The modeling part is fine with me. My main concerns are relative to the comparison of the model outcomes with the empirical observations. As is, this comparison is not fully convincing and some conclusions may sound too speculative.
Below I summarize my main concerns.

- FIGURE 1a. In Figure 1a, the authors compare the GR laws from different sites (and different catalogs). In my experience this can be very misleading. In fact, the b-value depends on the kind of magnitude used and by the transformation used to get a homogeneous magnitude (as made in this paper according to the supplementary material; page 7). If the local magnitudes have been obtained in different ways, also their linear transformation may not be homogeneous and not allowing b-values to be easily compared. Moreover a linear transformation from Ml to Mw may leave unaltered some non-linearity problems of Ml as it has be noted by Herrmann and Marzocchi that may bias the b-value estimation (SRL, 2021); for this reason some transformation between Ml and Mw is nonlinear. As a consequence, the whole discussion on the probability of large events has to be carefully reshaped, highlighting these possible caveats.

- FIGURE 1a. In Figure 1a the Basel catalog has 2591 events (Table S2), but, if I remember well, the Herrmann catalog is much richer (hundreds of thousands?). What is the reason of this difference?

- FIGURE 1b. In Figure 1b, the b-value has a clear trend, but it is not clear to me how many data have been used to calculate the b-value in each time window. Using non-overlapping time windows of 0.2 days, and about 3,000 earthquakes (Figure 1a), the number of earthquakes in each bin could be about few tens (of course, this is just a rough average because I guess that the rate is not uniformly distributed in time). This would make the b-value highly sensitive to the largest earthquake in the bin.
I would suggest adding the 2 sigma confidence level to Figure 1b, to appreciate if the variations noticed are really significant and, consequently, to give a direct idea of how many data you are using to calculate the b-value. This is important to evaluate the significance of the b-value drop before the shut-in.

- FIGURE 1b. I guess that the authors pay attention to the completeness before calculating the b-value. But it could be worth showing, at least in the supplementary material, that the magnitudes are really exponentially distributed (otherwise the calculation of the b-value does not make too much sense). In my experience, I have noted often strange behaviors of the GR when small magnitudes are considered (high-resolution catalog). This may be helpful also to address my first point.

- LINES 129-132. Although I understand the logic followed by the authors (and I may agree with that, at least in principle), this conclusion, which is one of the main conclusions of the paper, sounds a little bit too speculative. In essence, they say that the b-value drop before the shut-in is caused by the fact that the seismic sequence "recognizes" the presence of a fault, making the risk higher. Reading the paper is not clear to me how much this result is stable varying the several input parameters of TOUGH2. I think it is important to communicate clearly that what you get is not an overfitting of the model. Just to be clear, I am not against this conclusion, but I would like to understand more how much the results of the model support (or not) this speculation. Another possibility (that does not exclude the previous one) is to rephrase this part with more caution.

- LINES 230-231. I like this idea. Ensemble forecasting is quite diffuse in many fields. Besides the weights, it is important how to create the ensemble; and this is not a trivial issue. No changes needed here, but just my opinion on this very important point.

- MAXIMUM SIZE OF INDUCED SEISMICITY. This section is rather fuzzy for me. It is not clear what is the message. If we would be able to set a maximum to the magnitude this would be a huge step ahead; but we cannot. It seems that the authors are saying that since we are not able to define a max magnitude, the monitoring of the b-value becomes more important. If so, this part can be shortened a lot.

- LINES 289-290. As said before, this conclusion seems a little bit too speculative.

Reviewer #3 (Remarks to the Author):

<B>Review of "Transient evolution of the b-value as a risk indicator for induced seismicity" by Vanille A. Ritz, Antonio P. Rinaldi, and Stefan Wiemer</B>

The authors present an extended and improved model (relative to the previously published study of Bachmann et al, 2012) for the Basel EGS project injection and initial post-shut in phases to model the occurrence and frequency-magnitude distribution of the project associated induced seismicity. The authors find clear spatiotemporal variation in the data which the model reproduces. The authors thus make a good case for a physical cause of the variations and subsequent implications for hazard and risk assessment and real-time monitoring in future similar cases.
I find the paper overall well written, the research well presented, and a valuable addition I have a few concerns and some minor comments, as indicated below, but overall suggest a minor revision of the paper prior to accepting it for publication.
The authors present a spatiotemporal development of the observed induced seismicity. The approach is based on binning of the data in time. I have a few concerns with respect to the approach:
• The approach basically only assesses a temporal evolution, as no spatial component is included in the analyses. Off course, given the spatial development of the seismicity with time, a spatial component is implicit. My question concerns the presence of the fault influencing the spatial development of the seismicity, as shown by the model and observed in the data (as stated in lines 74-75). To what extend would this spatial component affect the analysis, given the limitation to the temporal binning only?

• In addition to this: the authors show the implication of binning the data in 0,2 day or 3000 event bins. Can the authors discuss why the binning size choice of 0,2 days was selected? What prior assumptions underlie this choice? Why differ from the previous choice?

• The authors do not present any statistical evidence that the b-values are indeed statistically significantly different and could not still stem from the same background distribution. I would like to see evidence that the b-value during injection is indeed statistically significantly different from the background rate to which it returns after shut-in, as this is an important motivation for the whole study and recommendations in the discussion.

I find the idea of real-time tracking of the b-value interesting and the model implicates a validity for this. However, it is well known that deriving b-values, especially b-value variations from data is implicitly difficult as large numbers of events are required to derive reliable b-values from data (see e.g. Marzocchi et al, 2020). I feel the authors should fully address this issue when proposing the approach in section "Using the b-value for near-real-time hazard assessment and forecasting".

The paper assumes that the risk/hazard strongly depends on the b-value. This implicitly assumes that the maximum magnitude that could occur due to injection is as large as can be statistically expected (Van der Elst et al, 2016). I think it would be worthwhile to at least mention this and preferably verify, as this is a major contributor to the hazard/risk, even more so than the b-value. The result of the verification could be reported in the supplemental information.

In general, the quality of the text in the figures should be improved, it seems a bit blurry, and the legends and axis titles are barely readable.

Minor comments:

• Figure 1: Please include also the uncertainty band on the b-values derived so the reader can assess the statistical significance of the small scale variations compared to the larger trend.

• Line 74-75: I suggest the authors include a figure of the spatial distribution of the seismicity showing the alignment with the fault zone. This will help your readers fully appreciate the complexity of the problem.

• Figure 2-c: Please explicate whether the figure represents the base case or the fault case. In the text please indicate you will address the difference between the two models next or later on in the paper.

• Line 83: "The evolution of the pressure in both time Figure 2-a and space." The sentence feels unfinished. Please rephrase.

• Line 83-85: "Figure 2-d shows an initial sharp pressurisation close to the injection. Further gradual pressurisation happens over time with the propagation of the pressure front and the surge in pressure associated with increases in the injection rate." Figure 2-d is a snapshot after 10 days, hence does not show the initial sharp pressurisation nor a temporal development. I presume the authors mean Figure 2-a?

• Line 85-87: "The permeability follows a similar pattern with an initial enhancement followed by successive increases after the injection rate increases, with each monitoring point further from the injection seeing a similar pattern, albeit with a delay." I would suggest to explicitly refer to Figure 2-b to assist the reader.

• Line 111: "…resulting in a larger the number of seeds reactivated." Please remove the "the".

• Line 209-219: Some references to TLS systems, there benefits and drawbacks should be included.

• Line 220-223: "Approaches where seismic data are fed into multiple models from purely statistical to fully physical are weighted and evaluated to provide robust ensemble forecasting of induced seismicity that can then be computed into hazard and operational control" The sentence only gives only a description of the approaches, but what about them? If I place comma's, It would read "Approaches, where seismic data are fed into multiple models from purely statistical to fully physical, are weighted, and evaluated to provide robust ensemble forecasting of induced seismicity that can then be computed into hazard and operational control," and I have an incomplete sentence. Please rephrase.

• Line 328-329: "The extent of the rupture area is based on the average expected magnitude from the power law distribution with the b-value of the given seed." For the sake of reproducibility, it would be good to explicate the relation between the rupture area (in 3D) and the average expected magnitude.

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Review file: Transient evolution of the b-value as a risk indicator for induced seismicity

Vanille A. Ritz, Antonio P. Rinaldi, Stefan Wiemer
Swiss Seismological Service at ETH Zürich, Zürich, Switzerland

Rebuttal letter

Dear reviewers,

We are thankful for your careful reading of our manuscript and constructive comments that helped us clear out potential misunderstandings arising from the manuscript. In this rebuttal letter, we provide a point to point answer to all raised comments, reporting the original comment in black and our reply in blue. All additional figures provided to answer the comments have been added to the Supplementary material and/or the main manuscript where applicable.
We would also like to point out that we have improved the quality of the figures in the manuscript and reorganised the Supplementary to better follow the order of the manuscript.

Vanille A. Ritz, Antonio P. Rinaldi, and Stefan Wiemer.

Reviewer 1 8/6/2022

The paper “Transient evolution of the b-value as a risk indicator for induced seismicity” presented by Ritz et al., demonstrates the b-value is the main driver for risk/hazard through simulating seismic activity of an EGS project in Basel with a loosely coupled hydro-mechanical-stochastic model. They proposed using transient b-values as a diagnostic tool to assess and mitigate seismic hazard. They show the change of b value during reservoir stimulation for different injection site conditions and injection strategies. The paper is well written and presented. There are two questions need to be addressed.

- It is little hard to physically understand the low transient b value is related with high probability of a large earthquake. Based on the method part, the b value is determined by the differential stress which depends on both pore pressure and stress drop of activation of seeds. Mathematically, it is right that the b value is large for high pore pressure and small for small pore pressure. However, it is hard to understand when the pore pressure is small, it would have a relative high probability to induce a large event.
  The relationship between low b-values and higher probabilities of a large magnitude earthquake is simply the mathematical definition of the frequency-magnitude power law: $log(N_{\geq M}) = a - bM$; which states that a lower b-value will correspond on average to fewer large magnitude events. In our model, the b-value and pore pressure are not linked, it’s the differential stress that is directly linked to the b-value. The differential stress is allowed to change with reactivation (stress drop) or stress transfer from reactivated neighbouring seeds. The pore pressure is the mechanisms by which effective normal stress on a given seed is lowered, potentially leading to reactivation (Mohr-Coulomb criterion).
  We have added the following sentence to the Methods section to make the reactivation process clearer:
  "The effective normal stress (σ_n^{eff}) on the seeds is changed by the pore pressure, if this this change of σ_n^{eff} is enough to exceed the critical shear stress (referred to as shear strength), the seed is reactivated."

- For earthquake hazard assessment and forecasting using the b-value, it seems the transient b is important during the reservoir stimulation, but b value would return to the long-term b value after the stimulation. Is there a time limitation for using transient b value for the forecasting?
  The reviewer raises an important question with practical implications, but one that is not easy to answer, mainly because of data limitations but also because in our understanding there is a continuous gradient between ’tectonic’ and ’induce/triggered’ (analog to the ambiguity in ’declustering’. We use the transient b-value to assess processes during the injection phase (re-pressurisation of previously pressurised zones, clustering on a fault etc). The long-term b-value is of course the most relevant one to assess the risk at the site. There are certainly limitations to using transient b-values, first of which being the number of induced events needed to have robust evaluation of the b-value during and after the stimulation phase. This limitation is linked to the triggering mechanism with pore pressure perturbation, as once the increase in pore pressure has been diffused, no more events are triggered and we cannot calculate a b-value anymore. This "quiet zone" is exemplified in the case of Pohang: The stretch of time between the end of the stimulation and the M5.5 was very quiet, rendering an analysis of the evolution of the b-value impossible. However, this lack of events in the post shut-in period is not a guarantee that there will be no more earthquakes later on. Moreover in Pohang the b-value during the injection was extremely

low and this observation early on could have raised alarms regarding the safety of the stimulation.

As for forecasting long-term b-value trends, we would suggest that cyclic injections are the ones to give us the best estimate via the seismicity decay phase that they create between each cycle allowing us to assess "stable" b-values but also τ of the Omori and Båth laws. We have added a sentence to the Discussion (section "Using the b-value for near-real-time hazard assessment and forecasting") to raise caution about the need for significant numbers of recorded events necessary to track transient b-values:

"This monitoring of the b-value, however, requires robustly generated magnitudes and generous catalogues recorded and generated in near-real-time to provide trustworthy signals from the underground."

Reviewer 2 8/6/2022

The paper aims at showing how the b-value can be used in induced seismicity to assess the risk during injections. The paper is well written and clear. The model is rooted in a solid well-known software package, TOUGH2, which is successfully used in many fields. The modeling part is fine with me. My main concerns are relative to the comparison of the model outcomes with the empirical observations. As is, this comparison is not fully convincing and some conclusions may sound too speculative. Below I summarize my main concerns.

- FIGURE 1a. In Figure 1a, the authors compare the GR laws from different sites (and different catalogs). In my experience this can be very misleading. In fact, the b-value depends on the kind of magnitude used and by the transformation used to get a homogeneous magnitude (as made in this paper according to the supplementary material; page 7). If the local magnitudes have been obtained in different ways, also their linear transformation may not be homogeneous and not allowing b-values to be easily compared. Moreover a linear transformation from Ml to Mw may leave unaltered some non-linearity problems of Ml as it has be noted by Herrmann and Marzocchi that may bias the b-value estimation (SRL, 2021); for this reason some transformation between Ml and Mw is nonlinear. As a consequence, the whole discussion on the probability of large events has to be carefully reshaped, highlighting these possible caveats.
  We do recognise that the "one size fits all" approach to standardising the magnitudes to Mw is flawed for detailed analysis. However, with Figure 1-a we aim at providing a global context of the observed range of a- and b-values during past injections, and to highlight the widely varying behaviour. For this goal and illustration, we think that the error on the magnitude due to the general Ml/Md to Mw conversion is not so relevant. We have added a sentence in the supplementary to raise these caveats to the readers and refer the reader to the detail of the supplementary in the figure caption.
  For Figure 1-a, the local magnitudes are converted to moment magnitude using Equation S1; and duration magnitudes converted to moment using Equation S2. This generic conversion is done to compare the sites but can introduce errors in the magnitude distribution depending on the specificities of the local and duration magnitude scales used for the sites.

- FIGURE 1a. In Figure 1a the Basel catalog has 2591 events (Table S2), but, if I remember well, the Herrmann catalog is much richer (hundreds of thousands?). What is the reason of this difference?
  We chose to show the GEL catalogue in Figure 1-a and not the template-matched-catalogue of Herrmann et al., (2019)[1] to allow for a fairer comparison between the different sites. We have made it clearer by referencing Häring et al., (2008)[2] which presents the non-template-matched catalogue in the figure. We also updated Supplementary Table 1 to show the characteristics for the GEL and template-matched catalogues should someone want to compare them directly.

- FIGURE 1b. In Figure 1b, the b-value has a clear trend, but it is not clear to me how many data have been used to calculate the b-value in each time window. Using non-overlapping time windows of 0.2 days, and about 3,000 earthquakes (Figure 1a), the number of earthquakes in each bin could be about few tens (of course, this is just a rough average because I guess that the rate is not uniformly distributed in time). This would make the b-value highly sensitive to the largest earthquake in the bin. I would suggest adding the 2 sigma confidence level to Figure 1b, to appreciate if the variations noticed are really

significant and, consequently, to give a direct idea of how many data you are using to calculate the b-value. This is important to evaluate the significance of the b-value drop before the shut-in.
In Figure 1-b we show the data from the template-matched catalogue of Basel by Herrmann et al., 2019[1] using a fixed time window of 0.2 days with 0.1 days overlap , we have rephrased in the manuscript to make it clearer. The $\pm$ one and two standard deviation of the b-value (using the classical $\sigma = \frac{b}{\sqrt{N}}$) binned with a fixed time window is shown in the figure below that now replaces Figure 1-b (the comparison between fixed number of events and fixed time window is now shown in Supplementary Figure S1). The uncertainty range for the fixed number of events can be found in Herrmann et al. 2019[1], we think adding it here would overcrowd the figure and not be of great benefit. As for the number of event and magnitude of completeness, we have added the Mc(t) to Supplementary Figure S1 that already contained the normalised a-value for the fixed time-window (see next point).

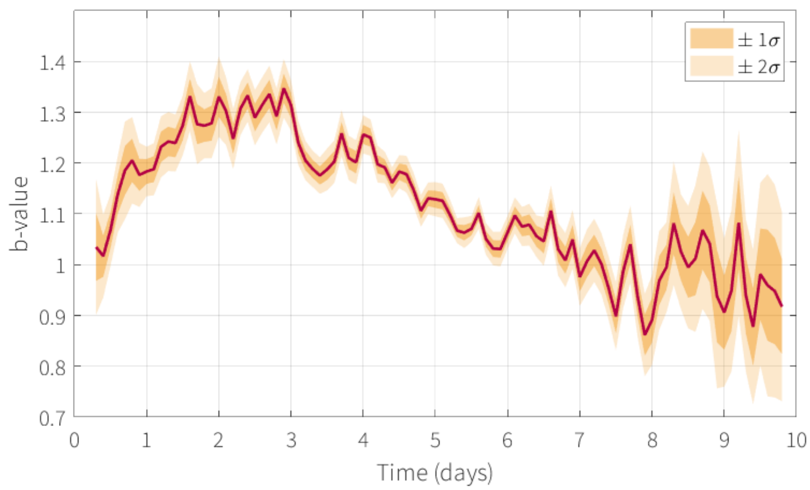


Figure 1-b

- FIGURE 1b. I guess that the authors pay attention to the completeness before calculating the b-value. But it could be worth showing, at least in the supplementary material, that the magnitudes are really exponentially distributed (otherwise the calculation of the b-value does not make too much sense). In my experience, I have noted often strange behaviors of the GR when small magnitudes are considered (high-resolution catalog). This may be helpful also to address my first point.
For the calculation of the b-value, we do use the magnitude of completeness determined using the 'maximum curvature methods' (Woessner et al., 2005[3]), adding the recommended 'safety margin' of +0.2 to the calculated Mc in the bin (as done by Herrmann et al., 2019[1]) for the catalogue when using a fixed number of events binning. We have added to Supplementary Figure 1 the evolution of the magnitude of completeness in time for both fixed number of event window and fixed time window.

- LINES 129-132. Although I understand the logic followed by the authors (and I may agree with that, at least in principle), this conclusion, which is one of the main conclusions of the paper, sounds a little bit too speculative. In essence, they say that the b-value drop before the shut-in is caused by the fact that the seismic sequence "recognizes" the presence of a fault, making the risk higher. Reading the paper is not clear to me how much this result is stable varying the several input parameters of TOUGH2. I think it is important to communicate clearly that what you get is not an overfitting of the model. Just to be clear, I am not against this conclusion, but I would like to understand more how much the results of the model support (or not) this speculation. Another possibility (that does not exclude the previous one) is to rephrase this part with more caution.

The section on the fault construction (Supplementary Figures S11.1-3) has been improved to show the stability of the "fault detection" with different levels of complexity in the model.

We have added a sensitivity analysis of the host-rock/fault permeability difference, which shows that even with only one order of magnitude of difference between the host-rock and fault permeability (purple line), the fault is clearly detected in the b-value signal.

We have included a reference to the detail of the fault build-up and the stability of the transient b-value with different hydraulic and geomechanical properties in the Methods section.

We also want to point out that the drop in b-value observed in Basel had already been interpreted as a clustering on the fault by Herrmann et al., (2019)[1] where they point out that a spatial shift of the recorded seismicity (from 3D growth to 2D clustering) coincides with the drop in b-value from the 5th of December (our "day 3" in Figure 1-b). We have added the following sentence to the manuscript to strengthen our suggestion of a "fault recognition" in the b-value and to acknowledge the work of Herrmann and colleagues more fairly. *"This drop in b-value in Basel from day 3 has been correlated to a shift from a three-dimensional spread of the seismic cloud to a two-dimensional growth aligned with the main fault by Herrmann and colleagues."*

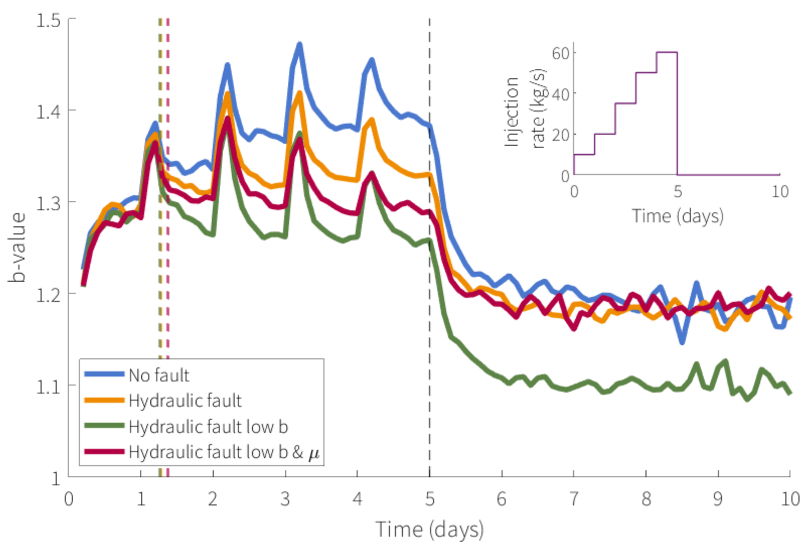


Figure S11.1. Step-increasing injection: temporal evolution of the b-value with different level of complexity of the fault placed 100 m from injection (fixed time-window 0.2:0.1 days). The dashed line denotes when the fault gets reactivated in each case (colour-coded)

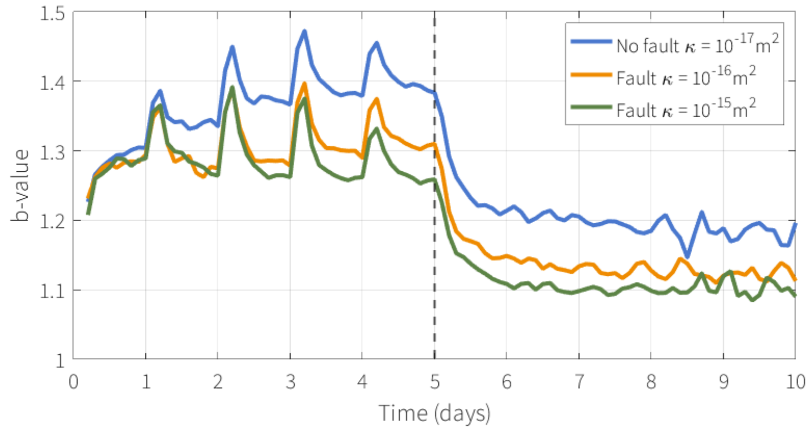


Figure S11.2. Step-increasing injection: temporal evolution of the b-value with different permeabilities of the fault (fixed time-window 0.2:0.1 days).

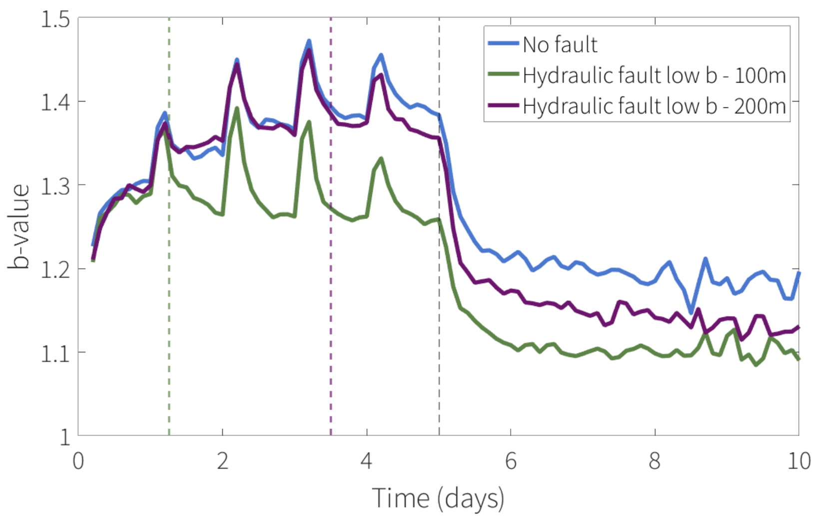


Figure S11.3. Step-increasing injection: temporal evolution of the b-value with the fault at different distances (fixed time-window 0.2:0.1 days).

- LINES 230-231. I like this idea. Ensemble forecasting is quite diffuse in many fields. Besides the weights, it is important how to create the ensemble; and this is not a trivial issue. No changes needed here, but just my opinion on this very important point.
  Thanks for the comment. We do think that ensemble modelling can provide benefits to forecasting but also to study the physics and relative weights of different processes behind induced seismicity.
- MAXIMUM SIZE OF INDUCED SEISMICITY. This section is rather fuzzy for me. It is not clear what is the message. If we would be able to set a maximum to the magnitude this would be a huge step ahead; but we cannot. It seems that the authors are saying that since we are not able to define a max magnitude, the monitoring of the b-value becomes more important. If so, this part can be shortened a lot.
  We have reworked the section to make clear that the maximum magnitude is as large as statistically expected (Van der Elst et al., 2016[4]; Gischig and Wiemer, 2013[5]) and a

parameter of the site that cannot be controlled, putting emphasis on the need to monitor the site to quantify this Mmax as soon as possible (for example with transient b-values). We also made clearer the role played by the injected volume in the determination of the expected size of seismicity and added a sensitivity analysis of the influence of the volume to the supplementary (Supplementary Figure S7).

"For a given regional tectonic context, the injected volume seems a logical first-order control on , and observations tend to agree in wastewater disposal and hydro-fracturing cases. Some models based on a direct control of the volume on the number of events have been proposed and used in near-real-time or to introduce risk informed thresholds. The injected volume can play a tremendous role on the size of the seismicity (Supplementary Figure S8). In this study, we chose to look beyond this first-order control with constant injection rates for all the cases presented. Indeed, the example of Pohang (2017, South Korea) demonstrates that there is more than just the volume at play, with only a fraction of the expected needed volume injected for an of magnitude M_w 5.5 (Grigoli et al., 2018[6], Woo et al, 2019[7])."

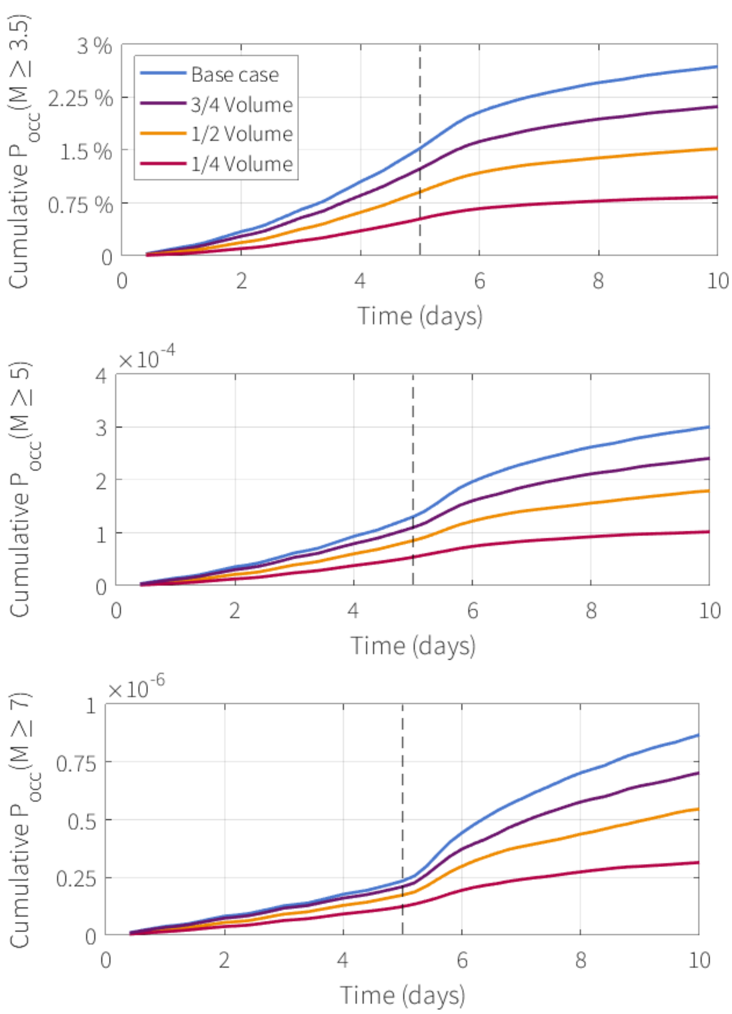


Figure S7. Cumulative probability of occurrence of a magnitude greater or equal to 3.5 (a), 5 (b) and 7 (c) for the base case without fault and fractions of the base case injected volume

- LINES 289-290. As said before, this conclusion seems a little bit too speculative.
  We have rephrased the sentence to make it clear that transient b-values on their own are not enough to detect faults, but that coupled with real-time analysis of the event location, changes in the b-value could help detect features.
  "In parallel to event localisation, monitoring the evolution of the b-value over time may enable detection of such a fault prior to major events and could lead to operational techniques noticing a deviation from an expected b-value pattern."

Reviewer 3 8/6/2022

The authors present an extended and improved model (relative to the previously published study of Bachmann et al, 2012) for the Basel EGS project injection and initial post-shut in phases to model the occurrence and frequency-magnitude distribution of the project associated induced seismicity. The authors find clear spatiotemporal variation in the data which the model reproduces. The authors thus make a good case for a physical cause of the variations and subsequent implications for hazard and risk assessment and real-time monitoring in future similar cases. I find the paper overall well written, the research well presented, and a valuable addition I have a few concerns and some minor comments, as indicated below, but overall suggest a minor revision of the paper prior to accepting it for publication. The authors present a spatiotemporal development of the observed induced seismicity. The approach is based on binning of the data in time. I have a few concerns with respect to the approach:

- The approach basically only assesses a temporal evolution, as no spatial component is included in the analyses. Off course, given the spatial development of the seismicity with time, a spatial component is implicit. My question concerns the presence of the fault influencing the spatial development of the seismicity, as shown by the model and observed in the data (as stated in lines 74-75). To what extend would this spatial component affect the analysis, given the limitation to the temporal binning only?
  The spatial component is implicit in our model and has already been shown by Goertz-Allmann and Wiemer (2013)[8] and Bachmann et al. (2012)[9]. We observe similar results as they did in our model. Looking at different times, we see that the b-value close to the injection increases during the stimulation (increase relative to the reference post-shut-in b-value, indicative of retriggering) while the outer regions (and fault in the case with a fault) exhibit lower b-values, indicating that "new" seeds/fractures are being activated with their intrinsic b-values close to the regional value. The maps of the change in b-value at different times for the base case without and with fault has been added to the supplementary (see S4 and S5 below).
  To apply transient b-values monitoring to real-time detection of a fault, one would need to couple it with monitoring of the location of events, which is not trivial in real-time but not completely unrealistic, at least with preliminary locations. Shifts in the localisation of events could be noticed, such as a shift from 3D growth of the seismic cloud to 2D with potential lineaments. An example is the shift from three-dimensional to fault-clustered noticed by Herrmann et al., 2019[1] around December 5th 2006 in Basel which also corresponds to the drop in b-value at day 3 in Figure 1-b. We have added a sentence to the manuscript to add this observation of shift in the seismic cloud growth coinciding with the drop in b-value.
  "This drop in b-value in Basel from day 3 has been correlated to a shift from a three-dimensional spread of the seismic cloud to a two-dimensional growth aligned with the main fault by Herrmann and colleagues."

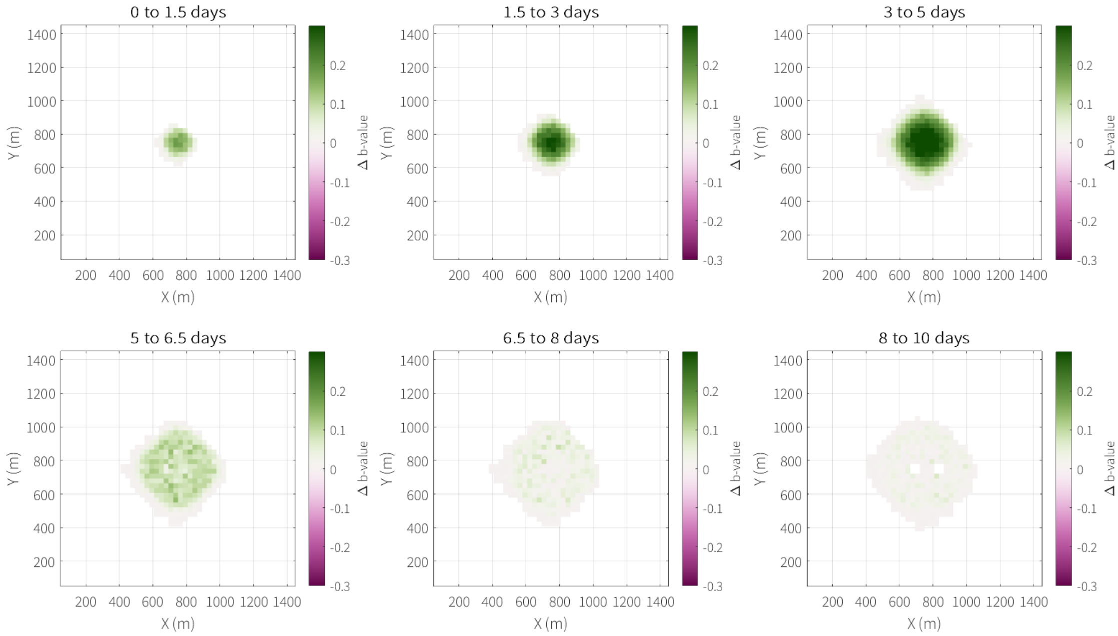


Figure S4. No fault case. Changes in b-value for different time period.

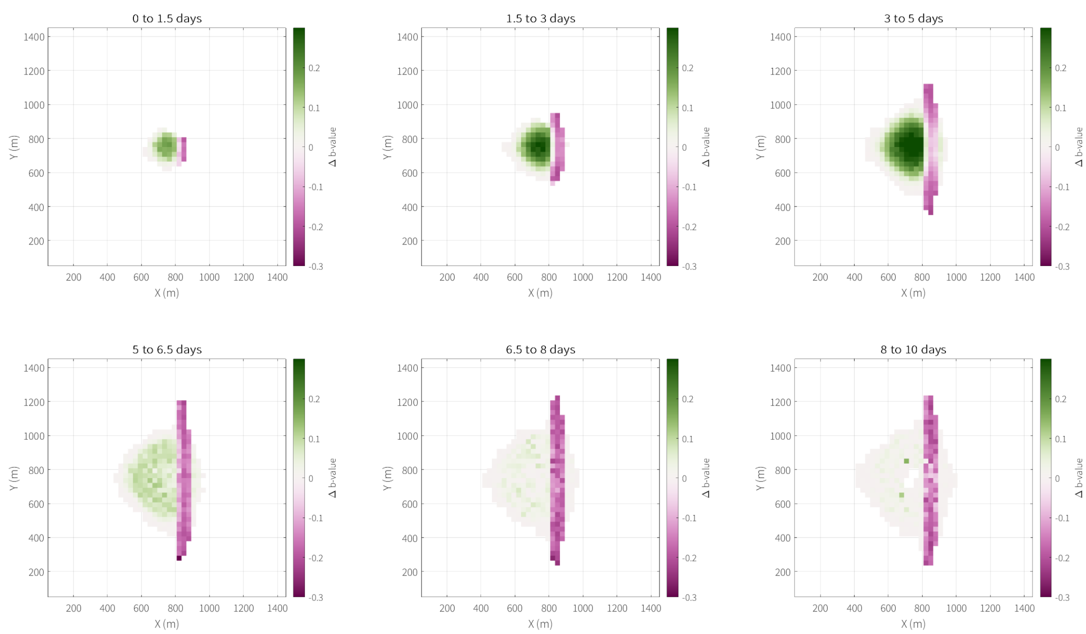
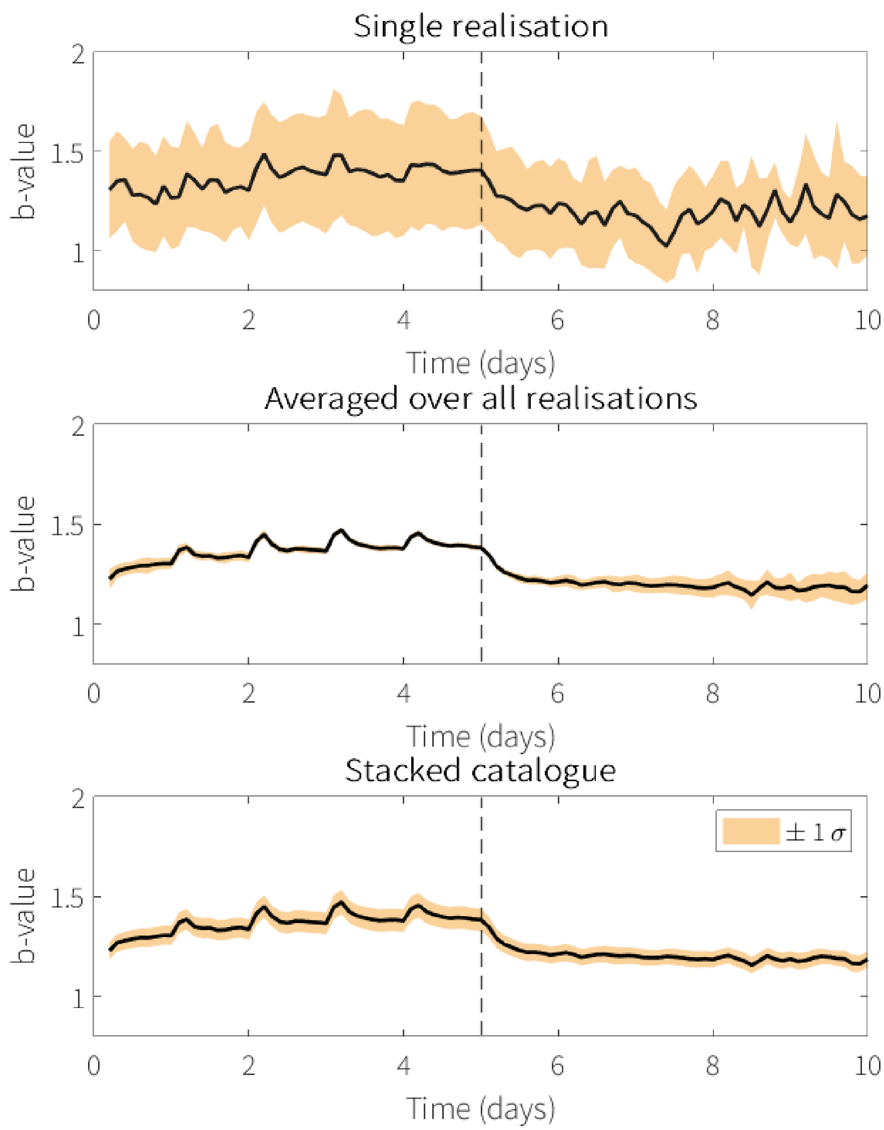


Figure S5. Fault case. Changes in b-value for different time period. To highlight the fault the changes in b-value are computed with the non-fault rock/stress properties as reference.

- In addition to this: the authors show the implication of binning the data in 0,2 day or 3000 event bins. Can the authors discuss why the binning size choice of 0,2 days was selected? What prior assumptions underlie this choice? Why differ from the previous choice?

The choice of 0.2 days bins is a pragmatic one allowing us to have regularity in the temporal distribution of our data points. The choice of Herrmann et al., 2019[1] to go with 3000 events bin is only made possible by the very large and more complete catalogue they created with template matching. In our model, we do not produce hundred of thousands of earthquake (which would be computationally very heavy), we have around 3000 events, which allows us to run the models in a reasonable time. Moreover, the b-value is not calculated from simulated magnitudes, but from the differential stress relationship and averaged over the events in the time-bin. We have confidence in the robustness of the b-values we show as we average over 48 catalogues (stochastic realisations) to smooth out outliers and get the signal of the b-value as strong as possible. The figure below shows the b-value and its $\pm$ one standard deviation for the base case without fault for one single realisation picked at random (top panel), the averaging over all realisations (middle panel) and when considering only one large stacked catalogue (bottom panel).



- The authors do not present any statistical evidence that the b-values are indeed statistically significantly different and could not still stem from the same background distribution. I would like to see evidence that the b-value during injection is indeed statistically significantly different from the background rate to which it returns after shut-in, as this is an important motivation for the whole study and recommendations in the discussion.
  For the models, we can separate the co-injection and post-shut-in phase, and look at the distribution of b-values of each population (Fig. 2-d, also presented below with mean and $\pm 1\sigma$ intervals as dashed lines). Then, we can use a T-test to evaluate their similarities. The null hypothesis is rejected with a p-value of 1.95e-4, confirming the statistically significant difference of the b-value during and after the injection phase.
  For the statistical analysis of the b-values observed in Basel, we would like to point out the very thorough analysis conducted by Herrmann et al., 2019[1]. We have added the p-value and statistical significance to the manuscript.
  Figure 3-d illustrates this natural state (dark) and the higher co-injection b-value distribution (light), driven up by repeating events. Using a T-test to evaluate the distributions of b-values co-injection and post-shut-in, we confirm the statistically significant difference of the b-value during and after the injection phase with a p-value of 1.95e-4.

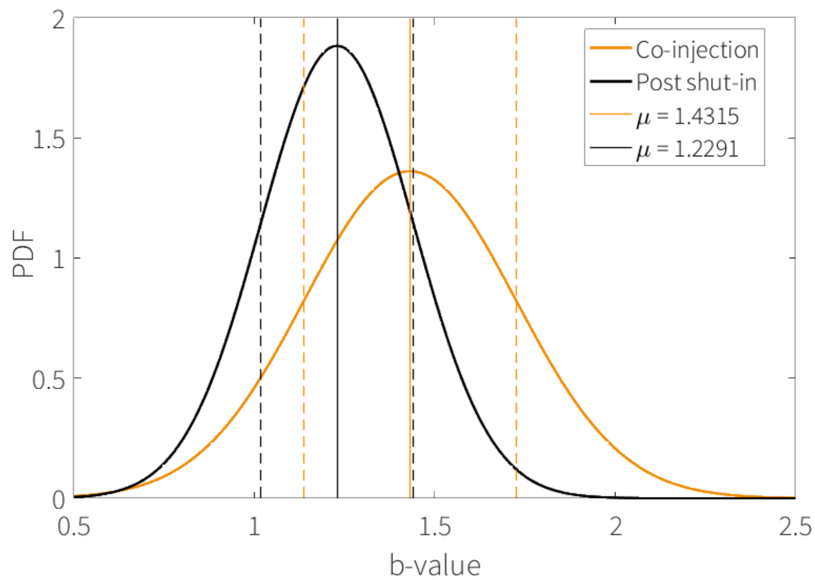


Figure 3-d. Distribution of the b-value co-injection and post-shut-in with mean (solid vertical lines) and $\pm 1\sigma$ (dashed vertical lines)

I find the idea of real-time tracking of the b-value interesting and the model implicates a validity for this. However, it is well known that deriving b-values, especially b-value variations from data is implicitly difficult as large numbers of events are required to derive reliable b-values from data (see e.g. Marzocchi et al, 2020). I feel the authors should fully address this issue when proposing the approach in section “Using the b-value for near-real-time hazard assessment and forecasting”.
We have added a sentence to emphasise the need of robust and complete catalogues to assess the b-value in near-real time in the "Using the b-value for near-real-time hazard assessment and forecasting" section.
"This monitoring of the b-value, however, requires robustly generated magnitudes and generous

catalogues recorded and generated in near-real-time to provide trustworthy signals from the underground."

The paper assumes that the risk/hazard strongly depends on the b-value. This implicitly assumes that the maximum magnitude that could occur due to injection is as large as can be statistically expected (Van der Elst et al, 2016). I think it would be worthwhile to at least mention this and preferably verify, as this is a major contributor to the hazard/risk, even more so than the b-value. The result of the verification could be reported in the supplemental information.

We agree that injected volume plays a major role in the risk associated with a deep underground injection and have made the "Maximum size of induced seismicity" clearer to reflect this fact. We strongly show that the site conditions (or local b-value) is the main driver of risk/hazard. The maximum magnitude is a parameter of the site and is independent of the b-value, but the probability to have an event of magnitude M does depend on the b-value.

We conduct our study with the same total injected volume for every case in order to look beyond the volume control and compare different settings (injection, fault) without having to account for the change induced by different volumes. We do verify that the risk/hazard is indeed controlled by the injected volume by running the base case without fault with fractions of the injection rate and get lower transient probabilities of any given magnitudes (see figure below that has been added to the supplementary). By using the different rate simulations (three quarters, half and a quarter of the base case volume) we see that the probability of a magnitude greater or equal to M scales with the b-value and also a-value so the Mmax is as large as statistically expected.

We have also rephrase section "Maximum size of induced seismicity" :

"For a given regional tectonic context, the injected volume seems a logical first-order control on , and observations tend to agree in wastewater disposal and hydro-fracturing cases. Some models based on a direct control of the volume on the number of events have been proposed and used in near-real-time or to introduce risk informed thresholds. The injected volume can play a tremendous role on the size of the seismicity (Supplementary Figure S7). In this study, we chose to look beyond this first-order control with constant injection rates for all the cases presented. Indeed, the example of Pohang (2017, South Korea) demonstrates that there is more than just the volume at play, with only a fraction of the expected needed volume injected for an of magnitude M_w 5.5 (Grigoli et al., 2018[6], Woo et al., 2019[7])."

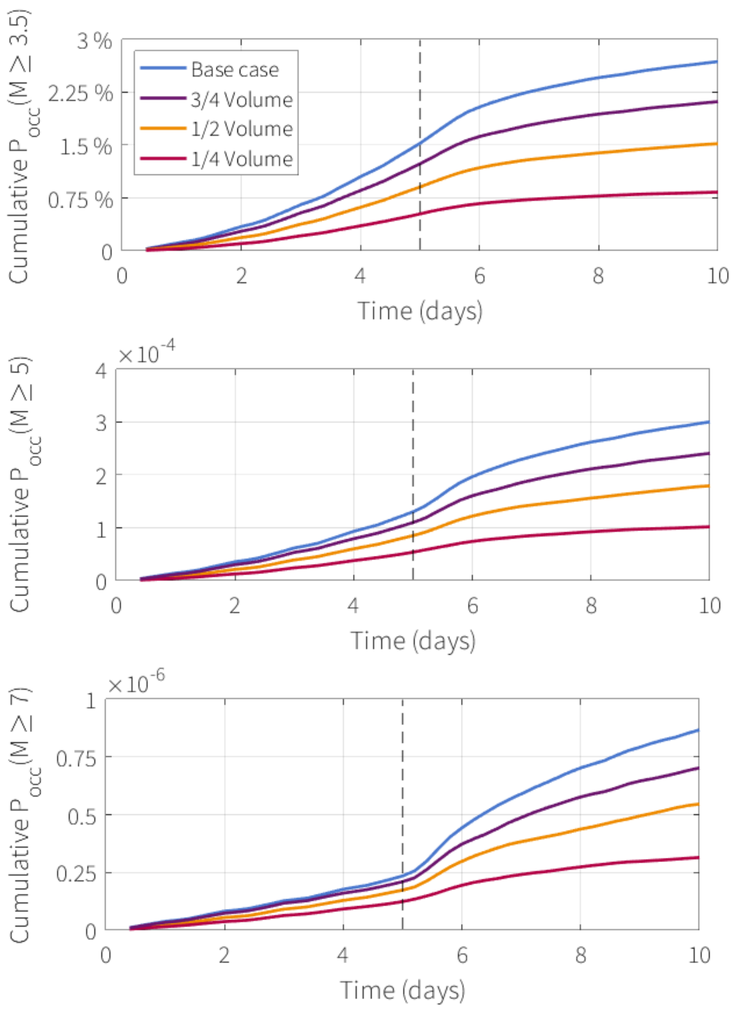


Figure S7. Cumulative probability of occurrence of a magnitude greater or equal to 3.5 (a), 5 (b) and 7 (c) for the base case without fault and fractions of the base case injected volume

In general, the quality of the text in the figures should be improved, it seems a bit blurry, and the legends and axis titles are barely readable.

All figures have been prepared for the revised version using the guidelines of the Nature journal group

Minor comments:

- Figure 1: Please include also the uncertainty band on the b-values derived so the reader can assess the statistical significance of the small scale variations compared to the larger trend.
  The uncertainty bands at $\pm$ one and two standard deviation of the b-value binned with a fixed time window has replaced Figure 1-b (the comparison of the fixed time window and fixed number of events has been moved to Supplementary Figure S1, also below).

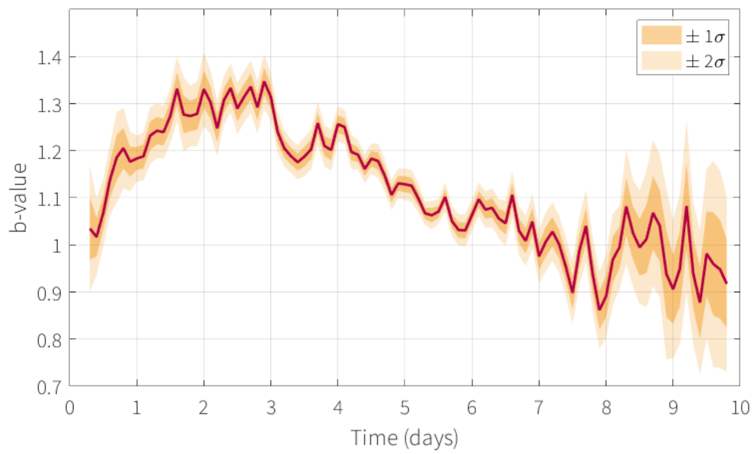


Figure 1-b

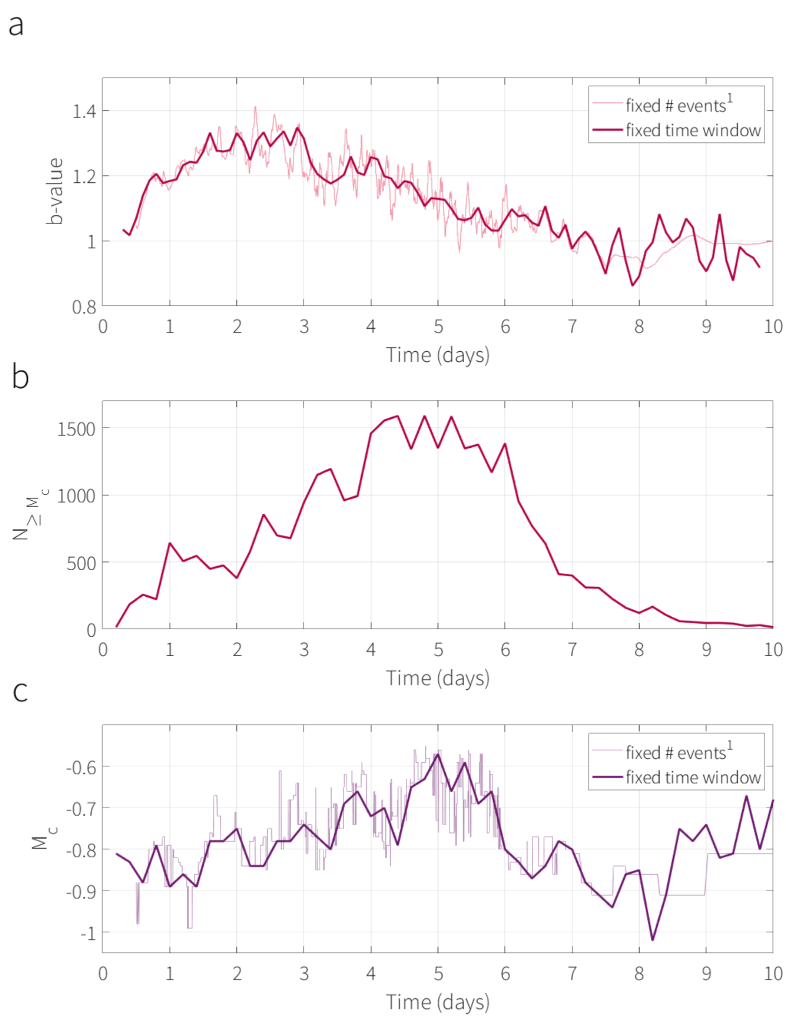


Figure S1

- Line 74-75: I suggest the authors include a figure of the spatial distribution of the seismicity showing the alignment with the fault zone. This will help your readers fully appreciate the complexity of the problem.
  Fig 2-g shows the map-view distribution of the seismicity (normalised over all realisations for the base case with fault). It highlights the clustering of seismicity on the fault. We have added the figure below to the supplementary material to show the distribution of seismicity in a vertical slice X-Z for the base case with and without fault

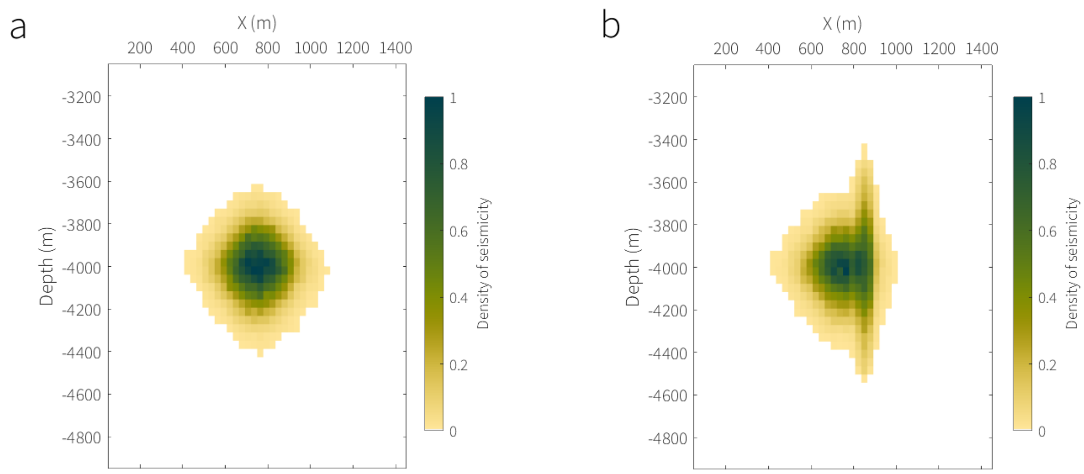


Figure S2

- Figure 2-c: Please explicate whether the figure represents the base case or the fault case. In the text please indicate you will address the difference between the two models next or later on in the paper.
  2c represents the base case without fault. We have changed Figure 2 to remove the confusion the R-T plots could create and have moved the R-T plot for the base case without and with fault to the supplementary material (S3). The fault case (b) clearly shows different triggering and back fronts of seismicity revealing its different hydraulic diffusivity from the host-rock.

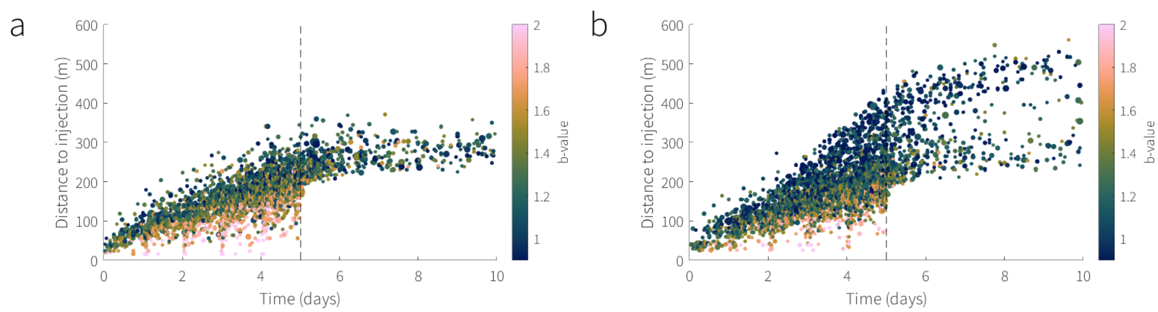


Figure S3. R-T plot for one realisation of the base case without a fault (a) and with a fault (b)

- Line 83: "The evolution of the pressure in both time Figure 2-a and space." The sentence feels unfinished. Please rephrase.
  The sentence has been reworked to be more understandable.

"The evolution of the pressure at different monitoring points (Figure 2-a) shows an initial sharp pressurisation close to the injection and further gradual pressurisations happening over time with the propagation of the pressure front and the surge in pressure associated with increases in the injection rate."

- Line 83-85: “Figure 2-d shows an initial sharp pressurisation close to the injection. Further gradual pressurisation happens over time with the propagation of the pressure front and the surge in pressure associated with increases in the injection rate.” Figure 2-d is a snapshot after 10 days, hence does not show the initial sharp pressurisation nor a temporal development. I presume the authors mean Figure 2-a?
  Yes, the referencing should have pointed to Figure 2-a, thank you for catching that one.

- Line 85-87: “The permeability follows a similar pattern with an initial enhancement followed by successive increases after the injection rate increases, with each monitoring point further from the injection seeing a similar pattern, albeit with a delay.” I would suggest to explicitly refer to Figure 2-b to assist the reader.
  reference to Figure 2-b has been added

- Line 111: “. . . resulting in a larger the number of seeds reactivated.” Please remove the “the”.
  extra "the" has been removed

- Line 209-219: Some references to TLS systems, there benefits and drawbacks should be included.
  We have added a pointer to Grigoli et al., 2017[10] which discusses in great length the advantages ans limitations of TLS.

- Line 220-223: “Approaches where seismic data are fed into multiple models from purely statistical to fully physical are weighted and evaluated to provide robust ensemble forecasting of induced seismicity that can then be computed into hazard and operational control” The sentence only gives only a description of the approaches, but what about them? If I place comma’s, It would read “Approaches, where seismic data are fed into multiple models from purely statistical to fully physical, are weighted, and evaluated to provide robust ensemble forecasting of induced seismicity that can then be computed into hazard and operational control,” and I have an incomplete sentence. Please rephrase.
  The sentence was indeed confusing and has be reformulated to be more understandable.
  "New approaches are being developed, where seismic data are fed into multiple models - from purely statistical to fully physical - which are then weighted and evaluated to provide robust ensemble forecasting of induced seismicity that can then be computed into hazard and operational control."

- Line 328-329: “The extent of the rupture area is based on the average expected magnitude from the power law distribution with the b-value of the given seed.” For the sake of reproducibility, it would be good to explicate the relation between the rupture area (in 3D) and the average expected magnitude.
  The detail of the calculation of the area of stress redistribution has been added to the method.
  In the distributed case, the stress drop occurs within a radius defined by the magnitude of the reactivated seed. Equation 2 details the calculation of the rupture area according to Brune’s model, with M_0 the seismic moment calculated from the picked magnitude

(Equation 3), and $\Delta\tau$ is the stress drop applied to the reactivated seed.

$$R_{eq} = \sqrt{\frac{7}{16}\frac{M_0}{\Delta\tau}} \tag{2}$$

$$M_0 = 10^{\frac{3}{2}M+9.1} \tag{3}$$

References

1. Herrmann, M., Kraft, T., Tormann, T., Scarabello, L. & Wiemer, S. A Consistent High-Resolution Catalog of Induced Seismicity in Basel Based on Matched Filter Detection and Tailored Post-Processing. *J. Geophys. Res. Solid Earth* **124**, 8449–8477, DOI: 10.1029/2019JB017468 (2019).
2. Häring, M. O., Schanz, U., Ladner, F. & Dyer, B. C. Characterisation of the Basel 1 enhanced geothermal system. *Geothermics* **37**, 469–495, DOI: 10.1016/j.geothermics.2008.06.002 (2008).
3. Woessner, J. & Wiemer, S. Assessing the quality of earthquake catalogues: Estimating the magnitude of completeness and its uncertainty. *Bull. Seismol. Soc. Am.* **95**, 684–698, DOI: 10.1785/0120040007 (2005).
4. van der Elst, N., Page, M., Weiser, D., Goebel, T. & Hosseini, S. Induced earthquake magnitudes are as large as (statistically) expected. *J. Geophys. Res. Solid Earth* **121**, 4575–4590, DOI: 10.1002/2016JB012850 (2016).
5. Gischig, V. S. & Wiemer, S. A stochastic model for induced seismicity based on non-linear pressure diffusion and irreversible permeability enhancement. *Geophys. J. Int.* **194**, 1229–1249, DOI: 10.1002/2013JB010597 (2013).
6. Grigoli, F. *et al.* The November 2017 M w 5.5 Pohang earthquake: A possible case of induced seismicity in South Korea. *Science* **5**, 1003–1006, DOI: 10.1126/science.aat2010 (2018).
7. Woo, J. U. *et al.* An In-Depth Seismological Analysis Revealing a Causal Link Between the 2017 MW 5.5 Pohang Earthquake and EGS Project. *J. Geophys. Res. Solid Earth* **124**, 13060–13078, DOI: 10.1029/2019JB018368 (2019).
8. Goertz-Allmann, B. P. & Wiemer, S. Geomechanical modeling of induced seismicity source parameters and implications for seismic hazard assessment. *Geophysics* **78**, KS25–KS39, DOI: 10.1190/geo2012-0102.1 (2013).
9. Bachmann, C. E., Wiemer, S., Woessner, J. & Hainzl, S. Statistical analysis of the induced Basel 2006 earthquake sequence: introducing a probability-based monitoring approach for Enhanced Geothermal Systems. *Geophys. J. Int.* **186**, 793–807, DOI: 10.1111/j.1365-246X.2011.05068.x (2011).
10. Grigoli, F. *et al.* Current challenges in monitoring, discrimination, and management of induced seismicity related to underground industrial activities: A European perspective. *Rev. Geophys.* **55**, 310–340, DOI: 10.1002/2016RG000542 (2017).

24th Aug 22

Dear Ms Ritz,

Your manuscript titled "Transient evolution of the b-value as a risk indicator for induced seismicity" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

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0000-0003-3729-0139

Joe Aslin
Locum Chief Editor,
Communications Earth & Environment
https://www.nature.com/commsenv/
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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The revised version solved all the questions very well. No further questions.

Reviewer #2 (Remarks to the Author):

The authors addressed satisfactorily my main concerns.

Review file: Transient evolution of the b-value as a risk indicator for induced seismicity

Vanille A. Ritz, Antonio P. Rinaldi, Stefan Wiemer
Swiss Seismological Service at ETH Zürich, Zürich, Switzerland

Rebuttal letter - August 2022

Dear reviewers,

We would like to thank you for your careful reading and review of our work. Your constructive suggestions helped us improve the manuscript.

Sincerely,

Vanille A. Ritz, Antonio P. Rinaldi, and Stefan Wiemer

Rebuttal letter - July 2022

Dear reviewers,

We are thankful for your careful reading of our manuscript and constructive comments that helped us clear out potential misunderstandings arising from the manuscript. In this rebuttal letter, we provide a point to point answer to all raised comments, reporting the original comment in black and our reply in blue. All additional figures provided to answer the comments have been added to the Supplementary material and/or the main manuscript where applicable.
We would also like to point out that we have improved the quality of the figures in the manuscript and reorganised the Supplementary to better follow the order of the manuscript.

Vanille A. Ritz, Antonio P. Rinaldi, and Stefan Wiemer

Reviewer # 1 8/6/2022

The paper “Transient evolution of the b-value as a risk indicator for induced seismicity” presented by Ritz et al., demonstrates the b-value is the main driver for risk/hazard through simulating seismic activity of an EGS project in Basel with a loosely coupled hydro-mechanical-stochastic model. They proposed using transient b-values as a diagnostic tool to assess and mitigate seismic hazard. They show the change of b value during reservoir stimulation for different injection site conditions and injection strategies. The paper is well written and presented. There are two questions need to be addressed.

- It is little hard to physically understand the low transient b value is related with high probability of a large earthquake. Based on the method part, the b value is determined by the differential stress which depends on both pore pressure and stress drop of activation of seeds. Mathematically, it is right that the b value is large for high pore pressure and small for small pore pressure. However, it is hard to understand when the pore pressure is small, it would have a relative high probability to induce a large event.
  The relationship between low b-values and higher probabilities of a large magnitude earthquake is simply the mathematical definition of the frequency-magnitude power law: $log(N_{\geq M}) = a - bM$; which states that a lower b-value will correspond on average to fewer large magnitude events. In our model, the b-value and pore pressure are not linked, it’s the differential stress that is directly linked to the b-value. The differential stress is allowed to change with reactivation (stress drop) or stress transfer from reactivated neighbouring seeds. The pore pressure is the mechanisms by which effective normal stress on a given seed is lowered, potentially leading to reactivation (Mohr-Coulomb criterion).
  We have added the following sentence to the Methods section to make the reactivation process clearer:
  "The effective normal stress (σ_n^{eff}) on the seeds is changed by the pore pressure, if this this change of σ_n^{eff} is enough to exceed the critical shear stress (referred to as shear strength), the seed is reactivated."

- For earthquake hazard assessment and forecasting using the b-value, it seems the transient b is important during the reservoir stimulation, but b value would return to the long-term b value after the stimulation. Is there a time limitation for using transient b value for the forecasting?
  The reviewer raises an important question with practical implications, but one that is not easy to answer, mainly because of data limitations but also because in our understanding there is a continuous gradient between ’tectonic’ and ’induce/triggered’ (analog to the ambiguity in ’declustering’. We use the transient b-value to assess processes during the injection phase (re-pressurisation of previously pressurised zones, clustering on a fault etc). The long-term b-value is of course the most relevant one to assess the risk at the site. There are certainly limitations to using transient b-values, first of which being the number of induced events needed to have robust evaluation of the b-value during and after the stimulation phase. This limitation is linked to the triggering mechanism with pore pressure perturbation, as once the increase in pore pressure has been diffused, no more events are triggered and we cannot calculate a b-value anymore. This "quiet zone" is exemplified in the case of Pohang: The stretch of time between the end of the stimulation and the M5.5 was very quiet, rendering an analysis of the evolution of the b-value impossible. However, this lack of events in the post shut-in period is not a guarantee that there will be no more earthquakes later on. Moreover in Pohang the b-value during the injection was extremely

low and this observation early on could have raised alarms regarding the safety of the stimulation.
As for forecasting long-term b-value trends, we would suggest that cyclic injections are the ones to give us the best estimate via the seismicity decay phase that they create between each cycle allowing us to assess "stable" b-values but also τ of the Omori and Båth laws.
We have added a sentence to the Discussion (section "Using the b-value for near-real-time hazard assessment and forecasting") to raise caution about the need for significant numbers of recorded events necessary to track transient b-values:
"This monitoring of the b-value, however, requires robustly generated magnitudes and generous catalogues recorded and generated in near-real-time to provide trustworthy signals from the underground."

Reviewer # 2 8/6/2022

The paper aims at showing how the b-value can be used in induced seismicity to assess the risk during injections. The paper is well written and clear. The model is rooted in a solid well-known software package, TOUGH2, which is successfully used in many fields. The modeling part is fine with me. My main concerns are relative to the comparison of the model outcomes with the empirical observations. As is, this comparison is not fully convincing and some conclusions may sound too speculative. Below I summarize my main concerns.

- FIGURE 1a. In Figure 1a, the authors compare the GR laws from different sites (and different catalogs). In my experience this can be very misleading. In fact, the b-value depends on the kind of magnitude used and by the transformation used to get a homogeneous magnitude (as made in this paper according to the supplementary material; page 7). If the local magnitudes have been obtained in different ways, also their linear transformation may not be homogeneous and not allowing b-values to be easily compared. Moreover a linear transformation from Ml to Mw may leave unaltered some non-linearity problems of Ml as it has be noted by Herrmann and Marzocchi that may bias the b-value estimation (SRL, 2021); for this reason some transformation between Ml and Mw is nonlinear. As a consequence, the whole discussion on the probability of large events has to be carefully reshaped, highlighting these possible caveats.
  We do recognise that the "one size fits all" approach to standardising the magnitudes to Mw is flawed for detailed analysis. However, with Figure 1-a we aim at providing a global context of the observed range of a- and b-values during past injections, and to highlight the widely varying behaviour. For this goal and illustration, we think that the error on the magnitude due to the general Ml/Md to Mw conversion is not so relevant. We have added a sentence in the supplementary to raise these caveats to the readers and refer the reader to the detail of the supplementary in the figure caption.
  For Figure 1-a, the local magnitudes are converted to moment magnitude using Equation S1; and duration magnitudes converted to moment using Equation S2. This generic conversion is done to compare the sites but can introduce errors in the magnitude distribution depending on the specificities of the local and duration magnitude scales used for the sites.

- FIGURE 1a. In Figure 1a the Basel catalog has 2591 events (Table S2), but, if I remember well, the Herrmann catalog is much richer (hundreds of thousands?). What is the reason of this difference?
  We chose to show the GEL catalogue in Figure 1-a and not the template-matched-catalogue of Herrmann et al., (2019)[1] to allow for a fairer comparison between the different sites. We have made it clearer by referencing Häring et al., (2008)[2] which presents the non-template-matched catalogue in the figure. We also updated Supplementary Table 1 to show the characteristics for the GEL and template-matched catalogues should someone want to compare them directly.

- FIGURE 1b. In Figure 1b, the b-value has a clear trend, but it is not clear to me how many data have been used to calculate the b-value in each time window. Using non-overlapping time windows of 0.2 days, and about 3,000 earthquakes (Figure 1a), the number of earthquakes in each bin could be about few tens (of course, this is just a rough average because I guess that the rate is not uniformly distributed in time). This would make the b-value highly sensitive to the largest earthquake in the bin. I would suggest adding the 2 sigma confidence level to Figure 1b, to appreciate if the variations noticed are really

significant and, consequently, to give a direct idea of how many data you are using to calculate the b-value. This is important to evaluate the significance of the b-value drop before the shut-in.
In Figure 1-b we show the data from the template-matched catalogue of Basel by Herrmann et al., 2019[1] using a fixed time window of 0.2 days with 0.1 days overlap , we have rephrased in the manuscript to make it clearer. The $\pm$ one and two standard deviation of the b-value (using the classical $\sigma = \frac{b}{\sqrt{N}}$) binned with a fixed time window is shown in the figure below that now replaces Figure 1-b (the comparison between fixed number of events and fixed time window is now shown in Supplementary Figure S1). The uncertainty range for the fixed number of events can be found in Herrmann et al. 2019[1], we think adding it here would overcrowd the figure and not be of great benefit. As for the number of event and magnitude of completeness, we have added the Mc(t) to Supplementary Figure S1 that already contained the normalised a-value for the fixed time-window (see next point).

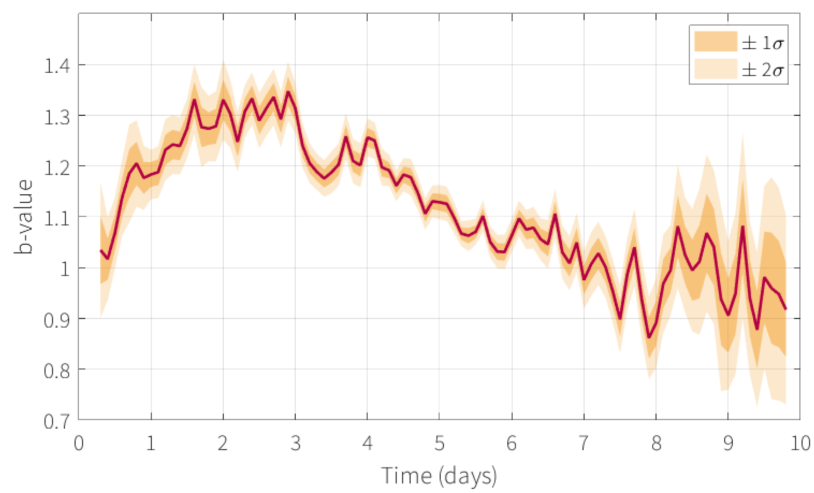


Figure 1-b

- FIGURE 1b. I guess that the authors pay attention to the completeness before calculating the b-value. But it could be worth showing, at least in the supplementary material, that the magnitudes are really exponentially distributed (otherwise the calculation of the b-value does not make too much sense). In my experience, I have noted often strange behaviors of the GR when small magnitudes are considered (high-resolution catalog). This may be helpful also to address my first point.
  For the calculation of the b-value, we do use the magnitude of completeness determined using the 'maximum curvature methods' (Woessner et al., 2005[3]), adding the recommended 'safety margin' of +0.2 to the calculated Mc in the bin (as done by Herrmann et al., 2019[1]) for the catalogue when using a fixed number of events binning. We have added to Supplementary Figure 1 the evolution of the magnitude of completeness in time for both fixed number of event window and fixed time window.
- LINES 129-132. Although I understand the logic followed by the authors (and I may agree with that, at least in principle), this conclusion, which is one of the main conclusions of the paper, sounds a little bit too speculative. In essence, they say that the b-value drop before the shut-in is caused by the fact that the seismic sequence "recognizes" the presence of a fault, making the risk higher. Reading the paper is not clear to me how much this result is

stable varying the several input parameters of TOUGH2. I think it is important to communicate clearly that what you get is not an overfitting of the model. Just to be clear, I am not against this conclusion, but I would like to understand more how much the results of the model support (or not) this speculation. Another possibility (that does not exclude the previous one) is to rephrase this part with more caution.
The section on the fault construction (Supplementary Figures S11.1-3) has been improved to show the stability of the "fault detection" with different levels of complexity in the model.
We have added a sensitivity analysis of the host-rock/fault permeability difference, which shows that even with only one order of magnitude of difference between the host-rock and fault permeability (purple line), the fault is clearly detected in the b-value signal.
We have included a reference to the detail of the fault build-up and the stability of the transient b-value with different hydraulic and geomechanical properties in the Methods section.
We also want to point out that the drop in b-value observed in Basel had already been interpreted as a clustering on the fault by Herrmann et al., (2019)[1] where they point out that a spatial shift of the recorded seismicity (from 3D growth to 2D clustering) coincides with the drop in b-value from the 5th of December (our "day 3" in Figure 1-b). We have added the following sentence to the manuscript to strengthen our suggestion of a "fault recognition" in the b-value and to acknowledge the work of Herrmann and colleagues more fairly. *"This drop in b-value in Basel from day 3 has been correlated to a shift from a three-dimensional spread of the seismic cloud to a two-dimensional growth aligned with the main fault by Herrmann and colleagues."*

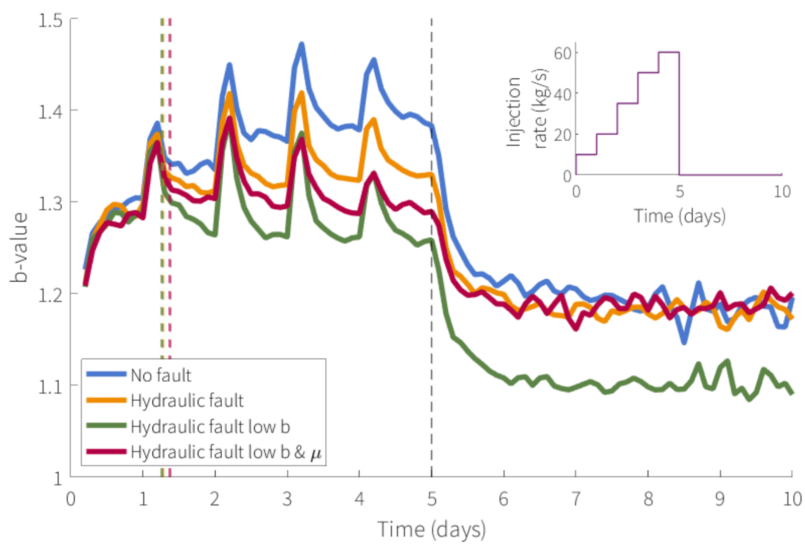


Figure S11.1. Step-increasing injection: temporal evolution of the b-value with different level of complexity of the fault placed 100 m from injection (fixed time-window 0.2:0.1 days). The dashed line denotes when the fault gets reactivated in each case (colour-coded)

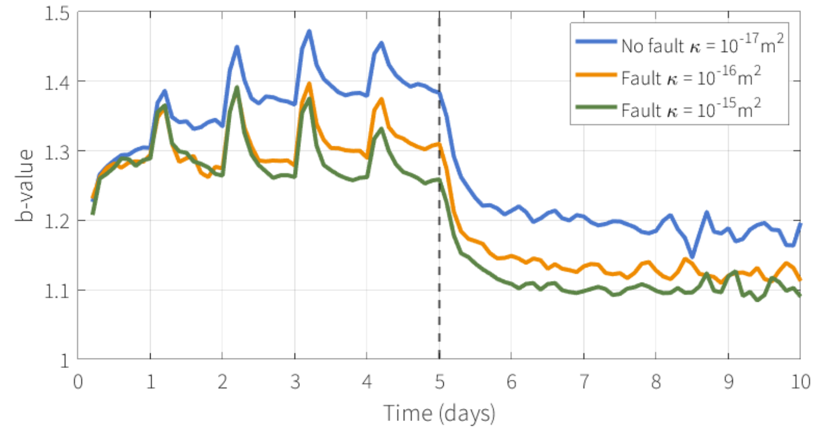


Figure S11.2. Step-increasing injection: temporal evolution of the b-value with different permeabilities of the fault (fixed time-window 0.2:0.1 days).

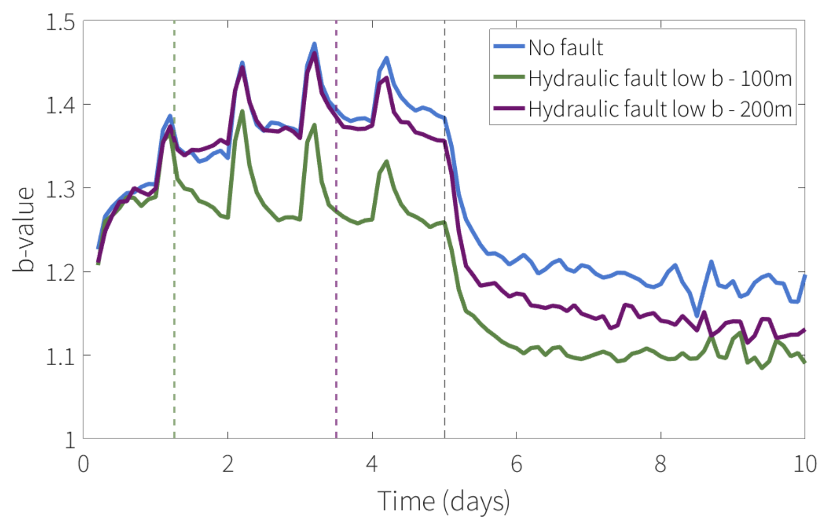


Figure S11.3. Step-increasing injection: temporal evolution of the b-value with the fault at different distances (fixed time-window 0.2:0.1 days).

- LINES 230-231. I like this idea. Ensemble forecasting is quite diffuse in many fields. Besides the weights, it is important how to create the ensemble; and this is not a trivial issue. No changes needed here, but just my opinion on this very important point.
  Thanks for the comment. We do think that ensemble modelling can provide benefits to forecasting but also to study the physics and relative weights of different processes behind induced seismicity.
- MAXIMUM SIZE OF INDUCED SEISMICITY. This section is rather fuzzy for me. It is not clear what is the message. If we would be able to set a maximum to the magnitude this would be a huge step ahead; but we cannot. It seems that the authors are saying that since we are not able to define a max magnitude, the monitoring of the b-value becomes more important. If so, this part can be shortened a lot.
  We have reworked the section to make clear that the maximum magnitude is as large as statistically expected (Van der Elst et al., 2016[4]; Gischig and Wiemer, 2013[5]) and a

parameter of the site that cannot be controlled, putting emphasis on the need to monitor the site to quantify this Mmax as soon as possible (for example with transient b-values). We also made clearer the role played by the injected volume in the determination of the expected size of seismicity and added a sensitivity analysis of the influence of the volume to the supplementary (Supplementary Figure S7).

"For a given regional tectonic context, the injected volume seems a logical first-order control on , and observations tend to agree in wastewater disposal and hydro-fracturing cases. Some models based on a direct control of the volume on the number of events have been proposed and used in near-real-time or to introduce risk informed thresholds. The injected volume can play a tremendous role on the size of the seismicity (Supplementary Figure S8). In this study, we chose to look beyond this first-order control with constant injection rates for all the cases presented. Indeed, the example of Pohang (2017, South Korea) demonstrates that there is more than just the volume at play, with only a fraction of the expected needed volume injected for an of magnitude M_w 5.5 (Grigoli et al., 2018[6], Woo et al, 2019[7])."

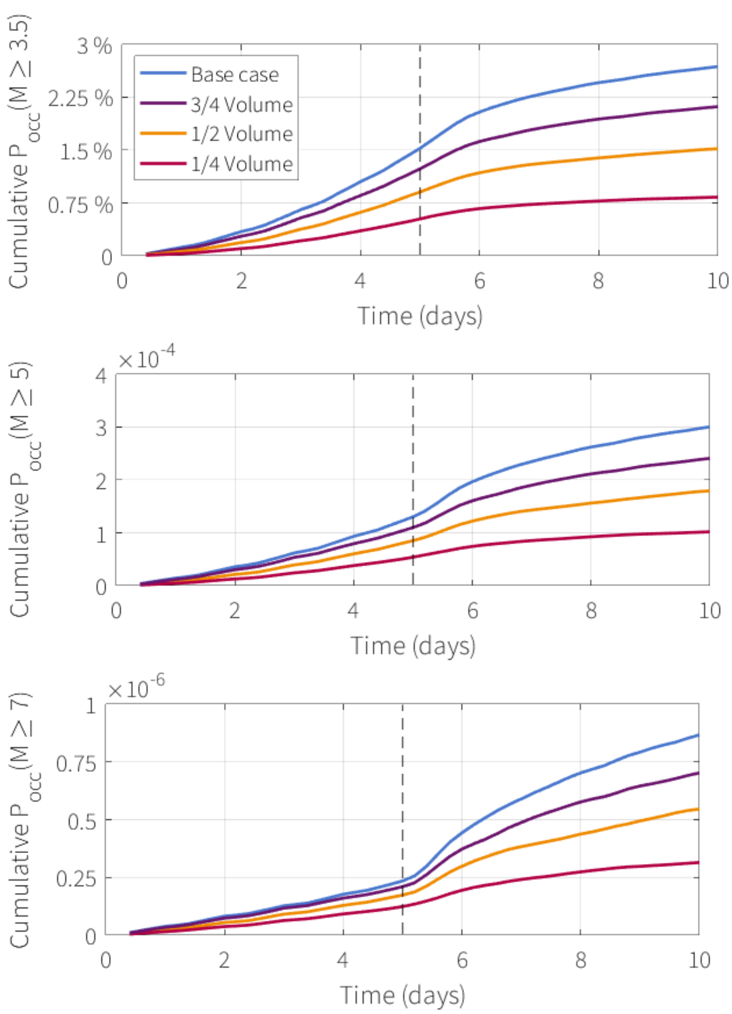


Figure S7. Cumulative probability of occurrence of a magnitude greater or equal to 3.5 (a), 5 (b) and 7 (c) for the base case without fault and fractions of the base case injected volume

- LINES 289-290. As said before, this conclusion seems a little bit too speculative.
  We have rephrased the sentence to make it clear that transient b-values on their own are not enough to detect faults, but that coupled with real-time analysis of the event location, changes in the b-value could help detect features.
  "In parallel to event localisation, monitoring the evolution of the b-value over time may enable detection of such a fault prior to major events and could lead to operational techniques noticing a deviation from an expected b-value pattern."

Reviewer # 3 8/6/2022

The authors present an extended and improved model (relative to the previously published study of Bachmann et al, 2012) for the Basel EGS project injection and initial post-shut in phases to model the occurrence and frequency-magnitude distribution of the project associated induced seismicity. The authors find clear spatiotemporal variation in the data which the model reproduces. The authors thus make a good case for a physical cause of the variations and subsequent implications for hazard and risk assessment and real-time monitoring in future similar cases. I find the paper overall well written, the research well presented, and a valuable addition I have a few concerns and some minor comments, as indicated below, but overall suggest a minor revision of the paper prior to accepting it for publication. The authors present a spatiotemporal development of the observed induced seismicity. The approach is based on binning of the data in time. I have a few concerns with respect to the approach:

- The approach basically only assesses a temporal evolution, as no spatial component is included in the analyses. Off course, given the spatial development of the seismicity with time, a spatial component is implicit. My question concerns the presence of the fault influencing the spatial development of the seismicity, as shown by the model and observed in the data (as stated in lines 74-75). To what extend would this spatial component affect the analysis, given the limitation to the temporal binning only?
  The spatial component is implicit in our model and has already been shown by Goertz-Allmann and Wiemer (2013)[8] and Bachmann et al. (2012)[9]. We observe similar results as they did in our model. Looking at different times, we see that the b-value close to the injection increases during the stimulation (increase relative to the reference post-shut-in b-value, indicative of retriggering) while the outer regions (and fault in the case with a fault) exhibit lower b-values, indicating that "new" seeds/fractures are being activated with their intrinsic b-values close to the regional value. The maps of the change in b-value at different times for the base case without and with fault has been added to the supplementary (see S4 and S5 below).
  To apply transient b-values monitoring to real-time detection of a fault, one would need to couple it with monitoring of the location of events, which is not trivial in real-time but not completely unrealistic, at least with preliminary locations. Shifts in the localisation of events could be noticed, such as a shift from 3D growth of the seismic cloud to 2D with potential lineaments. An example is the shift from three-dimensional to fault-clustered noticed by Herrmann et al., 2019[1] around December 5th 2006 in Basel which also corresponds to the drop in b-value at day 3 in Figure 1-b. We have added a sentence to the manuscript to add this observation of shift in the seismic cloud growth coinciding with the drop in b-value.
  "This drop in b-value in Basel from day 3 has been correlated to a shift from a three-dimensional spread of the seismic cloud to a two-dimensional growth aligned with the main fault by Herrmann and colleagues."

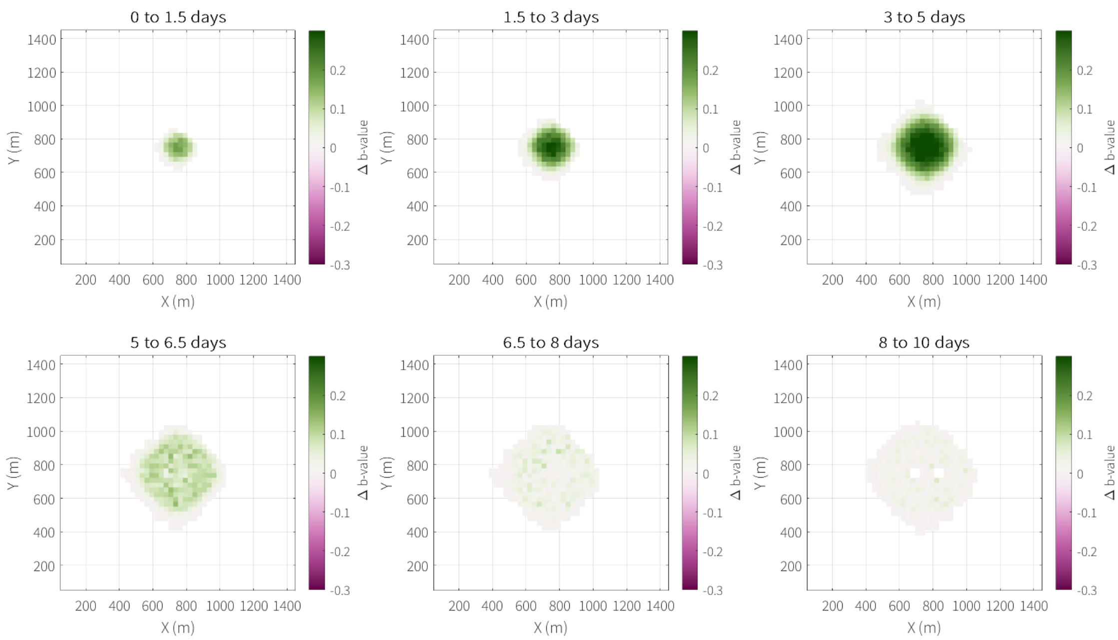


Figure S4. No fault case. Changes in b-value for different time period.

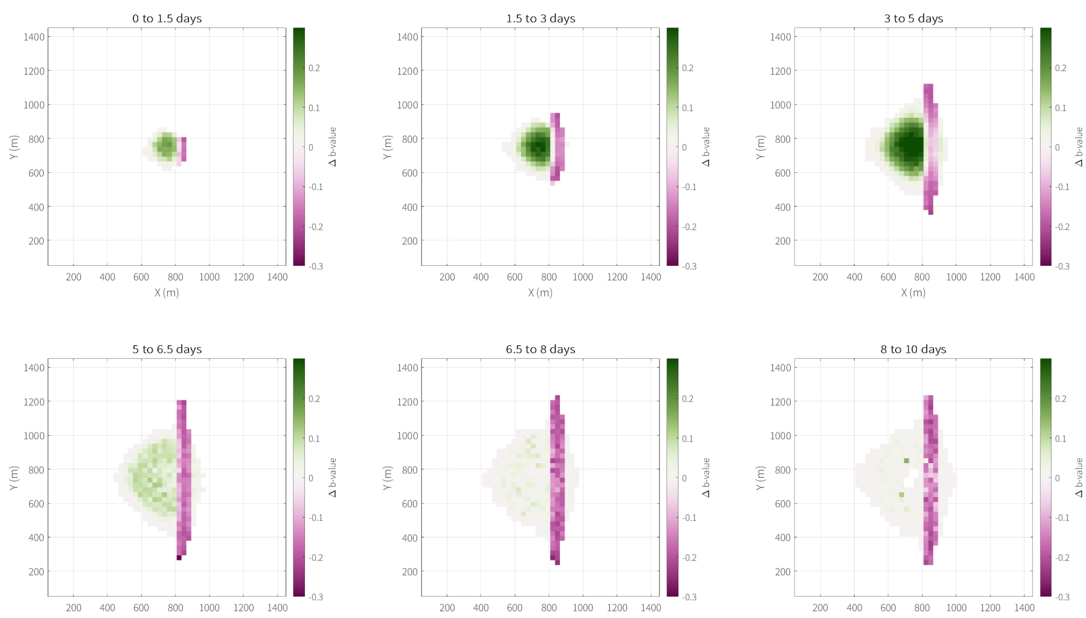
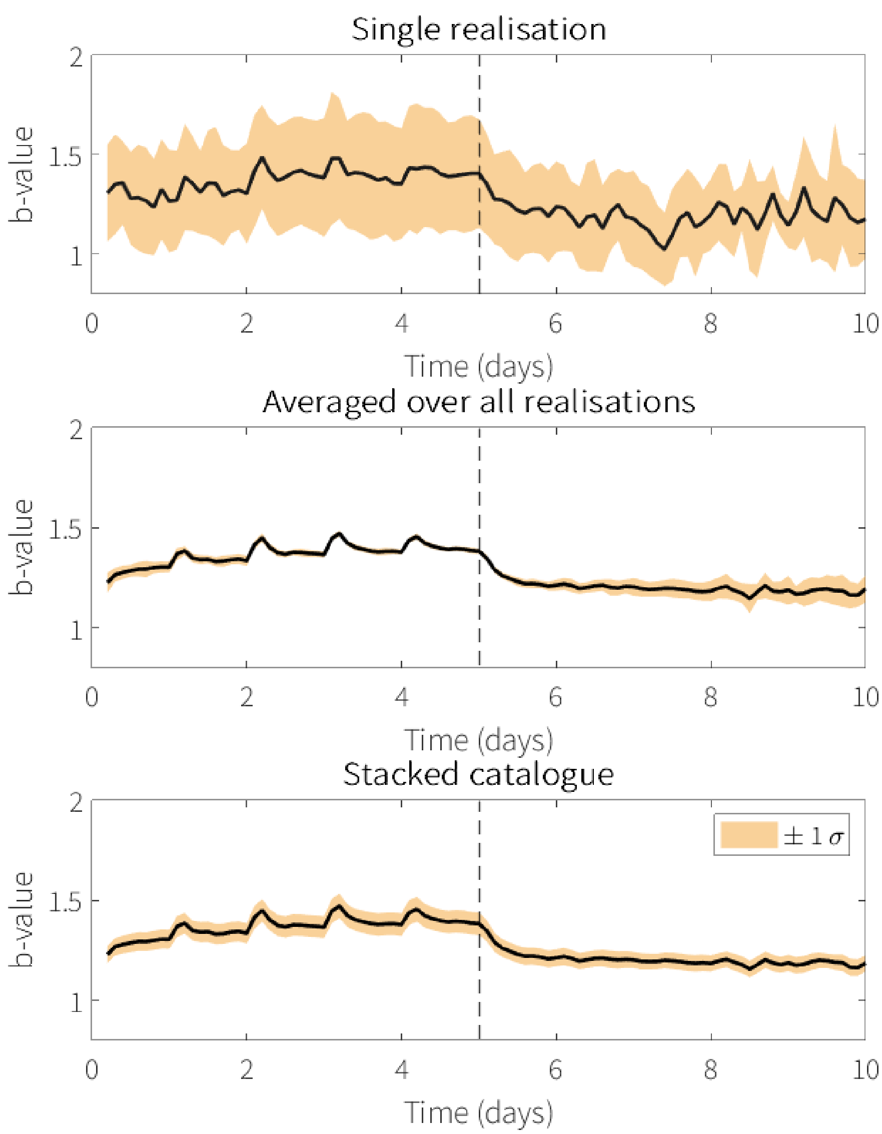


Figure S5. Fault case. Changes in b-value for different time period. To highlight the fault the changes in b-value are computed with the non-fault rock/stress properties as reference.

- In addition to this: the authors show the implication of binning the data in 0,2 day or 3000 event bins. Can the authors discuss why the binning size choice of 0,2 days was selected? What prior assumptions underlie this choice? Why differ from the previous choice?

The choice of 0.2 days bins is a pragmatic one allowing us to have regularity in the temporal distribution of our data points. The choice of Herrmann et al., 2019[1] to go with 3000 events bin is only made possible by the very large and more complete catalogue they created with template matching. In our model, we do not produce hundred of thousands of earthquake (which would be computationally very heavy), we have around 3000 events, which allows us to run the models in a reasonable time. Moreover, the b-value is not calculated from simulated magnitudes, but from the differential stress relationship and averaged over the events in the time-bin. We have confidence in the robustness of the b-values we show as we average over 48 catalogues (stochastic realisations) to smooth out outliers and get the signal of the b-value as strong as possible. The figure below shows the b-value and its $\pm$ one standard deviation for the base case without fault for one single realisation picked at random (top panel), the averaging over all realisations (middle panel) and when considering only one large stacked catalogue (bottom panel).



- The authors do not present any statistical evidence that the b-values are indeed statistically significantly different and could not still stem from the same background distribution. I would like to see evidence that the b-value during injection is indeed statistically significantly different from the background rate to which it returns after shut-in, as this is an important motivation for the whole study and recommendations in the discussion.
  For the models, we can separate the co-injection and post-shut-in phase, and look at the distribution of b-values of each population (Fig. 2-d, also presented below with mean and $\pm 1\sigma$ intervals as dashed lines). Then, we can use a T-test to evaluate their similarities. The null hypothesis is rejected with a p-value of 1.95e-4, confirming the statistically significant difference of the b-value during and after the injection phase.
  For the statistical analysis of the b-values observed in Basel, we would like to point out the very thorough analysis conducted by Herrmann et al., 2019[1]. We have added the p-value and statistical significance to the manuscript.
  Figure 3-d illustrates this natural state (dark) and the higher co-injection b-value distribution (light), driven up by repeating events. Using a T-test to evaluate the distributions of b-values co-injection and post-shut-in, we confirm the statistically significant difference of the b-value during and after the injection phase with a p-value of 1.95e-4.

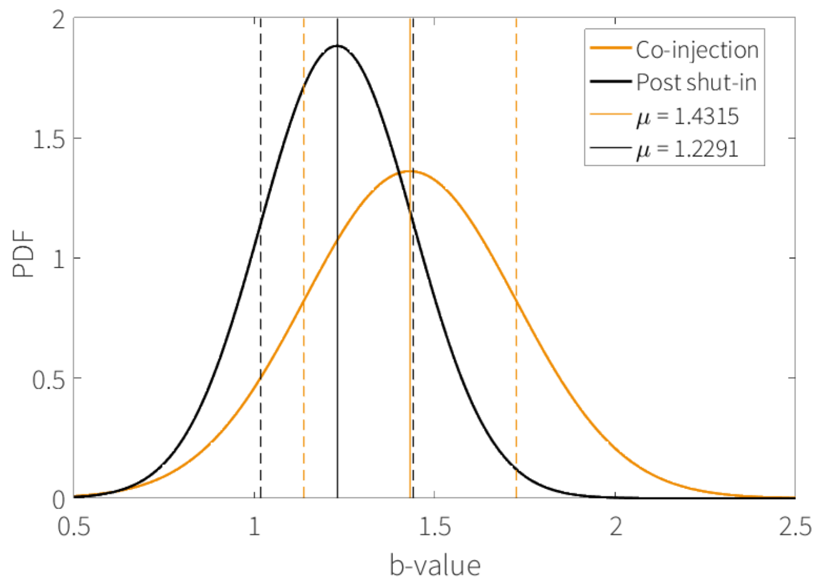


Figure 3-d. Distribution of the b-value co-injection and post-shut-in with mean (solid vertical lines) and $\pm 1\sigma$ (dashed vertical lines)

I find the idea of real-time tracking of the b-value interesting and the model implicates a validity for this. However, it is well known that deriving b-values, especially b-value variations from data is implicitly difficult as large numbers of events are required to derive reliable b-values from data (see e.g. Marzocchi et al, 2020). I feel the authors should fully address this issue when proposing the approach in section "Using the b-value for near-real-time hazard assessment and forecasting".
We have added a sentence to emphasise the need of robust and complete catalogues to assess the b-value in near-real time in the "Using the b-value for near-real-time hazard assessment and forecasting" section.
"This monitoring of the b-value, however, requires robustly generated magnitudes and generous

catalogues recorded and generated in near-real-time to provide trustworthy signals from the underground."

The paper assumes that the risk/hazard strongly depends on the b-value. This implicitly assumes that the maximum magnitude that could occur due to injection is as large as can be statistically expected (Van der Elst et al, 2016). I think it would be worthwhile to at least mention this and preferably verify, as this is a major contributor to the hazard/risk, even more so than the b-value. The result of the verification could be reported in the supplemental information.

We agree that injected volume plays a major role in the risk associated with a deep underground injection and have made the "Maximum size of induced seismicity" clearer to reflect this fact. We strongly show that the site conditions (or local b-value) is the main driver of risk/hazard. The maximum magnitude is a parameter of the site and is independent of the b-value, but the probability to have an event of magnitude M does depend on the b-value.

We conduct our study with the same total injected volume for every case in order to look beyond the volume control and compare different settings (injection, fault) without having to account for the change induced by different volumes. We do verify that the risk/hazard is indeed controlled by the injected volume by running the base case without fault with fractions of the injection rate and get lower transient probabilities of any given magnitudes (see figure below that has been added to the supplementary). By using the different rate simulations (three quarters, half and a quarter of the base case volume) we see that the probability of a magnitude greater or equal to M scales with the b-value and also a-value so the Mmax is as large as statistically expected.

We have also rephrase section "Maximum size of induced seismicity" :

"For a given regional tectonic context, the injected volume seems a logical first-order control on , and observations tend to agree in wastewater disposal and hydro-fracturing cases. Some models based on a direct control of the volume on the number of events have been proposed and used in near-real-time or to introduce risk informed thresholds. The injected volume can play a tremendous role on the size of the seismicity (Supplementary Figure S7). In this study, we chose to look beyond this first-order control with constant injection rates for all the cases presented. Indeed, the example of Pohang (2017, South Korea) demonstrates that there is more than just the volume at play, with only a fraction of the expected needed volume injected for an of magnitude M_w 5.5 (Grigoli et al., 2018[6], Woo et al., 2019[7])."

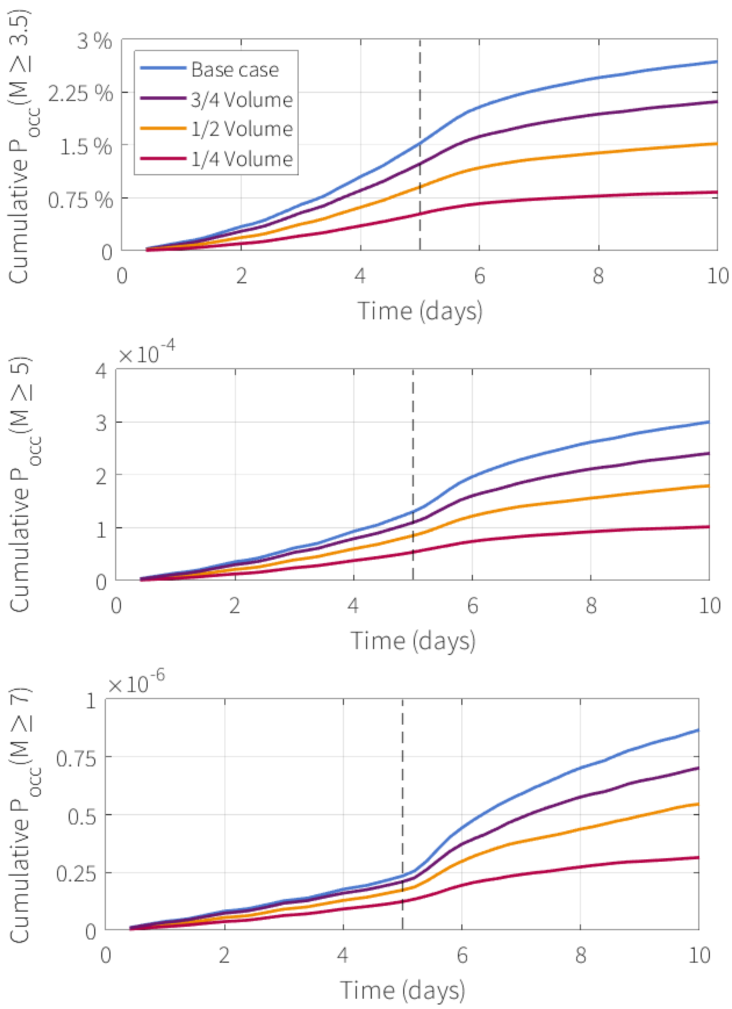


Figure S7. Cumulative probability of occurrence of a magnitude greater or equal to 3.5 (a), 5 (b) and 7 (c) for the base case without fault and fractions of the base case injected volume

In general, the quality of the text in the figures should be improved, it seems a bit blurry, and the legends and axis titles are barely readable.

All figures have been prepared for the revised version using the guidelines of the Nature journal group

Minor comments:

- Figure 1: Please include also the uncertainty band on the b-values derived so the reader can assess the statistical significance of the small scale variations compared to the larger trend.
  The uncertainty bands at ± one and two standard deviation of the b-value binned with a fixed time window has replaced Figure 1-b (the comparison of the fixed time window and fixed number of events has been moved to Supplementary Figure S1, also below).

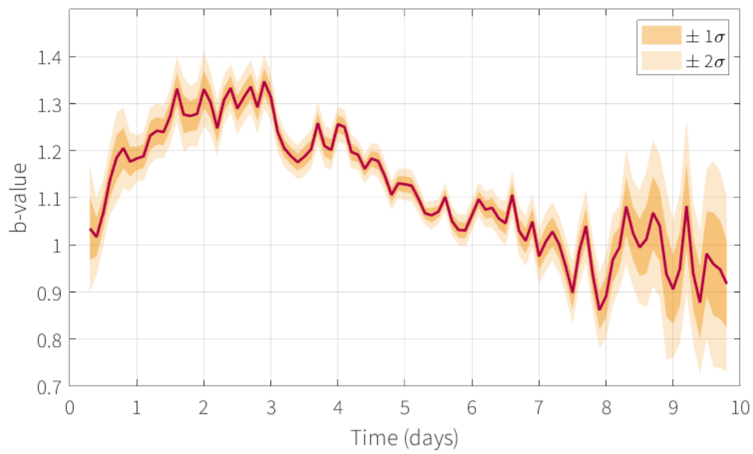


Figure 1-b

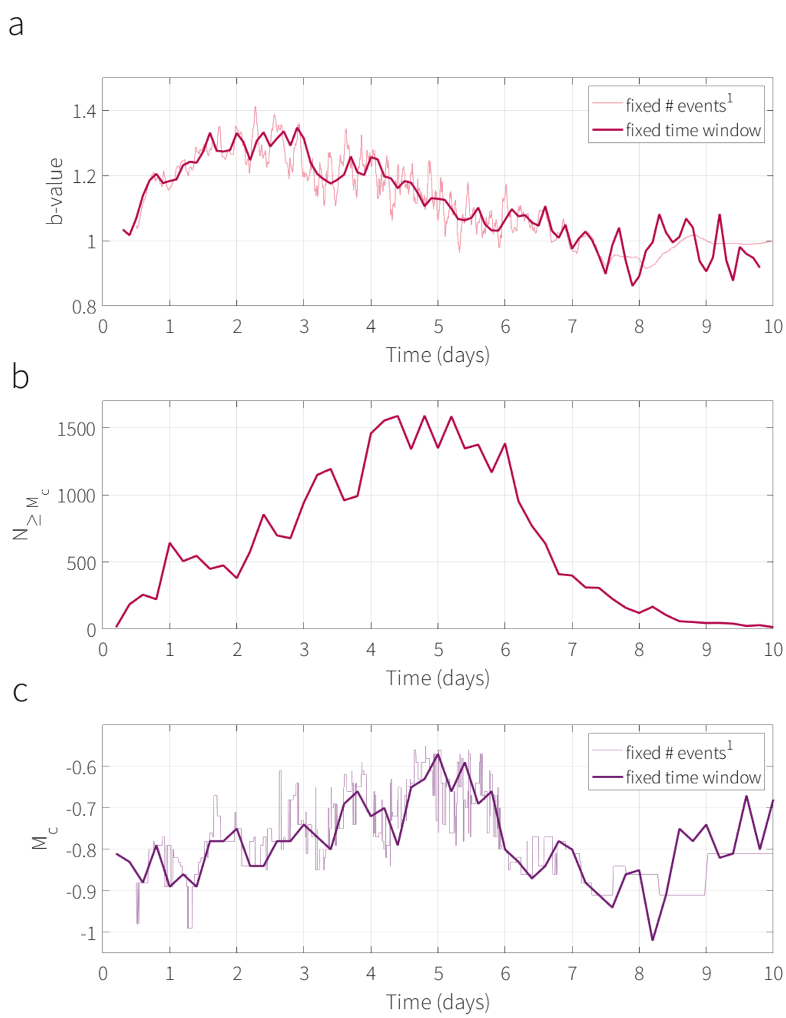


Figure S1

- Line 74-75: I suggest the authors include a figure of the spatial distribution of the seismicity showing the alignment with the fault zone. This will help your readers fully appreciate the complexity of the problem.
  Fig 2-g shows the map-view distribution of the seismicity (normalised over all realisations for the base case with fault). It highlights the clustering of seismicity on the fault. We have added the figure below to the supplementary material to show the distribution of seismicity in a vertical slice X-Z for the base case with and without fault

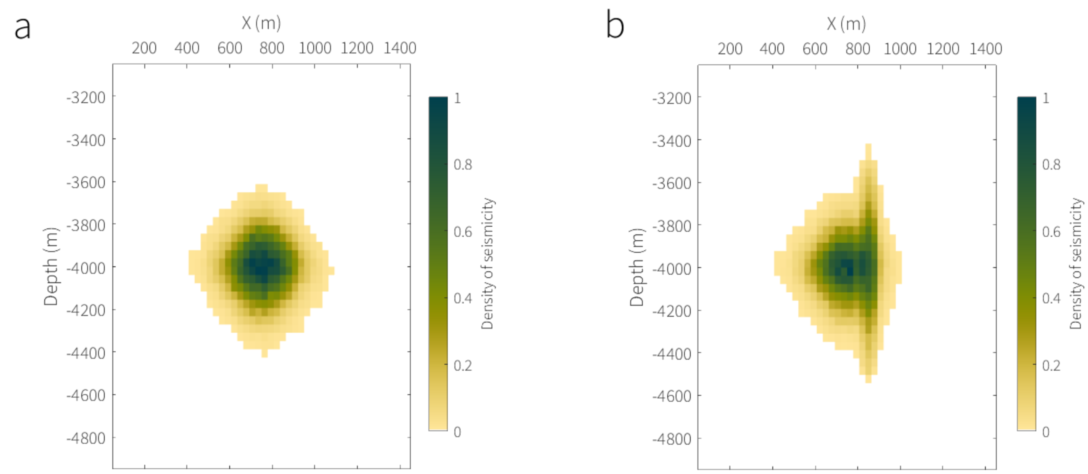


Figure S2

- Figure 2-c: Please explicate whether the figure represents the base case or the fault case. In the text please indicate you will address the difference between the two models next or later on in the paper.
  2c represents the base case without fault. We have changed Figure 2 to remove the confusion the R-T plots could create and have moved the R-T plot for the base case without and with fault to the supplementary material (S3). The fault case (b) clearly shows different triggering and back fronts of seismicity revealing its different hydraulic diffusivity from the host-rock.

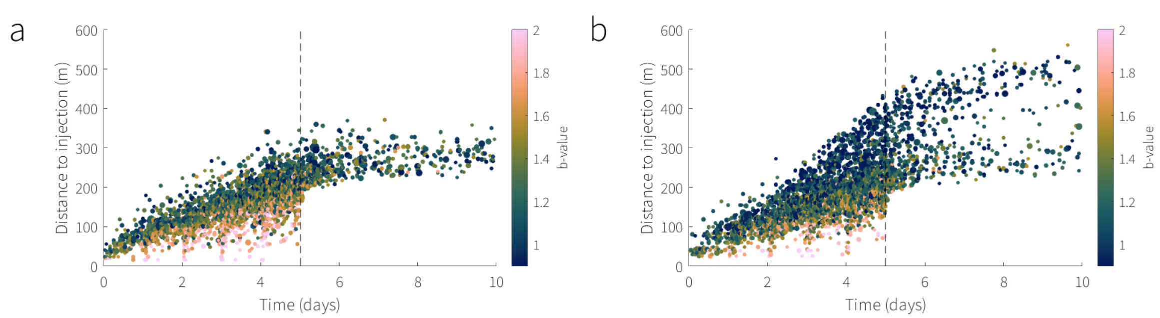


Figure S3. R-T plot for one realisation of the base case without a fault (a) and with a fault (b)

- Line 83: "The evolution of the pressure in both time Figure 2-a and space." The sentence feels unfinished. Please rephrase.
  The sentence has been reworked to be more understandable.

"The evolution of the pressure at different monitoring points (Figure 2-a) shows an initial sharp pressurisation close to the injection and further gradual pressurisations happening over time with the propagation of the pressure front and the surge in pressure associated with increases in the injection rate."

- Line 83-85: "Figure 2-d shows an initial sharp pressurisation close to the injection. Further gradual pressurisation happens over time with the propagation of the pressure front and the surge in pressure associated with increases in the injection rate." Figure 2-d is a snapshot after 10 days, hence does not show the initial sharp pressurisation nor a temporal development. I presume the authors mean Figure 2-a?
  Yes, the referencing should have pointed to Figure 2-a, thank you for catching that one.
- Line 85-87: "The permeability follows a similar pattern with an initial enhancement followed by successive increases after the injection rate increases, with each monitoring point further from the injection seeing a similar pattern, albeit with a delay." I would suggest to explicitly refer to Figure 2-b to assist the reader.
  reference to Figure 2-b has been added
- Line 111: "...resulting in a larger the number of seeds reactivated." Please remove the "the".
  extra "the" has been removed
- Line 209-219: Some references to TLS systems, there benefits and drawbacks should be included.
  We have added a pointer to Grigoli et al., 2017[10] which discusses in great length the advantages ans limitations of TLS.
- Line 220-223: "Approaches where seismic data are fed into multiple models from purely statistical to fully physical are weighted and evaluated to provide robust ensemble forecasting of induced seismicity that can then be computed into hazard and operational control" The sentence only gives only a description of the approaches, but what about them? If I place comma's, It would read "Approaches, where seismic data are fed into multiple models from purely statistical to fully physical, are weighted, and evaluated to provide robust ensemble forecasting of induced seismicity that can then be computed into hazard and operational control," and I have an incomplete sentence. Please rephrase.
  The sentence was indeed confusing and has be reformulated to be more understandable.
  "New approaches are being developed, where seismic data are fed into multiple models - from purely statistical to fully physical - which are then weighted and evaluated to provide robust ensemble forecasting of induced seismicity that can then be computed into hazard and operational control."
- Line 328-329: "The extent of the rupture area is based on the average expected magnitude from the power law distribution with the b-value of the given seed." For the sake of reproducibility, it would be good to explicate the relation between the rupture area (in 3D) and the average expected magnitude.
  The detail of the calculation of the area of stress redistribution has been added to the method.
  In the distributed case, the stress drop occurs within a radius defined by the magnitude of the reactivated seed. Equation 2 details the calculation of the rupture area according to Brune's model, with M_0 the seismic moment calculated from the picked magnitude

(Equation 3), and $\Delta\tau$ is the stress drop applied to the reactivated seed.

$$R_{eq} = \sqrt{\frac{7}{16}\frac{M_0}{\Delta\tau}} \tag{2}$$

$$M_0 = 10^{\frac{3}{2}M+9.1} \tag{3}$$

Reviewer # 1 - 24/8/22

The revised version solved all the questions very well. No further questions.

Reviewer # 2 - 24/8/22

The authors addressed satisfactorily my main concerns.

References

1. Herrmann, M., Kraft, T., Tormann, T., Scarabello, L. & Wiemer, S. A Consistent High-Resolution Catalog of Induced Seismicity in Basel Based on Matched Filter Detection and Tailored Post-Processing. *J. Geophys. Res. Solid Earth* **124**, 8449–8477, DOI: 10.1029/2019JB017468 (2019).
2. Häring, M. O., Schanz, U., Ladner, F. & Dyer, B. C. Characterisation of the Basel 1 enhanced geothermal system. *Geothermics* **37**, 469–495, DOI: 10.1016/j.geothermics.2008.06.002 (2008).
3. Woessner, J. & Wiemer, S. Assessing the quality of earthquake catalogues: Estimating the magnitude of completeness and its uncertainty. *Bull. Seismol. Soc. Am.* **95**, 684–698, DOI: 10.1785/0120040007 (2005).
4. van der Elst, N., Page, M., Weiser, D., Goebel, T. & Hosseini, S. Induced earthquake magnitudes are as large as (statistically) expected. *J. Geophys. Res. Solid Earth* **121**, 4575–4590, DOI: 10.1002/2016JB012850 (2016).
5. Gischig, V. S. & Wiemer, S. A stochastic model for induced seismicity based on non-linear pressure diffusion and irreversible permeability enhancement. *Geophys. J. Int.* **194**, 1229–1249, DOI: 10.1002/2013JB010597 (2013).
6. Grigoli, F. *et al.* The November 2017 M w 5.5 Pohang earthquake: A possible case of induced seismicity in South Korea. *Science* **5**, 1003–1006, DOI: 10.1126/science.aat2010 (2018).
7. Woo, J. U. *et al.* An In-Depth Seismological Analysis Revealing a Causal Link Between the 2017 MW 5.5 Pohang Earthquake and EGS Project. *J. Geophys. Res. Solid Earth* **124**, 13060–13078, DOI: 10.1029/2019JB018368 (2019).
8. Goertz-Allmann, B. P. & Wiemer, S. Geomechanical modeling of induced seismicity source parameters and implications for seismic hazard assessment. *Geophysics* **78**, KS25–KS39, DOI: 10.1190/geo2012-0102.1 (2013).
9. Bachmann, C. E., Wiemer, S., Woessner, J. & Hainzl, S. Statistical analysis of the induced Basel 2006 earthquake sequence: introducing a probability-based monitoring approach for Enhanced Geothermal Systems. *Geophys. J. Int.* **186**, 793–807, DOI: 10.1111/j.1365-246X.2011.05068.x (2011).
10. Grigoli, F. *et al.* Current challenges in monitoring, discrimination, and management of induced seismicity related to underground industrial activities: A European perspective. *Rev. Geophys.* **55**, 310–340, DOI: 10.1002/2016RG000542 (2017).