Peer Review File

Manuscript Title: A process-based approach to understanding and managing triggered seismicity

Editorial Notes:

Redactions – Third Party Material
Parts of this Peer Review File have been redacted as indicated to remove third-party material.

Redactions – unpublished data
Parts of this Peer Review File have been redacted as indicated to maintain the confidentiality of unpublished data.

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

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Referee #1:

Seismicity triggered by wastewater injection into a deep well in the Val d'Agri oil field has been the subject of both numerous scientific studies and public concern. New approaches to manage the hazard are thus important to both develop and test. This study leverages unusually complete information on the oil field to create a detailed process-based geomechanical model to forecast induced seismicity rates. The model predicts that almost all areas within the Val d'Agri oil field are stabilized by production, with the exception of the Costa Molina Fault (CMF), the host of most of the events since 2006 induced by injection into the Costa Molina 2 disposal well (CM2). History-matching of operational data and seismicity were used to develop a forecast model based on Rate-and-State theory. According to the authors, maintaining the future injection rate in CM2 to less than 2000 m3/d will avoid nuisance seismicity in the future. (It might be noted that this disposal volume is very modest compared with wells in the U.S.)

Comparing the results and conclusions of this study with earlier investigations of induced seismicity at Val d'Agri and with other disposal related cases raises a number of questions in the mind of this reviewer. One paper, in particular, on Val d'Agri suggests a different and perhaps more straightforward means for managing the hazard. This unreferenced paper, "A detailed analysis of wastewater-induced seismicity in the Val d'Agri oil field (Italy)" by L. Improta et al. (Geophysical Research Letters, 2015) presents detailed information on the spatial and temporal evolution of the seismicity and its correlation with injection parameters. They found that the most intense periods of seismic activity on the CMF correlate with peak injection pressure in CM2. Reduction of wellhead pressure largely quenched the seismicity.
Their Figure 4c displays the wellhead pressure and volume rate time series from 2006 – 2014. In mid-2010 before the most intense seismic sequence later that fall, the volume rate was approximately constant, but the pressure rose rapidly, peaking at the most intense part of the seismic swarm. Injection resumed after a several month-long shut-in 2011, with both pressure and volume rate increasing to the next spike in pressure and the next significant burst of seismicity. These examples show that seismicity management requires careful attention to pressure. It should also be noted that the simulated pressure history for CM2 shown in Figure S8a

more closely resembles the volume curve from Improta et al. Figure 4c than the pressure, calling into question the calibration.

There is also a suggestion in the pressure data presented by Improta et al. of the Kaiser effect – seismicity returns when the pressure exceeds the previous maximum. The same effect can be seen at the Paradox Valley Project, Colorado, USA, where saline brine is injected into a deep disposal well. Attempts to manage the seismicity by controlling injection rate, with scheduled shut-downs to allow pressure to diffuse, as described by Ake et al., (2005) succeeded for some years, but ultimately did not prevent the occurrence of a MW 4.0 in 2013 8 km from the well and a MW 4.5 in 2019 within a few hundred meters of the casing shoe. Mitigation by volume management alone failed to keep downhole pressure from rising triggering earthquakes that threaten to shutter the project (see also King et al, 2015).

While this paper focuses on management of seismicity on the CMF due to injection into CM2, changes in the Coulomb Failure Stress (CFS) are determined throughout the oil field. Figure 3a shows that, with the exception of the CMF and a few minor nearby locations, faults in the field are stabilized by production. Minor seismicity, with magnitudes up to M 3 occur within the footprint of the Val d'Agri field according to the INGV catalog. Although depths are unreliable (known events on the CMF commonly appear at 10 km in the catalog), it raises the question if the prediction of the model (declining seismicity beneath most of the field) is supported by the data. An affirmative answer to this question would bolster confidence in the CFS model presented here.

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Referee #2:

A: Summary of key results: This paper represents a breakthrough in the application of coupled, multi-physics numerical simulation to understand and forecast induced seismicity related to fluid withdrawal and injection into the subsurface.

B. Originality and significance: A wealth of original data, as well as the use of calibration and forecasting time intervals, are key, novel strengths of this work. The results are of major significance and high potential impact, especially inasmuch as a forecast model is available for future testing and model validation. However, I am not convinced that the authors' claim that triggered seismicity has been "managed" is fully supported, because (unlike the Rangely experiment, during which reservoir pressure was deliberately cycled above and below the forecasted seismicity triggering threshold), during the validation time interval in this study seismicity is virtually quiescent.

C-D. Data, Methodology, Uncertainties. The complex methodology is described cogently and completely, enabling this approach to be applied (at least in principle) by other research groups to cases where sufficient data are available. Overall, the methodology constitutes a monumental step forward, but I have a number of detailed comments as enumerated below.

1. Despite claims that this study surpasses the Rangely experiment, in that case deliberate cycling of reservoir pressure was applied. In the present study, it is not apparent that any deliberate management of induced seismicity was undertaken by the operator - rather it is both a retrospective and forward-looking analysis. Moreover, seismicity has been effectively quiescent since 2014. Therefore the validation period does not include active seismicity (unlike the Rangely experiment). This leads me to question whether seismicity really has been *managed*, as claimed.

2. Based on Figure 4, there are two time intervals during which the model seems to be inconsistent with the model:

a) An uptick in seismicity on the CMF occurred in 2011, following a pause to injection in well CM2.

After renewal of injection (and consequent increase in the Delta CFF level), the Delta CFF level does not surpass previous levels as described in the paper, yet seismic activity occurred.
b) A subsequent increase in injection rate in late 2012 yielded a record-breaking high level of Delta CFF, leading to a predicted increase in seismicity according to the model. However, this increase was not observed.

Overall, the authors focus on tuning their model such that the net cumulative seismic moment fits the observations, however some discussion of these apparent inconsistencies is warranted.

E. Conclusions: robustness, validity, reliability. The approach presented here robust and well documented, enabling repeatability, in principle. As noted above, my principal reservation is that the claim that induced seismicity has been "managed" seems over-stated.

F. Suggested improvements:

3. Figures and captions should be improved. In Figure 1, legend annotation below part b is not legible (font too small for figure resolution). It would be helpful to show the injection depth for well CM2 (bottom of the well?). Cross section A-B appears to be oblique to structural fabric, which makes it difficult to assess the true dip of faults. In the caption to Figure 2, does “reservoir contour” mean “surface outline of the oil field”? The content from Figure 3a (Delta CFF on faults) could be displayed in Figure 2 without loss of clarity (the use of colours to distinguish different faults in Figure 2 is unnecessary). Also, the clipped colour scale for Figure 3 is unsatisfactory.

4. How can we be confident that all potentially seismogenic faults are included in the model, given that the Costa Molina Fault was unknown prior to being activated by injection? Some discussion of this source of uncertainty is warranted.

5. Please define “triggered” seismicity. This term is variously defined in the literature, sometimes (but not always) to indicate fault movement that is due to an anthropogenic perturbation but primarily releases tectonic stress.

6. The modelling assumes that the S3 direction is perpendicular to normal faults, but Figure 1 shows that this is not exactly the case.

7. Showing at least one example of 3-D seismic data used to construct the model, to illustrate the seismic expression of faults, would help to advance foundational science discussed in this work.

8. Please convert to SI units of volume/rate, or at least show the conversion.

9. Statements that seismicity occurs when Delta CFF rises above its previous maximum value require further substantiation as this does not follow directly from the Coulomb criterion (and in fact differs from the conclusions from the Rangely experiment, where a threshold reservoir pressure was found). Is this due to the Kaiser effect? If so, this should be stated explicitly.

Minor comments:

10. Line 39: For clarity and completeness add “monitoring” to the statement about regulatory frameworks.

11. Line 109, “hydrocarbon thickness” should be “reservoir thickness”.

12. Line 241, it seems odd that only the optimal friction coefficient (0.55) is repealed here in the context of “different combinations of friction parameters”.

13. Line 268 “Operations” should be replace by “Oil and gas production” (since injection is also

part of the operations).

14. Line 464, elaborate on how relative slip is calculated. It is claimed to be based on Mohr-Coulomb, but that alone does not predict the amount of slip.

G. References: Appropriate credit is given to previous work.

H. Clarity and context. Abstract, Introduction and Conclusions are lucid and accessible to a general scientific audience.

David Eaton
University of Calgary

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Author Rebuttals to Initial Comments:

Amendments in Response to the Evaluation and Review from Referee #1:

The Referee states that "new approaches to manage the [induced seismicity] hazard are important to both develop and test," and that our study "leverages unusually complete information on the oil field to create a detailed process-based geomechanical model to forecast induced seismicity rates." We agree with the Referee on the vital importance on new approaches to induced seismicity, and their validation with field data—and of course agree that our study provides an extraordinary dataset to accomplish this. He/she raises, however, several substantive comments. We thank the Referee for his/her comments on our manuscript, some of which—as it will be apparent in this response—have been instrumental in strengthening the work, and indeed providing extraordinary evidence of the power of our multidisciplinary models in explaining and managing not only fault-scale induced seismicity, but also field-scale seismicity.

In what follows we respond to the Referee's specific comments (in *blue italics*), noting where we have made amendments to the manuscript.

Seismicity triggered by wastewater injection into a deep well in the Val d'Agri oil field has been the subject of both numerous scientific studies and public concern. New approaches to manage the hazard are thus important to both develop and test. This study leverages unusually complete information on the oil field to create a detailed process-based geomechanical model to forecast induced seismicity rates. The model predicts that almost all areas within the Val d'Agri oil field are stabilized by production, with the exception of the Costa Molina Fault (CMF), the host of most of the events since 2006 induced by injection into the Costa Molina 2 disposal well (CM2). History-matching of operational data and seismicity were used to develop a forecast model based on Rate-and-State theory. According to the authors, maintaining the future injection rate in CM2 to less than 2000 m3/d will avoid nuisance seismicity in the future. (It might be noted that this disposal volume is very modest compared with wells in the U.S.)

In the US, wells with injection greater than 300,000 bbl/month (1,590 m3/d) are considered high-rate injection wells (Weingarten et al., *Science* 2015). The injection rate in the CM2 well has been in the range 2,000 m3/d to more than 3,000 m3/d. Indeed, the paper by Improta et al. (*GRL* 2015), brought up by the Referee and which we discuss in detail below, states "Hence, CM2 can be classified as a long-term, high-rate disposal well [Frohlich, *PNAS* 2012]." Thus, while it is true that some wells in the US are very-high-rate, the CM2 disposal is not modest according to established usage.

Comparing the results and conclusions of this study with earlier investigations of induced seismicity at Val d'Agri and with other disposal related cases raises a number of questions in the mind of this reviewer. One paper, in particular, on Val d'Agri suggests a different and perhaps more straightforward means for managing the hazard. This unreferenced paper, "A detailed analysis of wastewater-induced seismicity in the Val d'Agri oil field (Italy)" by L. Improta et al. (Geophysical Research Letters, 2015) presents detailed information on the spatial and temporal evolution of the seismicity and its correlation with injection parameters. They found that the most intense periods of seismic activity on the CMF correlate with peak injection pressure in CM2. Reduction of wellhead pressure largely quenched the seismicity.

Because Luigi Improta has been so helpful to us, he was the first person listed in the Acknowledgements (now highlighted in yellow), and we are well aware of his work. Three of his

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papers are (highlighted) in our main reference list. We chose not to include the 2015 paper mentioned by the Referee because the most important results are superseded by the 2017 paper that we do reference (see figure below, extracted from Improta et al., *JGR* 2017).

[Figure redacted - third party material]

Their Figure 4c displays the wellhead pressure and volume rate time series from 2006 – 2014. In mid-2010 before the most intense seismic sequence later that fall, the volume rate was approximately constant, but the pressure rose rapidly, peaking at the most intense part of the seismic swarm. Injection resumed after a several month-long shut-in 2011, with both pressure and volume rate increasing to the next spike in pressure and the next significant burst of seismicity. These examples show that seismicity management requires careful attention to pressure. It should also be noted that the simulated pressure history for CM2 shown in Figure S8a more closely resembles the volume curve from Improta et al. Figure 4c than the pressure, calling into question the calibration.

[Figure redacted - third party material]

To facilitate the discussion, we reproduce Figure 4c of Improta et al. (*GRL* 2015) above, with the time interval in question highlighted in blue. Contrary to the Referee's assertion, the injection rate (blue line) increased by an amount comparable to the pressure (red line) during this period. After the first arrow, the pressure falls dramatically despite the injection rate not changing much. Also, note that the relative pressure "peak" at the second arrow is not nearly as prominent as the corresponding peak in injection rate.

At the time of the first arrow, operators realized that the efficiency of the CM2 injection had decreased such that it took more pressure to inject at the same rate. The pressure dropped rapidly because the operators carried out an "acid job" to clean out the near-well blockage. This is a typical activity, routinely performed to maintain wells performance. While CM2 well-head pressures are readily and immediately available, they are not accurate proxies for the pressures on CMF that trigger earthquakes, because are strongly influenced by wellbore flowing conditions and near-wellbore effects that are highly variable in time.

We believe that the Referee would agree with us that the key trigger of seismicity is the pressure increase *on the CMF*, not at the well-head. Thus, all our work is ultimately geared towards determining the pressure evolution on the fault where the events occur.
Our fluid-flow simulations provide the pressure evolution based on unusually detailed knowledge of the subsurface permeability and porosity, as well as a comprehensive history matching of pressure at tens of wells throughout the field over a period of decades. Ultimately, our model established a quantitative, time-varying link between the injection rate at the CM2 well (the "control") and the fluid pressure at the CMF (the "response").

Regarding the comment on Figure S8 (now Figure S9 in the revised manuscript), the green line reproduces the average pressure of the cell that contains the CM2 well completion and is therefore not directly comparable with the top-hole pressure (THP). For this reason, it is part of the best practices of reservoir engineering to calibrate reservoir models only with pressure measured at bottom hole (BHP) during shut-in periods of the wells (red dots in Figure S9), as we have done in our models. To make this clear, we have slightly modified the following sentence in the manuscript:

L 206 – 207: "We *calibrate* our models 1 Jan. 1993 – 31 Dec. 2016 using field production and injection rates and matching well-bottom pressures, surface deformation, and seismicity.")

In summary, we believe that our modeling approaches represent a significant step forward in understanding and reproducing induced and triggered seismicity. Indeed, in the words of the second referee, "a wealth of original data, as well as the use of calibration and forecasting time intervals, are key, novel strengths of this work", and our paper "represents a breakthrough in the application of coupled, multi-physics numerical simulation to understand and forecast induced seismicity."

We have modified the manuscript in a number of places to try to make clear that it is pressure changes on the faults, not injection rates, that are the source of the DCFF that triggers earthquakes. (Line numbers refer to the revised manuscript.):

L 41 – 45: Field experiments quantifying the effects of increased pore fluid pressure on triggering earthquakes carried out in the early 1970's at the Rangely, Colorado (USA) oil field[4] suggested that, if conditions in the subsurface are known sufficiently well, seismicity can be turned on or off by cycling subsurface fluid pressure above or below a threshold.

L 79 – 80: We calibrate these models based on observations through 2016, arriving—from multiple modeling approaches—at predictions of injection rates that keep pressures on the CMF from exceeding previous maxima.

L 225-227: Most seismicity occurs when CMF pressure increases such that ΔCFF rises above its previous maximum and the seismicity rate drops when ΔCFF decreases, consistent with the Kaiser effect.

L 248-250: The model predicts an increase in seismicity rate after the rapid pressure increase following shut-in in 2011, although the observed seismicity rate is somewhat higher (Figure S5).

L 271-274: In contrast to studies that rely on proxies such as wellhead pressures, our models enable physics-based calculations of reservoir volume changes and resulting stress and pressure changes on faults that are consistent with reservoir properties constrained by decades-long, field-specific history matching.

L 283-288: Injection of water at the CM2 well, close to a fault connected with the CMF, led to pressure increases that destabilized the CMF, triggering a sequence of events with $M_L \leq 2.0$, too small to be felt. Our model predictions of variations in seismic activity on the CMF in response to variations in Coulomb stress, primarily due to changes in pressure, match the observed variations remarkably well; 3) Most seismic events in our models occur when pressure on the CMF increases sufficiently that ΔCFF exceeds its previous maximum, consistent with the Kaiser effect.

There is also a suggestion in the pressure data presented by Improta et al. of the Kaiser effect – seismicity returns when the pressure exceeds the previous maximum. The same effect can be seen at the Paradox Valley Project, Colorado, USA, where saline brine is injected into a deep disposal well. Attempts to manage the seismicity by controlling injection rate, with scheduled shut-downs to allow pressure to diffuse, as described by Ake et al., (2005) succeeded for some years, but ultimately did not prevent the occurrence of a MW 4.0 in 2013 8 km from the well and a MW 4.5 in 2019 within a *few hundred meters of the casing shoe. Mitigation by volume management alone failed to keep downhole pressure from rising triggering earthquakes that threaten to shutter the project (see also King et al, 2015).*

We agree that our results are consistent with the Kaiser effect and have added mention of the Kaiser effect to the manuscript, as well as adding a reference to Kaiser's paper.

L 225-227: Most seismicity occurs when CMF pressure increases such that ΔCFF rises above its previous maximum and the seismicity rate drops when ΔCFF decreases, consistent with the Kaiser effect.

L 287-289: 3) Most seismic events in our models occur when pressure on the CMF increases sufficiently that ΔCFF exceeds its previous maximum, consistent with the Kaiser effect.

While this paper focuses on management of seismicity on the CMF due to injection into CM2, changes in the Coulomb Failure Stress (CFS) are determined throughout the oil field. Figure 3a shows that, with the exception of the CMF and a few minor nearby locations, faults in the field are stabilized by production. Minor seismicity, with magnitudes up to M 3 occur within the footprint of the Val d'Agri field according to the INGV catalog. Although depths are unreliable (known events on the CMF commonly appear at 10 km in the catalog), it raises the question if the prediction of the model (declining seismicity beneath most of the field) is supported by the data. An affirmative answer to this question would bolster confidence in the CFS model presented here.

The Referee makes an excellent point. We have taken it very seriously and, by addressing it fully, we believe that it has led to an even stronger case for the power of our models with respect to seismicity at the fault scale *and* at the field scale.

The answer to the Referee's question (whether the generalized decrease in CFF in the many extensive faults throughout the field has actually led to a decrease in seismicity) is indeed affirmative. To show this, we have undertaken two analyses:

1. *An analysis of the evolution of DCFF in all the field faults.* For each fault in the model, we computed the evolution of the DCFF. In the figure below, we show the DCFF distribution (histogram) at discrete times for the Caldarosa fault and the MF-MDF faults, as representative examples. The DCFF distribution is complex, reflecting the complex reservoir architecture and well locations, but the trend is that the histogram shifts to the left (negative DCFF) as time progresses.

[Figure redacted - unpublished data]

We then combine the DCFF values at individual faults to obtain an overall DCFF distribution at all the faults in the model. From this distribution we compute the mean DCFF and its standard deviation for all the model faults. In the figure below, we plot the mean +/- standard deviation of the DCFF distribution over time for all faults in the field combined. We do so with a higher time resolution, from 1992 to 2019. This plot demonstrates quantitatively that DCFF in the field faults is overall decreasing during this period as a result of production.

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[Figure redacted - unpublished data]

2. *An analysis of the seismicity within and around the field.* We determined all the earthquakes of magnitude M_L> 2.4 within a 5-km buffer region surrounding the outline of the Val d'Agri field. We chose a magnitude threshold of 2.4 because it guaranteed the completeness of the earthquake catalog throughout the period of interest, 1986-2019 (Stabile et al., *BGTA* 2015). We chose a buffer region around the field because this is a reasonable choice for the area subject to pressure and stress perturbations from reservoir operations (Hauksson et al., *TLE* 2015).

We combine the results of both analyses in the figure below, which shows the clear relationship between the decrease in CFF and the concomitant decrease in seismicity rate.

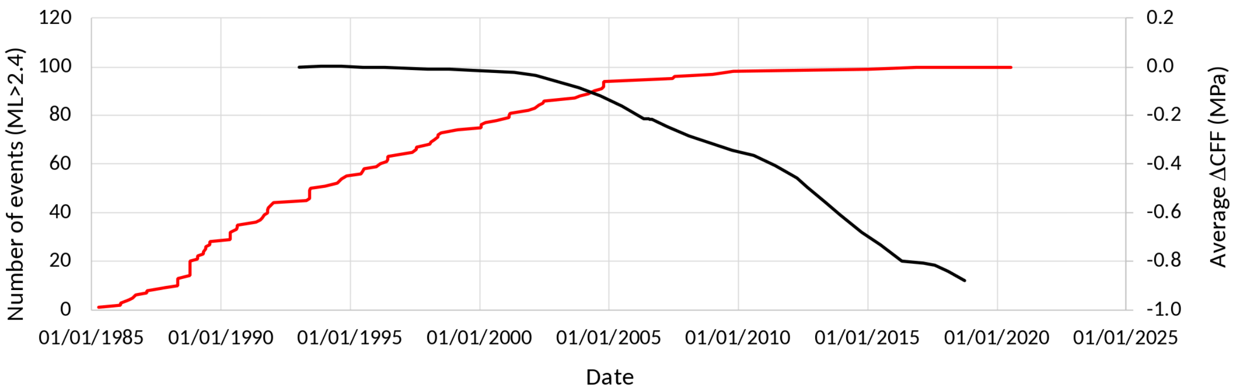

Figure 3b: Cumulative event count (red curve, left scale) and average DCFF on all model faults (black curve, right axis) plotted as a function of time. Events included are those from a region within a 5-km buffer region surrounding the reservoir boundary, with ML >2.4, the magnitude of completeness of the earthquake catalogue [Stabile et al., 2015].

While the emphasis of the current paper is on managing seismicity in the CMF triggered by injection, we believe that this unambiguous relation between DCFF and decline in field seismicity "bolsters confidence in the CFS model presented here," as suggested by the Referee. Therefore, we have added the figure above as a new panel in Figure 3 of the manuscript. We have also added a brief discussion in the main text:

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L 213-216: Production causes pressure declines in most of the reservoir and underlying aquifer, stabilizing almost all fault segments. The regional model predicts decreases in CFF from 0.5 MPa in the south to 1.5 MPa in the north (Figure 3a), with ΔCFF rates > 10 times tectonic loading rates (see Methods). The seismicity rate in the vicinity of the field is also observed to decrease (Figure 3b).

References cited in this response

Frohlich, C. (2012), Two-year survey comparing earthquake activity and injection-well locations in the Barnett Shale, Texas, *PNAS* 109 (35), 13934-13938.

Hauksson, E., T. Gobel, J.-P. Ampuero, and E. Cochran (2015). A century of oil-field operations and earthquakes in the greater Los Angeles Basin, southern California, *The Leading Edge*, June, 650-656.

Improta, L., L. Valoroso, D. Piccinini, and C. Chiarabba (2015), A detailed analysis of wastewater-induced seismicity in the Val d'Agri oil field (Italy), *Geophys. Res. Lett.* 42, doi:10.1002/2015GL063369.

Improta, L., S. Bagh, P. De Gori, L. Valoroso, M. Pastori, D. Piccinini, C. Chiarabba, M. Anselmi and M. Buttinelli (2017), Reservoir structure and wastewater induced seismicity at the Val d'Agri oilfield (Italy) shown by three-dimensional Vp and Vp/Vs local earthquakes tomography. *J. Geophys. Res. Solid Earth*, 122(11), 9050-9082.

Kaiser, J., (1953), Kenntnisse und Folgerungen aus der Messung von Geräuschen bei Zugbeanspruchung von metallischen Werkstoffen, *Arch. Für Isenhütten-Wesen*, 24, 43–45.

Stabile, T.A., A. Giocoli, A. Perrone, S. Piscitelli, l. Telesca, and V. Lapenna (2015), Relationship between seismicity and water level of the Pertusillo reservoir (southern Italy), *Bollettino di Geofisica Teorica ed Applicata*, 56(4), pp. 505-517, doi: 10.4430/bgta0161.

Weingarten, M., S. Ge, J. W. Godt, B. A. Bekins, and J. L. Rubinstein (2015), High-rate injection is associated with the increase in U.S. mid-continent seismicity, *Science* 348, 1336-1340.

Amendments in Response to the Evaluation and Review from Referee #2:

The Referee states that our paper "represents a breakthrough in the application of coupled, multi-physics numerical simulation to understand and forecast induced seismicity related to fluid withdrawal and injection into the subsurface," that "the results are of major significance and high potential impact," and that "the methodology constitutes a monumental step forward." We thank the Referee for this summary and for the positive assessment on the novelty, impact and rigor of our work—an assessment with which we agree.

The Referee, however, expresses that he is unconvinced about "the authors' claim that triggered seismicity has been 'managed' " because during the validation time interval in our study seismicity is virtually quiescent. He points to the Rangely experiment as an example of seismicity management, in the sense that the reservoir pressure was deliberately cycled up and down to trigger seismicity "on demand". In what follows we respond to this and other comments raised by the Referee. Since some of the authors of the present manuscript have recently revisited the original Rangely earthquake-control experiment using quantitative numerical modeling (Byrne et al., *GRL* 2020), we are in a position to offer a perspective on the fundamental advances of the present manuscript.

In what follows, we respond to the Referee's specific comments (in *blue italics*), noting where we have made amendments to the manuscript.

*1. Despite claims that this study surpasses the Rangely experiment, in that case deliberate cycling of reservoir pressure was applied. In the present study, it is not apparent that any deliberate management of induced seismicity was undertaken by the operator - rather it is both a retrospective and forward-looking analysis. Moreover, seismicity has been effectively quiescent since 2014. Therefore the validation period does not include active seismicity (unlike the Rangely experiment). This leads me to question whether seismicity really has been *managed*, as claimed.*

Although we understand the Referee's point of view, we disagree with his interpretation of "managed." In the context of an active oil field in a populated seismically active region, *"managing" seismicity means designing operations such that earthquakes do not occur.* This is not a pure scientific experiment in an unpopulated area, where one could hypothetically demonstrate the ability to "control" earthquakes by triggering them, as well as avoiding triggering them (as we have done here). There is a valid reason that our title refers to "management," while the title of the Rangely study refers to "earthquake control."

To emphasize this difference, we added the following sentence to the text:

L 300-301: Unlike for the Rangely experiment, our intent was to avoid triggering earthquakes in the Val d'Agri field, rather than to trigger them.

Despite not generating earthquakes during the validation phase, we believe that the level of control that we have accomplished via our process-oriented approach is demonstrably better than what was achieved in the Rangely experiment. In addition to the disparate level of sophistication and detail in the data and models between the Rangely paper and our study (which the Referee indeed acknowledges), we compare Figure 4 of our manuscript with Figure 7 of the Rangely paper (Raleigh et al., *Science* 1976), which is reproduced below to facilitate the discussion.

[Figure redacted - third party material]

Note that, although our plot shows both Coulomb stress (directly related to pressure) on the fault and seismicity continuously from pre-injection to the present, the Rangely plot has no information about pressure for the first two years of injection. In addition, the seismicity rate in the withdrawal interval in Feb. 1971, when the pressure is supposed to be sub-critical, exceeds the seismicity rate for 6 of the 7 months in 1972-1973 when the pressure is supposed to be above the threshold. Our ability to "manage" to keep the seismicity low appears to be better than that demonstrated in the Rangely experiment.

Therefore, we believe that our demonstrated ability to use process-based modeling to make proactive operational decisions that stop the generation of earthquakes while disposing of co-produced water in an active hydrocarbon reservoir is unprecedented—and thus that our claim of having managed triggered seismicity is warranted.

There is another aspect of seismicity management that was left unexplored in the original manuscript, but which we have investigated in response to one of the comments by another referee: the link between the decrease in Coulomb stress (CFF) on the field faults as a result of hydrocarbon production and the concomitant decrease in field-wide seismicity, as shown in the figure below (now Figure 3b in the revised manuscript).

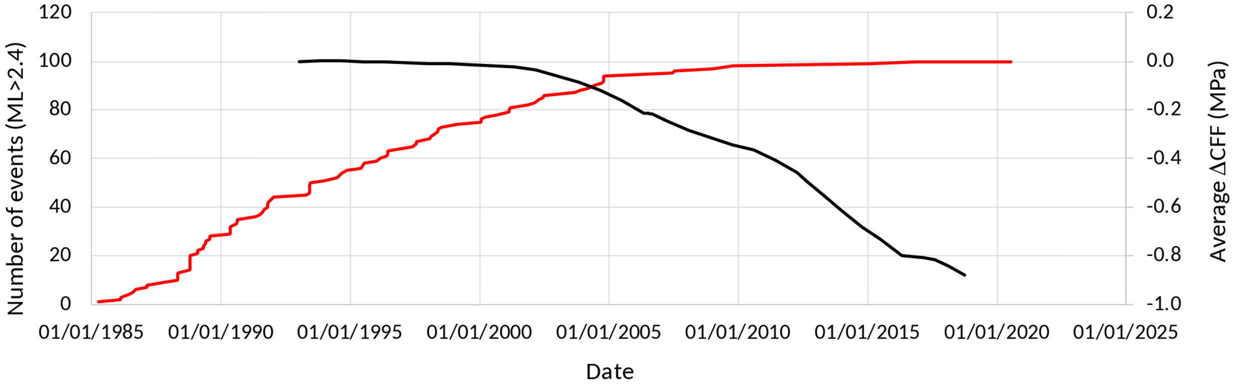

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Figure 3b: Cumulative event count (red curve, left scale) and average DCFF on all model faults (black curve, right axis) plotted as a function of time. Events included are those from a region within a 5-km buffer region surrounding the reservoir boundary, with ML > 2.4, the magnitude of completeness of the earthquake catalogue [Stabile et al., 2015].

While the emphasis of the current paper is on managing seismicity on the CMF triggered by injection, we believe that this unambiguous relation between ΔCFF and decline in field seismicity "bolsters confidence in the CFS model presented here," as suggested by the other referee.

2. Based on Figure 4, there are two time intervals during which the model seems to be inconsistent with the model:

a) An uptick in seismicity on the CMF occurred in 2011, following a pause to injection in well CM2. After renewal of injection (and consequent increase in the Delta CFF level), the Delta CFF level does not surpass previous levels as described in the paper, yet seismic activity occurred.

The reviewer is correct that the ΔCFF models do not show this uptick. The rate-state model does show an uptake, but not at as high a rate as observed. We added a brief statement to this effect:

L 246-250: We obtain even better agreement with the observed seismicity using the rate-state model (Figures 3d and 4d), which enables modeling of both seismicity rates and the spatial evolution of triggered earthquake sequences. The model predicts an increase in seismicity rate after the rapid pressure increase following shut-in in 2011, although the observed seismicity rate is somewhat higher (Figure S5).

b) A subsequent increase in injection rate in late 2012 yielded a record-breaking high level of Delta CFF, leading to a predicted increase in seismicity according to the model. However, this increase was not observed.

We respectfully disagree with this comment. There is an increase in seismicity in late 2012 accompanying the high level in ΔCFF (clearly shown by the histograms in Figure 4, all panels, and by red circles in Figure 4d, but less apparent in the moment release in Figure 4c). The models do slightly lag the observations at this time.

We note, however, that this comment led us to reexamine Figure 4. We discovered and fixed a minor problem - the monthly bars in the histograms in Figure 4 were plotted at the beginning of the month instead of at the end of the month, leading to the appearance of seismicity occurring late in the month preceding changes in ΔCFF, which was plotted daily.

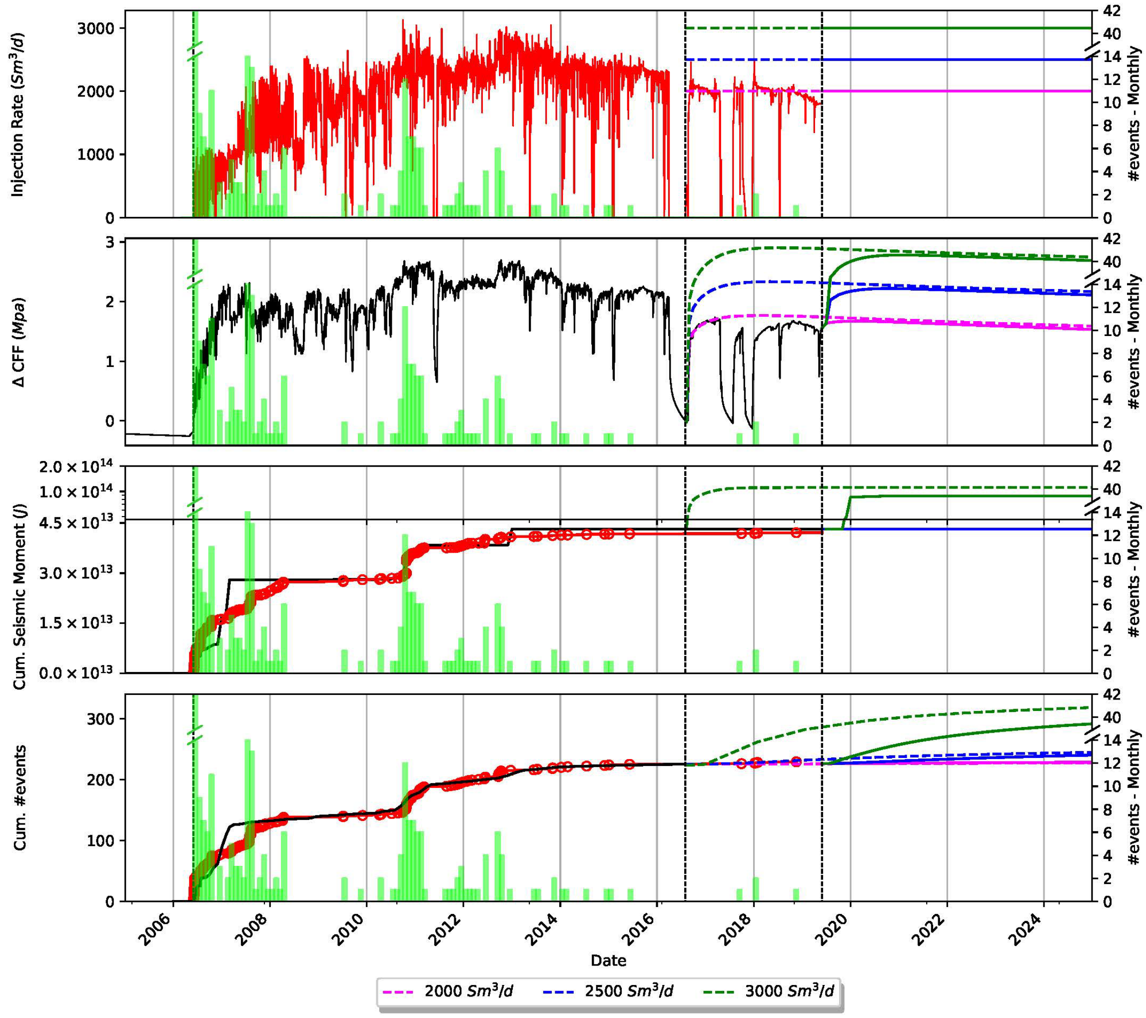

Figure 4: Temporal evolution of injection rates, observed monthly number of earthquakes (green bars, right axis), and model predictions. a) CM2 well injection rates: Actual (red), 2005.0 – 2018.5, and potential for 3 injection rates (fuchsia, blue and green), 2017.0 – 2025 (dashed) and 2019.5 – 2025 (solid); b) ΔCFF averaged over CMF for the local model (black), with forecasts in color. The causative pressure changes are equal to -1.82 ΔCFF; c) Cumulative moment release vs prediction. The red line represents the best estimate for the observed seismic moment. The black line is the model match, with forecasts in color. Red circles indicate events with $M_L \geq 0.5$; d) Rate-state prediction of the cumulative number of events (black) overlaid on the observed number (red), with forecasts in color.

Overall, the authors focus on tuning their model such that the net cumulative seismic moment fits the observations, however some discussion of these apparent inconsistencies is warranted.

We added some discussion in the conclusions section.

L 239-240: Even though the model moment release occurs more abruptly, the total moment release is well captured.

L 248-250: The model predicts an increase in seismicity rate after the rapid pressure increase following shut-in in 2011, although the observed seismicity rate is somewhat higher (Figure S5).

nature portfolio

E. Conclusions: robustness, validity, reliability. The approach presented here robust and well documented, enabling repeatability, in principle. As noted above, my principal reservation is that the claim that induced seismicity has been "managed" seems over-stated.

In view of our detailed discussion above, we hope to have convinced the Referee that "managed" is the correct description for the intervention on seismicity in the Val d'Agri field.

F. Suggested improvements:

3. Figures and captions should be improved. In Figure 1, legend annotation below part b is not legible (font too small for figure resolution). It would be helpful to show the injection depth for well CM2 (bottom of the well?). Cross section A-B appears to be oblique to structural fabric, which makes it difficult to assess the true dip of faults. In the caption to Figure 2, does "reservoir contour" mean "surface outline of the oil field"?

We thank the Referee for the detailed review. We have made these changes.

The content from Figure 3a (Delta CFF on faults) could be displayed in Figure 2 without loss of clarity (the use of colours to distinguish different faults in Figure 2 is unnecessary).

We have tried to incorporate this suggestion, but ultimately prefer to leave Figure 2 as in the original manuscript.

Also, the clipped colour scale for Figure 3 is unsatisfactory.

The color scale in the DCFF plots is something that we have given careful consideration before submission. In particular, for Figure 3a, the emphasis is on illustrating the effect of reservoir operations on all the faults in the field. If we allowed the DCFF on the CMF to define the color scale, much detail would be lost on the regional faults. In turn, the value of DCFF on the CMF is shown in great detail in Figure 4b.

4. How can we be confident that all potentially seismogenic faults are included in the model, given that the Costa Molina Fault was unknown prior to being activated by injection? Some discussion of this source of uncertainty is warranted.

The Referee raises a valid point. In general, it is unlikely that all seismogenic faults are included in a structural model, and our study is no exception. We have added discussion in the Supplementary Material section, as well as in the main text, addressing this point.

In the Supplementary material:

L 139 – 145: Our structural models, like any models, involve known and unknown simplifications of reality; it is important to consider how these simplifications might affect our interpretations about earthquake triggering. At the regional scale, we show that changes in stress on faults vary smoothly with position. Thus, our conclusion that hydrocarbon production leads to stabilization of major faults should apply to other nearby faults not included in our models. For the local model, pressure changes from injection into the CM2 well are localized to a small region, decreasing the likelihood of faults that have not yet been recognized or triggered.

In the main text:

L 277-283: 1) Hydrocarbon production results in a decrease in pore fluid pressure in most of the reservoir; because these faults allow equilibration of pore pressure across them, operations generally stabilize the faults within and surrounding the reservoir. Because, except near the CM2 well, ΔCFF does not vary rapidly with position, our conclusions about stabilization should hold on any unmodelled faults with geometries consistent with tectonic stresses. This major stabilization of regional faults is consistent with the observed decrease in regional seismicity rate;

5. Please define "triggered" seismicity. This term is variously defined in the literature, sometimes (but not always) to indicate fault movement that is due to an anthropogenic perturbation but primarily releases tectonic stress.

The Referee's description above is the definition that we adopt. We have changed the lead-off sentence of the manuscript to make this clear:

L 35 – 37: There is growing concern about seismicity triggered by human activities, where small stress increases bring tectonically stressed faults to failure, resulting in significantly greater stress release.

6. The modelling assumes that the S3 direction is perpendicular to normal faults, but Figure 1 shows that this is not exactly the case.

We show the orientations of S3, along with the fault orientations, in Figure 1. In general, S3 is not perpendicular to the faults.

Indeed, our flow-geomechanical model at the full-field scale does not assume that S3 is perpendicular to the normal faults. Such assumption would be incompatible with the fact that the regional faults are complex surfaces that change orientation and dip angle. Instead, the stresses are initialized by solving a boundary-value problem on the model domain, which is aligned with the principal tectonic stresses (Chiarabba et al., 2005), as shown in the figure below.

[Figure redacted - unpublished data]

Figure. Schematic of the imposition of boundary conditions in the model, in the model coordinates (x,y,z), aligned with the principal directions of tectonic stress.

We impose displacement boundary conditions on the lateral boundaries that ensure that the initial stresses from solving the momentum-balance equation are identical to the target tectonic stresses. In particular, this stress-initialization procedure results in stresses on the faults that generally have a spatial distribution of all three components: left-lateral, updip, and effective normal stress.

We have added a subsection 8.2.4 in the Methods to describe the stress initialization of the geomechanics model:

L 611-616: The stresses in the geomechanical model are initialized by solving a boundary-value problem on the model domain, which is aligned with the principal tectonic stresses. We impose displacement boundary conditions on the lateral boundaries that ensure that the initial stresses from solving the momentum-balance equation are identical to the target tectonic stresses. This stress-initialization procedure results in stresses on the faults that generally have a spatial distribution of all three components: left-lateral, updip, and effective normal stress

7. Showing at least one example of 3-D seismic data used to construct the model, to illustrate the seismic expression of faults, would help to advance foundational science discussed in this work.

We have followed this suggestion, adding Extended Figure S8:

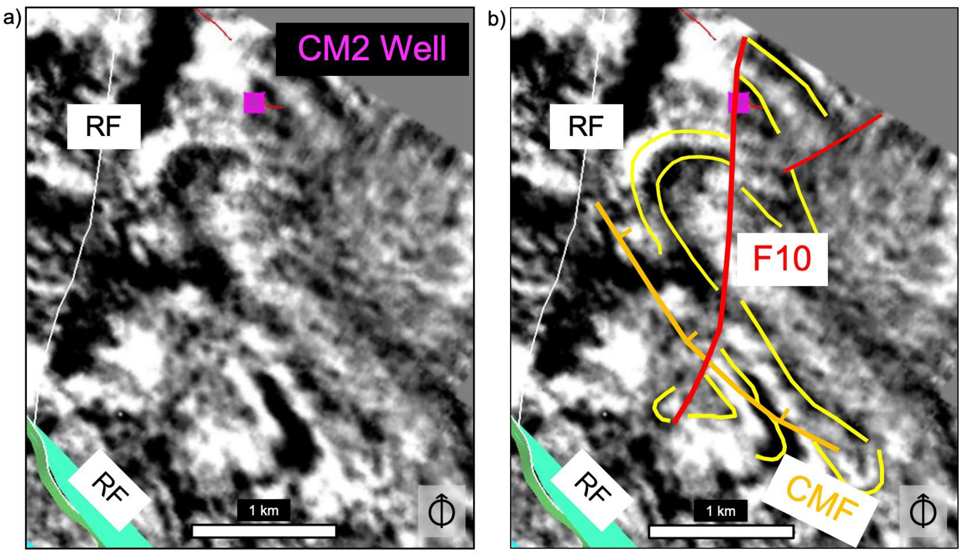

Figure S8. Seismic time slice at 2300ms from Val d'Agri 3D seismic reflection volume, showing constraints on the F10 and Costa Molina (CMF) faults. a) Image showing CM2 well and other reservoir faults (RF) included in the structural model. b) Interpreted seismic reflections (yellow), and F10 and CMF traces. Note that the primary constraint on the CMF location is from seismicity.

8. Please convert to SI units of volume/rate, or at least show the conversion.

We have changed Figure S6 to SI units.

9. Statements that seismicity occurs when Delta CFF rises above its previous maximum value require further substantiation as this does not follow directly from the Coulomb criterion (and in fact differs *from the conclusions from the Rangely experiment, where a threshold reservoir pressure was found). Is this due to the Kaiser effect? If so, this should be stated explicitly.*

When DCFF rises above its previous maximum, the effective stress decreases, decreasing the Coulomb failure stress and triggering fault slip. We modified the Methods text to address this:

L 494-497: The relative slip of node pairs is calculated on the basis of a zero cohesion, purely frictional Mohr-Coulomb failure criterion. The CFF at each fault node evolves in time in response to changes in shear and effective stress. If the CFF exceeds 0, slip occurs such that the CFF value at each fault node returns to a value ≤ 0..

We agree that our results differ from Rangely and are approximated by the Kaiser effect:

L 287-293: 3) Most seismic events in our models occur when pressure on the CMF increases sufficiently that ΔCFF exceeds its previous maximum, consistent with the Kaiser effect. In addition, our rate-state model provides an explanation for seismicity occurring at a finite rate even at ΔCFF lower than the previous maximum, in particular, associated with rapid ΔCFF increases. Our results differ from the interpretation of the Rangely experiment that seismicity is triggered at a constant threshold pressure[4]

Minor comments:

10. Line 39: For clarity and completeness add "monitoring" to the statement about regulatory frameworks.

We have followed the Referee's suggestion and this term has been added:

L 40 – 41: authorities have set up empirical or statistical regulatory monitoring-based frameworks, with varying success.

11. Line 109, "hydrocarbon thickness" should be "reservoir thickness".

We prefer to continue to use "hydrocarbon thickness". The reservoir is thicker than the hydrocarbon layer, which has brine beneath it.

12. Line 241, it seems odd that only the optimal friction coefficient (0.55) is repealed here in the context of "different combinations of friction parameters".

This is a good point. We have revised the discussion in the Methods section to make it more clear that the friction coefficient in the rate-state formulation always enters in combination with other parameters and is not possible to constrain uniquely. Thus, the value of 0.55 is assumed, rather than somehow "optimal."

L 711-727: The remaining parameters needed for the simulations are r, a, and the initial value of γ in equation 3, and $\mu - \alpha$ in equation (5). The term $(r/\dot{S}_r)$ scales the amplitude of the rate solution. Typically, in modeling observed seismicity, $r/\dot{S}_r$ is evaluated empirically, where r is estimated from the observed background rate of seismicity that occurs at the tectonic stressing rate $\dot{S}_r$, which we infer from geodetic observations or can also be inferred from recurrence times of large earthquakes. However, in the case of the Costa Molina fault, because the earthquake catalogue is incomplete below M_L = 2.4, we have no reliable determination of the background seismicity rate prior to the start of Val d'Agri field operations. Consequently, the cumulative number of events in the simulation is simply scaled to the total number of observed earthquakes with *M* above a threshold value. Because a, μ, and α enter the equations in combination, we set the rate-state parameter a = 0.010, as representative of laboratory determinations, and μ = 0.55 to be consistent with the moment release model. The parameter α is not well-characterized in laboratory experiments, so we leave it, together with the initial value of γ, as free parameters that we vary by trial-and-error to find solutions that best fit the observed seismic rates (Supplementary Dataset 1). For our preferred model, α =0.38 and the initial value of γ is 2.0 MPa^{-1}. At this value, the seismicity rate before injection is negligible, but rapidly increases once the fluid pressures begin to rise.

13. Line 268 "Operations" should be replace by "Oil and gas production" (since injection is also part of the operations).

We agree with this suggestion, and we have replaced "Operations" with "Hydrocarbon production."

14. Line 464, elaborate on how relative slip is calculated. It is claimed to be based on Mohr-Coulomb, but that alone does not predict the amount of slip.

This point is similar to one addressed above:

When DCFF changes, the effective stress changes, changing the Coulomb failure stress. If the resulting stress state is unstable, fault slip results to bring the fault back to critical. We modified the Methods text to address this:

L 494-497: The relative slip of node pairs is calculated on the basis of a zero cohesion, purely frictional Mohr-Coulomb failure criterion. The CFF at each fault node evolves in time in response to changes in shear and effective stress. If the CFF exceeds 0, slip occurs such that the CFF value at each fault node returns to a value ≤ 0.

.

G. References: Appropriate credit is given to previous work.

H. Clarity and context. Abstract, Introduction and Conclusions are lucid and accessible to a general scientific audience.

We thank the Referee for these positive comments.

References cited in this response

Alghannam, M., and R. Juanes (2020), Understanding rate effects in injection-induced earthquakes. *Nature Comm.* 11, 3053.

Byrne, H., J. A. Silva, A. Plesch, R. Juanes, and J. H. Shaw (2020), The groundbreaking experiment in earthquake control at Rangely, Colorado, revisited. *Geophys. Res. Lett.*, 47, e2020GL088257.

Chiarabba, C., L. Jovane, and R. DiStefano (2005), A new view of Italian seismicity using 20 years of instrumental recordings. *Tectonophys.* 395, 251–268.

Kaiser, J. (1953), Kenntnisse und Folgerungen aus der Messung von Geräuschen bei Zugbeanspruchung von metallischen Werkstoffen, *Arch. Für Isenhütten-Wesen*, 24, 43–45.

Raleigh, C. B., J.H. Healy and J.D. Bredehoeft (1976), An experiment in earthquake control at Rangely, Colorado. *Science* 191(4233), 1230-1237.

Reviewer Reports on the First Revision:

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Referee #3:

The paper "A process-based approach to understanding and managing triggered seismicity in the Val d'Agri oil field, Italy" integrates geology, seismology, reservoir characteristics, and a mix of modeling approaches to show that anthropogenically triggered seismicity can be managed. The authors are provide access to industry standard data to constrain subsurface geometries; oil, gas and water production and injection at fairly fine temporal scales; stress orientations, and stressing rates; and earthquake monitoring data. With this information, and using the injection and production data for the field, they can design and validate a combined structural model with multi-phase fluid flow and geomechanical models and then tie the results of pressure and Coulomb Failure Functions to rate-and-state earthquake physics-based models to reproduce earthquake history. Because they have a 30-month validation dataset, they can also draw conclusions as to what injection volume can be maintain to manage seismicity (here define as reaching a near zero seismicity rate rather than maintaining magnitudes below felt thresholds). I agree with previous reviews that this study represents a significant advancement that will be of broad interest to readers of Nature.

I was asked to comment on the authors' response to previous reviews, in addition to providing independent review. I found all of the scientific changes to the manuscript, most notably inclusion of Figure 3b, led to substantive improvements and strengthen the conclusions. I agree with the authors that the comments and discussion leading to Figure 3b, which documents a decreasing seismicity rate on regional faults as production continues and actually stabilizes most faults in the field, was a key extension of the findings. A minor comment – is the DAY/MO/YEAR format on the x-axis of Figure 3b allowed for Nature?

The figures were revised following comments by an initial reviewer. I printed all figures and found the font size remains very small. Figure 3c,d at 200% on a screen still had text that could barely be read. Figure 1 poses similar concern. The scientific content of the figures are excellent.

In summary, I find the paper presents a compelling case for successful management of seismicity at the Val d'Agri oil field and points a way forward for integrated modeling-based mitigation of hazard in areas of induced earthquakes. It is a data rich paper and the authors should be commended on the care being taken to provide public supporting information. The data quality and integration provided in this study may be hard to mimic, however, particularly in some US basins where many companies compete for economic advantage and regulatory requirements do not provide data of the spatial and temporal resolution granted to these authors by Eni. The paper should be published in current form by Nature.

****
Referee #4:

A - the authors use coupled fluid-flow and geomechanical modeling to compute stress changes within an oil field. This in turn is combined with a rate-and-state model to make predictions on the likelihood of future induced seismicity based on various injection histories

B - I'm in 2 minds about the originality and significance, in particular given their ending (lines 304-310).

304 The effort here is unique and stands at the frontier of seismic hazard analysis. We are unaware
305 of any other petroleum field in the world where physics-based, data-constrained models are
306 being used to assess changes in the seismic potential of faults, and inform simulations that
307 forecast scenarios for future seismic activity for potential operations plans. We expect that
308 implementing our approach elsewhere where the required information is available should
309 substantially improve both understanding and management of earthquakes associated with
310 human activities.

It is true that the amount of benchmark information used here is unusual in the context of public-domain data. Operators have routine access to these data though. As a consequence, fault stability analyses of this scale are unusual in the scientific literature but service companies like Schlumberger, Halliburton perform these studies regularly. The SPE publishes some of these as well. Although they rarely integrate rate-and-state friction. Two other studies that go into this direction are by Konstantinoskaya. I'm sure I can find more if I dig more in the SPE literature.

Present-day stress analysis of the St. Lawrence Lowlands sedimentary basin (Canada) and implications for caprock integrity during CO2 injection operations
E Konstantinovskaya, M Malo, DA Castillo
Tectonophysics 518, 119-137, 2012

CO2 storage and potential fault instability in the St. Lawrence Lowlands sedimentary basin (Quebec, Canada): Insights from coupled reservoir-geomechanical modeling
E Konstantinovskaya, J Rutqvist, M Malo
International Journal of Greenhouse Gas Control 22, 88-110, 2014

It also looks like this field is very mature; production has stabilised the stress state essentially everywhere except on a single and now well-known fault (Fig 3), making management and forecasting relatively straightforward.

If I exclude lines 304-310 and 45-51 then this becomes a work of very high quality research, combining many disciplines, rarely seen in the widely accessible research literature. I would not class it as a breakthrough though. Like all excellent research it raises many questions - how sensitive are your forecasts to the inverted model parameters? Can you provide forecasting uncertainties? How robust are the forecasts if you vary your model parameters by 1-4%? Is the history matching affected?

My personal view is that this is a very nice case history - it has significant scientific value by focusing on the benchmarking + validation strategy for other regional scale studies.

C- Data and methods are well described. It is indeed good to emphasize that the injection pressures (surface or bottom) and injection rates are important but ultimately solely the in situ stress state at a fault determines if it will reactivate, requiring often such coupled modeling approaches

D - I could not determine how the benchmarking + validation was done exactly: what information was excluded from the benchmarking, only to be used in validation exercises? Eg was history matching done on all wells, or were some used for validation? Similar question for the various stress indicators etc and the geomechanical parameters per layer, the GPS info etc

E - the conclusion that the production has stabilized the field everywhere except on a single fault is well shown. I'd dispute the implied statements that this approach is a breakthrough. Yet the study shows what is possible given extensive data and robust benchmarking methodologies.

F - How sensitive are your forecasts on variations in your modeling parameters? History matching is non-unique. Are your conclusions unchanged if the model parameters are somewhat varied? Also what does slip tendency analysis or fault slip potential predict for the fault? Is it largely unstable? In other words, does your coupled methodology show a different response from a simpler method?

G - references are fine

H - very well written

Mirko van der Baan

*******************************END*******************************

Author Rebuttals to First Revision:

Response to Reviews

We thank the Editor for his effort during these challenging times in finding insightful reviewers for our manuscript, as well as the opportunity to respond to these reviews. We thank Referee #3 for his general enthusiasm for our work, as well as his care in evaluating the formatting of the figures. And we thank Referee #4 for his constructive comments that made it clear that our communication of the significance of our results could be improved. We hope that the Editor will agree that our further revisions, discussed in detail below, convincingly show, as Referee #3 states, that our "*paper presents a compelling case for successful management of seismicity*," that it "*represents a significant advancement that will be of broad interest to readers of Nature,"* and that "*the paper should be published in current form by Nature*."

In what follows, the referees' comments are in *blue italics*. Line numbers in the previous manuscript are indicated as PMnnn. Line numbers in the current, revised manuscript are indicated as CMnnn.

Amendments in Response to the Editor

This section follows point-by-point the Editor's comments in his e-mail of 8 January 2021, identified by the beginning of each paragraph.

Please note that the presentation of a Nature paper has undergone significant changes to improve its readability and navigability online.

We have integrated the material that was previously in the Supplementary Information section into the Methods section in the main text, changing the reference order as needed.

As a signatory of the https://copdess.org/enabling-fair-data-project/commitment-statement-in-the-earth-space-and-environmental-sciences/”>Enabling FAIR Data in

Earth, space and environmental sciences, Nature is committed to supporting FAIR principles in data sharing and citation.

We support the FAIR principles in data sharing and citation and commit to putting as much of the data as possible in a data repository (lines CM997-1003), "Source data for the graphs are provided in linked files, including the two earthquake catalogues used. Remaining relevant data are available at the public research data repository figshare (citation TBD) including the computational meshes with embedded stratigraphic horizons and fault surfaces for the regional and local models, pressure history on the CMF, input file and script for the seismicity rate model, and source data for Figures 1c and 3. Total field monthly oil and gas production as tabulated by the Italian Ministry of Economic Development are included in the figshare repository."

Our planned directory structure and files for the figshare site that will be populated prior to publication is given below. Folders are in bold italics, files are in plain text. Indented items are in the folder above them.

CM2 data
README.txt
Injection_rate_well_head_pressure.xls
Field Production data
README.txt
Field_Oil_Gas_Total_Production.xls
Gocad meshes for computational models and figures
Regional mesh
README.txt
3-D Mesh coordinates and topologies with embedded horizons and fault surfaces
Local mesh
README.txt
3-D Mesh coordinates and topologies with embedded CMF surface
Local model pressure evolution on the Costa Molina Fault surface
CMF_fault_pressure.xls
Rate-state model
Readme.txt
R script
Script and input file for chosen parameters
Figures (non-graph)
Figure 3a
Readme.txt
DCFF values on faults in Gocad format

We appreciate Referee #3's comment "*It is a data rich paper and the authors should be commended on the care being taken to provide public supporting information*." The information provided will enable others to reproduce the key findings of our study. We emphasize that along with the earthquake catalogues, we are providing also the calculated pressure fields on the Costa Molina Fault that are used in the calibration and forecasts of seismic moment release and seismicity rate and subsequent validation. These data provide a clear path for others to reproduce and evaluate our main results, and serve as an extremely valuable open resource for future studies by anyone interested in developing quantitative models of earthquake source physics and management of triggered seismicity. Moreover, we provide a path for access to all additional information, as we note in lines CM1004-1007, "Some input data for the flow-geomechanics models contain proprietary information, made available by Eni for the current study under confidentiality agreement. These data are available from author S.M. (stefano.mantica@eni.com) with permission of Eni." We hope that you find that this meets the FAIR principles expectations of *Nature*.

Length: At about 3140 words, the main text length of your manuscript is above our usual guidelines for a short Nature Article with four display items. Please therefore do not allow the overall length of the main text to increase significantly in revision.

The length of the main manuscript is now 313 lines, only 3 lines longer than the previous version. Additions needed to address the comments of Referee #4 were nearly balanced by removal of other text.

Order: The general order in which your text file (without embedded figures or Extended Data items) should be presented is as follows:

We have put the components in order.

Title: The present title is somewhat longer than we can accommodate (and indicates that your results might not be of wide use). Would simply "A process-based approach to understanding and managing triggered seismicity" suffice?

We have shortened the title as suggested, which now better reflects its broad appeal, and thank the Editor for his specific suggestion.

Summary paragraph:
Sub-headings:
Figure legends:
Methods:
References:
Display items:
Figure formatting:
Extended Data:
Source Data:
Code availability statement:
Editorial policies:
ORCID

We believe that our manuscript is now in compliance with all of the above.

Our point-by-point responses to the two referees is on the remaining pages of this document. The response to each referee begins on a new page.

Amendments in Response to the Evaluation and Review from Referee #3

In what follows, the referee's comments are in *blue italics*. Line numbers in the previous manuscript are indicated as PMnnn. Line numbers in the current, revised manuscript are indicated as CMnnn.

The paper "A process-based approach to understanding and managing triggered seismicity in the Val d'Agri oil field, Italy" integrates geology, seismology, reservoir characteristics, and a mix of modeling approaches to show that anthropogenically triggered seismicity can be managed. The authors are provide access to industry standard data to constrain subsurface geometries; oil, gas and water production and injection at fairly fine temporal scales; stress orientations, and stressing rates; and earthquake monitoring data. With this information, and using the injection and production data for the field, they can design and validate a combined structural model with multi-phase fluid flow and geomechanical models and then tie the results of pressure and Coulomb Failure Functions to rate-and-state earthquake physics-based models to reproduce earthquake history. Because they have a 30-month validation dataset, they can also draw conclusions as to what injection volume can be maintain to manage seismicity (here define as reaching a near zero seismicity rate rather than maintaining magnitudes below felt thresholds). I agree with previous reviews that this study represents a significant advancement that will be of broad interest to readers of Nature.

I was asked to comment on the authors' response to previous reviews, in addition to providing independent review. I found all of the scientific changes to the manuscript, most notably inclusion of Figure 3b, led to substantive improvements and strengthen the conclusions. I agree with the authors that the comments and discussion leading to Figure 3b, which documents a decreasing seismicity rate on regional faults as production continues and actually stabilizes most faults in the field, was a key extension of the findings. A minor comment – is the DAY/MO/YEAR format on the x-axis of Figure 3b allowed for Nature?

We now use only YEAR for the x-axis label of Figure 3 – we feel that there is no need for a finer division at the scale of the figure.

The figures were revised following comments by an initial reviewer. I printed all figures and found the font size remains very small. Figure 3c,d at 200% on a screen still had text that could barely be read. Figure 1 poses similar concern. The scientific content of the figures are excellent.

We appreciate the remark on scientific content and especially appreciate the Referee's effort in identifying the formatting problems with Figures 1, 3c, and 3d. We have changed the font size in these figures and checked that the font is legible upon reduction.

In summary, I find the paper presents a compelling case for successful management of seismicity at the Val d'Agri oil field and points a way forward for integrated modeling-based mitigation of hazard in areas of induced earthquakes. It is a data rich paper and the authors should be commended on the care being taken to provide public supporting information. The *data quality and integration provided in this study may be hard to mimic, however, particularly in some US basins where many companies compete for economic advantage and regulatory requirements do not provide data of the spatial and temporal resolution granted to these authors by Eni. The paper should be published in current form by Nature.*

We thank the Referee for the decisively positive comments and recommendation. In particular, we agree on the vital importance of the extraordinarily rich field dataset, which has allowed us not only to build multidisciplinary high-fidelity models but, crucially, perform a true field *validation* of the forecasts and, ultimately, our process-based approach to managing induced seismicity in the Val d'Agri field.

Amendments in Response to the Evaluation and Review from Referee #4

In what follows, the referee's comments are in *blue italics*. Line numbers in the previous manuscript are indicated as PMnnn. Line numbers in the current, revised manuscript are indicated as CMnnn.

The Referee states that our work is "*very high quality research, combining many disciplines, rarely seen in the widely accessible research literature*" and that it is "*very well written*", but expresses some reservations on the "*originality and significance*" of our study. Given this contrast to Referee #3's evaluation, we are concerned that we did not adequately communicate the novelty and significance of our work. Some of the comments from the Referee point to our not having explained well enough the key contributions from our study. We have made strategic modifications to the manuscript to address these points. We hope that now the Referee will be convinced that our study (in the words of the other Referee) "*presents a compelling case for successful management of seismicity … and points a way forward for integrated modeling-based mitigation of hazard in areas of induced earthquakes."*

Addressing the specific issues raised:

A - the authors use coupled fluid-flow and geomechanical modeling to compute stress changes within an oil field. This in turn is combined with a rate-and-state model to make predictions on the likelihood of future induced seismicity based on various injection histories.

While this summary is correct, we feel that it does not include the contributions from our study that are most important and unique:

1. In addition to computing stress changes, we accurately determined what pressure and stress changes were required to trigger the sequence of seismic activity observed. This cycling of load and resulting variation in seismic moment and seismicity rate allowed *in situ* calibration of the failure criterion.
2. The demonstration that triggered earthquake rates can be modeled using physical inputs in the seismicity rate models, thus extending the methods of quantitative probabilistic seismic hazard analysis (PSHA) to triggered earthquakes. This makes possible assessment of the probability of triggering an earthquake of a given magnitude and calculation of the probability distribution of ground shaking and damage.
3. After the initial calibration, we did not only use the models for "*mak[ing] predictions on the likelihood of future induced seismicity based on various injection histories."* A crucial further step is validating the models over the subsequent 30 months by comparing the observed to predicted moment release and seismicity rates for the actual injection rates.
4. The demonstration that our multidisciplinary process-based approach has permitted managing injection-triggered seismicity at the fault scale, that is, that seismicity on the CMF almost entirely ceased while water injection was allowed to continue with a rate selected by the operator based on the model forecasts.

5. The observation of the quantitative temporal link between the reduction in tectonic seismicity at the field scale and the reduction in Coulomb stress (ΔCFF) on the main field faults as a result of oil and gas production.

B - I'm in 2 minds about the originality and significance, in particular given their ending (lines PM304-310).

304 The effort here is unique and stands at the frontier of seismic hazard analysis. We are unaware
305 of any other petroleum field in the world where physics-based, data-constrained models are
306 being used to assess changes in the seismic potential of faults, and inform simulations that
307 forecast scenarios for future seismic activity for potential operations plans. We expect that
308 implementing our approach elsewhere where the required information is available should
309 substantially improve both understanding and management of earthquakes associated with
310 human activities.

We are concerned that we had not adequately communicated the originality and breakthrough nature of our work. In rereading lines PM304 – 310 with fresh eyes, we now see that this passage may not adequately communicate the novelty and significance of our work, as it might be interpreted as our claiming novelty for what has been done previously. We and others have previously used physics-based models to assess changes in the seismic potential of faults. And these studies have been constrained by various kinds of data. But not seismicity rates and moment release. Calculating fault stability is only an initial step in what we do – our main accomplishment, the one that makes our work a significant advance, is calibrating and validating what injection rates can be maintained without triggering additional seismicity.

We discuss in more detail in our response to the comments below the novel advances that we have made. We thank the Referee for pointing out that these lines could be easily misunderstood and have modified these lines as follows to more clearly describe what is novel (lines CM307-313):

The effort here is unique and stands at the frontier of management of triggered seismicity in active fields. We are unaware of any other petroleum field in the world where such comprehensive and detailed physics-based models, calibrated by history matching of reservoir observations and observed seismicity rates and moments, are being used to forecast scenarios for future seismic activity for operations plans. We expect that implementing our approach elsewhere where the required information is available should substantially improve both understanding and management of earthquakes associated with human activities.

It is true that the amount of benchmark information used here is unusual in the context of public-domain data. Operators have routine access to these data though. As a consequence, fault stability analyses of this scale are unusual in the scientific literature but service companies like Schlumberger, Halliburton perform these studies regularly. The SPE publishes some of these as well. Although they rarely integrate rate-and-state friction. Two other studies that go into this direction are by Konstantinoskaya. I'm sure I can find more if I dig more in the SPE literature.

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Present-day stress analysis of the St. Lawrence Lowlands sedimentary basin (Canada) and implications for caprock integrity during CO2 injection operations, E Konstantinovskaya, M Malo, DA Castillo, Tectonophysics 518, 119-137, 2012

CO2 storage and potential fault instability in the St. Lawrence Lowlands sedimentary basin (Quebec, Canada): Insights from coupled reservoir-geomechanical modeling, E Konstantinovskaya, J Rutqvist, M Malo, International Journal of Greenhouse Gas Control 22, 88-110, 2014

The two papers that the Referee mentions use flow-geomechanics modeling to carry out fault stability analysis in order to address the seismic potential of faults. We are familiar with this literature and have contributed a number of papers to it ourselves (citations given at the end). The specific papers cited above, and many others in this category, use simplified 2D (or sometimes 3D) academic representations of the subsurface and *hypothetical* injection scenarios, without implementation of real-world field constraints on the outcome of the injection scenario assumed. That is, they do not test their models by field experiments that either do or do not trigger seismicity.

We note that the first model approach that we use, calculation of ΔCFF, provides a compact quantification of fault stability analysis. Fault stability analyses can have great value, as we demonstrate in Figure 3b, showing that pressure decreases and poroelastic stress changes have generally resulted in most reservoir faults having increased stability.

However, traditional fault stability analysis predicts neither the moment that is released nor the seismicity rate that results from a given evolution of effective stress. Predicting these requires more advanced models such as those we used. In particular, our 3D geologically-constrained models are a significant and necessary improvement compared to the 2D models in the papers mentioned by the Referee. As the paper cited and other papers by Rutqvist have noted, 2D models are not able to predict seismic moment or magnitude. It is only by doing our work in high-fidelity 3D representations of the subsurface that the calibration of our models of moment release and seismicity rate is possible.

Returning to the text of the comment, "*Operators have routine access to these data though. As a consequence, fault stability analyses of this scale are unusual in the scientific literature but service companies like Schlumberger, Halliburton perform these studies regularly.*" We are not aware of service companies or operators carrying out history matching and fault stability analysis at the level that we present here, and certainly not demonstrating the ability to reproduce seismicity rates and moment release to manage seismicity.

In summary, what sets our current study apart from all previous ones is the demonstration of using a physics-based approach in 3D to forecast and thereby successfully manage injection-induced seismicity, and to do so in an active oil field in a seismically-active region.

In the revised manuscript, we have added text to highlight the novelty and breakthrough nature of our study (lines CM84-88):

What sets this study apart is the use of a physics-based approach in a high-fidelity, geologically-constrained 3D representation of the subsurface to: (1) reproduce observed seismicity rates and moment release caused by fluid injection, and (2) forecast and successfully manage injection-induced seismicity, and to do so in an active oil field in a seismically-active region.

It also looks like this field is very mature; production has stabilised the stress state essentially everywhere except on a single and now well-known fault (Fig 3), making management and forecasting relatively straightforward.

Even if there is a single injection well near a single seismogenic fault, the forecasting and management may seem straightforward *only after* one has developed a high-fidelity structural representation, and carefully calibrated the flow models with production history and bottom hole pressures, the geomechanics models with surface deformation, and the fault frictional parameters with the observed seismicity. It is true that having a decades-long history of production and years-long recorded seismicity allows for robust calibration, which in turn enables our physics-based models to be used for guiding field operations, making our study the first demonstration of management of seismicity in a working hydrocarbon field. Indeed, our models explain a situation in which subsurface operations are responsible for an *increase in seismicity locally* (at the CM2 fault) due to water injection, and a concomitant *decrease in seismicity regionally* (in the entire field) due to oil and gas production. Only the integration of the correct physics with reservoir and geophysical data allows one to reach this quantitative understanding, and the ability to *manage* the seismicity by limiting the injection rate to a level determined by the modeling forecasts. We do not think that this can be considered straightforward.

If I exclude lines 304-310 and 45-51 then this becomes a work of very high quality research, combining many disciplines, rarely seen in the widely accessible research literature. I would not class it as a breakthrough though.

We have discussed lines PM304-310 above. For reference, lines PM45-51 are given here:

"We report here development, testing, and implementation of a new methodology for managing triggered seismicity using unusually detailed information about the subsurface, including hydrocarbon production and water injection data, to inform geomechanical and earthquake source physics models. We employ our approach in the Val d'Agri oil field in southern Italy, providing the first documented case of successful management of triggered seismicity based on a process-based method applied to a producing hydrocarbon field. Applying our approach elsewhere could help to manage and mitigate triggered seismicity."

We have modified this text to emphasize calibration, validation, and management of field operations in a seismically active region (lines CM43-51):

We report here development, testing, and implementation of a novel multidisciplinary methodology for managing triggered seismicity using unusually comprehensive and detailed information about the subsurface to calibrate geomechanical and earthquake source physics models. We then validate these models by comparing their predictions to subsequent observations made after calibration. We employ our approach in the Val d'Agri oil field in seismically active southern Italy, providing the first documented case of successful management of triggered seismicity based on a process-based method applied to a producing hydrocarbon field. Applying our approach elsewhere could help to manage and mitigate triggered seismicity.

Like all excellent research it raises many questions - how sensitive are your forecasts to the inverted model parameters? Can you provide forecasting uncertainties? How robust are the forecasts if you vary your model parameters by 1-4%? Is the history matching affected?

The question of forecasting uncertainties and robustness of the results to perturbations in the model parameters is an excellent one. We address this point in detail in our reply to Point F.

My personal view is that this is a very nice case history - it has significant scientific value by focusing on the benchmarking + validation strategy for other regional scale studies.

We agree that benchmarking (calibration) and validation set a new standard for such studies.
Based on a unique case history, we have developed a novel quantitative approach integrating different disciplines and datasets to explain and manage triggered seismicity, which can be applied elsewhere.

C- Data and methods are well described. It is indeed good to emphasize that the injection pressures (surface or bottom) and injection rates are important but ultimately solely the in situ stress state at a fault determines if it will reactivate, requiring often such coupled modeling approaches.

We agree with the Referee that coupled flow-mechanics models are needed, precisely to determine the state of effective stress on the fault, as this will determine the propensity for the fault to slip. This aspect distinguishes our study from other studies in the Val d'Agri field (e.g., Improta et al. 2015, 2017), and a number of papers on induced seismicity elsewhere, from the classic study of Raleigh et al., 1976) to very recent work (e. g., Kwiatek et al., 2019), which only establish correlations between injection rates or wellhead pressures and recorded seismicity.

D - I could not determine how the benchmarking + validation was done exactly: what information was excluded from the benchmarking, only to be used in validation exercises? Eg was history matching done on all wells, or were some used for validation? Similar question for the various stress indicators etc and the geomechanical parameters per layer, the GPS info etc.

The issue of validation is a crucial point of our study, and we welcome the opportunity to clarify it. Most studies refer to calibration and validation periods rather arbitrarily, given that all the data is available at the outset. This was not the case here. The models were calibrated exclusively with field data available up to the end of 2016. Based on those calibrated models, forecasts were made with 3 different injection rates for the period 2017-2025, concluding that a water injection rate of 2,000 Sm^3/d was sustainable from the point of view of managing seismicity. This was indeed the injection rate adopted by the operator of the field during the period 2017-2019.5 (see the actual injection rate in Figure 4a), while data continued to be collected. Thus, the forecasts were completely blind to the data in the validation period, as this period was actually in the future.

In Extended Data Figure 9 of the paper, we show sample history matches for the *calibration* phase. In Figure 1 (below), we show, for some representative wells, how the regional model, history matched during the *calibration* phase against measured pressures (red dots), blindly reproduces the pressure data acquired during the *validation* phase (blue dots).

[Figure redacted - unpublished data]

Figure 1: Well bottom hole pressure: simulated (green curve), measured in the *calibration* phase (red dots) and measured in the *validation* phase (blue dots)

To clarify that the validation aspect of our study occurred after calibration and the initial forecasts were done, we have added the following text (lines CM203-215):

We *calibrate* our models during the period 1 Jan. 1993 – 31 Dec. 2016 using field production and injection rates and matching well-bottom pressures, surface deformation, and seismicity. The constraint on the coefficient of friction provided by the calibration of the timing of the initial earthquake and subsequent seismic moment release was crucial to limit the uncertainties in parameters. Based on those calibrated models, we *forecast* the seismicity expected during the period 1 Jan. 2017 – 31 Dec. 2025 for three different injection rates: 2,000, 2,500, and 3,000 Sm^3/d, concluding that a water injection rate of 2000 Sm^3/d was sustainable from the point of view of managing seismicity. This was indeed the injection rate adopted by the operator of the field during the 30-month period 1 Jan. 2017 – 30 Jun. 2019 (Figure 4a), while data continued to be collected. We *validate* our models by comparing the predicted and observed seismicity during this 30-month post-calibration period; thus, the forecasts used to decide the injection rate were completely blind to the data in the validation period, as this period was then in the future.

We discuss in more detail in response to point F below that all individual model parameters have uncertainties. These include uncertainties in the initial state of stress from different stress indicators, uncertainties in elastic parameters, and uncertainties in Biot coefficient from the GPS comparison. Without calibration, these uncertainties could propagate into disparate model forecasts. In our study, the parameter uncertainty is greatly reduced for all models as a result of the sharp sensitivity to the rapid variation in seismicity, which turns on and off in response to variations in injection rate. As noted below, we added lines CM987-994 to the Methods section to address this point.

E - the conclusion that the production has stabilized the field everywhere except on a single fault is well shown. I'd dispute the implied statements that this approach is a breakthrough. Yet the study shows what is possible given extensive data and robust benchmarking methodologies.

We do not claim that our conclusion that production stabilized the field is unique; however, we suggest that the comprehensive nature of the datasets, novel aspects of the methodology outlined in the paper and this response, and the demonstration of the management of seismicity on the CMF during continued injection are indeed ground breaking contributions.

F - How sensitive are your forecasts on variations in your modeling parameters? History matching is non-unique. Are your conclusions unchanged if the model parameters are somewhat varied?

Also, referring back to the related issue in section B, *- how sensitive are your forecasts to the inverted model parameters? Can you provide forecasting uncertainties? How robust are the forecasts if you vary your model parameters by 1-4%? Is the history matching affected?*

The Referee raises an important point, one that we had investigated while doing our study, but had not specifically reported on in our manuscript. As noted in our response to comment B, multiple model parameters affect whether a given change in pressure and stress will trigger an earthquake. For example, the coefficient of friction and the initial stress state are both important and there is a strong tradeoff among these parameters. However, once the additional observations of spatial and temporal variations in seismicity rate or moment release are used as a constraint, the range of admissible parameters is greatly reduced.

To address the Referee's question about the uncertainties in forecasts of moment release and seismicity rate variations, we present here simulation results from our models with different parameter values.

First, we show the sensitivity of moment release to the coefficient of friction in our 3D coupled flow-geomechanical model of injection near the Costa Molina fault—see Figure 1 in this reply. In this figure, which is similar to Figure 4c in the manuscript, we compare the observed variation of seismic moment release with time (black curve) with that predicted for our preferred value of coefficient of friction, 0.55 (red curve) and those predicted for coefficients of friction that vary from this value by ± 0.01 (~2%). It is clear from this figure that, for the given pressure field, the total moment release during the calibration stage is very sensitive to the coefficient of friction. For $\mu = 0.54$ (blue curve), the total moment release over the calibration interval is about twice that observed. For $\mu = 0.56$ (green curve), the total moment release is about a factor of two too low. We note that none of these models predict additional seismic moment release after 2012, including during the validation and forecast periods.

[Figure redacted - unpublished data]

Figure 1. Sensitivity of moment release predicted by the 3D coupled flow-geomechanics model to the value of the coefficient of friction, with pressure history and initial stress held fixed.

Second, we illustrate the sensitivity of our seismicity-rate model predictions to the rate-state friction parameters—see Figure 2 in this reply. The figure shows the tradeoff between the frictional parameters μ and *a*, this time with μ varying by ± 0.05 about the nominal value of 0.55. These 20% variations in μ, when accompanied by variations in the parameter *a* by about a factor of 2, match the cumulative seismicity (red curve). Remarkably, the three fits (black lines) to the variations in seismicity during the calibration phase are barely distinguishable, as are the post-calibration *forecasts* of seismicity rates. This example illustrates that, after proper calibration to constrain the values of the parameter sets, the model forecasts of seismicity are robust to variations in the model parameters.

[Figure redacted - unpublished data]

Figure 2. Sensitivity of predictions of the seismicity rate model to the values of parameters (μ and a) of rate-state friction. The model is calibrated to the observed cumulative seismicity (red curve) for three different values of the coefficient of friction μ, 0.55$\pm$0.05 (three black curves). In each case, parameter a is identified to match the cumulative number of earthquakes with M $\geq$ 0.5 during the calibration period only (2006 through 2016). After calibration, the model simulations with different parameter sets lead to very similar forecasts of triggered seismicity.

We have incorporated revisions in the manuscript to summarize the discussion above on this important point raised by the Referee. In particular, we have replaced the original Extended Data Figure 5 by Figure 2 above. We have also added the following paragraph to the manuscript (lines CM985-993):

As in traditional fault stability analyses, individual model parameters have uncertainty. Without data to constrain them, this uncertainty can propagate into disparate model forecasts. In our study, the parameter uncertainty is greatly reduced for all models as a result of the sharp sensitivity to the rapid variation in seismicity, which turns on and off in response to variations in injection rate. This opportunity for calibration of the models to the observed cumulative moment release and seismicity rates has a two-fold impact. First, it enables fairly precise identification of parameter sets that exhibit strong tradeoffs, and second, it makes model forecasts robust to variations in the values of individual parameters (see Extended Data Figure 5).

Also what does slip tendency analysis or fault slip potential predict for the fault? Is it largely unstable? In other words, does your coupled methodology show a different response from a simpler method?

The use of simplified geomechanical analyses can be very informative. Indeed, as part of the initial stages in the assessment of the recorded seismicity, we conducted a wide spectrum of analyses based on the Mohr-Coulomb criterion. In particular, we evaluated the influence of *in situ* tectonic stress and static friction coefficient on the propensity of the Costa Molina fault to be unstable (see Figure 3 in this document).

These analyses, however, are analytical exercises that cannot incorporate the wealth of field data available. For example: (1) the analytical pressure diffusion model cannot capture the complex reservoir architecture and is unconstrained by measured pressure data; (2) poroelastic effects (which are crucial to ascertain the behavior of the bounding faults at the field scale) are ignored; and (3) fault slip potential says nothing about the actual fault slip. In contrast, as shown above, our simulations allow calibrating the friction parameters sufficiently accurately to predict the response to different injection scenarios. Therefore, simplified ΔCFF /fault stability analyses—while informative in general—are inadequate to address the key objective of our study: managing injection-induced seismicity.

[Figure redacted - unpublished data]

Figure 3. Stability analysis of the Costa Molina fault using a simplified analytical procedure based on the Mohr-Coulomb criterion. Here, all parameters are fixed except for the *in situ* tectonic stresses. Different scenarios of tectonic stresses lead to different values of the fault friction coefficient μ_{crit} associated with fault slip. This exercise suggests that, because the CMF was not slipping prior to injection, $\mu > 0.45$. But it is not sufficiently realistic to allow reliable determination of the *in situ* value of μ, or the injection rate that does or does not trigger seismicity.

G. References are fine.

H. Very well written.

We thank the Referee for these positive comments.

References referred to in this response

Byrne, H., J. A. Silva, A. Plesch, R. Juanes, and J. H. Shaw (2020), The groundbreaking experiment in earthquake control at Rangely, Colorado, revisited. *Geophys. Res. Lett.*, 47, e2020GL088257.

Dieterich, J. H. (1992), Earthquake nucleation on faults with rate- and state-dependent friction. *Tectonophys.*, 211, 115-134.

Improta, L., L. Valoroso, D. Piccinini, and C. Chiarabba (2015), A detailed analysis of wastewater-induced seismicity in the Val d'Agri oil field (Italy), *Geophys. Res. Lett.* 42, doi:10.1002/2015GL063369.

Improta, L., S. Bagh, P. De Gori, L. Valoroso, M. Pastori, D. Piccinini, C. Chiarabba, M. Anselmi and M. Buttinelli (2017), Reservoir structure and wastewater induced seismicity at the Val d'Agri oilfield (Italy) shown by three-dimensional Vp and Vp/Vs local earthquakes tomography. *J. Geophys. Res. Solid Earth*, 122(11), 9050-9082.

Jha, B., and R. Juanes (2014), Coupled multiphase flow and poromechanics: A computational model of pore pressure effects on fault slip and earthquake triggering. *Water Resour. Res.*, 50(5), 3776-3808.

Juanes, R., B. Jha, B. H. Hager, J. H. Shaw, A. Plesch, L. Astiz, J. H. Dieterich, and C. Frohlich (2016), Were the May 2012 Emilia-Romagna earthquakes induced? A coupled flow-geomechanics modeling assessment. *Geophys. Res. Lett.*, 43(13), 6891-6897.

Juanes, R., D. Castiñeira, M. C. Fehler, B. H. Hager, B. Jha, J. H. Shaw, and A. Plesch (2017), Coupled flow and geomechanical modeling, and assessment of induced seismicity, at the Castor Underground Gas Storage Project. Report to the Spanish Ministry of Energy.

Kwiatek, G. et al. (2019), Controlling fluid-induced seismicity during a 6.1-km-deep geothermal stimulation in Finland. *Science Advances*, 5, eaav7224.

Raleigh, C. B., J.H. Healy, and J.D. Bredehoeft (1976), An experiment in earthquake control at Rangely, Colorado. *Science*, 191(4233), 1230-1237.

Reviewer Reports on the Second Revision:

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Referee #4 (Mirko van der Baan):

A Summary of the key results

The revised version presents a compelling case history on how coupled fluid-geomechanical modeling, combined with proper calibration and validation, can provide pertinent insights into fault reactivation potential and therefore management of seismicity in an active hydrocarbon field.

B Originality and significance: if not novel, please include reference

The revised version addresses various questions and suggestions I raised in the previous round. It now clearly states the novelty of the study; it clearly differentiates between the calibration, validation and forecasting periods (and corresponding data usage); it discusses the effect of uncertainty in estimated parameters on the final forecasts. I believe this is an outstanding (best-in-class) research study that deserves publication in Nature.

C Data & methodology: validity of approach, quality of data, quality of presentation

The methodology is well described. The amount of data used is extensive, covering multiple disciplines, with FAIR usage provisions for public sharing. The article is very well written with high-quality figures

D Appropriate use of statistics and treatment of uncertainties

The revised version and additional figures in the reply to the reviewer reports now addresses the effect of parameter uncertainty in their forecasts. The authors demonstrate that some uncertainty in estimated models parameter does not change their conclusions about recommended injection rates to mitigate seismicity.

E Conclusions: robustness, validity, reliability

The revised text better explains the novelty of this work compared with the existing literature. The previous revision already convincingly demonstrated validity of the approach. This has been further enhanced in this version

F Suggested improvements: experiments, data for possible revision

Nothing comes to mind

G References: appropriate credit to previous work?

Yes

H Clarity and context: lucidity of abstract/summary, appropriateness of abstract, introduction and conclusions

This is a highly accessible and well written manuscript despite the depth and amount of scientific work performed. I recommend publication in Nature.

********************************END********************************