Reviewer A Comments

For author and editor

Review of “Assessing earthquake rates and b-value given spatiotemporal variation in catalog completeness: Application to Atlantic Canada” submitted to Seismica by A. P. Plourde.

This manuscript introduces a new methodology to predict Magnitude of Completeness (Mc) taking into account the network geometry. In addition, the Mc variability in space and time is considered in a new b-value estimator, and last, the ratio of detected to predicted earthquakes as a function of the Mc is used to create a smoothed event-rate map. The methodologies are applied to an area with a sparse seismic network and show an overall improvement in the Mc, and Frequency-Magnitude Distributions.

Overall, I find this study interesting and adds to our knowledge on magnitude of completeness and b-values. My major comment regards the derived equation of Mc(d,t), and its robustness over several hundreds of km. In addition, a test or comparison with other methods might strengthen the conclusions of this study. My comments are listed below.

(1) Line 27. Perhaps add a reference at the end.

(2) Section 2 “Data and regional seismotectonics”. I don’t quite agree with the title. To me seismotectonics means discussing about faults, showing maps with moment tensors and/or focal mechanisms, etc. The text in that section mostly describes the seismicity patterns in the area. I’d recommend to rephrase the section title.

(3) Line 53. “Moment-rate is much less the biased due spatial Mc variation”. Perhaps consider rephrase this sentence.

(4) Figure 1. I’d recommend to split Figure 1 into to two figures. Figure 1a-c, is mostly related to section 2. There I’d like to see a map with the network coverage, similar to 1a but without d, which will also include the seismicity in the area. That will help to better understand figures 1b and 1c.

Please also consider adding a scale on the maps, and also use the same projection everywhere.

(5) Line 80. Here I’d introduce the new Figure 2 which shows the d distribution (similar to 1a).

(6) As the study includes areas in high latitude, I'm wondering whether the distance is calculated using spherical coordinates. If not, I'd suggest to try this or document how the distance is measured.

(7) Section 3.1 – Figure 2c. This figure shows the relation of Mc with distance (d). For the first ~100 km the fit seems to be good, then it breaks for a few km ~150-170 km and then around ~180-200 km is good again. For distances between 200 to 1000 km I find only a few points being consistent with the model.

Because this equation is used for the next sections, I think it should be revised, in order to prove its robustness. For example, I'd suggest to try and fit the first 200km and then the rest (200-1000 km), and explore whether the constants remain the same. Perhaps it might also be necessary to try and fit between 0-100 km or so.

In case that more than one equation is necessary to describe the Mc(x,t), then sections 3.2 and 3.3 should be revised. That also means that the final results and interpretations, after considering the new equations, might be more robust.

(8) Section 3.2. It is not clear to me what Mc is used. Is the one from Fig. 2c?

(9) Section 3.3. I think that Figure 1d should be re-numbered and cited here as Figure 4 (?).

(10) One suggestion is to compare the b-vaule results with a newly proposed methodology which is suitable for variable Mc (van der Elst, 2021). To me this will help to improve the discussion section and better place the proposed methodologies.

I hope the author will find this review helpful.

Maria Mesimeri

Swiss Seismological Service & Bedretto Laboratory | ETH Zurich

maria.mesimeri@sed.ethz.ch

References

van der Elst, N.J., 2021. B-positive : A robust estimator of aftershock magnitude distribution in transiently incomplete catalogs. J. Geophys. Res. Solid Earth 1–19. https://doi.org/10.1029/2020jb021027

Reviewer B Comments

Review of "Assessing earthquake rates and b-value given spatiotemporal variation in catalog completeness: Application to Atlantic Canada"

This paper illustrates some (claimed) new procedures to compute the magnitude of completeness, the b-value of the Gutenberg-Richter law, and the earthquake rates using the Atlantic Canada earthquake catalog.

PRO:

- this work is an example of what each seismologist has to do in order to properly estimate the b-value and the earthquake rates, starting from the magnitude conversion and including the magnitude of completeness computation;

- some nice maps and plots;

- an interesting mixture between the Ogata and Katsura 1993 method with the Mignan et al. 2011 method (although non completely well explained);

CONS:

- the claimed new procedure to estimate the b-value was already published in 2021 (Taroni 2021 – GJI https://academic.oup.com/gji/article-abstract/224/1/337/5911583): this is not a big issue, you have just to properly quote the previously published paper;

- some parts of the paper are not clear, e.g. how do you take into account the temporal part of the variation of the magnitude of completeness, or the equation nr. 5 in chapter 3.3;

- some important figures are missing, i.e. the figures describing the catalog used in the computation (the classical epicentral map of events, and the time vs magnitude plot of events);

SPECIFIC COMMENTS:

Introduction:

- you have to introduce here (and also consider in your computation) the so-called Short Term Aftershock Incompleteness (STAI) problem regarding the b-value estimation (see e.g. Stallone and Falcone 2021 on this topic https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020EA001481);

Data and regional seismotectonics:

- please add a figure to illustrate your seismic catalog (see previous comment in CONS section);

- what about the depth of the events? Did you set a threshold?

- the moment rate map (Fig. 1c) is not useful at all;

- Fig. 1d is one of the results of your study, I think that you have to move this figure after Fig. 3;

- I appreciate your choice of not declustering the seismic catalog, but you have to discuss this choice more into detail (see e.g. Mizrahi et al 2021 on this topic:

https://pubs.geoscienceworld.org/ssa/srl/article/92/4/2333/594794/The-Effect-of-Declustering-on-the-Size?casa_token=C2jKFwKc5lsAAAAA:QTc8EojBbCNK_7AxrpemSojnvhpj_nmkRUJQ8uVaXDs65cnkbyOSxCwym7dEGMKqI1KOgrw);

Chapter 3.1

- here is not clear how did you take into account the possible temporal variation of the magnitude of completeness Mc; usually Mc decreases through the years due to possible improvement of the seismic network, and increases for some days after the strong event in the catalog (see the previous comment about STAI);

- here it seems that the Mc estimated with the Ogata and Katsura 1993 method also includes the b-value estimation, which consequently can change with space (and maybe time), but in the following chapter you assumed that the b-value is the same for all the catalog; this inconsistency not prevent to have correct results, but it must be discussed more into detail;

Chapter 3.2

- this very important result of eq. 4 was already published (see previous comment in CONS section);

- the estimation of the b-value with the least-square method is not used anymore, you can remove this part and the comparisons shown in Fig. 3 (green lines);

Chapter 3.3

- this part of the paper is the least clear; you must better explain how it is possible to obtain the eq. 5 and why this equation is important to obtain the earthquake rates (how do you can produce Fig. 1d from this equation?); you must also compare your equation to some other equations used to estimate the seismicity rate in the case of a catalog with variable Mc (e.g. equation 10 of Kijko and Smit 2012 https://pubs.geoscienceworld.org/ssa/bssa/article/102/3/1283/326893/Extension-of-the-Aki-Utsu-b-Value-Estimator-for?casa_token=Znar5BxH1gcAAAAA:jsS3TkqxSw49ZrkDuv8u-YzoxMs-5ZqcCSd8ooOa6SlIwolSq1E6Zy4w64C3FH3vszYgcFI) ;

Discussion and Conclusions:

Lines 167-169: are you sure that this “non–log-linear bend” for the CSZ catalog is significant? It seems that you have about 30 events above magnitude 3.0: it is not a large dataset to infer departures from the Gutenberg-Richter law…

To conclude, given all these points raised by my review, I suggest a major revision for this paper.

I'd like to thank both reviewers for their constructive feedback. It took me a while to address all their concerns, as some of them prompted me to make some pretty significant changes—notably changing how the $M_c(d)$ function is computed, adding related Supplemental Info, and adding some new figure parts that compare b and the choice of M_c to complement the Gutenberg-Richter plots. I also updated the catalog to include data up to May 12, 2023, and extended the discussion of my new b estimator in Appendix 2—including adding correction for a maximum-magnitude (which isn't very relevant to the conclusions, but makes b-values in the new Figure 4g more robust). So much of the text is changed or re-ordered that "track changes" gets pretty messy, so rather than a full "track changes" I've highlighted important changes in the PDF markup (with a few comments).

My specific responses are below, please note that I put Reviewer comments in blue my responses in black.

Reviewer A

This manuscript introduces a new methodology to predict Magnitude of Completeness (Mc) taking into account the network geometry. In addition, the Mc variability in space and time is considered in a new b-value estimator, and last, the ratio of detected to predicted earthquakes as a function of the Mc is used to create a smoothed event-rate map. The methodologies are applied to an area with a sparse seismic network and show an overall improvement in the Mc, and Frequency-Magnitude Distributions.

Overall, I find this study interesting and adds to our knowledge on magnitude of completeness and b-values. My major comment regards the derived equation of Mc(d,t), and its robustness over several hundreds of km. In addition, a test or comparison with other methods might strengthen the conclusions of this study. My comments are listed below.

(1) Line 27. Perhaps add a reference at the end.

Thanks, we added references to recent review papers by Schultz et al. and Cheng et al., that focus on hydraulic fracturing and CO2 storage, respectively.

(2) Section 2 "Data and regional seismotectonics". I don't quite agree with the title. To me seismotectonics means discussing about faults, showing maps with moment tensors and/or focal mechanisms, etc. The text in that section mostly describes the seismicity patterns in the area. I'd recommend to rephrase the section title.

Good point, I think simply changing "seismotectonics" to "seismicity" is more accurate.

(3) Line 53. "Moment-rate is much less the biased due spatial Mc variation". Perhaps consider rephrase this sentence.

Thanks, I have corrected to "Spatial M_c variation biases moment-rates much less than event-rates, but..."

(4) Figure 1. I'd recommend to split Figure 1 into to two figures. Figure 1a-c, is mostly related to section 2. There I'd like to see a map with the network coverage, similar to 1a but without d, which will also include the seismicity in the area. That will help to better understand figures 1b and 1c. Please also consider adding a scale on the maps, and also use the same projection everywhere.

I have added a new Figure 1 with a scatter plot map of earthquake hypocenters to address a comment by reviewer 2, I considered plotting stations on there as well, but it becomes quite cluttered. My hope was that keeping Figure 2a alongside 2b–d makes it sufficiently easy to compare them.

I have also added a scale bar to Figure 2a (formerly Figure 1a), thanks for pointing out that omission. I'm not sure what you mean by "use the same projection everywhere"—the maps are made with an Albers equal area projection centered on 59 deg W. Please see my later responses re. Figure ordering.

(5) Line 80. Here I'd introduce the new Figure 2 which shows the d distribution (similar to 1a).

Please see my response to your comment about Figure 1d (now 2d). My preference is to leave all the maps together so that they can be easily compared side-by-side. I reworked much of this section to address various comments, I hope you find where I introduce both map figures to be reasonable.

(6) As the study includes areas in high latitude, I'm wondering whether the distance is calculated using spherical coordinates. If not, I'd suggest to try this or document how the distance is measured.

Yes, distances are calculated using actual geographical coordinates, so they are not affected by latitude or the choice of map projection. I think this might be too tedious a detail to warrant discussion in the text, but if you disagree I can include a comment.

(7) Section 3.1 – Figure 2c. This figure shows the relation of Mc with distance (d). For the first 100 km the fit seems to be good, then it breaks for a few km 150-170 km and then around 180-200 km is good again. For distances between 200 to 1000 km I find only a few points being consistent with the model.

Because this equation is used for the next sections, I think it should be revised, in order to prove its robustness. For example, I'd suggest to try and fit the first 200km and then the rest (200-1000 km), and explore whether the constants remain the same. Perhaps it might also be necessary to try and fit between 0-100 km or so.

In case that more than one equation is necessary to describe the Mc(x,t), then sections 3.2 and 3.3 should be revised. That also means that the final results and interpretations, after considering the new equations, might be more robust.

I've changed a few things about the method trying to address these concerns and improve the M_c–$d(x,t)$ model fit, notably:

1) I now bin earthquakes in a much simpler way. Previously I had a range of log-linearly spaced d_{max}, taking all earthquakes in the range $d_{max}/2 < d(x,t) < d_{max}$, and then assigning the resulting M_c to the 90th percentile d_{90}. I now simply sort earthquakes by $d(x,t)$ and bin them in groups of 300, with 50% overlap, and assign the resulting M_c to d_{max}. 300 is large enough according to studies such as Mignan et al. (2011), and the smaller groups alleviate the need to distinguish between d_{max} and something like d_{90}.

2) I put bound limits on b during the Ogata and Katsura (1993) model fitting: $0.85 \leq b \leq 1.05$. I previously noticed that the b output by my Ogata and Katsura (1993) code varied a lot, more so than I expect to be real, so I expect this should improve results. I explain this choice in the text.

Unfortunately these efforts still didn't produce a good power-law model fit, in fact in got a bit worse! The lower-than-predicted M_c over ~100–500 km appears to real feature of those MFDs, and to get a power-law model reasonably similar to that of Mignan et al. (2011) I had to exclude these data (I consider the Mignan et al. (2011) models quite robust given that they had so much more data—not that this study area should match their result precisely). It is still possible that a true $M_c(d)$ function follows a power-law more closely and non–log-linear character of MFDs in the $\sim$100–500 km range result in an artificially low output M_c. I actually think this is likely but, given that that seems like a big assumption, in the main results I instead introduce a smooth, increasing non-analytical $M_c(d)$ model fit that matches my results much more closely. The way I estimate the ratio $r(M_c)$ makes the final, predicted event-rate map fairly insensitive to this choice, and I demonstrate this in new supplemental figures S1–S4, which are computed using the power-law fit.

(8) Section 3.2. It is not clear to me what Mc is used. Is the one from Fig. 2c?

Yes, I am assigning an $M_c(x,t)$ to each earthquake based on their $d(x,t)$ and the model fit from Figure 3d (formerly 2c). I have tried to make this more clear in the text:

"We can evaluate $d(x,t)$ for each earthquake in the dataset using the distribution of seismometers that were active when it occurred." and later with the sentence "...i.e. we consider $M_{cE} = M_c(x,t)$, evaluated using $d(x,t)$ for each earthquake."

(9) Section 3.3. I think that Figure 1d should be re-numbered and cited here as Figure 4 (?).

I have definitely strayed from standard figure ordering/placements here by showing the results (i.e. Fig 1d) long before they are discussed. However, the alternatives (that I have thought of) are either:

1) Display Figure 1d as a separate Figure later on—this makes it much less convenient for the reader to compare the maps currently presented as Figs 1b,1c,1d. I think seeing them side-by-side is critical to be able to distinguish differences effectively.

2) Repeat maps from Figure 1b,1c in a later figure with the result-map—this lengthens the paper / increases file size somewhat unnecessarily.

Given that these alternatives aren't ideal, and the added wrinkle that having only three maps with this aspect ratio and level-of-detail makes it difficult to make an efficient use of page-space, my preference is to leave the "Predicted event rate" map as Figure 2d (previously 1d), but I will change it if it is deemed necessary by the editor. I have added the statement "The main result of this study (Section 3): ..." to part (d) of the figure caption" to help clarify.

(10) One suggestion is to compare the b-vaule results with a newly proposed methodology which is suitable for variable Mc (van der Elst, 2021). To me this will help to improve the discussion section and better place the proposed methodologies.

Thanks, I was previously unfamiliar with the van der Elst (2021) paper, and have now tried applying it to my data. The method was mainly intended to resist bias from changes in $M_c(t)$ during aftershock sequences, and an assumption is that the completeness magnitude is "sufficiently similar between any two successive earthquakes"—this is not the case for my "Full Atlantic Canada" catalog where there is significant M_c variation in space, not just time, but nevertheless I test B-Positive (their Equation 8) on the four MFDs in my Figure 4. I find that if I do not restrict to $M > M_c$ (either $M_W > M_c$ or $M* > Mc*$), the method returns extremely low b-values and essentially doesn't work—it returns $b = 0.50^{+0.02}_{-0.01}$, $0.50^{+0.01}_{-0.01}$, $0.46^{+0.01}_{-0.01}$, and $0.59^{+0.01}_{-0.02}$ for Figure 4 a,b,e,f. Perhaps I am misinterpreting their meaning of what data to include, but I cannot figure out how. Alternatively, if I restrict to $M > Mc$, (again either $M_W > M_c$ or $M* > Mc*$), I compute $b = 0.91^{+0.15}_{-0.13}$, $0.99^{+0.13}_{-0.11}$, $0.96^{+0.58}_{-0.29}$, and $1.02^{+0.12}_{-0.10}$, for Figure 4 a,b,e,f, respectively—similar to my original results but with larger confidence intervals given the fewer data.

From what I can tell, B-Positive is not useful for these datasets, but I'm not 100% sure that I've applied it as the author would intend so I don't think it would be useful/fair to include these results in the paper. I hope this explanation is satisfactory, but if not I am certainly willing to try further things to compare to B-Positive or other methods.

I hope the author will find this review helpful.

I did, thank you very much for the constructive feedback.

Maria Mesimeri Swiss Seismological Service Bedretto Laboratory, ETH Zurich maria.mesimeri@sed.ethz.ch

References van der Elst, N.J., 2021. B-positive: A robust estimator of aftershock magnitude distribution in transiently incomplete catalogs. J. Geophys. Res. Solid Earth 1–19. https://doi.org/10.1029/2020jb021027

Recommendation: Revisions Required

Reviewer B

This paper illustrates some (claimed) new procedures to compute the magnitude of completeness, the b-value of the Gutenberg-Richter law, and the earthquake rates using the Atlantic Canada earthquake catalog.

PRO: - this work is an example of what each seismologist has to do in order to properly estimate the b-value and the earthquake rates, starting from the magnitude conversion and including the magnitude of completeness computation; - some nice maps and plots; - an interesting mixture between the Ogata and Katsura 1993 method with the Mignan et al. 2011 method (although non completely well explained);

CONS: - the claimed new procedure to estimate the b-value was already published in 2021 (Taroni 2021 – GJI https://academic.oup.com/gji/article-abstract/224/1/337/5911583): this is not a big issue, you have just to properly quote the previously published paper;

Oh no, I missed this paper! You're right that my formula is basically the same as that of Taroni (2021), with the exception of the $\Delta M/2$ correction which I now explain in the Appendix (also, I was actually including the (N-1)/N correction before, but neglected to write it, it is now in the Appendix as well). Thanks for bringing this to my attention. I cite Taroni (2021) in section 3.2 and in the Appendix. In Section 3.2, I state that "we further generalize their method by allowing spatial variation of Mc...".

- some parts of the paper are not clear, e.g. how do you take into account the temporal part of the variation of the magnitude of completeness, or the equation nr. 5 in chapter 3.3;

$M_c(x,t)$ is evaluated for each earthquake based on its $d(x,t)$, using the model(s) fit from Figure 3d, and given the distribution of seismometers active when it occured. I have tried to clarify these aspects of the method in the text.

"We can evaluate $d(x,t)$ for each earthquake in the dataset using the distribution of seismometers that were active when it occurred."

- some important figures are missing, i.e. the figures describing the catalog used in the computation (the classical epicentral map of events, and the time vs magnitude plot of events);

Thanks, I now include a new Figure 1 with parts a and b, containing a scatter-plot style map and a time vs.

magnitude plot, respectively.

SPECIFIC COMMENTS:

Introduction: - you have to introduce here (and also consider in your computation) the so-called Short Term Aftershock Incompleteness (STAI) problem regarding the b-value estimation (see e.g. Stallone and Falcone 2021 on this topic https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020EA001481);

I don't think that STAI is very relevant to this dataset, given that most of the area has modest seismicity rates and high M_c. The entire dataset has $\sim$13000 events, over a huge area and over 30 years, vs. the Stallone and Falcone (2021) dataset which has a simlar number over $\sim$1 month and a much smaller area. My data therefore has much less potential for overlapping waveforms, and therefore less potential for STAI to be present. However, I do agree it is worth introducing STAI and explaining this in the text, and have added a reference to Stallone and Falcone in section 2: "We also assume that, given the vast geographic scale and generally low event rates and high M_c of this dataset, that temporal variation of M_c due to short-term aftershock incompleteness (Stallone and Falcone, 2021; van der Elst, 2021) is not a significant issue. " I reference it again in the final paragraph of the discussion: "Although we do not expect it to be a major factor in this dataset, short-term aftershock incompleteness (Stallone and Falcone, 2021) can also cause M_c variation in time."

Data and regional seismotectonics: - please add a figure to illustrate your seismic catalog (see previous comment in CONS section);

We have added a new Figure 1 with a "scatterplot" map and a magnitude-time plot.

- what about the depth of the events? Did you set a threshold?

I did not set any depth threshold. It might be interesting to add depth to the Mc function, i.e. Mc = Mc(x,z,t), although many earthquakes in this study don't even have a true depth estimate (and are assigned 5 km or 18 km depth based on a quick comparison of body/surface wave size), so this isn't really a good dataset to test that.

- the moment rate map (Fig. 1c) is not useful at all;

This comment seems a little harsh to me. The moment-rate map has advantages and disadvantages compared to the event-rate map (Figure 2b), as I discuss in the paper. It is less spatially-biased from the distribution of stations than the event-rate map, which can seen comparing Figs 2b,c,d. I haven't seen very many examples of other mapping moment rate, but a couple examples are found in:

Jin, S., Park, PH. Strain accumulation in South Korea inferred from GPS measurements. Earth Planet Sp 58, 529–534 (2006). https://doi.org/10.1186/BF03351950

Duplančić Leder, Tea. (2015). Geodesy in Croatia, 2011–2014: Report submitted to the international association of geodesy of the international union of geodesy and geophysics. Geofizika. 32. 133-141

although I find that it is more common to see publications list the moment rates for different areas, without actually mapping.

- Fig. 1d is one of the results of your study, I think that you have to move this figure after Fig. 3;

Please see my response about this to Reviewer 1 above. I prefer to leave these maps together so that they can be easily compared, but will separate them if deemed necessary.

- I appreciate your choice of not declustering the seismic catalog, but you have to discuss this choice more into detail (see e.g. Mizrahi et al 2021 on this topic):

Thanks very much for sharing this paper, it is certainly a good one to reference if opting not to decluster, as it shows the complications (and potentially bias) that declustering can introduce. I added two sentences after the exist comment on not declustering:

"Declustering is often performed as part of probabilistic seismic hazard analysis to remove foreshocks and aftershocks, such that the remaining earthquakes can be considered a Poisson process in time (Gerstenberger et al., 2020). However, declustering techniques require arbitrary thresholds to define what constitutes a foreshock or aftershock, and can cause unintended bias of b and hazard level estimates (Gerstenberger et al., 2020; Mizrahi et al., 2021)."

Chapter 3.1 - here is not clear how did you take into account the possible temporal variation of the magnitude of completeness Mc; usually Mc decreases through the years due to possible improvement of the seismic network, and increases for some days after the strong event in the catalog (see the previous comment about STAI);

For this study, I only consider M_c to be a function of the station distribution at a given time, I have tried to clarify that with the new sentence in section 3.1 "We can evaluate $d(x,t)$ for each earthquake in the dataset using the distribution of seismometers that were active when it occurred." and then "... we consider $M_{cE} = M_c(x,t)$, evaluated using $d(x,t)$ for each earthquake." in section 3.2. You raise some important ways that M_c can vary that would not be captured by my method. I consider those largely out of my scope, but I have expanded the final paragraph of the Discussion to address the assumptions / areas to next steps more clearly.

- here it seems that the Mc estimated with the Ogata and Katsura 1993 method also includes the b-value estimation, which consequently can change with space (and maybe time), but in the following chapter you assumed that the b-value is the same for all the catalog; this inconsistency not prevent to have correct results, but it must be discussed more into detail;

Good point, I have added the sentence "Note that here we ignoring variation of b with space to get average results over broad areas and times, despite observing varying b the previous section." Then in the Discussion section talking about the GR plots, I add "The full Atlantic Canada catalog is an extreme case, where M_c varies greatly in both space and time, and there may be interesting b variation within the sample. Nevertheless, these observations...". I went with "interesting" here rather than something like "significant"...basically I'm just thinking that any geophysical measurement is an average over some space and/or time sample, and whether that's important depends on the purpose of the study.

Chapter 3.2

- this very important result of eq. 4 was already published (see previous comment in CONS section);

Thanks again, we now reference Taroni (2021).

- the estimation of the b-value with the least-square method is not used anymore, you can remove this part and the comparisons shown in Fig. 3 (green lines);

Thanks, I have removed it in favour of the new b vs. M_c plots. My thinking before was that the least-squares fit help demonstrate the sensitivity to the choice of M_c, I hope the new plots are more effective.

Chapter 3.3 - this part of the paper is the least clear; you must better explain how it is possible to obtain the eq. 5 and why this equation is important to obtain the earthquake rates (how do you can produce Fig. 1d from this equation?); you must also compare your equation to some other equations used to estimate the seismicity rate in the case of a catalog with variable Mc (e.g. equation 10 of Kijko and Smit 2012) ;

I have really overhauled this section to try to explain the method more clearly, including writing the equation differently. I hope you agree it is more clear now.

It's difficult to compare results directly with the Kijko and Smit (2012) equation 10, which they write as:

$$\hat{\lambda} = \frac{n}{\sum_{i=1}^{s} t_i \exp[-\hat{\beta}(m_{\text{min}}^i - m_{\text{min}})]}, \tag{1}$$

or using notation more similar to mine (and multiplying by total catalog duration t to get an absolute number of events rather than a rate):

$$N_{\text{pred}}(M_c) = \frac{tN_{\text{obs}}}{\sum_{i=1}^{s} t_i 10^{-b(M_c^i - Mc)}}. \tag{2}$$

In these equations, n or N_{obs} only include earthquakes with magnitudes greater than their corresponding M_c^i. Using a similar line of reasoning to my section 3.2, we can consider the case where each event has a unique $M_{cE} = M_c(x, t)$ and predict the number of $M_W \geq 1$ earthquakes as:

$$N_{\text{pred}}(M_W = 1) = \frac{N_{\text{obs}}}{\text{mean}(10^{-b(M_{cE}-1)})}. \tag{3}$$

There are a few different ways you could treat event-averaging (rather than time-averaging), but the equation is effectively a linear extrapolation of the GR fit in log-space. For our event-rate mapping purposes, we could use this N_{pred} to define the ratio $r(M_c)$, but the denominator must still include all $M_W \geq 1$ earthquakes (otherwise we would lose the vast majority of earthquakes offshore, e.g. Figure 3c):

$$r(M_c) = \frac{N(M_W \geq M_c)10^{b(M_c-1)}}{N(M_W \geq 1)}. \tag{4}$$

The difference between this and the formula I was using already is really just whether the Ogata and Katsura model is used to predict the numerator (i.e. whether $M < M_c$ events are used in that estimate), or whether that estimate is derived only from $M \geq M_c$ events. This is now discussed briefly in the text, and

Discussion and Conclusions:

Lines 167-169: are you sure that this "non–log-linear bend" for the CSZ catalog is significant? It seems that you have about 30 events above magnitude 3.0: it is not a large dataset to infer departures from the Gutenberg-Richter law…

Valid point, and no, I'm certainly not sure that there is a significant bend in my dataset. I wasn't actually trying to infer a departures from the GR using those 30 events, really I was inferring the bend based on previous studies (e.g. Kolaj et al., 2020) that estimate much lower b from $M > 3$ events. However, not all studies do estimate a much lower b, and in hindsight, this all seems like a distraction from the main focus of this paper, so I've entirely removed this paragraph.

To conclude, given all these points raised by my review, I suggest a major revision for this paper.

Thanks for your constructive feedback, I hope my revisions are satisfactory.

References

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Reviewer A (Maria Mesimeri):

I'd like to thank the author for their detailed response. They adrressed all my comments.

Reviewer B:

This second version of the manuscript contains a lot of improvements, and also new important Figures. The author clarified all the points raised by my review. The new Supplemental Info is useful and guarantees the stability of the obtained results. A very minor point is related to Figure 1 a): yellow colors for large events, and red colors for small events are not intuitive (usually, the darker the stronger the events), but it is just a subjective consideration. I think that now the paper is ready for publication!