Empirical Relationships Between Ground Motion Intensities and JMA Seismic Intensity Scale: Rebuttal To Reviews

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We thank both Reviewers for their considered comments and have answered them in turn below. Both Reviewers recommended publication after minor revisions, and praised our work identifying and tackling an out-standing issue in earthquake risk science and our clear writing style. Whilst minor revisions typically suggests a few small changes to the language and discussion, two of the Reviewer's comments have led us to reconsider our approach and produce several substantive updates to our manuscript, which we have outlined here for clarity:

- Reviewer 1 commented that we did not fully explain how we processed our accelerograms. In answering this comment, we discovered an article published in *Seismica* in June 2026 by Loviknes et al. (2026b). This paper describes a detailed methodology for processing KiK-net and K-Net seismic data for the calculation of seismic intensity parameters and is paired with a dataset of published PGA and Sa(T) in RotD50 orientation Loviknes et al. (2026a). We have determined that these data are a more robust dataset with which to parameterise I_{JMA} and have redone all of our analyses accordingly. Unfortunately, with the exception of PGA, they do not provide the horizontal components of each seismic intensity metric and therefore we reached out to the lead Author, Karina Loviknes, who agreed to become a co-author of our paper and re-run her seismic processing scripts to calculate the geometric means of each component. This change in our approach has created a cascade of small improvements to the resultant parameterisations, misfit, and loss results, but does not materially affect our discussion or conclusions.

- Reviewer 2 highlighted that, other than our cross-validation exercise, we have not tested our approach using out-of-sample data. To rectify this oversight, we have now calculated I_{JMA} for all records within the NGA-Subduction database that are not from Japan and generate $\mathrm{PGA}_{RotD50} >= 0.1$ g (98.0677 gal; Mazzoni, 2022; Mazzoni et al., 2023). We then fit these data using our chosen parameterisation. These results are shown and discussed within a revised Figure 4 in the main text.

We have also made some minor improvements to the manuscript that we believe increase the fit of our models and the robustness of our results. These include:

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- The Loviknes dataset does not include 2026 samples but we have added earthquakes between 1997 and 2000 to compensate for this loss of data - our final dataset includes 22,863 strong motion records from 112 earthquakes.
- We have changed the weighting approach within our regression model to increase the influence of samples with $I_{\mathrm{JMA}} \geq 4.5$. This choice improves the fit for samples with the most extreme shaking, which are the most material in an insurance context.
- Due to an erroneous dating filter, we did not include earthquakes which occurred on the 1st of January in our analysis. This error removed the Noto Earthquake and some of its key aftershocks. We have now included these events which has increased the number of samples with Shindo Class 6+ and 7.
- We have decided to change our most-parsimonious parameterisation from a two-component to three-component model. This change improves the overall fit of our model and all model options are still provided in the Supplementary Materials.
- I_{JMA} is always rounded to one decimal place. We now also round the outputs of our parameterisations to one decimal place in all figures.
- We now present our results in RotD50 orientation within the main text as it is more common than geometric mean in academic contexts. We now also present all the same figures using geometric mean in the Supplementary Materials.

With these improvements and the incorporation of other minor comments by the reviewers, we hope you will consider our manuscript ready for publication.

1 Reviewer A

- **Comment 1**
  "The authors sustains and proves that a model that accounts for source parameters, PGA and spectral accelerations actually estimates better the seismic intensity and reduces the risk bias assuming a synthetic case. Additionally, the authors suggest that the estimated intensities agreed with the Vs30 distribution in Kanto area. My question is why not using Vs30 values in the model instead? What about Vs10 and Vs5? There are some works that suggest that these are more representative of the site effects than Vs30 (e.g. Pulido and Matsuoka, 2006). Are there ground models that account for these velocities?"
  The primary objective of our article is to present empirical equations that allow insurers to convert the output of catastrophe models to Shindo Scale. We need an empirical conversion because these models are quite restrictive in the outputs they provide. Unfortunately, they do not output V_{s30}, V_{s10} or V_{s5} values, and so none of these metrics can be used in our equations. Our model does not perfectly capture the changing soil conditions as a result. As the reviewers suggests, in the discussion we show that considering soil conditions is vital to reducing risk bias. Insurers should consider metrics such as V_{s10} or V_{s5} when pricing risks and use parametric triggers that are sensitive to soil conditions.

- **Comment 2**
  "For the synthetic test, the authors used a database that consider health care facilities and power stations. There are also amplification effects that may arise due to the height of buildings, producing some damage. In addition, different materials from different houses may not agreed in the same response as health centers and power stations. Is it possible to account for that effects on a synthetic case? "
  In our test, we chose health care facilities and power stations because they are often insured using parametric insurance and their placement across Japan roughly mimics residential and industrial exposure. Using parametric insurance clauses disassociates the payout received from the damage incurred. In reality, the limits and index triggers chosen within the insurance contract would account for differences in building vulnerability to shaking. In our synthetic example, we decided to keep all the contracts the same to enhance the interpretability of the results. The point of the synthetic test is to show the difference in payout using the different empirical conversions between shaking metrics and Shindo class, rather than to create the best possible insurance contract for each building. To improve the clarity of our methodology, have now reiterated this point within the main text - "All assets are allocated uniform insured values, or limits, of 10 billion JPY to create a 'flat portfolio' where limits are decoupled from true asset values. This normalisation removes confounding differences in underlying asset value and vulnerability to isolate the effect of I_{JMA} parameterisation.".

- **Comment 3**
  "Line 73. "... all recordings are corrected using scaling factors provided with the data." -> What do you mean? Did you performed instrument correction or ground amplification factor correction? Suggest to emphasize."
  Whilst addressing this comment, we discovered an article published in *Seismica* in June 2026 by Loviknes et al. (2026b). This paper describes a detailed methodology for processing KiK-net and K-Net seismic data for the calculation of seismic intensity parameters that is much more robust and sophisticated than our original approach. In addition, they have published PGA and Sa(T) in RotD50 orientation for all Japanese earthquakes $\geq 5\text{M}_w$ between May 1996 and December 2025 (Loviknes et al., 2026a). We have adopted these seismic intensity observations within our I_{JMA} parameterisations and have redone all of our analyses accordingly. Unfortunately, with the exception of PGA, they do not provide the horizontal

components of each seismic intensity metric and therefore we connot calculate $\mathrm{Sa(T)}_{GM}$. We reached out to the main author of this work, Karina Loviknes, who agreed to provide these data and become a co-author of our paper. This change in our approach has created a cascade of small changes to the methods, parameterisations, misfit, and loss results, but does not materially affect our discussion or conclusions.

- **Comment 4**
"Line 91. "three band-pass filters are applied". Although this information is already described in Kiyono et al. (1999) and Midorikawa et al. (1999), I suggest you put the corner frequencies you used for the three band-pass filters. Line 92. "The filtered components are then combined into a single waveform". How are these component combined? Did you sum the components? Did you compute the mean value? I suggest to clarify this point."
Both Reviewers requested the full explanation of how to calculate I_{JMA}, which we have now provided in the main text and it is repeated here:
"I_{JMA} is calculated from three-component acceleration time histories (Kiyono et al., 1999; Midorikawa et al., 1999). First, the acceleration time history in each shaking orientation is transformed into the frequency (f) domain using a Fourier transform. To isolate the range of ground motions associated with perceptible shaking and potential damage, a band-pass filter is applied to each spectra that combines a low-cut, high-cut, and periodic filter (F_{low}, F_{high}, and F_{period}, respectively):

$$
\begin{aligned}
F_{high} &= \sqrt{1 - \exp\left(\left(\frac{f}{0.5}\right)^3\right)}, \\
F_{low} &= \frac{1}{\sqrt{\begin{array}{l} 1 + 0.694\left(\frac{f}{10}\right)^2 + 0.241\left(\frac{f}{10}\right)^4 + 0.0557\left(\frac{f}{10}\right)^6 \\ \quad + 0.009664\left(\frac{f}{10}\right)^8 + 0.00134\left(\frac{f}{10}\right)^{10} + 0.000155\left(\frac{f}{10}\right)^{12} \end{array}}}, \\
F_{period} &= \sqrt{\frac{1}{f}}.
\end{aligned}
\tag{1}
$$

These filtered spectra are then transformed back into the time domain using an inverse Fourier transform, and combined using a root-sum-square approach into a single accelerogram. From this combined signal, a_0 is taken as the highest acceleration sustained for a minimum duration of 0.3 seconds. Finally, I_{JMA} is computed from a_0 (in $\mathrm{cm/s^2}$) using a logarithmic relationship:

$$
I_{\mathrm{JMA}} = \log_{10} a_0 + 9.4. \tag{2}
$$

Resulting I_{JMA} values are always reported to one decimal place. This calculation is performed using the shindo.py python library and are identical to those quoted by KiK-net and K-NET (RR-Inyo, 2021)."

2 Reviewer B

- **Comment 1**
  "Lines 32-35. Some common parameters used to trigger parametric earthquake policies are listed. It may be useful to also mention that there are often additional terms like time windows that apply to mainshocks and aftershocks, to help clarify the contractual terms of use, so to speak."
  We agree and have included this additional clarifying sentence - "These policies be triggered once on a single location and occurrence basis, or may include clauses that consider how losses aggregate across several events and/or properties within a portfolio, with the time and distance encompassed by an event strictly defined.".

- **Comment 2**
  "Line 39. It may be useful to specify it as "actual" damage in order to clarify which is viewed as correct and from which value the bias is measured."
  We agree and have included the word "actual".

- **Comment 3**
  "Line 52. "Several commonly reported ground motion intensity measures". It may be useful to specify these, since the specific references are provided at the end of the same sentence. "
  In the original draft we specify the intensity measures on line 58. We have rearranged this section so that they are joined to the citations.

- **Comment 4**
  "Line 73. To be clear, the value of I_{JMA} associated with each recorded measurement was directly provided by the JMA and not computed by the authors? Please clarify. I later note that this computed by the authors, but this could be stated earlier."
  The calculation for I_{JMA} is rigorously defined by the JMA and always rounded to the nearest 0.1. As a result, our calculated values and those provided with the data by the K-Net and KiK-net seismic networks are generally identical. "Resulting I_{JMA} values are always reported to one decimal place and this calculation is performed using the shindo.py python library (RR-Inyo, 2021)."

- **Comment 5**
  "Line 83. Please revise to clarify "where a is the recorded acceleration"."
  We have made this change.

- **Comment 6**
  "Line 83 and Equation 2. This adopts the RotD50 definition defined by Boore (2010), but it could be worth mentioning that Shahi and Baker (2014) proposed an empirical correction that allowed RotD50 values to be converted to corresponding RotD100 values, further increasing the applicability of the proposed model.
  Shahi, S. K., & Baker, J. W. (2014). NGA-West2 Models for Ground Motion Directionality. Earthquake Spectra, 30(3), 1285-1300. doi.org/10.1193/040913EQS097M"
  We have answered this comment in our reply to the Reviewer's Comment 17.

- **Comment 7**
  "Equations 1 and 2. Related to the previous comment, the definitions as written do not appear to match the standard forms and may contain errors. In Equation 1, the geometric mean is written as the maximum over time of the square root of the instantaneous product of the two components, which can require the square root of a negative value when the components have

opposite sign, whereas the geometric mean of PGA is normally the square root of the product of the two component peaks. In Equation 2, the RotD50 definition shows a product of the two rotated components rather than their sum, aNS cos theta plus aEW sin theta, taken before the maximum over time and the median over orientation. Since the PyRotD library computes these correctly, these are likely typesetting issues, but please correct them so the text matches what was computed."

We agree with the Reviewer's assessment of our PGA_{GM} and PGA_{RotD50} definitions and have made the appropriate changes. We apologies for our sloppy typesetting and have double checked the equations are correct in our code base.

- **Comment 8**

"Line 85 and throughout. Since spectral acceleration is a function of period (T) it may be more convenient to specify it as Sa(T) instead of Sa. Please consider."

We agree and have made this change.

- **Comment 9**

"Line 89. The choice of using gals as the unit for acceleration is a little unconventional, since fractions of acceleration due to gravity (g) have become standard practice. For the unfamiliar reader, it may be useful to specify that $1gal = 1cm/s^2$ *and its equivalence in units of g."*

We agree gal is not the standard in most academic contexts but it was opted for out of convenience since it is used to calculate I_{JMA} and remains a commonly used unit within the castastrophe modelling insurance community. We have decided to keep everything in gals but include a definition as suggested - "(equivalent to 1 cm/s^2 or 1/980.665 g)".

- **Comment 10**

"Lines 91-92. What are the frequency bands in Hertz filtered in this step? It is useful to know for others looking to recompute I_{JMA} *values for other scenarios."*

Both Reviewers requested the full explanation of how to calculate I_{JMA}, which we have now provided.

- **Comment 11**

"Line 93. It does not directly impact the work, but it may be useful to note that a similar kind of intensity definition based on sustained velocity pulses termed FIV3 has recently been proposed (Dávalos and Miranda, 2019) and has been shown to correlate quite well with observed damage. It applies a similar technique of filtering the signal and looking at the sustained pulse of loading, as the a_0 *definition here does."*

We agree that it is important to mention other intensity measures that could be applicable. But, for clarity of message, we believe it is more appropriate to address these options in the Discussion rather than the Methods section. We include the following statement in the discussion which now covers this point and cites the article in question: "More broadly, the parametric insurance market would benefit from wider adoption of instrumentally-derived intensity metrics that incorporate multiple spectral frequencies and align more closely with observed damage. Beyond I_{JMA}, promising candidates include spectral intensity, average spectral acceleration, and filtered incremental velocity, which could serve as effective triggers in other seismic settings (Wu et al., 2012; Dávalos and Miranda, 2019; Monteiro et al., 2026)"

- **Comment 12**

"Equation 4. The logic behind this functional form and also the way it is written is a little strange. For the case of $j = 1$*, the term* $x_ij = M_w$ *for observation i. However, the second*

term on the right hand side sums from $j = 1$ to 1, meaning there is no actual summation at all, and it could simply be written $b_j * x_i j$. Please comment on this. Additionally, why is a linear relationship with magnitude assumed but a log10 relationship assumed with the other parameters? Ground motion models are typically natural log models, but perhaps there is a specific reason for the choice adopted here. Please comment. "
We apologise for the confusion here. Originally, both M_w and D_{hyp} were considered as linear terms and there was a need for the first summation. With Equation 4 now only including M_w as a linear term, there is no need for the first summation and we have removed this unnecessary vestigial syntax.

We agree with the reviewer that many ground motion models use natural logs. We choose $\log_{10}$ because acceleration sits within a $\log_{10}$ term in the I_{JMA} calculation (Equation 2 above). Our hypothesis is that we are more likely to develop a well-fitting empirical equation if we treat our acceleration terms in the same way. This approach was also adopted by Karim and Yamazaki (2002). The treatment of non-acceleration terms, M_w and D_{hpy}, was decided through trial and error. Neither of the non-acceleration terms are material in our paper or its outcomes. We state our reasoning in the paper on line 109 in the first submission "The relationship between I_{JMA} and maximum acceleration a_0 is log linear and several previous authors have identified log linear relationships between I_{JMA} and both D_{hyp} and ground motion intensities (Campbell and Bozorgnia, 2011; Karim and Yamazaki, 2002; Midorikawa et al., 1999).".

- **Comment 13**
"Lines 125-126. You could mention that this multicollinearity is the basis for same-site IM correlation models used in ground motion record selection applications, reviewed in Monteiro et al. (2026), for example."
Similar to our response to Comment 11, we have made reference to this study in the discussion rather than here in the methods.

- **Comment 14**
"Figure 2a. This plots the Pearson correlation coefficient between the absolute values of the ground motion intensities, correct? Not the residuals with respect to some ground motion model as is typically done. Please clarify this."
We have added the following to the figure caption - "calculated using their respective absolute values".

- **Comment 15**
"Table 2 and Equations 7 and 8. There are small inconsistencies between the table and the equations that should be reconciled. The PGA coefficient is 1.27670 in Table 2 but 1.27690 in Equation 7, and the P2 intercept is 0.3832 in Table 2 but 0.38325 in Equation 8. "
The inconsistency between Equation 7 and Table 2 was an error which we have rectified - we thank the Reviewer for the diligence. The Equation 8 inconsistency stems from the true value being 0.383247953 which confusingly rounds to both 0.3832 and 0.38325. For the sake of consistency we have opted to display all values to 4 decimal places.

- **Comment 16**
"Figure 3c. The associated text only describes the accuracy in identifying Shindo classes 5- and 6- (reported as 72% and 73%, respectively). Why are the other classes discussed? Furthermore, how is this accuracy computed? For the 5- class, I would count this accuracy as 594/(594+118) = 0.834, since we are viewing the data vertically and with respect to the actual observed Shindo class on the horizontal axis. Please comment."

The Reviewer is correct that accuracy is calculated vertically. However, there are 123 Shindo Class 5− observations which are modelled as 4, as a result the accuracy would be calculated as 594/(594+118+123) = 0.71 (see figure below - note that this is with the old parameterisation from our previous submission). This is slightly lower than 0.72, since the text states the accuracy for observations ≥ 5−, which sums to 72%. For clarity, we have decided to instead quote the accuracy of ≥ 5+ and ≥ 6− which are visible within the original figure and the most relevant Shindo Scale observations.

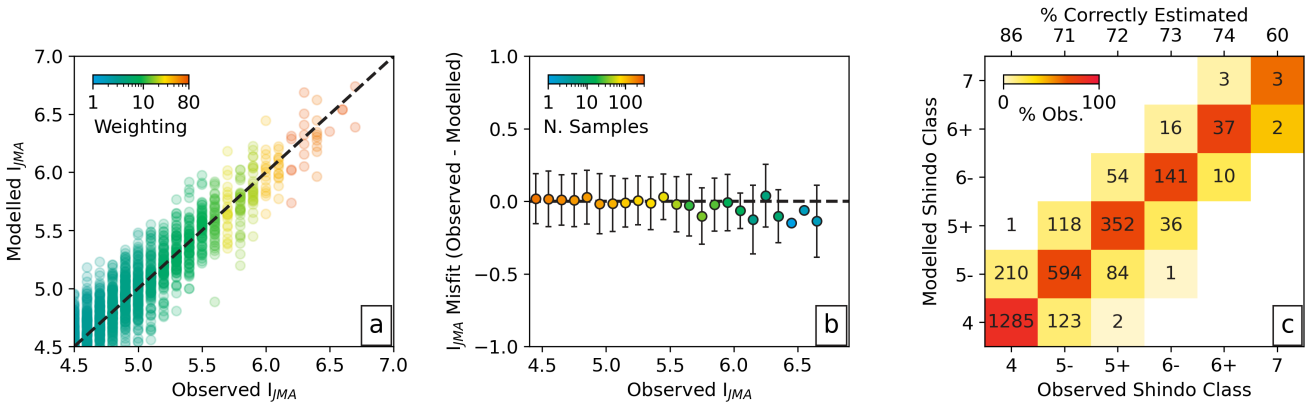


Figure 1: Comparison between observed and modelled I_{JMA} calculated using P1 (a) Modelled I_{JMA} as a function of observed I_{JMA} for all ground motions recordings with observed $I_{\mathrm{JMA}} \geq 4.5$; samples coloured by weighting during regression fitting; dashed line = 1:1 for comparison. (b) Circles and error bars = mean and standard deviation residual misfit between observed and modelled I_{JMA} for all samples in each 0.1 I_{JMA} interval; circles coloured by the number of samples. (c) A comparison between observed and modelled Shindo classes using conversion outlined in Table 1 of main text. Number and colour of each bin denotes the count and percentage of observed samples of the class, respectively.

- **Comment 17**
  "Line 185. Would this maximum resultant horizontal PGA be equivalent to the RotD100 component? It may be worth stating. Later on lines 192-193, the difficulty of moving between this RotD100 and GM or RotD50 was noted, but the previous work by Shahi and Baker (2014) can actually help resolve that issue to a certain extent."
  Resultant and RotD100 are slightly different metrics. PGA_R is calculated by combining the maximum of the first and second axes regardless of orientation, whereas RotD100 is the maximum combined when orientation is considered. As a result PGA_R is always equal to or larger than RotD100. These two metrics are identical for PGA but can vary considerably for Sa(T). Similar to the conversion between PGA_{GM} and PGA_R, small uncertainties in this conversion will have larger impacts on the final outcome and therefore using these conversions within our equations should be treated with caution. We have now cited Shahi and Baker (2014) within the discussion of these conversion terms. We do not know of earthquake risk models in insurance settings that rely on RotD100 and therefore we do not consider a fit using this metric unnecessary. Nevertheless, if the reviewer feels strongly that conversions between RotD100 and Shindo scale would be helpful to the community, we can consider estimating empirical equations in the same way as we do for PGA_{GM} and RotD50.

- **Comment 18**
  "Figure 4. Similar to the previous comment on the accuracy of the modelled versus observed Shindo class, it may be worth noting the accuracy of this existing model here also, and comparing it to your proposed one. Furthermore, you may insert the accuracy "per stripe" as

a value placed just above each Shindo class, or to the right of the plot. For example, the 5-Shindo class in Figure 4b could be computed as 444/(444+273+21) = 0.602 and placed above the plot. The same computation could be carried out for other classes and the mean accuracy across all classes computed for this model, which could then be compared to the corresponding statistic from your proposed model. It is rather crude, but it may give a better overall feeling for how much more accurate your proposed model actually is."

We have addressed the Reviewer's confusion over our uncertainty metric in Comment 16 and we agree with their suggestion to add a clarifying uncertainty metric to the figure. We have added the metric to each vertical column in Figure 2 and supplied percentages rather than raw sample numbers to the subplots of Figure 4 of the main text.

- **Comment 19**

"In addition, it is worth noting that the comparison is not entirely like for like, since the proposed model is fitted to this dataset whereas KY02 is not. Your own refit using only M_w and PGA reaches a wRMSE of about 0.33, close to the 0.358 of KY02, which suggests the improvement comes mainly from adding Sa(1.0 s) rather than from refitting on a newer and larger dataset. Bringing this like-for-like comparison into the main text, together with an out-of-sample check such as leave-one-event-out, would make the improvement more convincing. On a related note, the abstract states that the model outperforms previously published parameterisations, in the plural, but only KY02 is actually compared, so this could be softened or the other models compared. It is also worth stating that wRMSE is a weighted error and so is not directly comparable to unweighted RMSE values reported elsewhere. "

We agree with the Reviewer that using the dataset that we parameterised the model with to compare with the old model that was not fit using that dataset is slightly unfair. We also feel that our model would benefit with a robust test against out-of-sample data. We therefore now test our model and KY02 against non-Japanese strong motion data from the Next Generation Attenuation Subduction database (Mazzoni, 2022; Mazzoni et al., 2023). This analysis is now presented in Figure 4. We have also recalculated the wRMSE of the KY02 model using the five-fold cross validation approach we used in our analysis. We have also changed "previously published parameterisations" to "a previously published parameterisation" in the abstract.

- **Comment 20**

"Section 2.1. Since the ground motion intensity metrics are computed from the recorded histories, and so are the I_{JMA} values using the Python script described, is the study really limited to the Japanese ground motion databases KiK-net and K-NET? Could other databases be aggregated in to extend the number of samples? Please comment."

We agree with the Reviewer that additional data from other countries could be included, but the data would all need to be processed in the same way otherwise you introduce additional biases as shown by our updated discussion of Figure 4. We have decided to keep our analysis to just Japan but now include an out-of-sample test using NGA-Subduction data.

- **Comment 21**

"Lines 218-220. The Shindo class of the nearest station (within 20 km) was computed and assigned to the assets analysed. This can give the impression that there is some approximation or additional source of error in the case study, which is not actually the case since all methods adopt the same approach. To remove any apparent doubt, why not just say that the assets were assumed to be located exactly at the nearest recording station, hence the recording station time history is the exact shaking it endured. It is the same as what you have actually done, but phrased slightly differently. Please consider."

We agree with the Reviewer and have added the following - "Note that the shaking experienced by the insured locations may differ from that of the seismic stations they are linked to, this

additional basis risk is also present in real-world parametric insurance deals of this nature."

- **Comment 22**
  "Lines 232-239. It seems most of the errors in the model predictions are driven by the Tohoku-Oki event. Can you provide any reasons why this event was over predicted? Given the large magnitude of the event, was the model poorly fitted to this kind of data at very high intensities? Was this a motivation for the weighted approach adopted earlier in Equation 5? Please comment. "
  Tohoku-Oki is the largest event in the catalogue and therefore the event that generates the highest insured losses. As a result, while the percentage of model misses may not be any higher or lower, the total insured loss is much greater. Overall, we were slightly underestimating the shaking at the highest intensities, which should lead to an underestimate rather than an overestimate (see Figure 1 in this rebuttal). Figure 2 of this rebuttal shows Figure 6 in the previous submission of our manuscript. In Figure d, we show the residual misfit for seismometer locations, those associated with Tohoku payouts are shown in red. Three and two locations have large positive and negative misfits $I_{JMA} > 0.2$, respectively. Of those with positive misfits, two lead to increases in Shindo Class increases and therefore increases in insured loss. On the otherhand, neither of the large negative misfits lead to a class change. The net result is an overestimation of insured loss. We believe this result stems from the sharp stepped nature of using Shindo Class to trigger payouts - a fact we discuss later on in this section of our paper.
  Our weighting methodology was motivated by better fitting the most extreme shaking observations. We have updated our weighting approach which has improved the overall fit and reduced our model miss for Tohoku-Oki in our revised manuscript.

- **Comment 23**
  "Line 274. Remove the word "should". In the examples provided, they did reduce the basis risk. "
  We have made this change.

- **Comment 23**
  "Conclusions. This section feels a little light and refers to other material presented elsewhere. It would be more informative if it were self-contained and provided a short summary of the work carried out followed by a brief set of conclusions outlining the main takeaways of the work."
  We understand the Reviewer's point here, but we feel it is a matter of taste. We believe that a conclusion should be as short as possible and to make it self contained creates unnecessary repetition since anyone reading the conclusion should have read its context within the bulk of the paper. The abstract already acts as a self-contained summary of the work followed by a brief set of conclusions that outline its main take-aways.

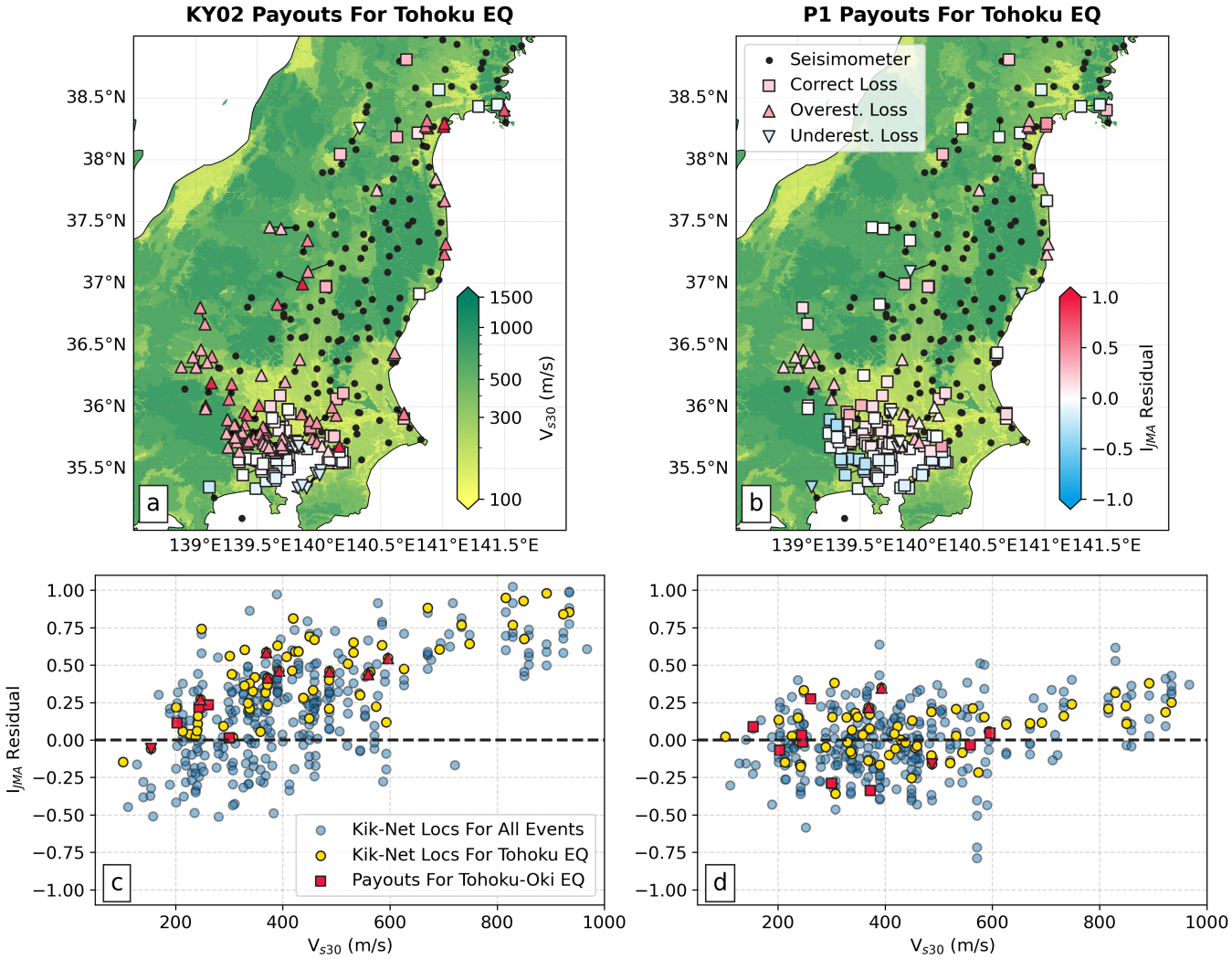


Figure 2: Parameterisation performance for the Tohoku-Oki Earthquake, 2011. (a) Map of Tohoku-Oki-earthquake-affected region with background grid coloured by V_{s30} value (Heath et al., 2020). Inverted-triangles, squares, and triangles are insured locations where triggered losses in the synthetic portfolio are under-, correctly and over-estimated using KY02, respectively. Symbols are coloured by the residual misfit between parameterised and observed I_{JMA} and black lines tie insured locations to their nearest seismometer. Black dots are locations of seismometers with Tohoku-Oki earthquake ground motion intensity recordings (Supplementary Table 1). (b) Same as Figure 2a with I_{JMA} values calculated using P1. (c) Difference between observed and parameterised KiK-net station I_{JMA} values ≥ 4.5 using Equation 14 in Karim and Yamazaki (2002) as a function of V_{s30} values provided by Zhu et al. (2020). Blue circles are all seismometer recordings for all earthquakes in Supplementary Table 1; yellow circles are recordings for Tohoku-Oki earthquake; red inverted-triangles/squares/triangles are recordings that trigger losses in the synthetic portfolio that are under-/correctly/over-estimated using KY02. (d) Same as Figure 2c but parameterised I_{JMA} calculated using P1 in Table 2 of main text.

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