

Reply to reviewer comments

Anonymous Review #1

The authors of this manuscript present a very in-depth explanation for the recalibration of the local magnitude scale used at the Royal Observatory of Belgium. The authors use a variety of methods to automate amplitude measurements as well as perform the main recalibration. The work is certainly useful, timely, and will help improve our understanding of small earthquakes in areas where small, induced earthquakes have been prevalent. The main goals of this paper are straightforward and I think the authors address them well but I do have some suggestions and questions for the authors that focus on the methodology, clarifying some of the data and methodological decisions, and organization. More specific comments can be found below.

I think it would be helpful to have an initial figure that shows the distribution of events and stations used in the study. The authors give a very in-depth description of the methodologies used to recalibrate magnitudes but do not go very far into describing the input data set. I think it would be helpful to see the distribution of available stations in particular, and to know at least the dataset size that the authors are working with. I would suggest moving figure 1 in the supplement to the main text and adding stations if possible.

We have moved the map in Figure S1 from the supplement to the main text, and added the location of the main stations used in our analysis, as suggested.

Line Specific Comments

24: Sentence sounds awkward, would suggest rewording to something like "... these results produce improved models of ..." or something similar

We changed this into: "these results have led to improved models of..."

47: When you refer to the "two distance terms" are you speaking about R_e appearing twice in the equation? As it is written now, it implies that there are two different distance variables which I'm not sure is what you meant?

Changed into "two R_e terms"

69: How many is several?

Numbers provided

110: Although not necessarily needed, it would be interesting to see a plot of this.

This is a hypothetical case

115: Can you be more specific as to how long is "a few years"

Replaced by "a decade"

124: Nowadays seems quite informal?

Changed to "now"

125: I assume “our main database” refers to the royal observatory of Belgium, but can you be more specific or state that? Or consider rewording to “We use data from the BELSHAKE database...” omitting the first part of the sentence.

This has been clarified: “... the database containing all routine seismic data of the Royal Observatory of Belgium (referred to as “ROB catalog” henceforth)”

128: How many events is this?

Number provided

Lines 134 – 154: I wonder if this could be moved to the supplement? The authors mention that it is a major challenge so perhaps that is why it is in the main text. However, I think that aside from adding context regarding the data quality, it doesn’t seem to be necessary for the main narrative of the manuscript. However, this may just be a personal preference, feel free to keep it in the main text if you disagree.

This has actually been described in more detail in another paper. We have shortened the section to two sentences, referring to the other paper for more details.

Line 161: Why not?

This is due to limitations of the current database structure.

Line 167: Do you mean velocimeter or accelerometer?

Changed to velocimeter

Line 171: What is the “small buffer”?

Changed into “buffer of 0.1 s”

Line 176 – 179: This is a long sentence that can be split into two: “... and data from other networks. Additionally, the start and end times...”

Done

Line 187: Earlier it is stated that you use the BELSHAKE database because amplitudes are not routinely stored in the ROB database. However here, you compare the automated amplitudes with manually determined amplitudes in the ROB database. I’m assuming you are just using whatever is available from ROB, if so, are the available amplitudes skewed in any way in terms of availability? i.e. newer events, larger events, etc.

This is mainly the case for older events, as mentioned in line 125, so the available amplitudes are mainly skewed by age. This also appears from Fig. 3

Line 194: Why do you only show amplitude results for one combination?

We already summarized the general trends obtained with the other combinations in the text, but we now also added the corresponding plots to the ES.

Line 198: Again, it would be helpful to know how large this dataset is.

Number provided

Line 215: I think table 1 could be moved to the supplement or the datapoints just omitted if the magnitudes used in comparison are not determined by ROB?

We made the table and figure again excluding events for which we are sure the ML value has been copied from other agencies.

Line 236: Is hypocentral location included in the BELSHAKE database? Did you perform relocation? What are the potential errors associated with depth?

Focal depth is included in the database. Many events have been relocated, as described in the paper describing the BELSHAKE database (Vanneste et al., 2026). The median and 95th percentile of the vertical location uncertainty are 1.2 and 5.7 km, respectively. These uncertainties are now mentioned explicitly in the paper, along with the horizontal uncertainties.

Line 248: Can you give more information about the 6th degree polynomial. Why did you choose this? Also, why do you include it in the manuscript if you don't report results in Table 2 or figure 5?

Polynomial regression is often used to fit data. The polynomial fit was not shown due to space constraints, and because it performs worse than the other fits. We decided to drop this entirely from the manuscript.

Line 304: This equation doesn't have a number.

There is a number for the two equations combined. We changed the order of the equations, and separated them by a semicolon to denote they are related.

Line 481: Is it actually increasing with decreasing magnitude or does it just seem that way because there are a lot more points at smaller magnitudes?

We had a closer look at this. It turns out that there is a slight increase in bias with decreasing magnitude, which is stronger than the change in variability. We modified the text accordingly.

Line 443: Is there any relationship between the station residuals and any physical components (i.e. soil type, depth, geographic location?) If there are interesting relationships as to which stations show higher residuals, it would be interesting to see that in a figure, even if just in the supplement. I also wonder whether this section could just be moved to the supplement since it mostly just describes and points to figures/table that are not in the main text.

We added a plot showing station residuals versus VS30. This plot illustrates that there is a correlation with site effects, which is stronger for the horizontal component. We think this is an interesting result, that we prefer to show in the main paper rather than in ES.

Line 449: This should refer to figure S3

Corrected

Line 460: by "original scale" are you referring to the equations 4-5 with the new automated amplitudes?

Indeed. We added a reference to these equations.

Figure 1: ROB is not yet defined in the text.

Solved by writing: "values determined by the Belgian network (labeled "ROB")"

Also, I wonder if it may be more useful to show MLRNS – MLROB as a function of MLROB to show whether the agreement between different networks is magnitude dependent?

Done. We also added a brief comment on the relation between variability and magnitude.

Figure 2: Text in the image is too small to read.

Font size of labels has been increased.

Figure 3: See comment for figure 1. I think it may be interesting to see how the amplitude measurements compare for small events compared to larger events. If the relationship is not earthquake size dependent feel free to ignore this comment.

Done

Figure 5: In the equations, epicentral distance and hypocentral distance are designated as R_e and R_h respectively. However, in this figure they are called R_{epi} and R_{hypo} . I would suggest reconciling them to avoid any confusion. Also, in figure 5c I'm confused why it is referred to as R_{epi_depth} fit when equation 7 in the text is based on hypocentral distance

X axis labels have been renamed to match the text, and we changed " R_{epi_depth} " into " R_{hypo_depth} "

We also adjusted the X axis labels in Fig. 6 and Fig. S3

Figure 10: I would recommend putting the HRF2020Q relation on the same plot as the two fits. It is hard to understand the comparison between all of these fits when they are on different plots.

We chose to show our new fits along with the XY plot in the left panel. The density plot in the right panel compares one of these new relations with two existing relations. Since one relation is present in both panels, comparison is possible.

In all of the histogram figures, you show average difference except Figure 3 which shows ratios. Why is that?

This is because magnitude is a logarithmic function of amplitude, and subtraction in the logarithmic domain corresponds to division in the non-logarithmic domain.

Anonymous Review #2

Review of 'Recalibration of the local magnitude scale of the Belgian seismic network: Improved models of geometrical spreading and path duration' by Kris Vanneste, Thierry Camelbeeck and Thomas Lecocq

This manuscript presents a recalibration of the local magnitude scale for the Belgian seismic network, updating a framework originally established in 1985. The original scale relied on 1-Hz Lg-wave displacement amplitudes on vertical paper records using epicentral distance. While adequate for regional earthquakes, this introduced significant limitations for modern operational seismology, particularly for microseismicity recorded at very short epicentral distance ($R < 20$ km). The authors successfully recalibrate the ML model, and evaluate suitability of horizontal component waveforms. A novel aspect of the work is the introduction of a Fourier-based (and therefore more physical) attenuation correction. The work has broad reference to the readership of *Seismica* and should be published after minor revision.

Please see comments in the attached annotated PDF. In addition, general comments below:

- the context of 'updating' the attenuation model without considering whether the existing ML values (based on a calibrated mode from ~30 years ago) could be wrong or biased needs better justification. Or the following rephrased to better define the objectives: "In this study, we aim to recalibrate the Belgian *ML* scale as a function of hypocentral distance, paying particular attention to short distances (few tens of km), *while ensuring a good agreement with the original scale at larger distances.*"

We chose to define the objectives more clearly, by adding: "Considering the good agreement (within ± 0.1 magnitude units) of the original ML scale with the scales of neighbouring agencies, and for reasons of continuity, we do not seek to establish an entirely new scale, but rather to recalibrate the original scale as a function of hypocentral distance."

- site amplification (station terms) are not adequately discussed given the mixture of hard and soft sites.

We have added a figure comparing station terms with VS30, and added a small description of the correlation between both.

- 10 km break in GM horizontal attenuation: authors acknowledge this is weakly constrained. I wonder if amplification effects could cause issues for few short distance records (perhaps some data collected during small, targeted surveys? etc.). Were sensitivity tests run to assess the need for a 10 km hinge point? Comparing GM horizontal and vertical models on the same figure would be helpful to assess if they are similar (and perhaps justify that there should be one model). In general, it is difficult to assess the differences without a residual-style plot (e.g. correcting y-axis amplitude for the prediction and plotting vs. distance).

See comment for line 339

- It would be helpful to clarify the final procedure adopted for the new scale in practice. ie. are A values based on equation 13, even if it is noted that the similarity with equation 10 is negligible? Or simply using the half peak to peak amplitudes? etc.

We added a sentence explicitly describing the preferred procedure in §5.

- The duration model - it is noted that site conditions vary: does this have any impact on the durations (e.g. plotting residuals vs Vs30)?

We only used bedrock stations for the duration model, as mentioned in the text.

- Comparison of Mw and ML: was orthogonal regression used to account for the uncertainty in both ML and Mw?

No, it was not, but we have performed the regression again using orthogonal regression. The result is slightly different, and the corresponding coefficients have been updated in Table 6.

The justification of the decay model used for sourcespec is missing (why not use the developed attenuation model alone and not the 1/R). Presumably details of the approach are detailed in the cited work (Vanneste et al., subm.), but some brief details (were Q/kappa, stressdrop etc. fixed) would be useful here.

Because we only studied S waves, we only have a specific geometrical spreading model for S waves. For P waves, we had to resort to the commonly used 1/R model. The final adopted MW value is the weighted average of both solutions. We rephrased this to clarify this.

- Discussion to other related studies (e.g. in terms of the attenuation model, magnitude scaling etc) seems a bit limited. A closely related recent study would seem relevant: Altindal et al (2026) <https://doi.org/10.1093/gji/ggag243>.

We were not aware of this similar analysis for Switzerland during our study, but we read this paper and added references to it in a few relevant places. We do note that these authors model attenuation with a single hinge distance, whereas we allowed for multiple hinge distances.

Specific comments:

Line 32 - For completeness - how is this calculated? i.e. Filter definition? Peak (half or full peak to peak or zero-to-peak etc.) amplitude. Are all records used and the peak is simply assumed to be Lg (irrespective of distance etc.)?

Answered in lines 156-158, as indicated by the reviewer

Line 40 - This is rather ambiguous - within the context of seismology I would not class these as 'high frequencies' though.

We replaced "relatively high frequencies" with "frequencies"

Line 50 - Why? Since A(Re) is bandlimited to 1 Hz (in the Belgian implementation) is it not simply related to Q(f=1 Hz)? Equally, for non-band limited signals it is related to the dominant frequency(ies) at which the peak A occurs - so it is more complicated than saying this is the frequency independent Q. Suggest to remove 'frequency-independent'.

We removed "frequency-independent".

Equation 3 - In other instances (1,4) this is R_e?

Indeed, but in this formula it can be both, so we prefer to keep the more generic R.

Line 66 - Many ML scales also simply use(d) the vertical component.

This is probably correct, but we don't have a good reference for this, and we simply want to highlight why this choice was made in Belgium.

Line 77 - Why? More data?

Indeed. We clarified this.

Line 91 - This is a little difficult to follow given the mixture of ML and mbLg. My understanding is that Belgian ML were originally developed as m_bLg (are they recorded as such or as ML)? Subsequent analysis at short(er) distance on S-waves shows equivalence and therefore the assumption is $ML = m_bLg$? Magnitudes are recorded as ML (even if from distant events, recorded at Lg distances?).

This is exactly what we are explaining here. We are not sure why this is confusing.

Line 92 - Presumably 'current ML scales'? As most earlier instruments were vertical?

Yes, we added "currently used"

Lines 109-111 - Would seem more intuitive (at least to me) to say that identical earthquakes at different depths will be assigned different magnitudes?

This is correct, but there are two sides to it: as you point out, identical earthquakes at different depth would generate different amplitudes at surface, and hence be assigned different magnitudes. On the other hand, earthquakes generating similar amplitudes at similar distances would be assigned the same magnitude, regardless of whether they are shallow or deep.

We kept the original phrasing, but added this statement as well.

Lines 120-121 - It is understandable why this is the ideal situation. But this is not particularly 'scientific'. The old model should be tested in light of the new data?

See 1st comment.

Line 158 - Acausal Butterworth filter?

No, but this would only be relevant if peak positions would be important, which is not the case.

Figure 2 - Text not possible to read.

Also indicated by reviewer #1. We increased the text size.

Line 213 - Which are?

This is explained in the lines before, but it was not mentioned explicitly for the peak estimation method. We now explicitly mention it.

Lines 231-232 - I don't follow why smaller differences in delta_ML than delta_A (auto vs. catalogue) indicated better reliability of ML? Can this be better explained?

We do not mean better reliability, but better consistency between A and ML, compared to the catalog, because we demonstrated there are some issues with amplitude data of older earthquakes in the catalog/database. We added "particularly for older (pre-2000) events", hoping this makes it a bit clearer.

Lines 236-237 - So are ML_event taken from the original ROB catalogue? Why not a joint inversion as you have noted amplitudes from the new approach are more robust?

No, we use both the newly determined amplitudes and ML values, as they are internally consistent. A joint inversion is a good idea, but out of scope for this revision. We mention it as a possible future refinement in the conclusions.

Line 236 - = 3? Why mix M_{bLg} and ML here? In the figures it is ML_{ref} , which makes more sense.

This notation refers to the same reference magnitude in Eq. 3. We changed it into ML_{ref} , and added “with $M_{L_{Ref}}$ equal to $m_{bLg_{Ref}} = 3$ (Eq. 3)”. We made the same change in Eq. 9

Line 240 - What is the logic behind this? The short distance stations are required to constrain the attenuation?

We do not want to include events that are determined only by short-distance stations, precisely because we argued before that the ML of shallow small earthquakes, recorded only by nearby stations, is likely biased.

We do include short-distance stations, but only for events that are covered by a sufficient percentage of far-away stations. Phrasing changed to: “To reduce the influence of events determined solely by short-distance stations”.

Lines 294-295 - Why is this more important? Is it not more important that the model matches the data better? Plotting in residual form (i.e. y-axis amplitude, corrected for prediction) would better show the model performance.

Yes, but the overall gain in performance is negligible (standard deviation of 0.219 compared to 0.220. We removed “more importantly”.

The better match at longer distance can clearly be observed in the plot. We added a specific analysis of the residuals with respect to distance, focal depth, event magnitude and VS30 in §4.3 (which was renamed to “Residual analysis”). The new plots are in electronic supplement.

Line 339 - This seems poorly constrained - related to relatively few short-distance records. Are these records well distributed? Could site amplification effects be an issue here (as horizontals could arguably be more susceptible to such effects)?

Indeed, this is acknowledged in line 422, and further discussed there. However, it is observed for both the vertical and the horizontal component. We added “However, a similar knickpoint was found for the vertical component, so it is likely not an artefact in the data.”

Lines 380, 386-387: State also in terms of Q? E.g. For $V_s = 3.5$ km/s $Q = 595$

Done

Figure 9 - In both cases a couple of points that lie > 3 sd. What is the cause of these - outliers?

This is mainly the case for the recalibration based on the GM component. There appeared to be two different causes for the outliers. For the first event, the outlier was associated to a station from another network, for which the instrument response is suspected to be wrong. We removed this station from the plot. The second event is a very shallow event (focal depth < 2 km), and the outlier was due to a too high velocity assumed for the R_g arrival, which resulted in a window that was too short to pick amplitudes. We solved this by assuming a lower R_g -wave velocity for events with shallow focal depth.

Line 516 - Why? Is your solution not a better model for Belgian events?

This seems to be a misunderstanding. We calculated two MW solutions, one using P-waves and one using S-waves, and the final value is the weighted average of both.

Line 527 - For the relationship $2/3ML : Mw$, Should refer to Deichmann for this: Deichmann, N., 2006. Local magnitude, a moment revisited. Bulletin of the Seismological Society of America, 96(4A), pp.1267-1277.

Who also shows that $ML = Mw$ at moderate magnitudes.

We did not find this in Deichmann (2006), but in Deichmann (2017). We therefore added the latter reference.

Summary of resulting changes

Most reviewer comments have resulted in small changes that can be evaluated from the annotated version (with the “track changes” of Word) of the manuscript.

In addition:

- One figure has been moved from electronic supplement to the manuscript, as requested by one of the reviewers;
- One figure has been moved from the manuscript to the electronic supplement;
- One figure was added, to allow evaluating the relationship between station residuals and site conditions (represented by the average shear-wave velocity in the top 30 m, V_{s30}), as requested by one of the reviewers;
- The “station residuals” section has been renamed and extended with an analysis of residuals as a function of different parameters (hypocentral distance, focal depth, event magnitude and V_{s30}), showing that the recalibrated scale does not show any bias with respect to these parameters. This has resulted in 3 additional figures, which are in electronic supplement;
- We added three tables to the electronic supplement, containing the covariance matrix of the coefficients for the three most important fitted relations.
- An important paragraph was added to the conclusions.

Reference:

Deichmann, N. (2017). Theoretical Basis for the Observed Break in ML/Mw Scaling between

Small and Large Earthquakes. *Bulletin of the Seismological Society of America*, 107(2),

505–520. <https://doi.org/10.1785/0120160318>