Review Reports

Reviewer A Comments

For author and editor

Dear Authors and Editor,

The manuscript of Dahm et al. presents the background and method for determining the moment magnitude Mw in a fast and efficient way from peak ground amplitudes. The method is highly interesting for the seismological community and can be readily applied. The authors are to be recommend for making the software public access. The manuscript is well written and the results and figures are well comprehensible. I recommend this manuscript for publication in Seismica after some minor edits and clarifications.

Minor comments and questions:
1. You mention in L205 and L227 that N should comprise a “sufficient number of records” to be able to average out errors and unknowns. So what is the the sufficient number you would recommend? Would it be possible to determine theoretically based on your framework a rule of thumb of how many records (N) of peak ground motion would be needed at least to achieve a well constrained estimate of Mw? This would surely be dependent on the magnitude and depth of the earthquake, as well of course on the station distances. Maybe you could use the simulations done for Figure 4 as a basis. It would also be possible to implement a bootstrap on the station records/simulations on your test cases and see how well Mw can be determined from a reduced range of N (see also comment 2).
2. Please clarify for me the implementation description of the in your abstract mentioned bootstrap: Do I understand correctly that you “only” bootstrap the source parameters? Please also explain the reason for choosing N=25 (L276) What about the recorded peak ground motion records? This can be faulty too, or influenced by strong site effects, maybe bootstraping should be done on them too to asses the uncertainty better.
3. I think you have chosen to fit only peak values to have an accessible framework for the larger hazard community. What do you think about using the exact same framework but an envelope fitting approach in contrast to fitting only the peak values? It could be more stable, especially for smaller magnitudes (Fig 7b), as the automatic extraction of peak values of both P- and S-waves for very small magnitudes at close stations can be challenging. You could drop a line or two more in the text about why you did not use an envelope based approach and mention such approaches (e.g. Eulenfeld et al. 2021).
4. Qseek gave me comparable results for a test area in southern Germany when comparing with an envelope based approach, but had a slight tendency for underestimation for Mw <1, which I also find in your figure 7a for smaller magnitudes. Do you expect this to be a systematic issue and do you have explantion for this?
5. Figure 7: Is the cause for the large error of smaller magnitudes maybe the chosen frequency

range, which appears to be fixed to 0.5-2 Hz?
6. Do you expect site effects to average out as well? Those effects can be quite substantial. Do you expect that 1.5D velocity model, such as the fomosto Green's functions with ConfigTypeB, would be helpful in your framework?

Minor editorial comments:
Figure 7: The caption mentions green circles, when the figure displays yellow circles. Please also explicitly state in the caption for what the 1 and 2 sigma is given (color wise I expect for the synthetic peak amplitudes?)
Figure 4: Why is the discontinuity at 2km so strongly visible? What is the size of the modelled earthquake?
Figure 10: Please indicate what the orange circles represent, I assume they are the measured data from Dost et al, 2016.
L 118: "multiply" should read "multiple"
L121: "ML-ML relationship" should read "ML-Mw"
L123: should read: "... in source properties, such as e.g. stress drop…"
There are number of extra double spaces, such as in L12, L101, L308, caption of figure 5, L358, L428
L228: "rupture" instead of fracture
L287: Double bracket in citation
L330: Bracket missing around Sen et al., 2023

best regards,

Andreas Steinberg

Reviewer B Comments

For author and editor

Revision of the manuscript titled:

"Earthquake Moment Magnitudes from Peak Ground Displacements and Synthetic Green's Functions" by Torsten Dahm, Daniela Kuhn, Simone Cesca, Marius Iksen

In this paper, the authors propose a new approach to estimate moment magnitude that is based on observed and simulated values of Peak Ground Displacement (PGD). Given the definition of Moment magnitude, the authors develop a framework in which the reference magnitude is associated to full waveform simulated data, that can be integrated with observed Peak Ground Displacement (PGD) to obtain estimates of Mw.

The method consists in calculating a massive dataset of Green's functions from different source models (location, moment tensor, etc…), giving simulated values of PGD. Assuming a circular crack model they express magnitude estimate as a function of the ratio between the observed PGD and the simulated PGD. After formulating the mathematical problem, the authors validate the approach on two different datasets, the first one consists in events registered in a coal mine in Germany, the latter contains events from the Groningen gas field in the Netherlands.

The method is interesting and leverages some known issues in moment magnitude estimation: the synthetics allow to simulate attenuation curves that can be also extrapolated at distances where no observations are available; and the error associated to moment magnitude estimates directly take into account the contribution of epistemic uncertainty coming from the explored parameters in the simulations. Also, the proposed formulation easily correlates to other magnitude scales, allowing for comparison when the same technique is used to the PGD.

Overall, the work is of interest for several reasons, and the authors also included the whole framework in public packages available for the community. Nevertheless, I think some relevant points should be addressed and/or clarified with further detail by the authors.

The main issues and perplexities are summarised in the following:

- Some figures are poorly described and miss some information in the captions. The authors should make an effort in clarifying and add more details throughout the text.
- The mathematical formulation has some typos, and in some formula some assumptions are made which should be verified/justified in more detail, maybe adding the calculation in the SI.
- The workflow description is not too clear and regular throughout the text. For example there is no clear description on how the real data are processed or how they extract

observed PGD. I also think that the authors should smoothen the description of the procedure.
- In the discussion the authors only focus on the strength points of the proposed approach, but they do not comment on possible limitations and/or shortcoming.

GENERAL QUESTIONS

- Some figures (e.g Fig 2 and Fig7, but It is a general comment) could benefit from some legend being added in the plots. Also, the text could be improved by adding more information to describe the figures and the overall workflow.
- The measure of the peak ground displacement (PGD) is one of the key points in the application of the proposed approach. In the text, the authors never detail how this measure is inferred. Is it extracted from whole trace or obtained isolating the phase of interest? Throughout the paper the authors refer many times to the selection of maximum amplitude in several frequency bands, but they never mention the values they use in the real data application of the proposed method, and how they are selected. Also, they should be clearer on the data processing used on the earthquake waveforms.
- In the discussion the authors do not mention any shortcoming or limitation of this approach. Even if the method looks promising for having error estimates that coherently account for the epistemic uncertainty, I still think that there are some points that were not discussed in detail. The criteria on how time windows are selected from the different stations and traces are not detailed, but this could have an influence on the PGD estimate, and consequently on the magnitude. This is a well-known problem for instance in source parameter estimation with standard spectral techniques (e.g. Bindi et al 2023, Bindi et al., 2024). The second point is related to the selection of the station reference magnitude $M_0^{(r)}$ which is often mentioned to be "as close as possible" to the real magnitude value. In the text it is not clear how this value is selected during the proposed framework, and also it is not discussed what is the consequence of selecting a reference value that is "far" from the correct one.
- The final formula for the estimation of moment magnitude is obtained in the assumption of a number of station large enough to neglect the last term of equation (8), but I think that the real constraining condition should be related to the azimuthal coverage of stations around the source, that is key to state that the directivity averages to 1. The validation is performed on two datasets where the stations cover the whole azimuthal range around the seismicity, and I wonder if the same results would be observed in a case where this condition do not apply.
- In the mathematical formulation some of the calculation is nontrivial, and some assumption must be carefully justified. Moreover, this formulation would benefit from a revision of the adopted notation, since the current one results ambiguous in some of the proposed formula, especially in referring to station and model averages.
- The discussion section should focus also on possible shortcomings of the approach. What happens if the model used to calculate synthetics is not accurate enough to produce seismograms that match the observations? What are the possible influences

coming from how the peak amplitudes are measured? What could be the effect the non-favouring geometries of the acquisition network?

LINE BY LINE COMMENTS

Line 171

“The slip direction is denoted by a vector γ"

Here if γ denotes only the slip direction, the authors should refer to it as a versor, otherwise they could just state that the slip is denoted by the vector γ.

Line 193

“The circular rupture models implicitly accounts for the source spectra scaling”

There is a typo, the verb is used at the singular form.

It is not clear which scaling the authors refer to. It is the scaling with distance? Also, the authors state that the rise time is about 1.5 times the rupture time. This observation come from the formula, or from another research paper? They should add a citation, if this is the case.

Equation 6

If I substitute the radius expression from eq. (2) into eq. (5) I find a factor $(M_0)^(2/3) 3(16/7\Delta\sigma)^(1/3) v_r w g_i$.

I think there could be a typo in the calculation. Please check the formula. Also, why omitting the component notation? I think it is the correct way to represent the formula, since it is valid for each component of the displacement.

Since equation 6 is the key equation that is used to estimate the seismic moment, and hence the moment magnitude, I think it would be useful to also express M_0 in a further equation, that could fit after Line 209. Here the authors could directly put the expression for the average magnitude at all stations, since it is also used later in equation (8). This expression would also be of interest since shows that M_0 is proportional to the ratio between the peak amplitude and the Green’s function giving a direct comparison with more classical formulations.

Line 199

Add the verb “are” in period “directivity effects considered”. Also the last part should be “...stress drop $\Delta\sigma$ as well as rupture velocity v_r are known”.

Line 212

Here the average simulated PGD is called $|u|^r$, while later is $|u^r|. Please, check and uniform the notation. Also in equation (7) the notation is different for the type of character (Non bold).

Since this reference value is evaluated station by station, the notation should also include an index varying from 1 to N.

Line 215

The authors state multiple times that the average of w is equal to 1. The first comment is that it is not clear on which domain this average is considered. It is the average only on the polar angle θ? Are they considering an average also on the azimuthal angle ϕ? This should be clarified. Are the authors sure that the average is 1 or some normalization coefficients should be considered?

Also, this angle is mentioned at line 188 as measured in the plane of the source, but from Sato and Hirasawa (1973) this angle should be measured with respect to the normal to the source plane.

Equation (7)

In this equation the term $M_0^(r)$ is introduced, but it is not described in the text. I assume this represent the reference magnitude “as close as possible to the one of the earthquake” for the selected station that is mentioned at Line 213. Also, how the reference is chosen it is not discussed.

Another comment is on the assumption that the directivity term is not considered in the model average, since the average of w is constant/is equal to 1. In the formula w is multiplied by several terms that change for every model realization, therefore this operation is not linear, and the authors should demonstrate that the average can be isolated from the summation over k.

Line 226

I think that the notation is poorly chosen to describe the problem. From this line n is the station index, and the reference has the apex r, but also the reference value is obtained for each of the N stations, and thus also the reference should have the station index for the notation not to be ambiguous.

Equation (8)

The quantity $M_0^(r)$ is not introduced in the text. The formulation results a bit confusing in this section.

The assumption that leads to equation (9) is that with increasing number of stations the ratio that appears in the argument of the logarithm in the last summation approaches 1, and thus this term can be neglected. Physically, the directivity effect averages to 1 if one considers a distribution of station that has complete angular coverage with respect to the source, and thus it depends more on the geometry of the network than on the number of stations. Also, the possibility to send this term to 1 in the average is only valid if the authors demonstrate it (See comment on eq (7)).

The second point is that, even if the term $w^(n)$ averages to 1, then It is nontrivial that the ratio goes to one as N increase. This implication still depends on the chosen parameters for the event at each station, in the assumption that the values in the simulation are distributed around these values for the event.

The authors could calculate the value of the last term as a function of station number N, selecting different narrow azimuthal coverages and showing how the factor changes as compared to the case in which the azimuthal coverage captures the whole domain of angle θ.

Line 237

The event moment magnitude is sometimes referred to as M_W and in other occasions as M_w. Also at line 151 and in eq (1) the notation is inconsistent. Can the authors please check this throughout the text?

Equation (10)

The evolution from eq (9) to eq (10) is not extremely clear to me. Could the authors add the explicit calculation in the SI? This formula should contain both the contribution of the variability of the observed PGD and the variability of the simulated/modelled quantities, but with the notation and the explanation in the text, it is difficult to interpret the result.

The final expression of the final variance on M_w contains the term $<Var>$, that according to the notation used previously (Line 220) should be the average over the K synthetic realizations at each station, but the Var as expressed in eq (7) is already accounting this contribution.

Figure 2

From the text it is not clear if the values represented in this plot are simulated, and if it’s the case, it is not clear how they are generated.

In the caption some green circles are mentioned, but I don’t see any green circle in the figure. Maybe the authors meant grey circles? I also note that no grey point is visible for the first and last point of the plot.

The limits on the y axis should be increased to include the whole uncertainty of the first point on the left. For this point no red bar for the error is visible on the plot, why this happens? Lastly, an horizontal dashed line appears, but neither in the caption or in the text it is explained what it represents.

I also find the caption of this figure and the text not so clear in explaining its content.

Line 261

In the comparison shown in Fig. S.1, why do the authors only compare the M_x scales for $M_w>4$?

Line 278

“Gidded” was intended to be “Gridded”?

Figure 4

An horizontal line appears at depth of 2km, and in the caption the author justify this as an effect on the velocity model. Is the velocity model used to simulate those values of PGD the same shown in fig 5b or Fig9a-b? If this is the case, why the only horizontal line appears at 2km depth?

Which is the reference magnitude used to simulate the shown PGDs?

Figure 5(b)

The label $v_p - v_s (km/s)$ is misleading in my opinion, it would be clearer if the authors could just add a legend relating the colours to the different velocities. Indeed, even if it is trivial to understand, in the caption there is no description on the difference in the colours of the two curves.

Figure 6

Since the displacement is divided by the seismic moment expressed in Nm, I think that the authors should correct the unity of measurement here.

Moreover, is the distance r here the epicentral or hypocentral distance?

Figure 7

Also here in the caption of Figure 7(a) green circles are mentioned but have no correspondence in the figure.

In Figure 7(b) the authors could think of adding shaded points in the background showing the distribution from which the binned values are retrieved.

Line 331

The authors frequently recall the possibility of applying bandpass filtering, but they do not give any precise indication on the frequency bands they explore to obtain PGD (peak ground displacement) estimates. Also the length of time windows they analyse is never mentioned. In the application to real data, more specific indications on the data processing should be provided.

Figure 10

In figure 10(a) plotting the dashed orange line on top of the blue one could help the visualization.

In figure 10(b) the authors show the theoretical amplitude decay with distance. The first point is about the notation, it is not clear in the figures whenever the distance is the hypocentral or epicentral distance. Could the authors check and uniform this throughout the text? The second point is about the amplitude decay shown for the P waves. Even if the authors comment at line 372 and following about the reason why the amplitude starts to increase at distances larger than 35km, they do not comment on the very steep decay of amplitudes in the first 15 km, which is very different for the usually assumed $1/r$ used in standard models for the far field radiation.

Line 438

In the abstract and here in the conclusion the authors state that the method could be easily transferred to DAS data, but the model they proposed is developed for peak displacements. Do they think this model from eq (6) is still valid for strain rate measured along fibres? If this is the case, the authors should add more details on that since they mention this point as a main perspective for the proposed method.

REFERENCES

Dino Bindi, Daniele Spallarossa, Matteo Picozzi, Adrien Oth, Paola Morasca, Kevin Mayeda; The Community Stress-Drop Validation Study—Part I: Source, Propagation, and Site Decomposition of Fourier Spectra. *Seismological Research Letters* 2023; 94 (4): 1980–1991. doi: https://doi.org/10.1785/0220230019

Dino Bindi, Daniele Spallarossa, Matteo Picozzi, Adrien Oth, Paola Morasca, Kevin Mayeda; The Community Stress-Drop Validation Study—Part II: Uncertainties of the Source Parameters and

Stress Drop Analysis. *Seismological Research Letters* 2023; 94 (4): 1992–2002.
doi: https://doi.org/10.1785/0220230020

Seismica Editor
Andrea Llenos

October 1, 2024

Re: Manuscript on earthquake moment magnitudes from peak ground displacements

Dear Ms. Llenos,

thank you and the reviewers for your comments and help in the review process. We herewith submit a revised manuscript where we addressed the comments by both reviewers as carefully and promptly as possible. Please find our answers as attachment to this letter.

Kind regards

Appendix: 1) Answer to reviewer A
2) Answer to reviewer B

Comments by reviewer A

Dear Authors and Editor,

The manuscript of Dahm et al. presents the background and method for determining the moment magnitude Mw in a fast and efficient way from peak ground amplitudes. The method is highly interesting for the seismological community and can be readily applied. The authors are to be recommend for making the software public access. The manuscript is well written and the results and figures are well comprehensible. I recommend this manuscript for publication in Seismica after some minor edits and clarifications.

Minor comments and questions:

1. You mention in L205 and L227 that N should comprise a "sufficient number of records" to be able to average out errors and unknowns. So what is the the sufficient number you would recommend? Would it be possible to determine theoretically based on your framework a rule of thumb of how many records (N) of peak ground motion would be needed at least to achieve a well constrained estimate of Mw? This would surely be dependent on the magnitude and depth of the earthquake, as well of course on the station distances. Maybe you could use the simulations done for Figure 4 as a basis. It would also be possible to implement a bootstrap on the station records/simulations on your test cases and see how well Mw can be determined from a reduced range of N (see also comment 2).

   The procedure of first estimating station magnitudes and then determining a mean magnitude representative of the event from an ensemble of station magnitudes is common practice. L210 (previously L205) refers to this common practice. In our method, we have the advantage of using additional uncertainties in the attenuation function. This should make the uncertainties more realistic. For example, even if a single station is used, our approach can estimate a realistic uncertainty (Fig. 2). L234 (previously L227) refers to the problem that the attenuation function itself usually has large uncertainties (e.g. from the effects of radiation pattern, rupture directivity or velocity). Therefore, the uncertainties in the magnitude can only be reduced by using more stations. We have simulated the reduction of the M uncertainty as a function of the number of stations in Fig. 2. While calculating the standard deviation from the residuals of station magnitudes saturates when the intrinsic scatter of peak amplitudes is large, our uncertainties can consider the uncertainties of source parameters and the standard deviation of the mean decreases if the number of stations increases. We have now further improved the text in this paragraph to explain this better.

2. Please clarify for me the implementation description of the in your abstract mentioned bootstrap: Do I understand correctly that you "only" bootstrap the source parameters? Please also explain the reason for choosing N=25 (L276) What about the recorded peak ground motion records? This can be faulty too, or influenced by strong site effects, maybe bootstrapping should be done on them too to asses the uncertainty better.

   Thank you for your comment. The abstract was possibly difficult to understand at this point, so we improved the text to clarify. A value of N=25 was selected after testing, but this parameter can be individually changed depending on the application. We clarify this in the manuscript. So far, we have refrained from simulating site effects in order to keep the computational effort within limits.

3. I think you have chosen to fit only peak values to have an accessible framework for the larger hazard community. What do you think about using the exact same framework but an envelope fitting approach in contrast to fitting only the peak values? It could be more stable, especially for smaller magnitudes (Fig 7b), as the automatic extraction of peak values of both P- and S-waves for very small magnitudes at close stations can be challenging. You could drop a line or two more in the text about why you did not use an envelope based approach and mention such approaches (e.g. Eulenfeld et al. 2021).

   Thank you for the suggestions. Inserted at the end of discussion.

4. Qseek gave me comparable results for a test area in southern Germany when comparing with an envelope based approach, but had a slight tendency for underestimation for Mw <1, which I also find in your figure 7a for smaller magnitudes. Do you expect this to be a systematic issue and do you have explanation for this?

   We are happy to hear that qseek is applied and that estimated Mw were comparable to estimates from other approaches (envelopes). Whether deviations for very small magnitudes < 1 are systematic or not is difficult to answer. Also, it may be that the envelope method overestimates Mw, for instance if the noise in seismic traces would be large. From our experience with qseek and small magnitude events, the selection of the filter can play a role, but high frequency also need to be contained in the GF database of synthetic seismograms. So we cannot answer the specific question for now but would be happy to provide assistance for qseek on request.

5. Figure 7: Is the cause for the large error of smaller magnitudes maybe the chosen frequency range, which appears to be fixed to 0.5-2 Hz?

   A careful analysis of this dataset has been done by Sen et al. (2013), who performed moment tensor inversion and Mw estimation. In that work, the authors considered a few frequency bands, with the frequency increasing up to 4 Hz for the smallest events studied. The quality of the inversion, however, was decreasing with smaller magnitudes and smaller amplitudes of the recorded signals, mostly because of the higher seismic noise contamination. Thus, we believe that larger errors for smaller magnitudes are mostly to be attributed to the decreasing signal-to-noise ratio.

6. Do you expect site effects to average out as well? Those effects can be quite substantial. Do you expect that 1.5D velocity model, such as the fomosto Green's functions with ConfigTypeB, would be helpful in your framework?

   If information is available on velocity variations, with different average 1D models for different stations, these can be considered in the Green's function database (e.g. 1.5D velocity models). We expect that this would improve the magnitude estimates and reduce the errors. We now mention this possibility in the conclusions. Whether site effects cancel out in general, or how to consider them best, is difficult to say. However, the problem is similar for both the conventional and the qseek scheme. We have not tested whether correction terms can be estimated from a large dataset, but this might be one way to empirically approach the problem.

<u>Minor editorial comments</u>

1. Figure 7:

   a) The caption mentions green circles, when the figure displays yellow circles.

Corrected.

b) Please also explicitly state in the caption for what the 1 and 2 sigma is given (color wise I expect for the synthetic peak amplitudes?)

Corrected.

2. Figure 4: Why is the discontinuity at 2 km so strongly visible? What is the size of the modelled earthquake?

   The relatively strong discontinuity in the velocity model at 2 km depth is visible in the simulated PGM map. This means that PGD for a source at 2 km generates slightly different PGM values in comparison to a source location above or below. The synthetic seismograms employ a full reflectivity method (qseis), which is accurate even if the source is placed on the interface. However, for a source close to interface, interaction between spherical waves emitted from the source and a plane interface becomes more complex, and likely increases the variability of the distance-dependent PGD pattern seen in the figure. The effect is not very relevant for the approach suggested in qseek, however, because we anyway average over a range of distances and depths. Physically more relevant is the general observation that sources located above the layer interface generate higher PGM than sources below, which is associated to layer surface waves. We improved the caption to explain better.

3. Figure 10: Please indicate what the orange circles represent, I assume they are the measured data from Dost et al, 2016.

   The orange circles are indeed observations by Dost et al. (2016). We slightly improved the caption to better indicate this.

4. L 118: "multiply" should read "multiple"

   Done.

5. L121: "ML-ML relationship" should read "ML-Mw"

   Done.

6. L123: should read: "... in source properties, such as e.g. stress drop..."

   Done.

7. There are number of extra double spaces, such as in L12, L101, L308, caption of figure 5, L358, L428

   Very well spotted, thanks! I hope we managed to remove them all now using "find and replace".

8. L228: “rupture” instead of fracture

   Done.

9. L287: Double bracket in citation

   Done.

10. L330: Bracket missing around Sen et al., 2023

    Done.

Comments by reviewer B

In this paper, the authors propose a new approach to estimate moment magnitude that is based on observed and simulated values of Peak Ground Displacement (PGD). Given the definition of Moment magnitude, the authors develop a framework in which the reference magnitude is associated to full waveform simulated data, that can be integrated with observed Peak Ground Displacement (PGD) to obtain estimates of Mw. The method consists in calculating a massive dataset of Green's functions from different source models (location, moment tensor, etc...), giving simulated values of PGD. Assuming a circular crack model they express magnitude estimate as a function of the ratio between the observed PGD and the simulated PGD. After formulating the mathematical problem, the authors validate the approach on two different datasets, the first one consists in events registered in a coal mine in Germany, the latter contains events from the Groningen gas field in the Netherlands. The method is interesting and leverages some known issues in moment magnitude estimation: the synthetics allow to simulate attenuation curves that can be also extrapolated at distances where no observations are available; and the error associated to moment magnitude estimates directly take into account the contribution of epistemic uncertainty coming from the explored parameters in the simulations. Also, the proposed formulation easily correlates to other magnitude scales, allowing for comparison when the same technique is used to the PGD. Overall, the work is of interest for several reasons, and the authors also included the whole framework in public packages available for the community. Nevertheless, I think some relevant points should be addressed and/or clarified with further detail by the authors. The main issues and perplexities are summarised in the following:

- Some figures are poorly described and miss some information in the captions. The authors should make an effort in clarifying and add more details throughout the text.
- The mathematical formulation has some typos, and in some formula some assumptions are made which should be verified/justified in more detail, maybe adding the calculation in the SI.
- The workflow description is not too clear and regular throughout the text. For example there is no clear description on how the real data are processed or how they extract observed PGD. I also think that the authors should smoothen the description of the procedure.
- In the discussion the authors only focus on the strength points of the proposed approach, but they do not comment on possible limitations and/or shortcoming.

We thank the reviewer for the careful and critical reading and the suggestions for improvement, all of which are very good. We considered all of the above points to improve the manuscript and readability, but will not respond to them here and instead address them in more detail below.

General questions:

1. Some figures (e.g Fig 2 and Fig7, but It is a general comment) could benefit from some legend being added in the plots. Also, the text could be improved by adding more information to describe the figures and the overall workflow.

   We tried to enhance both the figure captions and the description of the figures in the text, but at the same time tried to minimise repetitions between both. Figures have in *general been improved considering the suggestions, and legends were added.*

2. The measure of the peak ground displacement (PGD) is one of the key points in the application of the proposed approach. In the text, the authors never detail how this measure is inferred. Is it extracted from whole trace or obtained isolating the phase of interest? Throughout the paper the authors refer many times to the selection of maximum amplitude in several frequency bands, but they never mention the values they use in the real data application of the proposed method, and how they are selected. Also, they should be clearer on the data processing used on the earthquake waveforms.

   Thank you for the comment, the information was indeed poor and is now improved. The extraction of peak amplitudes can actually be configured by the user depending on own preferences or data. It is realised by a general feature extraction tool provided in the pyrocko *package. The user has the option to extract peak values or peak-to-peak values and to configure the window lengths, tapers, frequency filters and other processing steps including the restitution or integration procedures. In addition to peak amplitudes, other features can be extracted and stored, such as envelopes, envelope peaks, peaks, etc. We have not tested these other features for magnitude computation, but used a default parameter set. The options have been described in the software package pyrocko and qseek, and an example script for pyrocko has now been added to the supplementary information.*

3. In the discussion the authors do not mention any shortcoming or limitation of this approach. Even if the method looks promising for having error estimates that coherently account for the epistemic uncertainty, I still think that there are some points that were not discussed in detail. The criteria on how time windows are selected from the different stations and traces are not detailed, but this could have an influence on the PGD estimate, and consequently on the magnitude. This is a well-known problem for instance in source parameter estimation with standard spectral techniques (e.g. Bindi et al 2023, Bindi et al., 2024). The second point is related to the selection of the station reference magnitude M_0^r which is often mentioned to be "as close as possible" to the real magnitude value. In the text it is not clear how this value is selected during the proposed framework, and also it is not discussed what is the consequence of selecting a reference value that is "far" from the correct one.

   We have now added a short paragraph on the problem of windowing and its possible impact on PGD in the discussion, with a link to the references mentioned. As explained above, the procedure is configurable by parameters and is not specific to qseek or pyrocko, but is generally applicable. The method is also designed to allow a geological or seismological survey to use observations and peak amplitudes as they do for their standard routine, and to employ these values in addition to the method proposed here. We think such course of action is important to increase the acceptance of the method.

4. The final formula for the estimation of moment magnitude is obtained in the assumption of a number of station large enough to neglect the last term of equation (8), but I think that the real constraining condition should be related to the azimuthal coverage of stations around the source, that is key to state that the directivity averages to 1. The validation is performed on two datasets where the stations cover the whole azimuthal range around the seismicity, and I wonder if the same results would be observed in a case where this condition do not apply.

   The problem of few observations and large azimuthal gaps can be severe and bias location and magnitude estimates. This is true in general and not a specific point of the proposed method. We believe that the most critical issue is the poor location that will result from *very few data. We now mention this general problem in the discussion. An advantage of our method is that location uncertainties can be taken into account in the estimation of M. A poor data set will lead to large uncertainties in the magnitudes, so it is ultimately not so critical if the 2nd term is completely or only partially missing.*

5. In the mathematical formulation some of the calculation is nontrivial, and some assumption must be carefully justified. Moreover, this formulation would benefit from a revision of the adopted notation, since the current one results ambiguous in some of the proposed formula, especially in referring to station and model averages.

   We have carefully checked the mathematical formulation and corrected typos and possible ambiguities (see also line by line comments).

6. The discussion section should focus also on possible shortcomings of the approach. What happens if the model used to calculate synthetics is not accurate enough to produce seismograms that match the observations? What are the possible influences coming from how the peak amplitudes are measured? What could be the effect the non-favouring geometries of the acquisition network?

   Thank you for the suggestions. The discussion is extended and now better considers the possible shortcomings.

Line by line comments:

1. Line 171: "The slip direction is denoted by a vector γ": Here if γ denotes only the slip direction, the authors should refer to it as a versor, otherwise they could just state that the slip is denoted by the vector γ.

   Done, thereby corrected the text description.

2. Line 193: "The circular rupture models implicitly accounts for the source spectra scaling":

   a) There is a typo, the verb is used at the singular form. *Done.*

   b) It is not clear which scaling the authors refer to. It is the scaling with distance?

      The sentence was unclear. We refer to the ω^{-2} model of the source spectra for frequency larger than the corner frequency of the source. The sentence in the manuscript is improved and a reference is given.

   c) Also, the authors state that the rise time is about 1.5 times the rupture time. This observation come from the formula, or from another research paper? They should add a citation, if this is the case.

      The ω^{-2} decay for frequencies larger than the corner frequency of the source is reproduced if an apparent rise time of 1.5 times the rupture time is used in Brune's model. This can be shown by numerical simulations of far field source spectra measured in different directions to the rupture plane. The sentence in the manuscript is improved and a figure has been added to the supplementary material.

3. Equation 6:

   a) If I substitute the radius expression from eq. (2) into eq. (5) I find a factor $(M_0)^{2/3}3(16/7\Delta\sigma)^{1/3}v_r w g_i$. I think there could be a typo in the calculation. Please check the formula.

Corrected, thank you.

b) Also, why omitting the component notation? I think it is the correct way to represent the formula, since it is valid for each component of the displacement.

We now keep the index i in equation 6 and explicitly state that it is valid for each component. However, we omit the component in the following equations to simplify the notations (note that we need another index for station distance later).

c) Since equation 6 is the key equation that is used to estimate the seismic moment, and hence the moment magnitude, I think it would be useful to also express M_0 in a further equation, that could fit after Line 209. Here the authors could directly put the expression for the average magnitude at all stations, since it is also used later in equation (8). This expression would also be of interest since shows that M_0 is proportional to the ratio between the peak amplitude and the Green's function giving a direct comparison with more classical formulations.

Thank you for the suggestion. We discussed this option and finally decide to not add an explicit formula, since the relation between M_0 and M_w is already given.

4. Line 199: Add the verb "are" in period "directivity effects considered". Also the last part should be "...stress drop $\Delta\sigma$ as well as rupture velocity v_r are known".

   Done.

5. Line 212:

   a) Here the average simulated PGD is called $|u|^r$, while later is $|u^r|$. Please, check and uniform the notation.

   We now improved the notation and introduced another term for the synthetic attenuation function, which is more intuitive. Also, the distance dependency of the attenuation function is now given in the text / notation.

   b) Also in equation (7) the notation is different for the type of character (Non bold).

   Corrected.

   c) Since this reference value is evaluated station by station, the notation should also include an index varying from 1 to N.

   Included now via distance dependence r_n, thank you.

6. Line 215:

   a) The authors state multiple times that the average of w is equal to 1. The first comment is that it is not clear on which domain this average is considered. It is the average only on the polar angle θ? Are they considering an average also on the azimuthal angle ϕ? This should be clarified.

   According to our understanding of the circular rupture front growth, the apparent source time function should be the same for each azimuth direction. Note that in this context, the azimuth is defined with respect to the source plane, and should not be confused with the station azimuth. We improved the text to avoid any misunderstanding.

   b) Are the authors sure that the average is 1 or some normalization coefficients should be considered?

The question on the role of the directivity term is answered below. In short: we now keep the term in the theoretical formulation, as it is larger than 1. In the numerical realisation, directivity is considered as an extended source and rupture is simulated.

c) Also, this angle is mentioned at line 188 as measured in the plane of the source, but from Sato and Hirasawa (1973) this angle should be measured with respect to the normal to the source plane.

Sato and Hirasawa use the polar angle Θ. Here, the latitude angle ϑ is used which is $\Theta - 90°$. Therefore, it is measured with respect to the plane of the source. This does not affect the results or anything else. We improved the text to clarify better.

7. Equation (7):

a) In this equation the term M_0^r is introduced, but it is not described in the text. I assume this represent the reference magnitude "as close as possible to the one of the earthquake" for the selected station that is mentioned at Line 213. Also, how the reference is chosen it is not discussed.

$M_0^{(r)}$ is the reference moment, and $M_w^{(r)}$ the reference moment magnitude. We now introduce both in the text (L220, L241-243). We also added some explanation how to choose them in practice. In theory, a reference magnitude of 0 could be used (this is done for the conventional magnitude scales to define a_0). However, the calculation of intrinsic uncertainties of the attenuation curve could be biased if the reference magnitude is very far from the ones to be determined. To have a close selection is even more important for the numerical approach, as we simulate synthetic seismograms including directivity and rupture effects for a given frequency, and these could be very different if the simulation is done for a reference magnitude of 5, and the studied event has a magnitude of 1. An optimal solution would be to select a reference magnitude for every earthquake, and use the one that is estimated in a first 'analysis'. We mention this now. However, in practice this would enhance the computational load, and a pragmatic solution is sufficient from our experience so far.

b) Another comment is on the assumption that the directivity term is not considered in the model average, since the average of w is constant/is equal to 1. In the formula w is multiplied by several terms that change for every model realisation, therefore this operation is not linear, and the authors should demonstrate that the average can be isolated from the summation over k.

Thank you for pointing this out, and we think you are right. The theory part was developed to support the numerical approach that is ultimately used in applications. In the numerical tool we simulate directivity effects, so they are included in the attenuation curves. This is also very important because filtering of seismograms (and synthetic seismograms) will affect the magnitude of the directivity effects. In the submitted version, we wanted to keep the formulas as simple as possible. But you are right, the term w cannot simply be omitted, even if it is constant, so we have now kept it in formula (7) and corrected the text accordingly. The theory is now more consistent and complete.

8. Line 226: I think that the notation is poorly chosen to describe the problem. From this line n is the station index, and the reference has the apex r, but also the reference value is obtained for each of the N stations, and thus also the reference should have the station index for the notation not to be ambiguous.

We changed the notation for the reference (synthetic peak amplitude), and added the distance dependency explicitely giving the dependency on distance r_n.

9. Equation (8):

   a) The quantity M_0^r is not introduced in the text. The formulation results a bit confusing in this section.

   $M_0^{(r)}$ appears first in equation (7) and is now better introduced in the paragraph before. In equation (8) we additionally use M_0^{ref}, which is a reference moment introduced by Kanamori to define the moment magnitude scale. We need this to use the $M_0 - M_w$ relation and to come to moment magnitudes (see equation 1). We are sorry about the complexity in the notation, but we don't see a possibility to omit these details.

   b) The assumption that leads to equation (9) is that with increasing number of stations the ratio that appears in the argument of the logarithm in the last summation approaches 1, and thus this term can be neglected. Physically, the directivity effect averages to 1 if one considers a distribution of station that has complete angular coverage with respect to the source, and thus it depends more on the geometry of the network than on the number of stations. Also, the possibility to send this term to 1 in the average is only valid if the authors demonstrate it (See comment on eq (7)).

   This is a very important point. We withdraw the statement that the last term is exactly one, and extend the discussion to the case that the term is constant but not 1. Therefore, a constant C is introduced to make this clear. In the data examples shown, we adjusted the reference magnitude slightly to calibrate the data. This calibration can now be better justified with the improved equations, for instance to account for a constant C, which would be very interesting.

   c) The second point is that, even if the term w^n averages to 1, then It is nontrivial that the ratio goes to one as N increase. This implication still depends on the chosen parameters for the event at each station, in the assumption that the values in the simulation are distributed around these values for the event. The authors could calculate the value of the last term as a function of station number N, selecting different narrow azimuthal coverages and showing how the factor changes as compared to the case in which the azimuthal coverage captures the whole domain of angle θ.

   We have withdrawn the statement that w^n averages to 1, and have changed the forumlas to keep the term with the radiation pattern and directivity. We state that the sum of the ratio will approach a constant if the number N of stations is large. The constancy implication is justified by the fact that the we randomise the radiation pattern (moment tensor orientation) for the synthetic data reference peak amplitude. This can be viewed equivalent (similar) to the situation that peak amplitudes are extracted from a network of stations at many azimuths. Please note that the assumption that the radiation pattern and directivity averages out is similarly suggested for the local magnitude procedure using an empirical instead of a synthetic attenuation function.

10. Line 237 The event moment magnitude is sometimes referred to as M_W and in other occasions as M_w. Also at line 151 and in eq (1) the notation is inconsistent. Can the authors please check this throughout the text?

Thank you for spotting this. We decided to use the notation M_w (in accordance with the Seismica style guide) and adapted it throughout the text.

11. Equation (10):

    a) The evolution from eq (9) to eq (10) is not extremely clear to me. Could the authors add the explicit calculation in the SI? This formula should contain both the contribution of the variability of the observed PGD and the variability of the simulated/modelled quantities, but with the notation and the explanation in the text, it is difficult to interpret the result.

    We revisited the formulas and corrected the description in the main text. The procedure is now much clearer and references are given.

    b) The final expression of the final variance on M_w contains the term $< Var >$, that according to the notation used previously (Line 220) should be the average over the K synthetic realizations at each station, but the Var as expressed in eq (7) is already accounting this contribution.

    This is a misunderstanding, and was also not described well in the text (sorry for this). We have improved and corrected the derivation and give references.

12. Figure 2:

    a) From the text it is not clear if the values represented in this plot are simulated, and if it's the case, it is not clear how they are generated.

    The example is a simulation to demonstrate the differences in uncertainty estimation, so it is somewhat "constructed". We incrementally increase the number of available station magnitudes, with the station magnitudes randomised to give a realistic example. The 'geometry' is similar to the HAMM case (mining, shallow induced earthquakes) which comes later. While the assumed station magnitudes in our example follow a Gaussian distribution, the uncertainties of the Green-function based attenuation curve for the HAMM case are calculated numerically using synthetic seismograms and bootstrapping source parameters (model, distance, depth distribution of events, etc.). In the plot, we compare the mean magnitude and its uncertainties for both the standard deviation of the ensemble and of the mean as used in the numerical approach of this paper. While the mean magnitude is the same for both statistical approaches, the uncertainties are different. The variance of M for a "conventional approach" is small when few stations are available and then increases until it saturates. The variance in our approach is large for few stations and then decreases continuously with the number of stations available. The figure shows this effect. Both the text in the manuscript and the figure caption have now been improved to better explain the test. The figure itself has also been improved. Thank you for pointing out the weaknesses in the previous version.

    b) In the caption some green circles are mentioned, but I don't see any green circle in the figure. Maybe the authors meant grey circles?

    Yes, indeed we meant the grey circles. Colours now changed and caption corrected.

    c) I also note that no grey point is visible for the first and last point of the plot.

    The first point coincides with the average, because the average of a single data point is the data point. The last point was by chance outside the range. We have now realised another random simulation and increased the plotting range.

d) The limits on the y axis should be increased to include the whole uncertainty of the first point on the left. For this point no red bar for the error is visible on the plot, why this happens?

Corrected. See also answer for (c).

e) Lastly, an horizontal dashed line appears, but neither in the caption or in the text it is explained what it represents. I also find the caption of this figure and the text not so clear in explaining its content.

We improved the caption and explain the horizontal dashed line.

13. Line 261: In the comparison shown in Fig. S.1, why do the authors only compare the M_x scales for $M_w > 4$?

The comparison of local magnitudes to M_w is described in the main part of the paper. Comparing other scales like m_B or M_S for very small events is not the topic of the paper, and would possibly need additional paragraphs and discussions.

14. Line 278: "Gidded" was intended to be "Gridded"?

Corrected.

15. Figure 4:

a) An horizontal line appears at depth of 2 km, and in the caption the author justify this as an effect on the velocity model. Is the velocity model used to simulate those values of PGD the same shown in fig 5b or Fig9a-b? If this is the case, why the only horizontal line appears at 2 km depth?

The horizontal line at 2 km is a result of the strong impedance contrast in the layered 1D velocity model at that depth. The here shown velocity model is the same as in Figure 9, which shows a strong velocity contrast at ∼2 km depth. The horizontal line appears only in Figure 4 because only there PGM is shown in two dimensions (source depth vs. distance). This illustrates the complexity of ground motions and its dependence on the (simple 1D) velocity model. We improved the Figure caption. See also answer to minor comment 2 of reviewer 1.

b) Which is the reference magnitude used to simulate the shown PGDs?

Peak amplitudes were extracted from full seismograms for a reference magnitude of 1. The distance attenuation for a source depth of 2.5 km is comparable to the red attenuation curve in Fig. 10, where S wave peak amplitudes are plotted over distance.

16. Figure 5(b): The label $v_p - v_s(km/s)$ is misleading in my opinion, it would be clearer if the authors could just add a legend relating the colours to the different velocities. Indeed, even if it is trivial to understand, in the caption there is no description on the difference in the colours of the two curves.

We corrected the label and introduced a legend to distinguish velocities.

17. Figure 6:

a) Since the displacement is divided by the seismic moment expressed in Nm, I think that the authors should correct the unity of measurement here.

The given unit of peak displacement is nanometers (now micrometers). However, *the labeling could be confusing, so we improved this and also introduced a legend.*

b) Moreover, is the distance r here the epicentral or hypocentral distance?

Distance is epicentral distance, as for all of our plots. Now mentioned explicitely in the caption.

18. Figure 7:

a) Also here in the caption of Figure 7(a) green circles are mentioned but have no correspondence in the figure.

We corrected and improved the figure and caption and also introduced a legend.

b) In Figure 7(b) the authors could think of adding shaded points in the background showing the distribution from which the binned values are retrieved.

Thank you for the suggestion. We discussed this but finally decided not to implement, as the figure might become too complex without enhancing the key points.

19. Line 331: The authors frequently recall the possibility of applying bandpass filtering, but they do not give any precise indication on the frequency bands they explore to obtain PGD (peak ground displacement) estimates. Also the length of time windows they analyse is never mentioned. In the application to real data, more specific indications on the data processing should be provided.

Originally, we thought we would leave this topic out of the manuscript, as the choice of frequency ranges, time windows, phases and components is very different depending on the magnitude scale, usually very precisely defined, and not a specific topic of this approach. At the reviewer's suggestion, we have now added some explanations in the discussion. When comparing with standard magnitudes and deriving $M_w - M_x$ *relations, we must strictly adhere to the specifications of the respective magnitude scale. The advantage of our method, however, is that we are free to choose whether to consider only the P-phase, only the S-phase or the whole wave train, and in which frequency range to work. We can even mix different wave modes and different filter ranges if, for example, the sensor types are different or the noise varies greatly. However, the choice of frequency range is limited by our GF database, both technically, because the effort required to generate high-frequency synthetic seismograms can be very high, and physically, because the subsurface models are generally not known at small scales. Therefore, the choice of parameters will always be a matter of application. However, we have now added a sample script in the appendix.*

20. Figure 10:

a) In figure 10(a) plotting the dashed orange line on top of the blue one could help the visualization.

We have added a legend and improved the labeling and the figure in general.

b) In figure 10(b) the authors show the theoretical amplitude decay with distance. The first point is about the notation, it is not clear in the figures whenever the distance is the hypocentral or epicentral distance. Could the authors check and uniform this throughout the text?

We clarified that we use epicentral distance for our attenuation curves - labeling was inproved.

c) The second point is about the amplitude decay shown for the P waves. Even if the authors comment at line 372 and following about the reason why the amplitude starts to increase at distances larger than 35 km, they do not comment on the very steep decay of amplitudes in the first 15 km, which is very different for the usually assumed $1/r$ used in standard models for the far field radiation.

The steep decay of amplitudes is due to a defocussing effect by the high-velocity anhydrite layer at the base of the Zechstein evaporites. We further detailed the explanation in the text and supplemented them with a citation.

21. Line 438: In the abstract and here in the conclusion the authors state that the method could be easily transferred to DAS data, but the model they proposed is developed for peak displacements. Do they think this model from eq (6) is still valid for strain rate measured along fibres? If this is the case, the authors should add more details on that since they mention this point as a main perspective for the proposed method.

    Strain rate can be calculated from gradients of ground velocity. If this is accounted for in a modified equation 6 (or numerically be providing Green functions for linear strain rate), the method should be applicable also to DAS. Another question is whether DAS recordings have a sufficient quality and linearity to be used for magnitude estimation. In the GF framework of pyrocko, however, we already prepared to store and use Green functions for strain rate, derived numerically from a displacement GF. However, this has so far not been tested and is not yet released in the open source version. But technically, it should work, we don't see a problem here. We added a some more explanations in the conclusion

References:

Dino Bindi, Daniele Spallarossa, Matteo Picozzi, Adrien Oth, Paola Morasca, Kevin Mayeda; The Community Stress-Drop Validation Study—Part I: Source, Propagation, and Site Decomposition of Fourier Spectra. Seismological Research Letters 2023; 94 (4): 1980–1991. doi: https://doi.org/10.1785/0220230019

Dino Bindi, Daniele Spallarossa, Matteo Picozzi, Adrien Oth, Paola Morasca, Kevin Mayeda; The Community Stress-Drop Validation Study—Part II: Uncertainties of the Source Parameters and Stress Drop Analysis. Seismological Research Letters 2023; 94 (4): 1992–2002. doi: https://doi.org/10.1785/0220230020

Both reference have been included in the reference list.

Review Round 2

Reviewer A Comments

Accept submission

Reviewer B Comments

For author and editor

Second revision of the manuscript titled:

"Earthquake Moment Magnitudes from Peak Ground Displacements and Synthetic Green's Functions" by Torsten Dahm, Daniela Kuhn, Simone Cesca, Marius Iksen

Dear editor and authors,

I have carefully read the new version of the manuscript and here are my comments.

First, I found that the authors answered most of the main points raised in the first round of review. Moreover, I think that this new version is clearer and far more robust, especially in the theoretical description of the problem.

In reading the manuscript I found some typos and few notation mistakes, thus I would ask the authors to carefully revise the text for the final version. All the minor comments can be found in the following. I think that these points can be directly addressed on the final version of the papers from the authors, without need for further revision.

Finally, I think that the manuscript is now suitable for publication in Seismica.

COMMENTS

- Abstract: Some periods appear blue as in the "track change version"
- Supplementary figure S1: there is a typo in the legend "mean spectrum" instead of spectrum
- Line 247: A typo, I guess. "The moment magnitude is …"
- Figure 7a: A green line is mentioned in the figure comment, but only grey and yellow lines are effectively on the plot. Please check.
- Line 442 & Line 446: Check consistency of the notation for Mw