

The deterministic behaviour of earthquake rupture beginning

Corresponding Author: Professor Aldo Zollo

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

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Dear Dr Colombelli,

Please allow us to sincerely apologise for the long delay in sending a decision on your manuscript titled "The deterministic behaviour of earthquake rupture beginning". It has now been seen by 2 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns and presents much stronger support for the conclusion of a deterministic relationship between rupture onset and magnitude than previous studies that have suggested this relationship.

In particular, this would require that you fully explain and justify (or adapt) the data selection approach, including the use of a single cut-off time, and demonstrate that there is no bias in your statistical results.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

When resubmitting, please provide a point-by-point response to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

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Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The manuscript 'The deterministic behavior of earthquake rupture beginning' by Longobardi et al. presents an important investigation into a fundamental question in seismology: the potential predictability of earthquake magnitudes from their initial rupture characteristics. They argue that statistically small earthquakes initiate faster than large earthquakes globally, based on analysis of near-field to regional-distance peak displacement time series recorded by seismic stations. While the research presents valuable insights into this significant topic, the manuscript would benefit from improved clarity in its presentation. Prior to recommending publication in Communications Earth & Environment, I respectfully suggest that the authors address the following points:

****Main comments****

1. Data: I feel some necessary details of how the data used are obtained are missing. The authors choose 200 earthquakes worldwide (Line 405). What are the criteria for selecting these events? Why are there only and exact 200 events selected? Where are the waveforms from (e.g. IRIS)? These are necessary pieces of information to help the readers understand how and where these data points are from, making the study reproducible.

A minor point on earthquake selection: maybe consider analyzing the shallow earthquakes only (e.g. depth < 50 km, those blue circles in Figure 1). Intermediate- and deep-focus earthquakes may feature different source properties than shallow earthquakes (e.g. Houston, 2001 JGR).

2. Methods: Why do the authors choose the time window $[t_{\min}, t_{\text{half}}]$ to evaluate the slope? Given the complexities in defining this time window, some explanations/justifications are needed. Readers might get questions such as "why don't we simply look at $[t_{\min}, t_{\min} + T]$ where T is a fixed window length, say 1 second?"

It would be interesting to put the typical duration of events in each magnitude bin as a reference time scale. It seems that t_{half} is a large portion of the duration for small earthquakes (M 4-5, 5-6) while it is only a small portion for larger earthquakes (M 7-8, 8-9). Does this potentially introduce a bias?

3. Assuming the observation is true, how to reconcile the new observation that small earthquakes initiate faster than large earthquakes with the previous but also recent observations that suggest identical onset across magnitudes (e.g. ref 16 and 19 in the main text, and Ide 2019 Nature)? These studies also look at near-field data. The authors seem to suggest this can be reconciled by considering different time scales (Line 226-239, Figure 4). However, it is not clear what the differences are

and what the breaking time scale is. Readers would appreciate and benefit from seeing a table that lists the key differences among these studies.

4. Regarding its potential for earthquake early warning, it seems to me t_{half} cannot be determined until a large portion of the time function is known, because we don't know what the maximum curvature is until the event ends.

****Minor comments****

Line 76-77: The description of station selection is not very clear to me the first time I read it. Consider adding an inset plot in Figure 1 or 2.

Line 139-142: This statement needs to be shown by a figure in the supplementary material.

Line 146-147: It is not clear how the slope is measured. The Methods section needs to be cited here. Make the description in the main text more explicit, e.g. "We identify the plateau time on the LPDT curves by analyzing the curvature and then measure the initial slope between $t_{\text{min}}=0.05$ s and t_{half} (half of the estimated plateau time) (See Methods for more details)."

Line 178: Define breakout phase. The difference between breakout phase and nucleation phase needs to be clarified. Are there any other papers that define this term clearly?

Line 193: It is not clear how equation (3) shows an exponential increase with rupture velocity V_R .

Line 198-214: Explanation of the different onset speed between small and large ruptures. This is very interesting discussion. However, this part looks a little weak because the only cited work (ref 28) is a preprint that is mostly about theory and models. It would be great to see a compilation of many earthquakes that demonstrate this rupture velocity -- magnitude relation, although this might not be easy, especially given that we are only looking at the very beginning of ruptures. If not possible now, it is still worth mentioning how future observations might bring new insights on how to test the current statement.

Line 234-238: How to reconcile the different onset speed with the self-similar scaling relationship (e.g. duration $\sim$ moment^{1/3})? I agree that this can be reconciled. It would be great to see exactly how the later stages would adjust to come back to the self-similarity result. For example, the authors can show the duration-moment scaling for their simulated events produced by the Sato and Hirasawa model (Figure S4) to check if self-similarity still holds.

Figure 1: Consider changing the color map or changing the color of the accelerator markers. Magenta squares and dark blue circles are too similar.

Figure 2: Consider connecting a line in the figure, or adding an equation to clarify how the slope is measured. Probably a good idea to explain LPDT and t_{half} in the caption too.

Figure 3: (1) What does "pf" in the caption mean (Line 363)? (2) Explain SE=Standard Error in the caption (line 367). (3) Here the meaning of t_{half} is explained. But readers might get confused because in the main text, it is explained as half of the time when LPDT reaches its plateau. I suggest to be consistent, or mention briefly how the determination of plateau time is related to curvature calculation, as suggested in the comments above.

Figure 4: Panel B needs more explanations. For example, explanation for the solid and dashed curves is missing in the caption. Also somehow the xlabel "Tens" is underlined.

Line 416-417: probably a good idea to make it clear that the average is done in the log peak displacement space not the linear space.

Text S1: this section needs to include more details. For example, consider reproducing the equations of Sato & Hirasawa here, or cite equations (1-3) in the main text. You should also cite Figure S4. The alpha ratio in Figure S4 is also not explained. I assume it is V_r/V_s .

Please fix " $V_p/V_s=\text{rad}^3$ " to display properly.

Use consistent decimal format. Use dot instead of comma for "Mw 6.5 mainshock".

Be consistent. Use " V_s " instead of " vs " in the last line.

Table S1: There are events with number of records fewer than 3. Maybe consider excluding these events from analysis.

Reviewer #2 (Remarks to the Author):

The authors examine the beginnings of 200 earthquakes and show that the beginnings of the smaller earthquakes they observe grow more quickly than the beginnings of the larger earthquakes.

This is an interesting result and seems a reasonably well done and well written paper. I do, however, have a number of comments below, and one major other significant issue: that M4-5 earthquakes often last less than a second. How do you avoid contaminating your results with the fact that some earthquakes are over in your window, and others aren't? Also, do you truncate the LPDT curves before analysis so as not to contaminate the data with information from 2 or 3 seconds into the rupture? The methods don't seem to say so.

Lines 37-46.

This isn't what many people mean by "cascade" and "pre-slip" models. Even in that paper by Joan Gomberg, the pre-slip is described as a mechanism for deterministic *nucleation,* not for deterministic *rupture.* The nucleation can be deterministic even if the actual rupture is not. The nucleation can happen in a small area dictated by accelerating aseismic slip, and then the rupture can spread outward.

Figure S3.

This figure is more convincing than Figures 2 and 3. Why not present it?

Line 121.

Rephrase this? You mean that your 200 earthquakes are selected from 7000 global earthquakes?

161.

Minor phrasing. I initially read the term in parentheses as a descriptor for the critical size, but I think you mean it as a descriptor for the frictional parameters. Changing e.g. to "such as" would make it clearer.

Lines 159-167.

That's great hypothesis for earthquake ruptures based on lab data, which would indeed be my working hypothesis for how ruptures start. It is not, however, known to be true, and shouldn't be presented as how earthquakes work.

Lines 166-167.

In particular, why does D_c relate to magnitude? Sure, in someone's model or lab results, but be specific about what's known about the earth and what you're using as a guiding hypothesis.

Lines 193-195.

I don't see how the rupture velocity and stress drop control the *shape* of the P wave radiation curve. It looks like they control its amplitude, but the shape is just t^2 . I think the curves in Figure S4 would have exactly the same shape if you plotted them on a log scale, which is how you've analyzed the data.

Line 201.

"Corroborated and proven." I'm not sure what proven means here, but this seems unnecessarily pushy. Why not just let readers read and interpret for themselves?

Lines 209-214.

What's the point of this paragraph?

Figure 4b.

Are these curves not reversed? The $\log(\text{slope})$ is higher for smaller events. If we approximate some portion of the curve as t^n , then we have $\log(t^n) \sim n$ larger for smaller curves, so more concave up for smaller events?

First paragraph and 241-248.

It's absolutely useful to think about using this for earthquake early warning. However, I think the potential for improvement is overstated. To avoid confusion, it'd be worth noting that EEW is already operational and that it's constrained by station distribution. We don't get the first second of the P wave right away.

The scatter in Figure 4a would also be interesting to discuss in the context of earthquake early warning.

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Version 1:

Decision Letter:

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Dear Professor Zollo,

Please allow us to apologise for the delay in sending a decision on your manuscript titled "The deterministic behaviour of earthquake rupture beginning" has now been seen again by Reviewer #1, and we include their comments at the end of this message. Unfortunately Reviewer #2 was unable to provide a second report. As you will see, Reviewer #1 retains some important concerns, particularly over the comparability of the length of analysis and the duration of smaller earthquakes. We remain interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

In particular, for us to consider the manuscript further, please ensure that in the revised manuscript you demonstrate that the time window you use can allow robust conclusions to be drawn even with respect to the smaller earthquake magnitudes, or adapt your conclusions and implications accordingly.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

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Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:

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Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have greatly improved the manuscript, particularly in clarifying technical details and defining key terms. However, I still have a major concern on whether the selected time window for analysis is representative for the very early breakout phase of the source time functions, especially for those earthquakes that are below magnitude 5. I would suggest publication only after the authors address this key issue.

Main comments

1. In line 146-147 of the new manuscript, the authors write "Indeed for $M=4$ the typical MRF duration is 0.5 seconds whereas for $M=7$ the typical MRF duration is 15 seconds". In the authors' reply to the original point #2 of the first review: "magnitude and the total duration may range from a few seconds for the small events ($M4-5$) to several tens of seconds for the very large earthquakes ($M>7-8$).". I would like to point out that these duration estimates for the small earthquakes ($M4-5$) are probably an overestimation. For example, a $M6.x$ earthquake typically lasts 3-10 s (e.g., Peng and Gombert, 2010). Based on Figure S7 from the Sato & Hirasawa model, the duration for a $Mw4$ earthquake with a 1 MPa stress drop gives a duration of ~0.3-0.4 s.

The point is the time window for analysis (t_{HALF}) seems a significant or even comparable to the total duration of those small earthquakes ($M4-5$). It is necessary to carefully examine the relative portion of the time analyzed compared to the total duration, both for analyzing earthquake physics and potential application for earthquake early warning. If the time window for analysis is longer than the time when the moment rate function reaches its peak, we cannot call them "rupture beginning" and the significance for early warning also would drop. Therefore, I suggest plotting the expected duration range for $M=4,5,6,7$ earthquakes on top of Figure 2A, 3B to allow direct comparison.

Minor comments

1. Figure S5 indeed shows that the deep earthquakes feature a slightly higher slope compared to a shallow earthquake of similar magnitude.
2. I appreciate that the authors add a discussion on various previous studies that may have posed different views or conclusions in the supplementary materials and in the introduction. I would like to point out that the analysis by Melgar and Hayes (2019) may be flawed because the time range for analysis is comparable to the peak time of the moment rate functions, as pointed out by Meier et al. (2021, GJI). Therefore, we must be cautious about their conclusion.
3. Lines 193-195: while I understand the general intuition here, this is a bit tricky because while larger earthquakes do spend longer time in the nucleation or breakout phase, they also release more moment in that period. It is unclear to me why this would necessarily lead to a smaller slope.
4. Lines 201-203: reference needed on the scaling of Dc with magnitude.

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Version 2:

Decision Letter:

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Dear Professor Zollo,

Your manuscript titled "The deterministic behaviour of earthquake rupture beginning" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Best regards,

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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have satisfactorily addressed my main concerns raised in the previous round. In particular, they have revised Figure 3 to include the range of peak time as a reference and have clearly articulated in the main text that their conclusions are robust with respect to the choice of time window used for evaluating the slope (e.g., t_{half} vs. t_{third}). I support the publication of this manuscript, as it is likely to stimulate further discussion on the important question of whether small and large earthquakes are statistically distinguishable during the onset phase of rupture.

A few final minor comments below:

Minor comments

1. line 150-155: the numbers here for durations also need to be updated to be consistent with the latest updates in the following lines.
2. line 180: this number probably also depends on magnitude.
3. In future work, it may be worthwhile to consider the effects of finite source dimensions for larger earthquakes. Given the current station selection criteria, larger events—owing to their greater rupture extents—are likely associated with relatively shorter source-to-station distances. In other words, the ratio of distance to source (d) to source dimension (R) tends to be smaller for larger earthquakes. This factor could potentially influence the observed trends and may be worth exploring in future investigations.

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[Authors] We wish to thank both Reviewer #1 and Reviewer #2 for considering our results a valuable contribution to the topic of earthquake rupture beginning. We appreciate the comments raised by both Reviewers and the constructive feedback they provided. Considering their comments, we made major modifications to the original work: we revised some of the original analyses, we added new results and improved the readability and clarity of the overall manuscript.

In the present document we provide a point-to-point response to all the issues raised by the Reviewers. Where applicable, we also indicate how the manuscript was revised, to incorporate the reviewer suggestion into the main text. We hope that the point-to-point response letter provides an exhaustive clarification to all the comments and that the new version of the manuscript satisfies both the two Reviewers and the Editor.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The manuscript 'The deterministic behavior of earthquake rupture beginning' by Longobardi et al. presents an important investigation into a fundamental question in seismology: the potential predictability of earthquake magnitudes from their initial rupture characteristics. They argue that statistically small earthquakes initiate faster than large earthquakes globally, based on analysis of near-field to regional-distance peak displacement time series recorded by seismic stations. While the research presents valuable insights into this significant topic, the manuscript would benefit from improved clarity in its presentation. Prior to recommending publication in Communications Earth & Environment, I respectfully suggest that the authors address the following points:

****Main comments****

[reviewer] 1. Data: I feel some necessary details of how the data used are obtained are missing. The authors choose 200 earthquakes worldwide (Line 405). What are the criteria for selecting these events? Why are there only and exact 200 events selected? Where are the waveforms from (e.g. IRIS)? These are necessary pieces of information to help the readers understand how and where these data points are from, making the study reproducible.

>> We clarify here how the dataset was built, and we have also added the same description to the revised version of the manuscript, in the *Methods* section.

We analysed 200 earthquakes with magnitudes between 4 and 9 that occurred worldwide. Waveforms were downloaded from all available catalogues, both at the European and global scale. Specifically: small-to-moderate Japanese events (in the range M 4-7) were downloaded from the Japanese Strong-motion Seismograph Networks (<https://www.kyoshin.bosai.go.jp/kyoshin/download/>); small-to-moderate (M 4-7) European and Californian events were downloaded from the European-Mediterranean Seismological Centre (<http://www.seismicportal.eu/fdsnws/event/1/>) and from the Northern California Earthquake Data Center (<https://service.ncedc.org/ncedcws/eventdata/1/>) respectively. Finally, for larger magnitude events (M $\geq$ 7) we downloaded the available records from the IRIS worldwide archive (<http://service.iris.edu/fdsnws/dataselect/1/>), selecting earthquakes that occurred between January 2010 and December 2023.

The resulting dataset ensured a sufficiently high number of events and waveform records (about 7000) distributed worldwide, with an adequate and balanced coverage of different magnitude ranges. At the same time, the dataset size remained manageable for the detailed analysis we performed, including manual picking of waveforms, which would not have been feasible at this stage with a larger dataset.

[reviewer] A minor point on earthquake selection: maybe consider analyzing the shallow earthquakes only (e.g. depth < 50 km, those blue circles in Figure 1). Intermediate- and deep-focus earthquakes may feature different source properties than shallow earthquakes (e.g. Houston, 2001 JGR).

>> We thank the reviewer for this suggestion. We now included a new figure in the Supplementary Material (Figure S5) showing the initial slopes of LPDT curves as a function of magnitude, with a different color for two different depth ranges (< 30 km and > 30 km). The figure shows that there is no significant contribution or effect of the event depth on the observed trend of slope with magnitude. We include the figure here, as well.

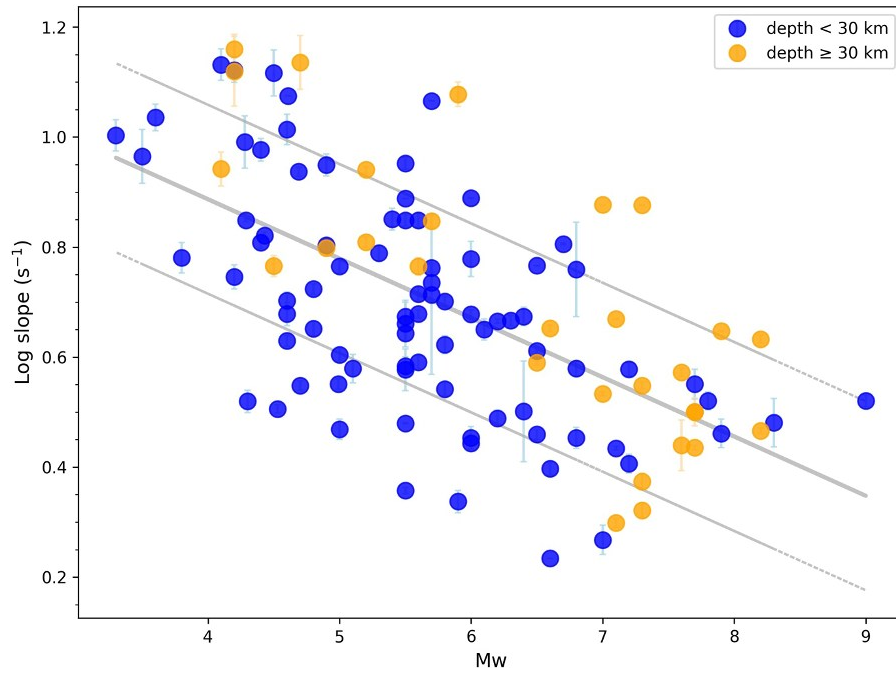


Figure S5. Initial slope of LPDT curves versus magnitude and depth. The figure shows the initial slope of the LPDT curves plotted as a function of magnitude in two different depth classes. Blue points correspond to event depth smaller than 30 km (shallow events) and orange points correspond to event depth higher than 30 km (deeper events). Grey solid and dashed lines are the empirical scaling relationship of initial slope vs magnitude and its standard error respectively.

[reviewer] 2. Methods: Why do the authors choose the time window $[t_{\min}, t_{\text{half}}]$ to evaluate the slope? Given the complexities in defining this time window, some explanations/justifications are needed. Readers might get questions such as "why don't we simply look at $[t_{\min}, t_{\min} + T]$ where T is a fixed window length, say 1 second?"

>> Reviewer #1 posed a very critical question, that we need to clarify here. The choice of the time at which the slope is measured is, indeed, crucial for the correct evaluation of the “early” slope of P-wave curves. Previous studies (e.g. Colombelli et al., 2014 and Colombelli and Zollo, 2015; Zollo et al., 2021; Nazeri et al., 2019) have already observed and highlighted the necessity of a variable time window with magnitude to be used to estimate the initial slope. In principle, the rupture process of earthquakes has a total duration that mainly depends on the magnitude and the total duration may range from a few seconds for the small events ($M4-5$) to several tens of seconds for the very large earthquakes ($M>7-8$).

With this in mind, when referring to the “early” rupture phase, the comparison of such different processes on the same, fixed, time scale could be inappropriate. Rather, the idea here (and of previous reference studies) is that each event has a proper time scale at which it can be seen and characterized. Following the same approach as suggested by these previous studies, here we evaluated the “initial” slope in a variable time window that depends on the earthquake magnitude.

The intrinsic difficulty in an automatic processing workflow is the risk of including portions of the curve that do not represent the “early” phase but include later times and approach the plateau time. To evaluate the initial slope of LPDT curves, we therefore needed to be sure that we were well before the plateau level of the LPDT curves and that we were considering only the portion of LPDT curves having a near-linear behavior.

We adopted an automatic criterion based on the LPDT curvature, described in the *Methods* section, to determine the time at which the Plateau Level of each LPDT curve is reached. As expected, this time is observed to vary, being generally larger for larger magnitude events. We then selected a sufficiently short time interval that ensures we were well before the plateau level of LPDT curves. The choice for selecting the half-time between the first-P arrival and the LPDT plateau time is arbitrary, since any shortest interval could

be used, provided that a sufficient number of data points are considered for the linear fitting of the initial slope (see also the following comments). The observed increase with magnitude of the time window used for the initial slope measurement (see fig 3) is a further confirmation that a variable size time window with magnitude is needed to measure the initial slope.

[reviewer] It would be interesting to put the typical duration of events in each magnitude bin as a reference time scale.

>> We added a line to clarify the typical durations for each event magnitude (line 146-147 in the revised version of the manuscript).

[reviewer] It seems that t_{half} is a large portion of the duration for small earthquakes (M 4-5, 5-6) while it is only a small portion for larger earthquakes (M 7-8, 8-9). Does this potentially introduce a bias?

>> As detailed in the previous reply, the choice of t_{half} is arbitrary, since any smaller time window could be appropriate, provided it does not include the plateau level while considering a sufficient number of data for the slope measurement. However, we have tested different time windows smaller than t_{half} (such as t_{third}) and the results confirmed the scaling of the initial slope with magnitude. We clarify that, while the overall scaling of the slope with magnitude is preserved even using smaller time windows, the absolute individual values of slope depend on the time window at which they are measured. Therefore, we do not interpret the physical meaning of absolute slope values but only focus and discuss the behavior of this parameter over the whole magnitude range. We added this new figure here and also to the Supplemental Material (Figure S6). We also included a comment in the main text (lines 148-153).

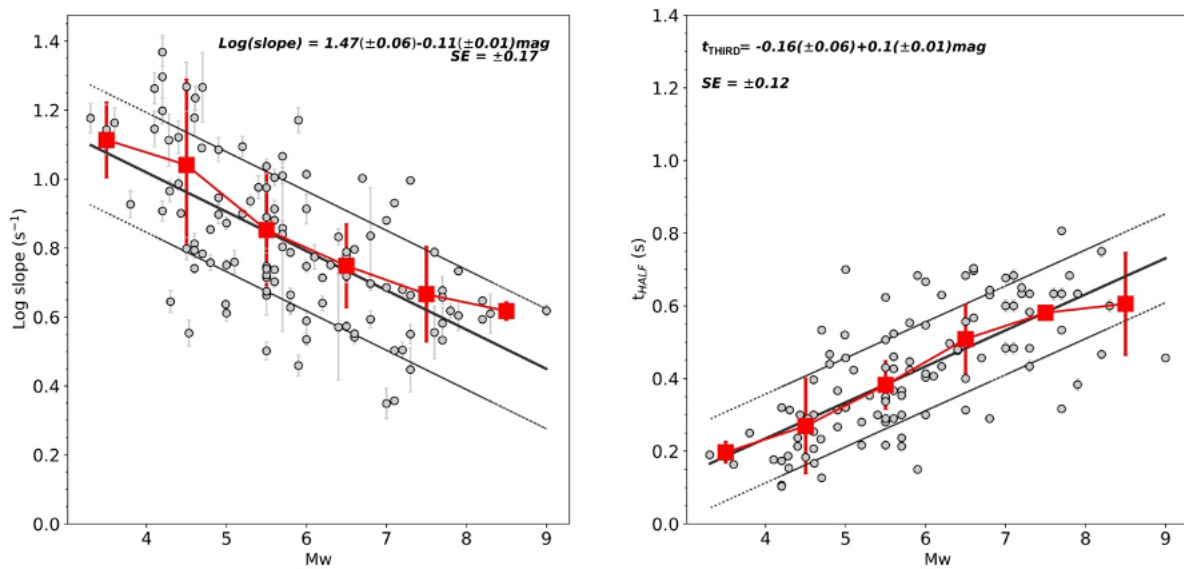


Figure S6. Initial Slope of LPDT curves and related time. Left: initial slope of LPDT curves as a function of magnitude. Right: the time at which LPDT curves reach one third of the maximum of their curvature (t_{THIRD}) as a function of magnitude. In both panels grey dots are single slope and time measurements, while red squares are the average values for each magnitude bin (bin width = 1). The solid black line represents the best fit line, along with $\pm$ one-SE (Standard Error) thin lines. The fit parameters and the SE values are shown at top left of the panel.

[reviewer] 3. Assuming the observation is true, how to reconcile the new observation that small earthquakes initiate faster than large earthquakes with the previous but also recent observations that suggest identical onset across magnitudes (e.g. ref 16 and 19 in the main text, and Ide 2019 Nature)? These studies also look at near-field data. The authors seem to suggest this can be reconciled by considering different time scales (Line 226-239, Figure 4). However, it is not clear what the differences are and what the breaking time scale is. Readers would appreciate and benefit from seeing a table that lists the key differences among these studies.

>> We sincerely appreciate the effort from Reviewer #1 to deeply understand the model we propose to justify our observations. After the Reviewer's comment, we realized that the original text did not clearly explain the

interpretation of results we meant to suggest. Since Reviewer #1 raised different concerns which were all related to this idea of “reconciling” our observations with other studies, we prefer providing here a unique and comprehensive response to all these similar comments. We hope that this new paragraph exhaustively explains the differences with previous studies and clarifies the issue raised by the Reviewer. We also added most of the following response in the revised manuscript (from line 155 on) and modified the original version of Figure 4. Finally, while appreciating the suggestion of Reviewer #1 to include a new table in the manuscript, we believe that a table listing differences among published studies in the main text would be too much technical and would shift the attention from the main focus of our work.

Our observations suggest that the initial displacement growth of small earthquakes is faster than for large earthquakes. There are two relevant aspects resulting from this result, that need to be clarified, and that are related to two different time scales during the rupture process.

At short time scale (e.g., within 1-2 seconds after the P-wave arrival): our observations contrast with the idea of a universal, identical onset for all earthquakes, that has been suggested by various authors so far (Meier et al., 2016; Trugman et al., 2019).

Although being conceptually like the initial amplitude rate measured here, none of the previous works measured the initial slope of the P-wave displacement amplitude with the same, quantitative, parameter (the slope), as we did here. Therefore, no direct comparison of such different results can be done. Moreover, significant differences in the processing of data may justify the observed differences in the initial rate of the P-wave displacement amplitude. We provide here a detailed report and discuss in the text only the most significant differences.

The analysis of *Meier et al. (2016)* mostly concerns few records per magnitude bin and a very limited observation distance (25 km) around the source, thus including possible biases due to the high variability of estimates of magnitude, depth, focal mechanism, directivity, etc., which can significantly affect the P-amplitude of a single or very few near source records.

In the work of *Trugman et al. (2019)* the peak displacement measurements are provided with a time step of 0.2 s, which does not allow to properly capture the variations at very short time scales. In our work, we computed the LPdT curves with a time step of 0.01 s, which allows to see amplitude variations on a very short time scale. This is especially true for small events, for which the largest, initial amplitude variations typically occur on a time scale smaller than 0.1s and the under-sampling with a time-step of 0.2s has the effect of introducing a smaller initial slope (Text S1 and Fig. S12 of the Supporting Information). Another difference is that *Trugman et al. (2019)* are mixing different events in magnitude bins of 0.5 to compute the curves, while in our analysis, we compute LPdT curve for single events and do not mix different earthquakes for the curve computation. Furthermore, in their Fig. 3, *Trugman et al. (2019)* showed the time evolution of the curves for different magnitude classes using a unique time and amplitude scale that compresses all the curves and does not allow to see the detailed behavior and the initial variations of the slope in a very short time range. When zooming in using a short time scale and shifting the curves at the same starting amplitude, the initial variations of the curves appear more evident, as discussed in Text S1 and Fig. S13 of the Supporting Information.

As for *Meier et al. (2017)*, they analysed a catalogue of teleseismic STF of large ($M \geq 7$) subduction zone earthquakes. Because of the signal low-frequency content, the analysed STFs have an intrinsic low time resolution at short periods (smaller than 1-2 sec). Moreover, the STFs were normalized using the centroid time, which is an assumption that requires that STFs have a triangular shape. As done in *Meier et al. (2016)*, the authors finally computed the average STFs for a large magnitude bin (7.7–9), for which remarkable differences in the source process can be existent.

Finally, *Odaka et al. (2003)* measured the initial rate of the P-wave amplitude and show that this is almost independent of the earthquake magnitude and variable with the recorded epicentral distance. Indeed, they propose the use of the initial rate of the P-wave to rapidly estimate the epicentral distance, shortly after the P-wave arrival. They used a fitting function of the type $Bt \cdot \exp(-At)$ that depends on two parameters (A and B), both estimated in a fixed, 3-s time window after the P-wave arrival. When discussing the A parameter, the authors declare that a positive value of A is typical for small earthquakes, indicating that the initial amplitude

increases sharply and decays quickly soon after the P-wave arrival. On the contrary, when A is negative, the initial amplitude increases exponentially with time, and this is a characteristic of large earthquakes, so that they expect that this parameter may be useful for distinguishing large and small earthquakes. As for the other parameter, the authors claim that the B value defines the slope of the initial part of the P waves and can be used to quantitatively characterize the initial rate of the P-wave amplitude. However, given the exponential form of the proposed model, the actual slope of the fitting function is a combination of both A and B. Because of the observed differences in the A parameter for small and large events, a potential scaling with magnitude of the initial slope is not discarded in the Okada et al. (2003) model.

At long time scale (several seconds to several tens of seconds, comparable to the peak of the MRF): *Melgar & Hayes (2019)* analysed a database of STFs (varying from large to very large events, M 7–9) and show that the events of different sizes can be distinguished after an initiation phase. They argue that, at some point in the rupture process, ground motion amplitudes are higher for M 9 than for M 7, suggesting the idea of an inverse scaling with magnitude, as compared to our observations. While we mainly focused our detailed analysis on the first few seconds after the P-wave arrival, we did not investigate the behaviour of LPDT curves at longer time scales, approaching of following their plateau level. Given the definition of LPDT (Max of log-modulus of Pd vs time), the decaying portion of LPDT is indeed not retrievable from our curves. For these reasons, with current data and methods, we do not have adequate elements to discriminate the growing behavior of MRFs at longer time scales, approaching the peak values. Our results, therefore, do not exclude the evolution of the source process over a longer time scale, as seen by other authors (Melgar & Hayes, 2019; Meier et al., 2016).

Another aspect to consider is that, whatever the initial behaviour of the source process is, self-similar scaling relationships (e.g. total duration vs. seismic moment) provide constraints to the macro source parameters, at the entire scale. We suggest potential hypothesis that could bring together observations at short time scales with observations done at long time-scales, while keeping the expected scaling relationships between source parameters.

Let us first distinguish the nucleation phase, during which slip slowly accumulates in a quasi-static way (Latour et al., 2013; Nielsen et al., 2010), until it reaches a critical size (related to the frictional parameters such as slip-weakening and critical distance, D_c) and the unstable break-out phase, during which rupture accelerates and finally triggers the dynamic propagation at seismic or sub-seismic velocities (Latour et al., 2013).

After that time and until the peak values of the source process (MRF), the amplitude could either **a) grow with an increasing rate with magnitude**, or **b) maintain the same growth rate with magnitude**, leading in both cases, to absolute peak amplitudes and related times which are consistent with those predicted from the scaling laws. In the first case (a), after the initial slower acceleration of the breakout phase, large events could then accelerate more than small events, reaching higher rupture velocities and even super-shear ruptures, as often estimated from the inversion of recorded seismograms (Melgar et al., 2023; Bouchon et al., 2001).

In the second case, small and large earthquakes could mainly differentiate only during the breakout phase, but then converge to a similar rupture velocity, common to all ruptures. As a support of the second case, we computed the duration-moment scaling produced by a Sato & Hirasawa circular rupture model where a linear decreasing rupture velocity is assigned for an increasing moment magnitude in the range Mw 4–8. We clarify that the Sato&Hirasawa model does not include the breakout phase and it only accounts for a unique value of rupture velocity for the entire duration of the process. We computed the average far-field S&H durations using equations in Zollo et al (2020), $V_S=3500$ m/s and V_R/V_S ratio ranging between 0.4 and 0.9, as shown in the figure below (also included in the new version of the Supplementary Material as Figure S7).

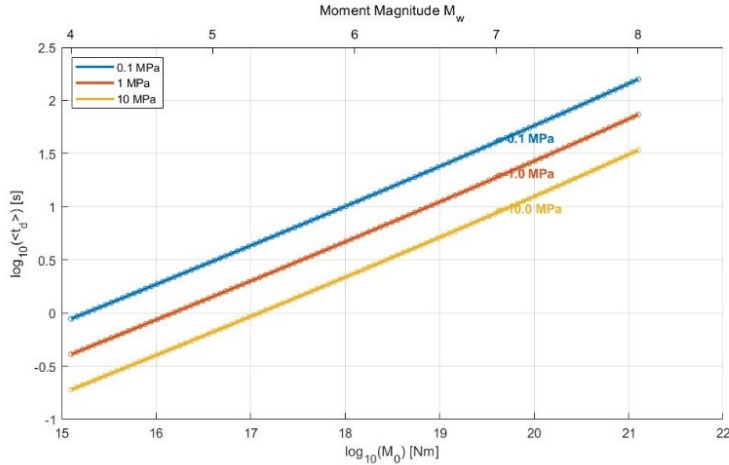


Figure S7: Duration-Moment scaling. The Figure shows the duration-moment scaling produced by a Sato & Hirasawa circular rupture model where a linear decreasing rupture velocity is assigned for an increasing moment magnitude in the range M_w 4-8.

We note that the theoretical duration of far-field displacement waveforms from uniform rupture speed, circular rupture is indeed model-independent, and it only uses the Fraunhofer approximation to evaluate the directivity modulation of the azimuthal varying pulse duration. The figure shows that even considering a decreasing rupture velocity with magnitude for the entire duration of the process, the final pulse duration would still obey to a self-similar, constant stress-drop scaling with magnitude. In a more realistic rupture model accounting for the breakout phase, due to the very limited duration of this unstable acceleration process (of the order of 1-2 seconds for M 7-8) as compared to the entire process (tens of seconds for the same magnitude range) no significant effects on the total duration would be produced. In this view, the observed differences between small and large events during the very early stage of the rupture process could be reconciled with the long-time scaling of earthquake source parameters.

Finally, we clarify that our observations are done within 1-2 seconds from the P-wave arrival and suggest that large events could potentially start with a slower amplitude increase, while small events may begin with a higher amplitude growth-rate. Since no direct observations of non-radiative processes (preceding the dynamic triggering) are currently available at the scale of natural earthquakes, our hypothesis is that the observed initial growth rates of P-wave amplitudes could be the footprint left by the early stages of earthquake rupture, including both the preparatory, quasi-static phase and the unstable break-out acceleration. Additional, high-resolution observations and modeling efforts will be essential to test and refine this hypothesis.

[reviewer] 4. Regarding its potential for earthquake early warning, it seems to me $\sigma_{t_{\text{half}}}$ cannot be determined until a large portion of the time function is known, because we don't know what the maximum curvature is until the event ends.

>> We thank the reviewer for the comment. Our analysis is indeed performed in off-line mode, meaning that the initial slope is measured in a time window whose limit is estimated upon the detection of the LPDT Plateau Level. We are currently working on the development of new strategies to obtain real-time estimates of the initial LPDT slope for fast magnitude characterization. The topic is pretty challenging since in real-time mode each curve will continuously evolve and may reach the plateau at different times. One possibility is to develop an evolutionary strategy, such that updated estimates of magnitude are obtained from slope measurements done on a progressively expanded P-wave time window while the alert is issued when a predefined threshold is exceeded. We added a few lines in the revised version of the manuscript to discuss the potential application of our results to EEW systems (from line 276 on).

****Minor comments****

[reviewer] Line 76-77: The description of station selection is not very clear to me the first time I read it. Consider adding an inset plot in Figure 1 or 2.

>> We rephrased the sentence (now line 75-78) and added an inset plot in Figure 2, showing a schematic sketch of stations selection.

[reviewer] Line 139-142: This statement needs to be shown by a figure in the supplementary material.

>> We added a new Figure here and in supplementary material (Figure S3) in which we show how the averaging on station azimuth is mitigating any potential directivity effect. Specifically, we selected the event occurred on 2014-08-24 at 10:20:44 with Mw 6 (table S1) having an azimuth coverage higher enough to include a wide range of records in the circular 25 km crown from the closest station to event epicenter, to evaluate the averaged LPDT curve.

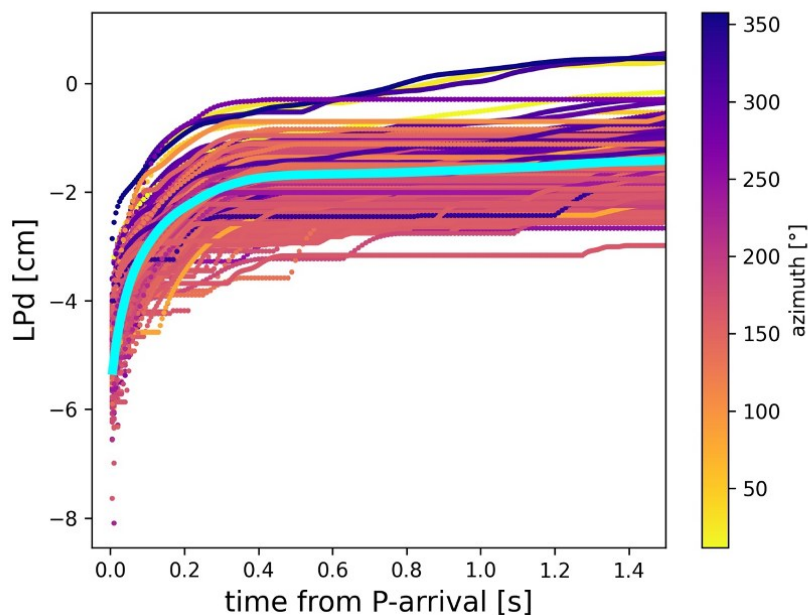


Figure S3: Azimuth average effect on example event 2014-08-24T10:20:44 Mw 6. The solid cyan is the averaged LPDT curve obtained from the single station LPDT curves in colored dotted line. The color bar follows the station azimuth values.

[reviewer] Line 146-147: It is not clear how the slope is measured. The Methods section needs to be cited here. Make the description in the main text more explicit, e.g. "We identify the plateau time on the LPDT curves by analyzing the curvature and then measure the initial slope between $t_{min}=0.05$ s and t_{half} (half of the estimated plateau time) (See Methods for more details)."

>> We substantially revised the *Methods* section and clarified the description of the slope evaluation, in the new version of the manuscript, as the Reviewer suggested.

[reviewer] Line 178: Define breakout phase. The difference between breakout phase and nucleation phase needs to be clarified. Are there any other papers that define this term clearly?

>> We substantially revised the *Discussion* section, adding a new paragraph explaining the differences between nucleation and breakout phase (line 185-216).

[reviewer] Line 193: It is not clear how equation (3) shows an exponential increase with rupture velocity V_R .
>> We have now rephrased the line in the new version of the manuscript (line 231). The rupture velocity factor grows as a power of v_R .

[reviewer] Line 198-214: Explanation of the different onset speed between small and large ruptures. This is very interesting discussion. However, this part looks a little weak because the only cited work (ref 28) is a preprint that is mostly about theory and models. It would be great to see a compilation of many earthquakes that demonstrate this rupture velocity -- magnitude relation, although this might not be easy, especially given that we are only looking at the very beginning of ruptures. If not possible now, it is still worth mentioning how future observations might bring new insights on how to test the current statement.

>> We appreciate the reviewer's insightful comment highlighting the importance of better observational support for the proposed relationship between rupture velocity and earthquake magnitude, especially during the initial rupture stages. We agree that this is a perspective path of investigation, though it currently remains limited by the availability of high-resolution rupture models, particularly for the very early moments of faulting. In our discussion, we aimed to highlight that quasi-dynamic rupture models have long suggested a link between rupture velocity, dynamic stress drop, and the early evolution of the radiated displacement field. However, we acknowledge that the theoretical basis alone is insufficient to fully substantiate this mechanism. A more systematic compilation and analysis of kinematic rupture models, particularly for moderate to large earthquakes, would be necessary to explore how initial rupture velocities vary with magnitude. While this is beyond the scope of our current work, we now clarify in the revised manuscript that future high-resolution observations and modeling efforts will be essential to test and refine this hypothesis (line 259-274). We report the new paragraph here as well.

The dynamic modelling of source processes is a valid tool for simulating the mechanical conditions for fault activation, including the friction law, the critical slip value of the nucleation size, the rupture velocity, and the role of these parameters in controlling the final earthquake size. Complementary, laboratory experiments may provide direct measurement of the parameters involved in the generation and propagation of seismic ruptures and the increasing availability of high-frequency records during laboratory micro-earthquake ruptures will pave the way for the future interpretation of natural, large scale seismic processes (Acosta et al., 2019). Finally, the increasing number of near-fault high-quality seismic and geodetic recordings will allow a deeper monitoring of the background seismicity and deformation evolution in the fault areas, resulting in a better understanding of the conditions leading to the preparation of large events, (including seismic sequences, foreshock activity and aseismic slip (Chiaraluce et al., 2022)).

[reviewer] Line 234-238: How to reconcile the different onset speed with the self-similar scaling relationship (e.g. duration $\sim$ moment^{1/3})? I agree that this can be reconciled. It would be great to see exactly how the later stages would adjust to come back to the self-similarity result. For example, the authors can show the duration-moment scaling for their simulated events produced by the Sato and Hirasawa model (Figure S4) to check if self-similarity still holds.

>> please, see response to comment #3.

[reviewer] Figure 1: Consider changing the color map or changing the color of the accelerator markers. Magenta squares and dark blue circles are too similar.

>> We changed the symbols for velocimeter locations in the map, as suggested (see new Figure 1).

[reviewer] Figure 2: Consider connecting a line in the figure, or adding an equation to clarify how the slope is measured. Probably a good idea to explain LPDT and t_{half} in the caption too.

>> We added the equation for slope evaluation in Figure 2 caption. We explained LPDT curves and time of half curvature (t_{half}) in figure caption.

[reviewer] Figure 3:

What does "pf" in the caption mean (Line 363)?

Explain SE=Standard Error in the caption (line 367).

Here the meaning of t_{half} is explained. But readers might get confused because in the main text, it is explained as half of the time when LPDT reaches its plateau. I suggest to be consistent, or mention briefly how the determination of plateau time is related to curvature calculation, as suggested in the comments above.

>> We have corrected the misspelled word; we have now added Standard Error in the caption; we have added a sentence in the *Methods* section to explain that the maximum curvature is expected at the time of the Plateau Level of the LPDT curves. We also added a panel in Figure 2 to emphasize the concept.

[reviewer] Figure 4: Panel B needs more explanations. For example, explanation for the solid and dashed curves is missing in the caption. Also somehow the xlabel "Tens" is underlined.

>> We have reviewed and simplified panel B of Figure 4. Moreover, the caption of the figure is now rephrased.

[reviewer] Line 416-417: probably a good idea to make it clear that the average is done in the log peak displacement space not the linear space.

>> We clarified that the average is performed on the logarithm of P-peak of displacement (LPD)

[reviewer] Text S1: this section needs to include more details. For example, consider reproducing the equations of Sato & Hirasawa here, or cite equations (1-3) in the main text. You should also cite Figure S4. The alpha ratio in Figure S4 is also not explained. I assume it is V_r/V_s . Please fix " $V_p/V_s=\text{radq3}$ " to display properly. Use consistent decimal format. Use dot instead of comma for "Mw 6.5 mainshock". Be consistent. Use "Vs" instead of "vs" in the last line.

>> We acknowledge the reviewer for this remark. As required, we added in Supplemental Material more details on the technique used to compute S&H far-field synthetic waveforms. Also the other text correction have been implemented.

[reviewer] Table S1: There are events with number of records fewer than 3. Maybe consider excluding these events from analysis.

>> We clarified that we excluded those events with less than 2 records, for which it is not possible to perform the average for the LPDT curve.

Reviewer #2 (Remarks to the Author):

[reviewer] The authors examine the beginnings of 200 earthquakes and show that the beginnings of the smaller earthquakes they observe grow more quickly than the beginnings of the larger earthquakes.

This is an interesting result and seems a reasonably well done and well written paper. I do, however, have a number of comments below, and one major other significant issue: that M4-5 earthquakes often last less than a second. How do you avoid contaminating your results with the fact that some earthquakes are over in your window, and others aren't?

>> We thank Reviewer #2 for considering our results interesting, and for the overall positive evaluation of our paper. We clarify here the major issue raised by the Reviewer, related to the contamination of results. We hope that our answer exhaustively addresses the question.

In our analysis, we focus on the initial rise of the LPDT curves and evaluate the initial slope between the first P-arrival time (the same for all the events) and a characteristic time t_{HALF} corresponding to half of the time at which each LPDT curve reaches the Plateau Level (see the description and meaning of t_{half} in our replies to rev1 comments). This means that the time window for slope evaluation is not fixed, but depends on the final earthquake magnitude, being in any case, much shorter than the time at which each LPDT curve reaches the Plateau Level. For example, for the M4-5 earthquakes, the initial slope measurement is typically done in a time window of the order of 0.1-0.2 seconds (see Figure 3 of the main text, revised version). In this way, we ensure no contamination in the slope measurement, at all magnitude ranges.

[reviewer] Also, do you truncate the LPDT curves before analysis so as not to contaminate the data with information from 2 or 3 seconds into the rupture? The methods don't seem to say so.

>> We clarify here the computation of the curves: starting from the P-wave arrival, we evaluate the logarithm of the P-peak of displacement in an expanding time window (with time steps of 0.01s). We stop the computation at about 30 seconds for each LPDT curve, so that we ensure the curves are long enough to reach the plateau level (peak of MRF), without extending unnecessarily far beyond that. Then we identify the plateau and focus on the early portion. We do not further truncate the LPDT curves before the slope analysis. Then the slope of LPDT is measured in a time window that ends before the plateau level to avoid contamination from later phases of the rupture. We clarified the data processing and the computation of the curves in the new version of *Methods* section.

[reviewer] Lines 37-46. This isn't what many people mean by "cascade" and "pre-slip" models. Even in that paper by Joan Gomberg, the pre-slip is described as a mechanism for deterministic *nucleation,* not for deterministic *rupture.* The nucleation can be deterministic even if the actual rupture is not. The nucleation can happen in a small area dictated by accelerating aseismic slip, and then the rupture can spread outward.

>> We sincerely thank the Reviewer for this comment, that helped us in clarifying and re-organizing the Introduction section. We realized that the two models mentioned in the original version of the manuscript did not properly concern the topic of our research. We therefore rephrased the entire paragraph, with a new one, more focused on the beginning of earthquake ruptures (lines 26-41).

[reviewer] Figure S3. This figure is more convincing than Figures 2 and 3. Why not present it?

>> We thank the Reviewer for this suggestion. We added figure S3 in the main text as panel B and C of Figure 2.

[reviewer] Line 121. Rephrase this? You mean that your 200 earthquakes are selected from 7000 global earthquakes?

>> The dataset description has been revised and the line is now rephrased. Our initial dataset consisted of more than 7000 seismic records referring to 200 global earthquakes.

[reviewer] 161. Minor phrasing. I initially read the term in parentheses as a descriptor for the critical size, but I think you mean it as a descriptor for the frictional parameters. Changing e.g. to "such as" would make it clearer.

>> Thanks, we changed the description in parenthesis.

[reviewer] Lines 159-167. That's great hypothesis for earthquake ruptures based on lab data, which would indeed be my working hypothesis for how ruptures start. It is not, however, known to be true, and shouldn't be presented as how earthquakes work.

[reviewer] Lines 166-167.

In particular, why does Dc relate to magnitude? Sure, in someone's model or lab results, but be specific about what's known about the earth and what you're using as a guiding hypothesis.

>> We thank the Reviewer for this comment. We have now rephrased the suggested lines (now moved to line 202-205), clarifying that this hypothesis (and in particular, the scaling of Dc with magnitude), is only observed at laboratory scale, and that the transposition of results to the scale of natural earthquakes is not trivial, also because, at the natural scale, no direct observations of Dc during moderate to large earthquakes are available.

[reviewer] Lines 193-195. I don't see how the rupture velocity and stress drop control the *shape* of the P wave radiation curve. It looks like they control its amplitude, but the shape is just t^2 . I think the curves in Figure S4 would have exactly the same shape if you plotted them on a log scale, which is how you've analyzed the data.

>> To better clarify the issue, we now present LPDT curves derived from synthetic simulations of the S&H circular fault model using a decreasing rupture velocity with increasing magnitude. In this representation the changing in the initial slope of LPDT curve as controlled by the rupture velocity is quite evident. In particular

we plot the measured initial slope on synthetics vs the magnitude to emphasize this behavior. Furthermore, in response to a comment by Reviewer #1, we evaluated the theoretical durations of S&H P pulses for different stress drop values and verified that at the scale of the total rupture duration the self-similar constant-stress drop scaling still holds

[reviewer] Line 201. "Corroborated and proven." I'm not sure what proven means here, but this seems unnecessarily pushy. Why not just let readers read and interpret for themselves?

>> We have now better rephrased this sentence.

[reviewer] Lines 209-214. What's the point of this paragraph?

>> In the new version of the manuscript, we substantially revised the paragraph (now at line 250-264). We hope that the new version better explains our point of view.

?

[reviewer] Are these curves not reversed? The $\log(\text{slope})$ is higher for smaller events. If we approximate some portion of the curve as t^n , then we have $\log(t^n) \sim n \log t$ larger for smaller curves, so more concave up for smaller events?

>> We thank the reviewer for raising this point. We would like to clarify that the curves of Figure 4b were intended to be as a mere, schematic representation of the Moment Rate Functions, but did not represent any quantitative modeling of the rupture process. When drawing the curves, we had in mind the simplified behaviour of the Sato & Hirasawa Moment Rate Functions, for which the amplitude increases quadratically with time, up to the peak value. In figure 4b, we meant to suggest how the observations at very short time scales could have been merged with the long-term observations, related to the absolute peak (and peak time) of the source process. We dedicated an exhaustive paragraph to the discussion about the potential merging of observation at short and long-time scale (see Response to comment 3 of Reviewer #1).

We provide a potential interpretation of the results in terms of the breakout phase of the rupture process (typically within 1-2 seconds from the P-wave arrival), during which large events could potentially start with a slower amplitude increase, while small events may begin with a higher amplitude growth-rate.

The Sato & Hirasawa model, however, does not include the breakout phase and it only accounts for a unique value of rupture velocity for the entire duration of the process. Based on these considerations, we realized that Figure 4b was misleading and decided to remove it from the main text. We left the detailed discussion in the revised version of the manuscript.

[reviewer] First paragraph and 241-248. It's absolutely useful to think about using this for earthquake early warning. However, I think the potential for improvement is overstated. To avoid confusion, it'd be worth noting that EEW is already operational and that it's constrained by station distribution. We don't get the first second of the P wave right away. The scatter in Figure 4a would also be interesting to discuss in the context of earthquake early warning.

>> We agree with the reviewer that the real-time implementation for an effective early warning strategy is a challenging task and needs additional steps of verification and validation. In response to a similar comment of rev.1 we depict the possible line of research that could be traced for using the results of this study for early warning. We have added a clarifying statement noting that EEW is currently in use and that our work is intended to complement and potentially enhance existing approaches, particularly by exploring the earliest portions of the P-wave. We also now explicitly acknowledge that the first second of the P-wave is typically not immediately available in real-time systems. We added a few lines in the revised version of the manuscript to discuss the potential application of our results to EEW systems (from line 276 on).

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REVIEWERCOMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have greatly improved the manuscript, particularly in clarifying technical details and defining key terms. However, I still have a major concern on whether the selected time window for analysis is representative for the very early breakout phase of the source time functions, especially for those earthquakes that are below magnitude 5. I would suggest publication only after the authors address this key issue.

[Authors] We thank the reviewer for appreciating the work we have done on the manuscript. We understand the major concern posed by the reviewer and try to provide additional elements to support our results.

Main comments

In line 146-147 of the new manuscript, the authors write “Indeed for $M=4$ the typical MRF duration is 0.5 seconds whereas for $M=7$ the typical MRF duration is 15 seconds”. In the authors’ reply to the original point #2 of the first review: “magnitude and the total duration may range from a few seconds for the small events ($M4-5$) to several tens of seconds for the very large earthquakes ($M>7-8$).” I would like to point out that these duration estimates for the small earthquakes ($M 4-5$) are probably an overestimation. For example, a $M 6.x$ earthquake typically lasts 3-10 s (e.g., Peng and Gomberg, 2010). Based on Figure S7 from the Sato & Hirasawa model, the duration for a $M_w 4$ earthquake with a 1 MPa stress drop gives a duration of $\sim 0.3-0.4$ s. The point is the time window for analysis (t_{HALF}) seems a significant or even comparable to the total duration of those small earthquakes ($M 4-5$). It is necessary to carefully examine the relative portion of the time analyzed compared to the total duration, both for analyzing earthquake physics and potential application for earthquake early warning. If the time window for analysis is longer than the time when the moment rate function reaches its peak, we cannot call them “rupture beginning” and the significance for early warning also would drop. Therefore, I suggest plotting the expected duration range for $M=4,5,6,7$ earthquakes on top of Figure 2A, 3B to allow direct comparison.

[Authors] As the reviewer suggested we evaluated the relative portion of the analyzed time window (t_{HALF}) compared to the time when the moment rate function reaches its peak (t_{PEAK}) for different magnitude earthquakes. To this purpose, for each magnitude we computed time of the peak of the moment rate function according to the Sato & Hirasawa model and exploring a broad range of stress drop and rupture velocity values. Specifically, we varied the stress drop between 0.1 and 10 MPa and the rupture velocity between 0.5 and 0.9 of the shear wave velocity (Abercrombie 1995, Allmann & Shearer 2004). As resulting from our simulations, the peak time of MRF varies between 0.2 and 0.6 s for $M=4$, between 0.3 and 1 s for $M=5$, between 0.8 and 3 s for $M=6$, between 2 and 10 s for $M=7$.

As the reviewer suggested, we plot a new version of figure 3 that compares the two time windows (t_{HALF} and t_{PEAK}) for each magnitude. The simulated times are shown as a shaded blue area that includes all the possible values resulting from the simulations. As it can be seen from the figure, for the very small events ($M<5$), our time window (t_{HALF}) is partially overlapped to the lower bound of the shaded area. This suggests that the analyzed time window can be of the same order of the peak of MRF only in case of very high values of both stress drop (10 MPa) and rupture velocity (0.9 vs). For $M>5$ the t_{HALF} time window is on average smaller than the lowest values of t_{PEAK} . As the magnitude increases the difference between the two times becomes more evident.

There are various aspects already mentioned in the revised version of the manuscript (lines 501-507), that we believe are worth to further discuss, considering the results of the new analysis suggested by the reviewer.

The choice of the time window (t_{HALF}) is arbitrary, since any shortest interval could be used, provided that a sufficient number of data points are considered for the linear fitting of the initial slope. In panel C and D of the new Figure 3 we compared the results in terms of slope and related time using a smaller time window, equal to t_{THIRD} of plateau level.

As expected, when using a shorter time window, the difference between t_{THIRD} and t_{PEAK} increases, avoiding the inclusion of portions of signal not related to the beginning of the earthquake rupture, while keeping the initial slope measurement still stable. As a direct consequence, we only focus on the scaling of the initial slope over the entire magnitude range, rather than interpret single slope values and time measurements.

Finally, concerning the application to EEW (which is however not the main goal of our work), even measuring the slope at the time of the peak of MRF for $M=4-5$, some warning would still be possible, considering that the peak of the most damaging waves is associated to the later arrival of S- or surface waves.

We thank the reviewer for suggesting this additional analysis, that we believe worth to be added to the manuscript. We replaced the original figure with the new version and also added a comment in the manuscript (line 153-172).

We hope that the analysis presented here are exhaustive to answer the major concern raised by the reviewer.

Minor comments

1. Figure S5 indeed shows that the deep earthquakes feature a slightly higher slope compared to a shallow earthquake of similar magnitude.

[Authors] We included a comment to Figure S5 in lines 135-138 that we report here also:

“Finally, we observe that deep earthquakes (depth > 30 km) tend to exhibit slightly higher slope values compared to shallow events of the same magnitude (see Figure S5), which may indicate different stress and/or friction conditions that govern the initial rupture process of deep, mainly subduction zone, earthquakes. Although the in-depth analysis of such differential mechanism is beyond the scope of our study, we note that the overall scaling of the initial slope with magnitude is preserved, independently on the event depth.”

2. I appreciate that the authors add a discussion on various previous studies that may have posed different views or conclusions in the supplementary materials and in the introduction. I would like to point out that the analysis by Melgar and Hayes (2019) may be flawed because the time range for analysis is comparable to the peak time of the moment rate functions, as pointed out by Meier et al. (2021, GJI). Therefore, we must be cautious about their conclusion.

[Authors] We rephrased the discussion regarding the paper by Melgar and Hayes (2019), as suggested by the reviewer. We report the rephrased line here also:

“Among them, at the time of scale of seconds-to-tens of seconds Melgar and Hayes ²² provided evidence that the initial growth-rate amplitude is higher for larger events and lower for smaller ones, although some questions raise about the time range of their analysis being comparable to the time peak of the moment rate function (Meier et al. 2021).”

3. Lines 193-195: while I understand the general intuition here, this is a bit tricky because while larger earthquakes do spend longer time in the nucleation or breakout phase, they also release more moment in that period. It is unclear to me why this would necessarily lead to a smaller slope.

[Authors] We are aware of the complexity of the proposed interpretation, as the relationship between moment release, duration of the breakout phase, and the resulting slope is not straightforward. We clarify that the interpretation provided in our manuscript still need to be verified and supported by quantitative analyses and measurements, both at the scale of real earthquakes, and at the scale of laboratory fractures.

According to our hypothesis, longer times and larger areas (i.e. large D_c) are needed to reach the condition for the rupture to propagate for large events, and this may lead to the observations of smaller initial slopes. Conversely, for small events, the smaller D_c value is reached in a shorter time on a smaller portion of the fault, with potential higher rates of initial amplitude growth.

While evidences of the unstable acceleration phase have been provided at the scale of laboratory experiments, no quantitative measurements of the moment release during the breakout phase is yet available. The quantitative measurement of the moment release during the breakout phase and its relation with the slope measured here is exactly the direction we wish to pursue with future works. By analyzing acoustic emissions at laboratory scale and comparing with detailed slope measurements, we intend to establish a robust framework to quantify the moment release during the breakout phase, that will enable a better understanding of the physical processes governing rupture dynamics.

4. Lines 201-203: reference needed on the scaling of D_c with magnitude.

[Authors] We added the reference as suggested by the reviewer.

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have satisfactorily addressed my main concerns raised in the previous round. In particular, they have revised Figure 3 to include the range of peak time as a reference and have clearly articulated in the main text that their conclusions are robust with respect to the choice of time window used for evaluating the slope (e.g., t_{half} vs. t_{third}). I support the publication of this manuscript, as it is likely to stimulate further discussion on the important question of whether small and large earthquakes are statistically distinguishable during the onset phase of rupture.

[Authors] We wish to thank the reviewer for the insightful comments that helped us to improve the original and the revised version of the manuscript. We sincerely hope that our work will stimulate the debate on the onset phase of earthquake ruptures. Following the suggestion provided by the reviewer - either in this last stage of review and in the previous phases - we will further investigate the evidence and the origin of these differences, including in the database finite fault ruptures as well as well microearthquakes at laboratory scale.

We made all the modifications the reviewer suggested in this last round of minor comments.

A few final minor comments below:

Minor comments

1. line 150-155: the numbers here for durations also need to be updated to be consistent with the latest updates in the following lines.

[Authors] Done

2. line 180: this number probably also depends on magnitude.

[Authors] Done

3. In future work, it may be worthwhile to consider the effects of finite source dimensions for larger earthquakes. Given the current station selection criteria, larger events—owing to their greater rupture extents—are likely associated with relatively shorter source-to-station distances. In other words, the ratio of distance to source (d) to source dimension (R) tends to be smaller for larger earthquakes. This factor could potentially influence the observed trends and may be worth exploring in future investigations.