Reviewer 1

Review of “A multiple asymmetric bilateral rupture sequence derived from the peculiar tele-seismic P-waves of the 2025 M_W 8.0 Mandalay, Myanmar earthquake” by Inoue & Yamaguchi, et al.

1) SUMMARY

This manuscript presents a very timely teleseismic-based analysis of the rupture process for the recent, devastating earthquake in Myanmar. The main finding of the paper is that the rupture propagation was complicated, involving phases of bilateral, supershear rupture, which can explain unusual teleseismic P-waveforms to the north and south of the rupture. I would say that the inverted potency-density tensor model doesn’t reveal too many new insights into the rupture process of this earthquake, but it does indeed appear to confirm a long total rupture length and phases of supershear rupture speed. Nevertheless, given that this result offers an early, formal scientific analysis of the 2025 Myanmar earthquake, it should be well-received by the seismology and earthquake science community. I congratulate the authors on their rapid efforts.

We appreciate your thorough evaluation and constructive suggestions, which have greatly improved the manuscript. Please find the revised manuscript as well as the responses below.

2) MAIN COMMENTS

My main general comment surrounds the multiple references to “short-period pulse-like phase and the tremor-like phases”. Much of the manuscript discusses the “peculiar” / “unusual” nature of these waves. First, I am not convinced about the use of “tremor-like” in the context of teleseismic waves. Perhaps the authors need to better describe exactly what is meant by this term, and explain better why they describe it as tremor-like in the context of the frequency content.

Thank you for your suggestion. We have rephrased the relevant sentence as:

“At the observation stations on the south side, multiple peaks without dominant pulses (here after we call these phases as "tremor-like phases") continued until about 90 s”

Regarding the high-amplitude phase at northward azimuths (in the opposite direction to the main rupture), it would be interesting to compare this pattern with other large strike-slip events. Could the lower amplitudes to the south be caused somehow by the supershear Mach cone? The described “peculiarities” might not be obvious to all readers, so it would be nice to see a synthetic test showing a simple unilateral southwards rupture – one with normal rupture velocity and another with supershear to quantify better the influence of rupture direction and velocity of the main features of the waveform.

We appreciate your critical insights. In this revision, following your suggestion, we have newly performed a synthetic test to evaluate the waveform amplitude according to the rupture scenario. Please find the newly added Fig. S4 and the relevant discussion as:

” Such a waveform signature can be seen in synthetic tests with variable rupture speed. By comparing the test that uses a normal rupture velocity with another one that produces supershear (Fig. S4), one can notice the relatively higher-amplitude phases at azimuths in the direction of the supershear rupture.”

3) MINOR COMMENTS

If possible, please include the final, integrated slip model as a supplementary file so that it can be used by the community.

We have included the integrated slip model in the zenodo repository.

I’ve noted that a preprint of this manuscript appears on EarthArXiv. The authors should note that Google Scholar may not choose the final paper published in Seismica as its “best” version (see e.g., Melgar et al., 2023). To try and avoid this issue, the authors might like to use a different title from the preprint version (e.g., see below) and then merge the two versions manually later in Google Scholar profiles.

Thank you for the advice. We have changed the title and will follow the procedure in Google Scholar profile.

Title and elsewhere: the formal name of the earthquake should probably be the “Mandalay, Myanmar earthquake” or the “Mandalay-Naypyidaw, Myanmar earthquake” – the title should include one of these.

Thank you for your suggestion. We have changed the title accordingly.

Title – change “tele-seismic” to “teleseismic”.

Fixed.

Please include the moment magnitude in the title

As we discussed in Section 5, the moment magnitude of our estimate is larger than that of the GCMT because of modeling strategy to represent the source complexity. Since our PDTI approach is not primarily targeted to estimate the moment magnitude, we are a bit hesitant to include the moment magnitude in the title, but instead, we included those explanations in the Discussion section.

Authors – is the fifth author’s first and family name the wrong way around?

Thank you for pointing that out. Fixed.

It would be good to mention the tectonic context (i.e., plates involved) and their relative velocities.

Added as:

"Around the Sagaing fault, India plate is moving against Eurasia plate at a rate of about 50 mm/yr (DeMets et al., 2010) (Fig. 1a). "

The second paragraph of the Introduction seems to include primary results – i.e., the azimuthal distribution of polarities, and the characteristics of the waveforms. This content should be given more background/context, or appear in a different section.
Thank you for the suggestion. We have decided to delete the corresponding lines regarding the polarity because they are not necessary in the context of our introduction and discussion contents.

L104 – better to say "node" instead of "knot"?
We agree. "knot" has been replaced with "node".

L108 – change "All of basis" to "all of the basis".
Fixed.

L108 – typo – "bset"
Fixed.

L108-109 – I don't entirely understand this sentence. Maybe rephrase it to make it clearer?
We have rephrased it to make it clearer.

L132 – change "dipping" to "dip"
Fixed.

L144-146 – "It is difficult to trace the rupture front propagating to the south" – is this because of the available station distribution, or because the southward-directed rupture was especially weak? Maybe the authors can comment/speculate.
This is because the southward-directed rupture was relatively weaker than the northward-directed rupture during E1. We have commented on this in the corresponding line.

L168 – Please state the final Mw value.
Added.

Figure 1
- Do the beachballs include aftershocks? If so, these should be shown differently (e.g., with a different colour).
They do not include aftershocks. We have clarified this point in the caption.

- The relocated historical earthquakes should ideally be labelled/annotated with their year and magnitude.
Labeled as suggested.

- In the caption, maybe change “relocated earthquakes” to “previous large events on the Sagaing fault (Hurukawa and Maung Maung, 2011)
Changed accordingly.

Figure 2
- Since this is a key figure, I recommend making it full-page size, which would help to reduce the along-strike overlap of beach balls in panel (c).
Thanks for the suggestion. We have enlarged the figure size and separated the snapshots into Fig. 3, so that we reduce the along-strike overlap of the beachballs in Fig. 2c.

- In the caption for panel (b), say that the grey lines represent the other models, and the black line is the best-fit model STF.
- In Panel (b), please put the y-axis label and tick-labels on the left-hand side … I nearly didn’t see them!
- It would be good to label the north and south directions on top of the first cross-section in panel (f).
- It would be useful to label Episodes E1-E3 on the figure.
Thank you for your suggestions. We have fixed all accordingly.

Reviewer 2

The manuscript presents a seismic potency density tensor inversion of the recent Myanmar earthquake. Overall, it is well-written, and the results suggest a bilateral rupture. While this interpretation differs from the USGS fault-plane solution, it offers a new perspective through an alternative approach. Here are a few main comments. Other comments are in the attached annotated pdf.
We greatly appreciate your careful evaluation. We have revised the manuscript by following your suggestions and comments, including the ones in the annotated PDF files.

Minor

1. Manuscript needs spelling check (usage of word like ‘bset-fitting’, theorical)

We have checked and modified the spelling and grammar together by following Reviewer 1’s suggestion.

2. Fig 2b is not cited and does not appear in text. Figure 1e is missing and cited in text.

Thank you for pointing these out. Fig. 2b has been cited. Fig. 1e was a typo, and the relevant sentence has been deleted by addressing the Reviewer 1's suggestion.

Introduction

1. Polarity variability at station to the north-south is not clearly described in the Introduction section. This is perhaps because a Figure 1(e) is missing.

Together by addressing the Reviewer 1's comment above, we have decided to delete the corresponding discussion and figure regarding the polarity.

2. A little more description of potency tensor to begin with.

We have added the description as:
", where the potency tensor is calculated by dividing the moment tensor by the rigidity of the medium (Shimizu et al., 2020)."

Data and Methodology section

1. Pre-processing steps need further clarification. Or maybe add details to the supplement.

By resolving your detailed comments on the annotated PDF, we have clarified the pre-processing steps.

2. What 4 models are being used is not clear either in manuscript or supplement? In the supplement it looks like 4 models are used together in one single simulation. I am guessing that 4 different models are used independently for 4 different test cases, for example, when I use southeast model, do you only use stations from southeast?

We use the same set of stations to evaluate the sensitivity of the different structural models, which were used separately for each modeling as presented in Figs. S2 and S4. We have clarified the description of the alternative models as:
"We also used four different velocity structure from CRUST1.0 (Laske et al., 2013) but using the same set of stations as used for the main model (Text S1)."
"The uncertainty and sensitivity of the solution to the different model settings are evaluated (Text S1 and Figs. S2, S3) using the same set of stations as used for the main model".

Results

1. Add E1, E2 and E3 on Figure 2b and 2c.

We have added the labels. Because E1, E2, and E3 are rupture episodes, which are primarily defined by both space and time of the potency-rate density tensors, we labeled them on Fig. 2c.

2. Many times numbers are being used in the results section that appear from nowhere. Please either show the calculation, or mark them on the map/figure
    1. Example 1: centered at 10 km and 110 km south of the epicenter

    2. Example 2: estimated rupture migration velocity from the start time and location of E2 is about 4.6–5.4 km/s,

3. Last 2 paragraphs of results need a little more refinement and connection with figures.

Thank you for your suggestion. We consider these two comments (2 and 3) to be relevant, and we have modified the figure and the associated explanation about those numbers accordingly, together by following your suggestions in the annotated PDF.

Conclusions

1. Your solution Fig 2a is very similar to the GCMT solution. If there are differences then please discuss. However, it brings me to Fig 1c, where a double-couple version of GCMT is plotted. Is there any specific reason for doing so?

We showed the GCMT solution in Fig. 1c as we intended to exhibit that the first-motion polarity is not explained by the moment tenor solution under the assumption of a point source, which is to emphasize a necessity of using our PDTI approach. However, by addressing the Reviewer 1's comment, we have deleted the associated figure in this revision.

Reviewer 3

I have read in detail, to some extent, the manuscript entitled "A multiple asymmetrical bilateral rupture sequence derived from the peculiar tele-seismic P-wave of the 2025 Myanmar earthquake", by Nahiro Inoue, Ryo Yamaguchi, Yuji Yagi, Ryo Okuwaki, Enescu Bogdan and Tira Tadapansawut. The authors assume that the Miyanmar 2025 earthquake fault rupture involves the rupture of several segments of the againg fault north and south of the epicenter, located close to Mandalay (central part). Their assumption is based on observation of teleseismic seismograms, specifically the P-wave, whose time domain signal pattern suggests the occurrence of multiple ruptures. Although the whole rupture is bilateral with directions north and south of the epicenter, the ruptures' propagation is mainly towards the south. Using a well established inversion method based on the potency density tensor (PDTI) projected over a model fault plane representing the whole segmented fault, the authors dentify three episodes of ruptures, each involving several fault segments. Finally, the authors produce a 24 seconds slip history of the earthquake from a 90 seconds potency rate density.

Considering that the manuscript intends to be a fast report on a very recent earthquake, and the fact that there are almost no near-field strong ground motion records of this earthquake, I recommend its publication. The authors' use of the PDTI method and their results are scientifically solid. I am convinced that this manuscript will contribute to the ongoing discussions in the scientific community to unveil the scientific aspects of the Myanmar 2025 earthquake.
On the other hand, I must emphasize that the manuscript cannot be published in the way it is written. I request that the authors address my comments listed below, which I consider minor, before publication.
Thank you for your positive evaluation. We have revised the manuscript based on your suggestions. We would like to note that we did not "produce a 24 seconds slip history of the earthquake from a 90 seconds potency rate density", but, as described in the Method, Data and Model setting section, we have estimated the spatiotemporal potency-rate density tensors in a model space allowing 90 s total duration of the event and each node of the model space has 49-s duration of the potency-rate density functions.

-To start, the title needs to include some basic information about the earthquake, such as its date and magnitude. Something like "A multiple asymmetric bilateral rupture of the Mw 7.7 March 28 2025 Myanmar earthquake, derived from teleseismic P waves". I see no need to include the word 'peculiar'.
Together by considering the suggestion from Reviewer 1, we have modified the title.

-Abstract: poorly written. Replace 'large' with Mw 7.7, i.e. "The Mw 7.7 earthquake that occurred in central Myanmar on March 28, 2025, exhibits a strike-slip rupture mechanism and aftershock distribution that suggests that the mainshock propagated mainly to the south of the epicenter. Eliminate '. However (it should not go after a period), i.e. " A pulse-like phase ...etc". Define here what is 'unusual' before using either word. Words like 'unusual' or 'peculiar' are subjective, unless defined for scientific purposes.
We have revised the abstract accordingly. As replied earlier to Reviewer 1, the estimates of Mw can be different from the methodologies or modeling strategies, so we are hesitant to include the moment magnitude, but instead, we included those explanations in the Discussion section.

-Introduction: poorly written; many grammar errors. I will not elaborate on those. Just to point out that in line 65, you refer to Fig. 1e. It must be 1d. Besides that, the issue that caught my attention here is that the authors do not refer to the paper by Xiong et al. (2017) on Coulomb stress transfer and accumulation on the Sagaing fault. This, and also the paper by Wang et al. (2014) on active tectonics and earthquake potential of the Myanmar region are, in my opinion, very relevant to the subject. In particular, figures 2 and 3 of Xion et al (2017).
We have included the suggested literature.

Overall, there are some terms that need to be a bit more elaborated. One is 'Green's function errors'. What is it? It is fine for the acquainted reader, but this 'fast report' may be read by the non-acquainted reader. They should be able to understand it without the need to go to the reference(s). Please elaborate more on the 'uncertainty of the Green's function', as well as on 'high degree of freedom' in this context of seismic source. How would these be affected if instead of teleseismic observation, you had near-field strong ground motion records?

We have added the explanation of "Green's function error" as: "In recent years, based on the approach of Yagi and Fukahata (2011) who considers the uncertainty of the Green's function originates from a discrepancy between the true and theoretical Green's functions due to inaccuracy of the underground structure". On the other hand, regarding the "high degree of freedom," it has already been explained in the main text as "a high-degree-of-freedom, the seismic source process by allowing changes in the focal mechanism and rupture speed and direction, including possible back-rupture propagation and supershear". To avoid possible misunderstanding, we have made minor revisions to the text.

As shown in the numerical experiments of Yagi et al. (2004, BSSA doi:10.1785/012003095), the uncertainty of the Green's function becomes a more serious problem for near-field strong ground motion records. In general, when using near-field strong ground motion records, the results obtained vary considerably depending on the assumption of the velocity structure, the setting of the fault plane, and the input data set. On the other hand, as our results show, PDTI with teleseismic P-waves provides stable results even when different velocity structures and fault planes are assumed, and even when different input data sets are used. This is because PDTI adequately solves the problems of Green's function uncertainty and fault geometry uncertainty. At present, there is no formulation that allows stable analysis with a highly flexible seismic source process model using near-field strong ground motion records, so PDTI cannot be applied to near-field strong ground motion records.

References:

-X. Xiong, B. Shan, Y.M. Zhou, S.J. Wei, Y.D. Li, R.J. Wang, and Y. Zeng, 2017, *Coulomb stress transfer and accumulation on the Sagaing Fault, Myanmar, over the past 110 years and its implications for seismic hazards*, GRL 10.1002/2017GL072770.

-Yu Wang, Kerry Sieh, Soe Thura Tun, Kuang-Yin Lai, and Than Myint, 2914, *Active tectonics and earthquake potential of the Myanmar region*, JGR 10.1002/2013JBO10762.