Dear Editor,

Thank you very much for the reviewing process. The reviewers' comments are very pertinent, containing model choices, implications, method limitations and so on. We revised some of the figures, added more explanations and references in the text. We reply to each of the comments in the following. We hope that our reply is sufficiently clear. We thank you again for your editorial work.

Best regards,

Victor Cruz-Atienza and Hideo Aochi

Reviewer B:

1. *Line 145: right-hand → left-hand*

   ANSWER: Corrected.

2. *Page 8: Many letters "s" are not printed properly.*

   ANSWER: We checked the PDF.

3. *Figure 2: Mahmoud et al. 2012 should be Mahmoud et al. 2013*

   ANSWER: Corrected.

4. *Equation 3: is not explained. And the numerator and denominator are inverted.*

   ANSWER Thank you. Corrected.

5. *Line 458: A typo error "#6".*

   ANSWER : It is removed.

6. *Line 472: The optimal directions mentioned here should be the direction of the fault rather than the direction of principal stress loading.*

   ANSWER: Thank you. Corrected.

7. *Line 474-475: In my opinion, we cannot say the optimal direction is close to the results of Aktug and Kilicoglu (2005) in the southern segment. They are still quite different.*

   ANSWER : it is true. We modified the expression as follows.

   "This variation is similar to the great-circle tangential direction deduced from the model of Aktuğ and Kiliçoğlu (2005), but there remains a significant difference in the middle by a difference of more than 45° (Figure 2)." (Page 26)

8. *Figure 8: The assumptions on the stress field are unrecognizable.*

   ANSWER: It is the conversion problem from the original files to a PDF file. We checked the generated PDF file.

9. *Line 511: three zones → four zones*

   ANSWER: We modified the expression as follows.

“To slow down the rupture process, we further adapt our source model by subdividing the stress field into four zones every 80 km length. Two of them are set with lower stress levels and the southernmost zone redirecting the stress is assumed to arrest the rupture.” (Page 28)

10. *Figure 9: The maximum resolvable frequency in the FDM simulation is 3.2 Hz, so I suggest to apply a low pass or band pass filter for waveform comparisons.*

    ANSWER: We made Figure 9 after low pass filter until 1 Hz.

11. *Line 558-559: The authors explain the relatively low stress zone as the effect of shadow part of the splay fault, this is reasonable. But can the author quantitatively calculate the value of stress reduction in the shadow area based on some existing models and compare it with their low stress zone settings?*

    ANSWER: We did not estimate the stress reduction due to the off-plane rupture. It is a statement derived from our parametric study. We modified the expression as follows.

    “Comparing to Figure 8b, a relatively low stress zone around X = -20 km was indeed necessary to keep sub-shear rupture propagation in this area, which corresponds to the shadow part of the splay fault where the Mw7.8 earthquake started. “ (Page 34-35)

12. *Line 670-673: In the dynamic model of the Kahramanmaraş earthquake, the non-planar fault geometry and the heterogeneity of initial stress orientation and magnitude are considered. So how can we determine whether the fault geometry or the heterogeneous stress field plays a more preponderant role during this earthquake?*

    ANSWER: Thank you for that comment, which is very pertinent. The rupture process is the result of a juxtaposition of these and other physical factors, which are indeed interrelated. The answer partly depends on how well one would like to describe the detail process of earthquake rupture. We added a general comment.

    “Our modelling framework represents the first attempt to integrate the non-planar fault geometry, a heterogeneous regional stress field and a three-dimensional variation of Dc over the source, all three factors derived from geological, tectonic and seismological observations. The resulting rupture process is thus a juxtaposition of these factors, which are in fact interrelated.“(L 737)

13. *The author concluded the seven-zone model is the optimal model. However, the waveform comparison results of this model were not shown in the paper.*

    ANSWER: We added the new Figure 12 with seismograms from the seven-zone model without and with horizontal variation of Dc.

About Supplementary materials:

1. *Figure S5: It seems that the normal stress almost not vary with the fault strike. Is this because of the color scale? I think the northern fault segment should exhibit high normal stress.*

    ANSWER: We changed the color scale of normal stress in Figure S5. The normal stress varies from zero (at surface) to a few hundred MPa (at depth). The high normal stress segment is represented by the fact that the highest color scale appears at a shallower depth.

Reviewer C:

Model Approach:

The modeling follows a two-step approach: first, a dynamic rupture scenario is generated for a homogeneous half-space using the Boundary Integral Equation Method (BIEM), and then the slip rate time history is applied as a boundary condition in a 3D Finite Difference (FD) model with a layered structure. If my understanding is correct, this approach disregards the influence of the layered structure on the rupture characteristics. Wouldn't the Green's function used in the BIEM vary accordingly? This could, in turn, affect the slip rate history.

If the authors consider this issue to be of limited importance, I suggest justifying this assumption for the benefit of the readership. While BIEM is efficient and well-suited for parametric studies, this major limitation should be acknowledged. Particularly with the availability of open-source numerical models and efficient methods that incorporate these complexities and benefit from high-performance computing, such as SeisSol, SPECFEM, and DRDG, it would be beneficial to discuss these options.

ANSWER: Thank you for this very proper comment. The BIEM is certainly a first order approximation, focusing on the fault geometry, neglecting other factors. We added clearly this issue.

"In general, a suite of BIEM is useful to consider the scaling issue and fracture problem, as meshing/remeshing is flexible (e.g. Ando et al., 2004; Ide and Aochi, 2005) and to provide different rupture scenarios in different fault systems emphasizing on the role of fault geometry (e.g. Aochi and Ulrich, 2015; Ando et al., 2018), while other volumetric methods such as finite element, spectral element and discontinuous Galerkin methods allow the simulations in a more complex medium (e.g. Tago et al., 2012; Jia et al., 2023; Gabriel et al., 2023). Our interest is focused on the ground motions in the vicinity of the fault, at very short distances, where pulse-like waveforms, mostly determined by the close rupture, dominate and where therefore the assumption of a homogeneous medium is a reasonable first approximation." (P. 16, section 2.4)

Novelty of Conclusions:

The authors present a comprehensive scenario for the earthquake dynamics; however, when compared to other recent papers (Jia et al., 2023; Ding et al., 2023; Gabriel et al., 2023; Wang et al., 2023), I did not observe a novel conclusion that clearly distinguishes this work. While it is true that the cited papers focus on the mechanisms allowing rupture transfer from the initial splay fault to the East Anatolian Fault (EAF), most of these studies were comprehensive in their modeling. They employed similar fault geometry and used inversion methods to constrain their models. The need for a non-uniform stress field to reproduce rupture characteristics has been reported by Wang et al. (2023) and, more recently, by Chen et al. (2024). Similarly, the transient supershear propagation observed in the current study has also been documented by Wang et al. (2023) and Gabriel et al. (2023). While the estimation of frictional *behavior Dc from near-fault observations is valuable and innovative, it has been reported earlier (Yao and Yang 2023) using the same methodology for a very similar set of near-fault stations.*

ANSWER: The primary objective of this research is not merely to describe the already documented phenomenology of the earthquake, but to gain further insight into the underlying dynamics. The procedures that we employed have not previously been utilized by other researchers in the manner that we did. It is worth noting the estimation of Dc from acceleration records and the energy balance of the rupture. As elucidated in the manuscript, the Dc estimates proposed by Ding et al. (2023) and He et al. (2024) are predominantly influenced by factors external to the stress breakdown process. In other words, the procedures they followed yielded values that are not constrained by the source itself in the vicinity of the seismic stations. The rigorous analysis of Dc resolution that we developed (lacking in the aforementioned works) provides sufficient confidence to identify even a lateral change of this constitutive property of the source, a result never previously observed in an earthquake. Such lateral variation provides an explanation for why the radiation efficiency is maximal at the location where the maximum PGAs were recorded. Furthermore, the comparison of the dynamic models with the kinematic solution of Delouis et al. (2023), which is arguably the most constrained model in the literature, is also absent from the papers cited by the reviewer. The congruence with our preferred model in terms of the lateral variation of the rupture velocity and the congruence with the observed PGAs indicate that the procedure for the construction of the initial stress state and for establishing the constitutive source parameters are close to the reality of the earthquake. For these reasons, we consider that the research provides innovative results of great scientific interest.

We cited Chen et al. (2024) and Yao and Yang (2023) in the paper. Chen et al. (2024) provides a finite source inversion. Yao and Yang (2023) estimated Dc'', however they integrated the acceleration from the P-wave arrival. This integration method they used is very likely to lead a large error in displacement because they integrate over 20s seconds beside ignoring the effect of the free surface, which makes the factor 2 introduced by Fukuyama and Mikumo inappropriate, as demonstrated by Cruz-Atienza et al. (2009). Our integration starts only just before the arrival of shock wave with a robust baseline correction (rupture front), and we think this is more reliable for the estimation of Dc''. We found only Yao and Yang (2023) investigation as a preprint on Eartharxiv (https://eartharxiv.org/repository/view/5316/) and not published in a peer reviewed journal.

Related to this issue, here there are two extracts from the main text:

"As numerous seismological/ geodetic/geological studies have already shown (e.g. Melgar et al., 2023; Jia et al. 2023; Barbot et al., 2023, Delouis et al., 2023, Chen et al., 2024), these large earthquakes are related to multiple fault segments with major surface ruptures along the East Anatolian fault zone." (Page 5)

"Following a similar strategy, Ding et al. (2023) and Yao and Yang (2023) estimated D_c from seismic records that led to higher estimates than ours. Unlike our approach, were the rupture-front shock wave was isolated prior to the double integration of acceleration via a baseline correction method, Ding et al. (2023) and Yao and Yang (2023) determined D_c'' from long displacement time series that suffer from the well-known baseline drift inherent in inertial accelerometers."(Page 42)

Ground Motion Predictions:

In line L433, the authors describe the ground motion predictions of the preferred model as "remarkable." However, a comparison between the observations and predictions in Figure 9 reveals significant discrepancies, particularly in regions expected to experience supershear rupture, such as stations 2712, 3137, and 3144. In contrast, Gabriel et al. (2023) produced ground motion predictions that are in much better agreement with the observations. It would be valuable for the readers if the authors investigated the reasons behind these discrepancies and provided a discussion in the text.

ANSWER: Please note that 2-zone model (Fig. 9a) is not presented as a good model. Figure 9 is now plotted both for X and Y components, corresponding to fault-parallel and fault-perpendicular components. The figure is intended to show how the 4-zone model is much better at explaining the arrival times of the shock wave associated with the rupture front. Our final 7-zone models are now shown in new Figure 12. We agree that Gabriel et al. (2023) succeeded a better waveform modelling of ground motion in a large area. As shown in Gabriel et al. (2023) (Their Figure 4a, filtered between 0.01 and 1 Hz) and by our simulations, near-fault waveforms are characterized by a dominant pulse accompanied by the rupture front. We are interested rather in the arrival times of rupture front along the fault line, which is a difficult task in our fault geometry that is more complex than theirs. We explained more in the text.

“Previously, Jia et al. (2023) and Gabriel et al. (2023) were also able to reproduce the general characteristics of near-field ground motions from dynamic rupture simulations, including five common stations as our simulations (4624, 4616, 2712, 2718 and 3139). The near fault ground motions are mainly characterized by a dominant pulse corresponding to the passage of the rupture front in all simulations. It is interesting to note that their and our models are well timed at station 2712, while the simulations are generally too early at station 2718. In our simulation it is difficult to reproduce well the fault parallel component at the positions between X = -100 km and X=-150 km, although it is still good in the fault perpendicular component. Thus, our simulation could capture the macroscopic rupture propagation well, but some local conditions could be further improved, as suggested by Kidoh et al. (2024), who propose a short pulse generation on this segment near station 3139.”(Page 29)

Specific comments:

L115-141: “To better understand …” The discussion of the Izmit earthquake does not clearly establish its relevance to the Kahramanmaraş earthquake. Please consider clarifying.

ANSWER: The situation of the two earthquakes is very similar in the adjacent fault boundaries and the motivation of our study is to apply the previously obtained knowledge on this new earthquake in order to study more in detail the lateral variation of the rupture process. We have modified the text as follows :

“From this perspective, the Mw 7.8 Kahramanmaraş earthquake, which has a similar faulting mechanism and was recorded by at least 11 near-fault accelerometers (i.e. within 3 km of the source), represents a globally unique opportunity to apply the similar analysis technique from the 1999 Izmit earthquake to study the dynamics of the rupture process and its impact on strong motions at the local scale, taking into account the non-planar fault geometry.” (Page 6)

L155-L160: Some of the letters are not showing in my reader. This could be an issue from my end, but I thought I would mention it nonetheless.

ANSWER: It should be conversion issue. We checked the generated PDF.

Table 1: Consider replacing the term "Material Rigidity (G)" with "Shear Modulus" for clarity, as the latter is more quantitative and accessible to a broader audience.

ANSWER: We have modified accordingly.

L285: How is the depth variation of Dc computed/constrained in the model? I understand that at a specific depth >4 km Dc is estimated based on S.31 but what is the motivation of 2m S3.1 I am not an expert on the topic but based on my reading of Fukuyama, Mikumo, Olsen BSSA 2023 (paragraph 7) the proposed method for estimating Dc requires, as an essential condition, the smoothness of the source process. Given the complex nature of the rupture propagation and the stress field shown in the model and kinematic inversions can this method be reliably applied?

ANSWER: The reviewer can refer to the investigations of Cruz-Atienza et al. (2009) and Cruz-Atienza and Olsen (2010) to understand the conditions under which it is possible to estimate Dc directly from near-fault strong motions. In this case, Dc = 80 cm corresponds to a value close to the average of the estimates at the 11 available seismic stations. This value is constant between 4 and 12 km depth in all simulations except for the case where we explore the lateral variation of Dc suggested by the data. Above 4 km, as mentioned in the manuscript, Dc grows linearly up to 2 m at the surface to account for a dissipative fault zone in the shallow crust that stabilizes rupture propagation, as suggested in several previous studies of rupture dynamics (e.g. Olsen et al., 2009; Aochi and Ulrich, 2015). We have clarified this on Page 15 : **"Above 12 km depth, based on the observations discussed in Section 3, we initially assume = 80 cm up to 4 km depth, where Dc begins to grow linearly to 2 m at the surface (z = 0 km)".** As seen in snapshot, the cohesive zone size is quite large (several km), so our setting would be good enough.

The contours in Figure 8, representing slip rate and stress state, are difficult to distinguish. Improving the visual quality would enhance clarity for the readers. Furthermore, from my end, the visuals of the stress state are not clear.

ANSWER: The contours shown are merely indicative and correspond to the rupture front (i.e. rupture times). In this figure, no stress state is reported except in the upper panels. On the other hand, there was indeed a problem with the letters at the top, which was due to format conversion. We checked on the generated PDF file.

L311-315: The nucleation of the rupture is modeled as an abrupt initiation with a sudden circular crack of a 3 km radius. According to Galis et al. (2014), achieving such a sudden nucleation would require substantial overstressing, potentially affecting the accuracy of estimated slip rates. Including more details on the specifics of nucleation in the appendix would be beneficial.

ANSWER: Our comprehensive parametric analysis, summarized in Figure 6, allowed us to identify the conditions under which the earthquake can propagate across the entire fault through geometric irregularities and prestress variations including the stress kickoff condition of the earthquake nucleation. In other words, the relationship proposed by Galis et al. would be insufficient to guarantee complete rupture propagation.

L315-316: The authors use a velocity structure reported in S1, which was also utilized in Aochi et al. 2017 for Marama region along the NAF. Given that the paper aims to correlate the findings with Delouis et al. 2023, I found it intriguing that the authors didn't use the same set of velocity structures (Güvercin et al. (2022)).

ANSWER: In the figure below, we show ground motion predictions from the 1D structure used by Delouis et al. (2023), close to Güvercin et al. (2022), which includes a relatively soft layer (Vsmin=2.18 km/s) like our model (c). However, such model generates too strong surface waves that we do not see in the data. Note that "the high-frequency cutoff is set to 0.3 Hz" in Delouis et al. (2023). We use quite different frequency ranges.

The tests in Güvercin et al. (2022) were carried out for moderate earthquakes at depth for focal mechanism inversion, while the 2023 Kahramanmaraş earthquake has a shallow rupture reaching the ground surface. Gürvercin et al. (2022) compares a simpler 1D model (homogenous until 20 km depth, see below figure), which is good enough in practice (Supplementary figures in Güvercin et al., 2022). Gürvercin et al. (2022)'s model can be good at regional scale wave propagation, but we discuss near-fault ground motion. The choice of the model is explained in detail. The structure is expected to be closer to the one used in the rupture simulation such that the homogeneous elastic medium hypothesis is good enough.

"In the following discussions, we adopt model (b) of Figure S1, hereafter called as our reference model, which has a slight shallow velocity variation and is close enough to the homogeneous model. This model improves the peak values at the near-source stations compared to the homogeneous model, and inhibits the generation of large surface waves behind the rupture front passage at frequencies 0.2-0.4 Hz."(Page 17, section 2.4)

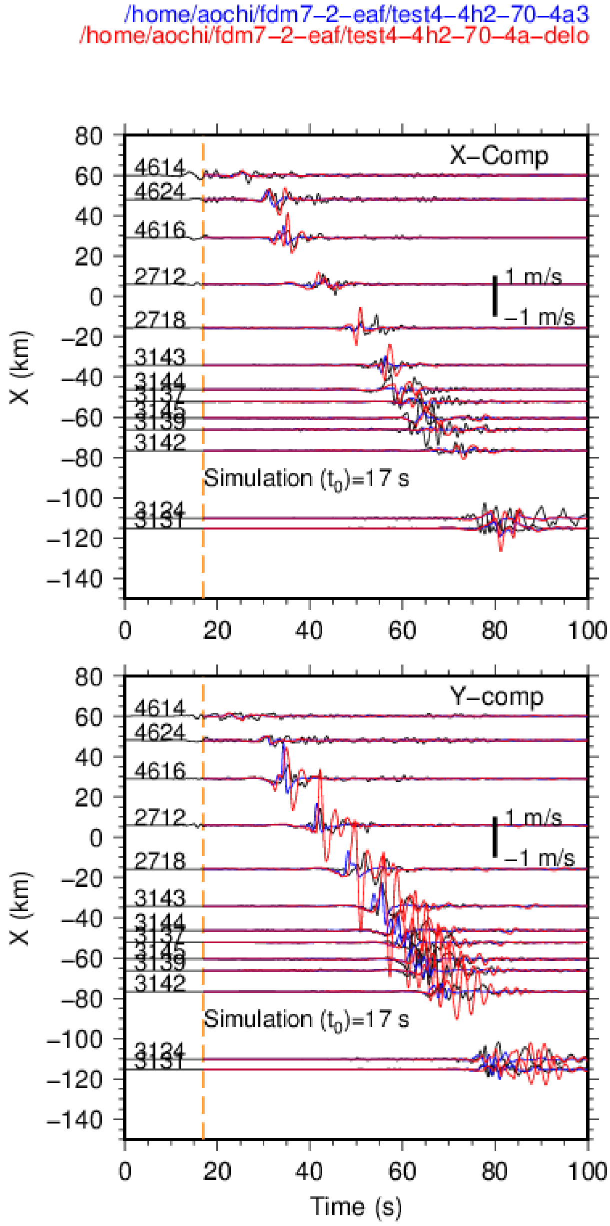

Figure A1 : Comparison of synthetic seismograms (red: 1D model from Delouis et al., 2023; blue: our reference 1D model) and the observations in black.

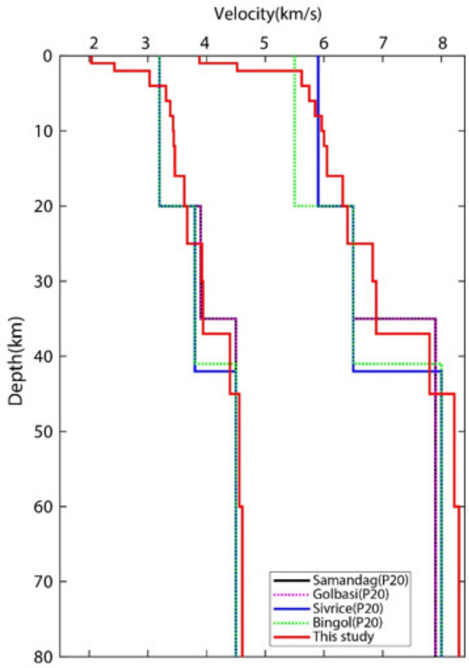

Figure S4: The comparison of the 1D velocity model obtained in this study and the 1D velocity models from Pousse-Beltran et al., (2020). Red thick lines represent the P and S velocities obtained in this study.

Figure A2 : Figure S4 from Gürvecin et al. (2022).

Furthermore, the author's choice of velocity structure ignores variation in velocity structure above 3km depth. The justification is that the inclusion of such variations leads to the resonance that is absent in records. Given the sequential approach, could this observation in S2 regarding resonance be attributed to rupture forcibly propagating faster than the shear wave speed within this media? Could perhaps damage accumulation or pre-existing damage account for this? Variation of frictional behavior between velocity strengthening and velocity weakening?

> **ANSWER:** This is an interesting remark. Indeed, a subshear rupture in our source model can lead to a radiation of supershear conical waves in the shallow soft-layer velocity structure. That is, the resonance in the wave propagation through the soft layer model can be enhanced by the trapping of shear and surface conical waves. For this reason, given the proximity of the stations to the source, it is most appropriate to consider a propagation model close to the homogeneous structure imposed on the source, to ensure as far as possible that the radiation and propagation of the waves are consistent and able to explain the observed waveforms, which are devoid of large resonant surface waves.

L384: assuming an average rupture speed of 3.5 km/s. Is there a justification for this choice? The shear wave speed in the proposed model is 3.464 km/s and the average rupture speed as mentioned by the author L515 and Delouis et al. 2023 is subshear shouldn't a sub-Rayleigh estimate be more appropriate?

> **ANSWER:** The reviewer's suggestion is pertinent. However, since the earthquake experienced some supershear transients, our choice of 3.5 km/s (i.e., the shear wave speed) is a midpoint between the Rayleigh velocity and sqrt(2)*beta (supershear regime). This average value was

used to estimate the possible range of variation of the cohesion zone considering a 40% uncertainty in the rupture velocity. This uncertainty (blue bars in Figure 5c) is large enough to encompass all the expected rupture velocities in this earthquake.

Responses to 2^{nd} round of review

We thank the reviewers for their work. We have improved the manuscript as much as possible according to their comments, and we think that our explanations and points are clear. We have modified Figures 9 and 12, and added Figure S9 (comparison of the 3-component seismograms). We added a discussion of the possible effects of heterogeneous medium on the rupture process and seismic radiation.

Answers to Reviewers (Line numbers correspond to the revised manuscript with tracked change).

Reviewer A:

Figure 9 and Figure 12: For waveform comparison, the observations and simulated ground motions should be filtered to the same frequency range. The authors only applied a low-pass filter to the simulated data. Therefore, we cannot evaluate which model is better.

Answer: Thank you for this comment. There does indeed appear to be a different spectral content between the observed and synthetic signals. However, they are both filtered in the same way. The numerical simulation seems smoother, because it is remarked by a single pulse related to the rupture front propagation, while the observation contains the later phases coming from the complexity of the medium.

Reviewer B:

I agree with the authors that ground motion at very short distances is dominated by the rupture process. However, I disagree with the assertion regarding the assumption of a homogeneous medium. I believe that rheology plays a significant role in altering rupture dynamics (please see the two papers below). For example, to capture this effect in an earlier section, the authors rightfully mentioned the need to introduce a larger shallow Dc to slow down the rupture. However, that is not the only possible scenario that could emerge due to the heterogeneity of material properties. It is thus important to demonstrate the reasonableness of this approximation. The impact of this approximation seems to manifest later in the ground motion response, requiring a structure as close to homogeneous elastic media as possible.

Abdelmeguid, M., Elbanna, A., & Rosakis, A. (2025). Ground motion characteristics of subshear and supershear ruptures in the presence of sediment layers. Geophysical Journal International, 240(2), 967-987.

Nico Schliwa, Alice-Agnes Gabriel, Yehuda Ben-Zion (2025). Shallow fault zone structure affects rupture dynamics and ground motions of the 2019 Ridgecrest sequence to regional distances. https://doi.org/10.31223/X5N412

One potential way to demonstrate that this assumption is reasonable is to consider the final realization (recommended model) and utilize any of the available volumetric methods to explore its impact on the dynamics of the rupture.

Answer: We thank the reviewer for these new papers, that we have now referred to in the manuscript. Of course, the medium properties determine both the rupture process and wave propagation, but the velocity reduction in the fault zone or shallow structure is not the topic in our paper. The impact of the shallow layer is very clear in the wave propagation even from the kinematic point of view, as we show with the amplification of ground motions. The influence on the rupture process depends strongly on the condition we impose. In our configuration, in contrast to the study by Abdelmequid, seismic radiation from the shallow part is moderated by the mechanical fault consideration of a large Dc, which damps the radiation of high frequencies as expected from the shallow rheology of the crust. But of course, nobody knows exactly how the rheology for this specific event is near the source, so different model assumptions are possible. If Dc is uniform in a homogeneous medium, then we also observe that the shallow rupture propagation becomes too dominant. Using a volume method to make other simulations goes beyond the scope of the present study. However, one might wonder in that case how much uncertain medium properties determine the constitutive friction parameters of the source leading to biased models. Therefore, we believe that our main conclusions are sufficiently robust compared to other approaches to this problem.

We added: “In this paper, we have emphasized the importance of Dc variations ….. “(L.795-806).

In L535, please clarify what metrics other than arrival time you deem remarkable in the ground motion prediction.

Answer: The comparison between (a) and (b) in Figure 9 highlights a major difference in the arrival times of the energetic seismic pulse. In panel (a), the model pulse arrives much earlier because rupture velocity is faster than the shear-wave speed. The amplitude itself does not change a lot between both cases, as it reflects the slip function on the fault, principally governed by the given frictional function. Therefore, what is remarkable in the comparison are both the arrival times and the overall amplitudes. We have clarified this in the text. We added “The amplitude itself does not change significantly between the two models.” (L. 538-539).

In Figure 9, if the shock arrival is the main parameter of interest, can you please add more quantitative analysis regarding the arrival time with appropriate error bars to indicate the *rupture front arrival? It is not directly clear in this figure, particularly for regions with overlapping stations. It would help readers identify how much faster/slower the rupture is relative to the observations. In Abdelmeguid et al. 2023, we also looked at arrival times for some of those stations (Supplementary Figure 6).*

Answer: Peaking the arrival times is tricky. Variations appear for different models. The arrival times from data were quite well identified in Section 3 (Figure 4 and Figure S3 in supplementary material). The reviewer can appreciate a clear comparison of the rupture travel time in Figure 10. We added the reference in Introduction.

In Figure 12, the ground motions are hard to discern in this layout. Stations 3143-3142 are all overlapping, so it is hard to see the agreement in ground motion referenced in L545.

Answer: We added Figure S9 to show the three components-seismograms.

Can you discuss what an early arrival at 2718 implies in the context of a supershear segment between 2712 and 2718?

Answer: This is already shown and discussed in Figure 10, in which super-shear happened at around station 2712 both in Delouis's model and our simulation.

If Figure 12 now represents the final (preferred) model, can you please move the discussion of the ground motion prediction to that figure?

Answer: We added the waveforms in Figure S9. We added: "(Figures 12, 13, and S9 in Supplementary material). Along strike variations of Dc estimated directly from the rupture front shock wave improved the model predictions both in terms of the expected locations of the supershear rupture transients and the spatial distribution of the observed PGVs." (L.715-718).

From my earlier review, I still believe that the manuscript would benefit from a discussion on what sets this set of kinematic inversion + dynamic rupture apart versus, say, Jia et al. or other similar literature.

Answer: We have already addressed this issue. Hopefully the reviewer will take the necessary distance to see the contributions of this work in comparison with others. We believe this is clear and justified in the manuscript. Besides introducing what we believe are the most rigorous Dc estimates in the literature for this event (which had a relevant implication in our final model), we brought a large discussion around the kinematic model of Delouis et al. (2023), among other original results.

Specific comments:

The visual errors highlighted in the previous revision are still present throughout this version of the manuscript. This includes the top panel in Figure 8.

L285-L290 The authors alternate between Dc and D_c, same for X and X

Answer: The critical slip distance D_c is originally a parameter in Equation, and it has become a common term such as b-value. We corrected in the text to use uniformly Dc as a term. We used different drawing tools, letting the characters slightly different. In particular, index becomes too small and we also prefer to put a small character. It is conventionally understandable.

Figure 2: The legend does not agree with the caption.

Answer: It is Mahmound et al. (2013). Corrected.

Figure 4: Please add labels to the x, and y axis in addition to the units.

Answer : We believe the figure is clear as it is.

L351 – L355 "Only ground motion …" Can you please clarify this sentence?

Answer: The reviewer may refer to Cruz-Atienza et al. (2009) to understand the sentence, which is clear.

Can the authors please provide how you compute the width of the process zone Lc within the text?

Answer: This is already stated in the text: "Lc values (given by Vr times Tc) incorporating both uncertainties vary between 4 and 12 km along most of the fault…" (L414-L417).

In the ground motion figure, can the authors please include a legend within the figure in addition to the caption?

Answer: We indicated now that the observation is in black and the synthetics in orange. (Figures 9 and 12).

In Figure 10, can you please clarify what Vs was used? If it is a scalar quantity based on average shear wave speed, it would be beneficial for interested readers to see how the rupture speed varies with local wave speed, as it has implications on the ground motion.

Answer: Vs = 3.464 km/s. We have indicated this in the figure legend.