Point by point responses to Reviewers of the Manuscript: "The extremely shallow M_W 4.9 2019 Le Teil earthquake, France: Main ground motion features and comparison with ground motion models"

Dear Pablo,

We are pleased to submit the revised version of our manuscript entitled "The extremely shallow M_W 4.9 2019 Le Teil earthquake, France: Main ground motion features and comparison with ground motion models".

We are grateful to the two reviewers for their comments and suggestions, which have helped us improve the manuscript. We have addressed each of their points in detail and provided point-by-point responses below. Our answers are in black, while the comments of each reviewer are in blue and are listed and numbered sequentially.

Here is a summary of the main changes:

1. **Clarification of the paper's structure:** We revised the introduction to better outline the objectives and structure of the paper. We have also modified several section titles to clarify the purpose of each part, particularly by emphasizing the "INTERPRETATION" section. We believe these changes will help guide the reader more effectively through the manuscript.

2. **Reorganization of content:** We have simplified the new "INTERPRETATION AND DISCUSSION" section to focus on two main aspects: (1) the systematic underestimation of low-frequency ground motions, which is linked to the generation of Rayleigh waves, and (2) the spatial variations of high-frequency ground motions, attributed to propagation effects. As a result, the clustering analysis has been moved to the section dealing with residual analysis, and the geological description has been simplified.

3. **Additional explanations:** We addressed specific reviewer comments by clarifying the use of epicentral distance, giving a better explanation of the frequency band selection, discussing the relevance of V_{S30} in the context of site effects, and explaining our choice of HN over HH signals. adding information about the clustering method. We also added information about the clustering method and expanded the discussion on the attenuation of surface waves relative to body waves.

We believe these revisions have significantly improved the clarity, coherence, and overall quality of the manuscript. We hope that they adequately address the reviewers' concerns and make the paper more accessible to readers.

We are also attaching the revised manuscript and electronic supplements, with all modifications highlighted in red.

Yours sincerely,

Aurore Laurendeau and Coauthors

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1 Reviewer K: Fabian Bonilla

The reviewer made comments directly on the PDF document. We have compiled these comments here, grouping together those that could be addressed together. To help keep track of the comments, I have indicated for each comment, the line number and the excerpt from the article's original version or the section concerned by the reviewer's comment, before repeating the reviewer's comment.

Main comment on the paper's structure

The reviewer made several comments that we considered related to the organization of the document's structure, so we grouped and addressed them together.

First, there is a series of comments in which the reviewer questions us about the physical explanations related to the observations described in the text. In the structure of the paper, all the questions he raised have been addressed in the chapter originally titled "DISCUSSION". Their comments are the following:

1.1 L 230: "The analysis of the spatial distribution of these intensity measures reveals distinct behaviours between the regions around the epicentre, with a clear separation between the northwest and southeast." What is the source of this spatial variability? The geology (site effects), the source or the path?

1.2 Section "4.1. Spectral Accelerations vs Distance". How do you explain the differences between the observed and the GMM results? Do you think that the radiated wave field is particular for this event that is very shallow? Please try to give some physics that will help for future events and also to improve the GMM models for cases like this.

1.3 Section "4.1. Spectral Accelerations vs Distance". The differences at distances larger than 100 km, are they produced by path or site effects? Another question, do you see the focal mechanism at high frequencies with the data?

1.4 L379: "GMMs underestimate low-frequency ground". Is there any relation with the fact that surface waves are strongly present in the data? (Figure 3)

1.5 Section "4.2.2. Within-Event Residuals ». 1. Do you think that this event produced ground motion that cannot be predicted at distances larger than 100 km? If so, why?

Second, there is a series of comments in which the reviewer suggested reorganizing the structure of the paper. Their comments are the following:

1.6 Section "4.2.2. Within-Event Residuals ». 2. I suggest to organize better the structure of the paper. First, please mention what you want to do, and how. You show sections of GMM results too fast. Why to use the residuals to explain the results? This would help to have a better reading of this work. 3. Finally, please try to give some physical explanations of why you have such results if possible.

1.7 L.576: "To summarize, a large part of the observed spatial variability at low frequencies is attributed to source effects, specifically the radiation pattern of Rayleigh waves generated by this shallow earthquake (1-2 km), which may dominate the signal." Nice, but to find the explanation now is almost late. I think that the paper can be shortened and put all elements together if possible. If you want to keep the current structure, please warn the reader when the full discussion and explanation will be found.

We noted that our structure was not clear to the reviewer. Therefore, we have revised both the statement of our objectives, and the outline provided in the introduction. In particular, we now explicitly refer to the names of the different sections in the outline. We also clarified in the introduction why we use

GMMs: they allow us to correct the data for the average path effects observed in other earthquakes, enabling us to analyse them within the same reference frame.

We also modified the section titles to make the objective of each part clearer, specifically by using the word "INTERPRETATION" in the section where the interpretations are presented. We have decided to maintain the overall structure of the paper, meaning that we kept the interpretations in a dedicated section. During the writing process, we experimented with various ways of presenting the results and interpretations, and this solution appeared to be the most suitable. In the sections preceding the “5. INTERPRETATION AND DISCUSSION” section, we now make it clearer that interpretation is deferred to that section (see for example Lines 280–283).

Additionally, in the “5. INTERPRETATION AND DISCUSSION” section, we now focus more directly on the two most important aspects of the paper: (1) the low-frequency observations, which are related to Rayleigh waves, and (2) the high-frequency observations, which are linked to propagation effects. As a result, we simplified the text by moving the clustering discussion into the residual analysis, and we integrated the geological discussions with the high-frequency analysis. We also relocated the discussion on stress drop and directivity, which were previously introduced at the beginning of the section, into the relevant high-frequency discussion, as these topics are more directly connected.

Overall, we made an effort to guide the reader more clearly through the structure of the paper and to improve the readability and coherence of the “5. INTERPRETATION AND DISCUSSION” section.

Other comments:

1.8 L.133. “(Résif-RLBP – FR) and the accelerometric (Résif-RAP – RA) networks”. Please check whether to use the current name of EPOS FR instead of RESIF. This is in case people would like to search for these data.

Thank you for this remark. We have replaced "Résif" with "Epos-France" throughout the text and updated the citations for the RLBP and RAP networks as well.

1.9 L.171. “(Perron *et al.*, 2018)”. You do not need to cite anyone. This is widely known.

The citation has been removed as suggested.

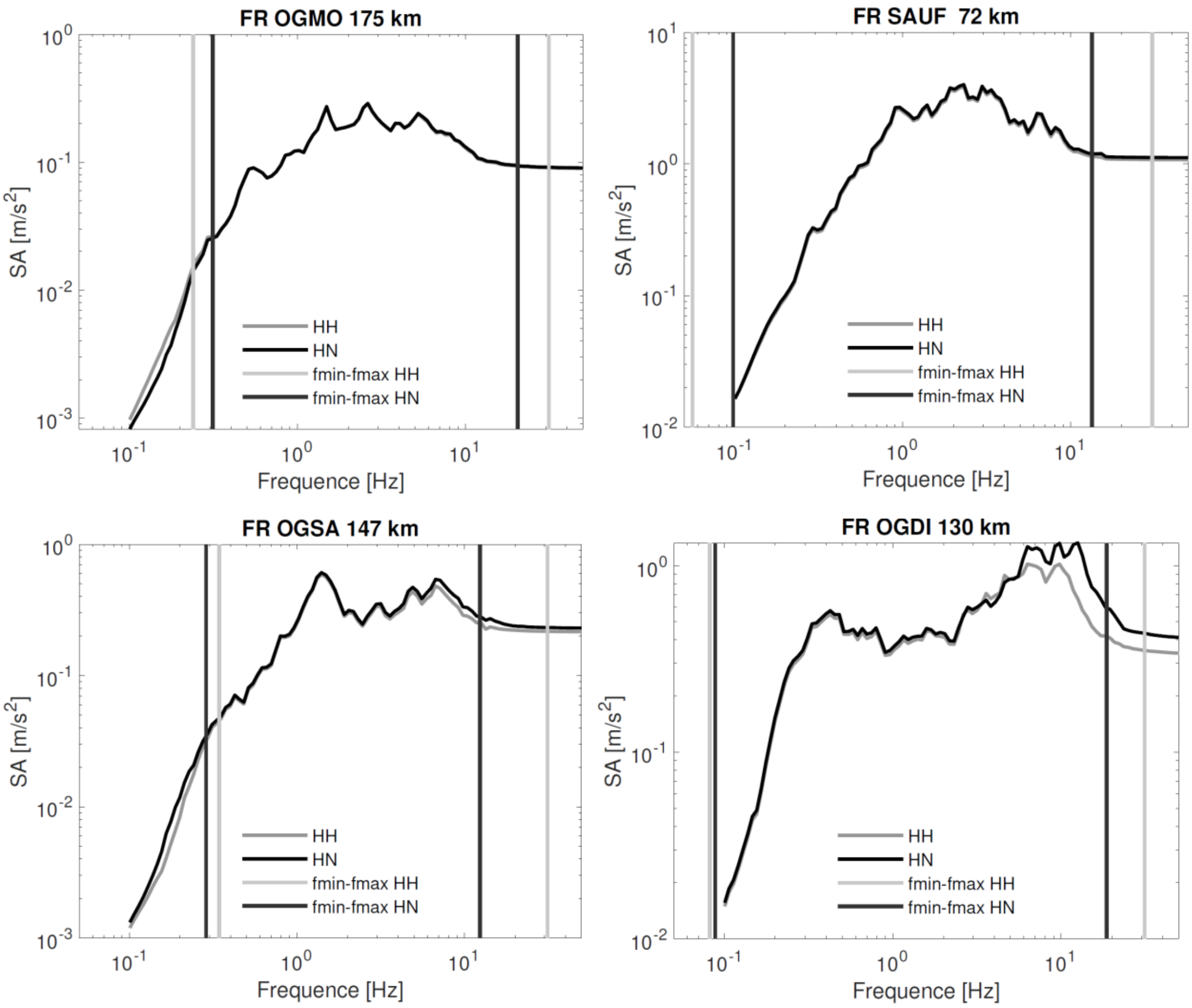
1.10 L. 175. “5 Hz”. Why 5 Hz is the maximum usable frequency? Is this for all stations or just to have a common value.

Indeed, in the text, we state: "Finally, only waveforms with a usable frequency band between 0.5 and 5 Hz are selected." Contrary to what seems to have been understood by both reviewers, this does not mean that we have no information beyond 5 Hz. This frequency band corresponds to the minimum valid frequency range that we wish to consider. It also matches the frequencies at which the figures in the paper are presented. The upper limit of 5 Hz further aligns with the maximum frequency of the simulations we intended to compare with. Since the text was unclear to both reviewers, we have modified it accordingly (L. 177 to 185).

1.11 L. 176. “the accelerogram records (HN) are preferred”. Why? If the distance is too large, the seismometer may have a better signal. Please explain better this choice.

Indeed, the signal-to-noise ratio is generally better for HH signals, particularly at long distances. For the majority of stations in our database, there is very little difference between the HH and HN response spectra, except that HH signals have a broader usable frequency range at high frequencies (see example below of OGMO and SAUF). However, broadband sensors can be buried, sometimes at depths of several tens of meters, which may lead to a deamplification of the seismic signal compared to surface recordings (see example below of OGSA and OGDI). This is discussed in the paper of Hollender et al. (2020). For this reason, we have systematically preferred HN signals over HH signals.

To further explain this choice in the paper, we have added elements to the text (L. 185 to 190).



1.12 L. 201. “a *VS30* value greater than 800 m/s”, While Vs30 is a valuable parameter, it is also important if soft shallow layers are also present. Site response can be controlled by them. Any comments?

Indeed, V_{S30} is a useful parameter for quantifying site effects. To address this comment, as well as that of the second reviewer, we propose first to move this section on V_{S30} to the part discussing GMMs, since we specifically analyse this parameter because it is required by the GMMs. Additionally, we have added further information on V_{S30} and its limitations, as suggested by both reviewers (see L. 313 to 324).

1.13 Section “3.2. Focus on Three Representative Stations”. Please discuss that PGV is in fact produced by surface waves, not body waves according to Figure 3. This might produce some problems to interpret GMM that use body wave data only (although this is not always clear).

The acceleration response spectra predicted by the GMMs are computed using the full waveform, with no distinction made between different wave types. GMMs are often based on body waves, particularly because the databases used to derive these models contain few, if any, shallow earthquakes like the Le Teil earthquake. For the three representative stations, PGV is predominantly carried by surface waves on the radial and transverse components, where they are observed. This is a direct consequence of the earthquake's shallow depth, which favours surface wave generation.

However, as mentioned earlier, we have chosen to discuss these points in the "5. INTERPRETATION AND DISCUSSION" section. In this section 3.2, we only made the observation that the PGV is carried by the surface waves at the OCOL and IRVG stations. Then, in the section "5.1. Underestimation of Low-Frequency Ground Motions", we notably discussed that at low frequencies, the GMMs underestimate the observations due to the presence of Rayleigh waves in the observations.

1.14 Figure 6. I am lost here, you mention that these sites have a Vs30 > 1400 m/s, and you use 800 m/s. Please explain this.

Indeed, since the V_{S30} value is not defined for most stations, we have chosen to fix the value at 800 m/s for all stations across all GMMs. For the stations with a measured V_{S30} value, a test was performed using the measured V_{S30} values in the models; this result is presented in the supplementary material (Figure S.10). To help the reader, we have added the prediction for the measured V_{S30} value in Figure 6 for the three stations. We have indicated this in the legend as "The purple dashed lines indicate, for reference, the prediction obtained when the measured V_{S30} value at each station (True V_{S30}) is used in the GMM, along with the corresponding epsilon value.". Additionally, we have included this reminder in the text to avoid confusion for the reader: "As a reminder, the Ko20 prediction is made using a fixed V_{S30} value of 800 m/s for all stations." (L. 386-387).

1.15 L 478. Does the numbering correspond to the clusters in Figure 8? If so, please change Figure 8 to begin at 1 and not at 0.

The numbering here does not correspond to the cluster but rather to the description of the main geological structures in the area. This part was ultimately removed in order to simplify the text. The link with geology is now addressed more directly in Section 5.2, which focuses on high-frequency observations.

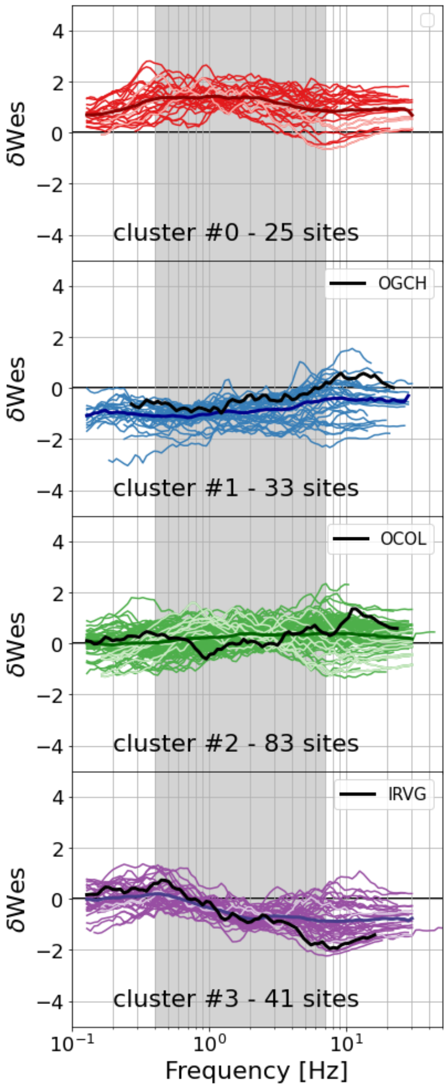
1.16 L497. "Firstly, cluster #0 (in red) groups". To make things easier, please match the formations mentioned before with the clusters. It is confusing as it is now.

As already mentioned, we no longer draw a direct connection between clustering and geology, since this link is not consistent across the full frequency range but is only relevant at high frequencies. We now address this connection in the introduction to the 5.2 section, about high frequency.

1.17 end of section "5.1. Spatial Distribution of Seismic Ground Motions: Clustering Analysis and Comparison with Geology". Please explain the sites that are at different geological settings and still is one cluster (SNCF stations?)

To help understand why sites are grouped in the same cluster despite being in different geological settings, we have provided further explanation of the clustering method in the text, which may have certain limitations. Specifically, k-means clustering is a data-driven method that assigns stations to clusters based on proximity to centroids, but stations near cluster boundaries may share characteristics with multiple clusters. This can make the final cluster assignments sensitive to small variations in the data or initial conditions, leading to potential ambiguities in the classification of sites with similar characteristics but different geological settings. This method thus provides a simplified categorisation that highlights dominant trends, rather than strict, sharply defined groupings (see L. 460-469).

Moreover, in the revised structure of the manuscript, the link between geology and clustering is now addressed differently. For cluster #0, we still explain it as potentially related to site effects. The Rhône Valley, where some of the stations in this cluster are located, is known for causing site effects. In Figure 8, as noted by the reviewer, we observe both stations from cluster #0 and stations from cluster #2 (in green) near the epicenter, particularly stations from the SNCF network. Below, we have reproduced the right-hand panel of Figure 8, highlighting the δW_{es} curves for all SNCF stations in light colors. These stations may be located at the edge of the canyon, and thus, may not be affected by the site effects discussed in the paper. The clustering was performed across the entire frequency range, and the SNCF stations in cluster #2 appear to be particularly influenced by high-frequency attenuation. Additionally, the amplification at these stations seems to occur closer to 1 Hz rather than at lower frequencies, and with a smaller amplitude. These two factors may explain why these stations in the Rhône Valley do not fall into cluster #0. Given the complexity of this explanation, we decided not to include it in the paper.



1.18 Section "5.3. Spatial Variations of High-Frequency Ground Motions". But the attenuation is different for body and surface waves, any comments on that in this section?

Indeed, surface waves attenuate more slowly than body waves, which can have a significant impact on long-distance seismic motions at low frequencies. We have now added a discussion on this point in the "5.1. Underestimation of Low-Frequency Ground Motions" section, which addresses surface waves (see L. 544-549).

1.19 L. 636. End of section “5.3.1. Local Geological Structure Southeast of Le Teil”. A transition phrase to next section?

We have added a transition sentence as requested (see L. 645-646).

1.20 Section “5.3.2. Effect of a Shallow Velocity Inversion on the Ground Motion Decay from Simulations”. What method did you use to perform the simulation?

Greens functions are computed using the AXITRA computer Package (Cotton and Coutant, 1997) and convolved with a source time functions, as described in the manuscript. These elements have been added to the revised version of the manuscript (see L. 689-691).

1.21 Conclusion . L780. What about sources from geothermal areas?

We have added a mention about induced earthquakes in the last sentence with a reference (see L. 792-794).

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2 Reviewer L:

The authors present a well-written and structured paper on France's very shallow 2019 earthquake. The paper presents some interesting data compilation and discussions, and thus, it should be published. However, the analysis seems less rigorous than expected for a reputed journal, and, in this reviewer's opinion, some statements and data need further justification before they can be published.

Major comments:

2.1 The authors indicate that the vertical ground motion exceeded 1g based on the calibrated numerical simulations, yet there is no detail on the calibration of those simulations. Is there any station that recorded even near gravity? Please indicate the maximum recorded vertical acceleration.

As stated in this sentence: "Based on numerical simulations calibrated with in-situ observations of displaced objects, Causse et al. (2021) demonstrated that vertical ground acceleration locally exceeded gravity." The study showing that ground motion could have exceeded gravity is that of Causse et al. (2021). Details on the calibration of the simulations are provided in that study.

While Causse et al. (2021) demonstrated through numerical simulations that vertical ground acceleration locally exceeded gravity, the highest recorded vertical acceleration observed at CAI55 (SNCF network), located 10 km from the rupture fault, was 39 cm/s². This value is significantly lower than the simulated values exceeding 1g. As for the horizontal components, the highest recorded PGA was 76 cm/s², also at CAI55. It is important to note that ground motion attenuates rapidly with distance from the fault, particularly for this type of shallow earthquake.

In the text, we chose not to include this information to keep the description concise. It is also worth noting that we provide an Excel table with the seismic motion indicators for the three components. Additionally, we have added this information at the beginning of the paragraph for clarity (L. 213-214).

2.2 The authors use epicentral distance as the unique metric for attenuation; dispite the low magnitude and shallowness of the earthquake, the studied event is not a point source nor at zero depth.

Can the authors associate the event with any known geologic structure? If there aren't any better distance metrics for your case you need to justify it.

The epicentral distance is used in Figures 1, 2, 7 to provide a visual reference for readers regarding the station locations. The fault has a maximum width of 5 km. Thus, the difference between the epicentral distance and Rjb is not significant. Then, to make the comparison with the GMMs, we took into account the rupture area of the Le Teil earthquake to calculate the site-to-fault distances as mentioned in the paper in L.308 to 313: “The input parameters required for these GMMs, or to compute them, are presented in Table 1. Site-to-fault distances (R_{JB}, R_{RUP}, and Rx) are computed by approximating the fault as a rectangular plane. The location, strike and length of the fault plane on the surface are determined based on the trace of the fault observed from InSAR analysis (Ritz et al., 2020) (approximately 5 km on the InSAR image). The fault width is estimated from the slip inversion results (Cornou et al., 2021; Vallage et al., 2021). "

To avoid confusion, we have removed "epicentral" from this sentence: L. 218-220 "As expected, PGA values decrease with increasing ~~epicentral~~ distance, although spatial variations are evident based on major geological domains (Figure 1 and Figure 2)."

2.3 The authors state: "*only waveforms with a usable frequency band between 0.5 and 5 Hz are selected.*" This must be a mistake. How are you studying PGA if you only use signals up to 5 Hz? Strong motion PGA comes from frequencies from 50 Hz and up. Also, why would you limit your ground motions to higher than 0.5 Hz? you then show some Fourier Spectra with records lower than 0.5 Hz. This data section needs rewritting in a more precise way.

This question is the same as the one from the first reviewer. Indeed, the text did not clearly explain what we had done. I invite you to read the response to the first reviewer to the question #1.10.

2.4 The authors associate Vs30 with site effects, which is not wrong, but site effects are so much more than Vs30 values. See the work of Castellaro, Rodriguez-Maerk, Bard, and so many other great researchers who have pointed out pitfalls of Vs30, proposed better alternatives, and study-specific cases (like yours) using site effects derived only from ground motion records (e.g., eHVSR p-wave arrival times, etc.) providing even better site effects prediction than Vs30 values. Of course, if and when shear-wave velocities are available, you can do much more than compute the variable to insert in a GMM, especially given the focus of your paper.

Similarly, this question has been addressed in the response to Reviewer K. I invite you to read the response to the first reviewer to the question #1.12.

2.5 One of the reasons the authors gave for the mismatch between the observations and predictions, as well as the spatial variability, is attenuation. One should wonder why not assessing the kappa values for those stations, or using published data for at least some of the 198 stations used. The computation is not hard to do.

Several previous studies, including those based on data from the Epos-France network, have highlighted regional differences in attenuation within our study area. In particular, Mayor et al. (2018) demonstrated spatial variations in high-frequency absorption. These works are discussed in the introduction of Section 5.2, 'Spatial Variations of High-Frequency Ground Motions' (see L. 594-610), and we consider it sufficient to compare our observations with the findings of such studies.

The kappa (κ) parameter, originally introduced by Anderson and Hough (1984), characterizes the high-frequency decay of ground motion spectra. It is typically estimated from the linear decay of the acceleration Fourier amplitude spectrum beyond the corner frequency, and ideally requires a large number of recordings across a wide range of epicentral distances and events to reliably separate source, path, and site effects. In particular, determining the site-specific component κ_0 involves modelling the distance-dependent decay κ(r) from multiple events and correcting for path effects.

However, our study area is characterized by low-to-moderate seismicity, and acquiring a sufficiently large dataset of recordings across a broad distance range for multiple events is not straightforward. In this context, estimating κ_0 reliably would be challenging. We therefore prefer to limit ourselves to comparisons with existing studies, which already provide insights into attenuation characteristics in the region, and consider that a robust kappa analysis would require a dedicated investigation beyond the scope of the present study.

Minor comments:

2.6 Abstract: "Athigh frequencies" seems to be wrong; instead, it should be "At high."

There is indeed a space in my Word version; it must be the PDF rendering that makes it look like there isn't one.

2.7 homogenize reference format: Either *Ritz et al., 2020 or Ritz et al. 2020*

Indeed, there were differences, which we have now harmonized.

2.8 Repeated statement (don't): "Our study suggests that a better understanding of the ground motions generated by such moderate extremely shallow earthquakes is important to improve seismic hazard assessment."

We did not understand this comment. We did not find any repetition.

2.9 The use of Signal-to-Noise Ratio >3 was not proposed by Perron et al. (2018). I'm unsure who used it first, but it dates from several years before (e.g., Bastias & Montalva, 2016)

The citation was an example of a study using a signal-to-noise ratio greater than 3, and it was indeed missing a clarification that it was just an example. As suggested by the first reviewer in question #1.9, we removed the reference, as selecting a signal-to-noise ratio greater than 3 is a widely recognized approach within the community.

2.10 For clarity, please include La Rouvière's fault in figures 1 and 2.

The Rouvière Fault is a small-scale fault that ruptured over 5 km during the Le Teil earthquake. Figures 1 and 2 are at too large a scale for the fault to be visible.

2.11 The authors state that the epistemic variability of GMMs is lower than the observed variability; this would be highly dependent on the selection of GMMs, but in any case I would ask you to clarify which is the observed variability you're referring to.

We followed the reviewer's suggestion and removed this paragraph. In fact, it was a brief summary of the previous points, which did not seem essential. Based on the reviewer’s comment, it appears that this paragraph may have led to more confusion than clarification for readers.

2.12 Line 635: Do you mean the velocity structure of the Le Teil earthquake AREA? Because the EQ cannot have a velocity inversion. Also, the technical word preferred now is velocity REVERSAL, because despite many authors using it in the past, an inversion means 1/Vs values which does not happen. This just a minor comment.

Indeed, we have added "area" as suggested by the reviewer.

We then chose to retain the term “inversion” instead of the term “reversal” suggested by the reviewer, as it is widely used in publications such as for example Castellaro S, Mulargia F (2009) The effect of velocity inversions on H/V. Pure Appl Geophys 166(4):567–59, and so we think it's easy for readers to understand.

2.13 Figure 12: The caption refers to Figure 1, which seems wrong because there is no Vs profile there.

Thank you for the remark. Indeed, it was not Figure 1, but Figure 10 the good reference.

Point by point responses to Reviewers of the Manuscript: “The extremely shallow M_W 4.9 2019 Le Teil earthquake, France: Main ground motion features and comparison with ground motion models”

Dear Pablo,

We are pleased to submit the second revised version of our manuscript entitled "The extremely shallow M_W 4.9 2019 Le Teil earthquake, France: Main ground motion features and comparison with ground motion models”.

We thank the reviewers again for their thoughtful comments and constructive suggestions. We have carefully considered all points raised during this second round of review and revised the manuscript and supplementary materials accordingly.

In particular, we have added new analyses and clarifications to address the concerns raised by Reviewer B regarding the treatment of site effects. These additions include sensitivity tests, further residual analyses, and expanded discussion of the methodological choices and their implications, which satisfy reviewer concern about reinforcing the robustness of our main conclusions.

Additionally, we took the opportunity to add references to other extremely shallow moderate-magnitude earthquakes, notably in northeastern South America (see lines 96–97). This addition complements the previously cited cases in China and Australia, and highlights the global relevance of studying ground motions associated with this particular type of shallow earthquake.

The revised manuscript and electronic supplements are attached, with all changes highlighted in red. A detailed point-by-point response to the reviewers’ comments is provided below. Our answers are in black, while the comments of each reviewer are in blue.

Yours sincerely,

Aurore Laurendeau and Coauthors

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1 Reviewer A: Fabian Bonilla

1. In the data and methods, I suggest to homogeneize the description of the arrays used in this study. This means, please put the number of stations of each array.

We thank the reviewer for this helpful comment. We have revised the section describing the datasets to harmonize the presentation of all arrays used in this study. Specifically, we now indicate the number of stations used from each network or deployment (FR, RA, IRSN/EDF, SNCF, XT, Z3) in the main text. We also clarified the spatial context of the different datasets (e.g., epicentral vs. far-field stations) and ensured consistent formatting and terminology throughout the paragraph (see revised version in the “2. DATA SELECTION AND PROCESSING” section, lines 139-173).

2. When discussing the stress drop for this event, the low value is due to the shallow depth? This is just a curiosity, you do not need to edit anything else.

The estimated stress drop for the Le Teil earthquake (1.0 MPa; Causse et al., 2021) lies within the typical range reported for intraplate events, though slightly on the lower end. The relatively shallow depth (~1 km) may indeed contribute to a lower stress drop, as shallow earthquakes tend to have lower effective normal stress and thus lower fault strength, which limits their capacity to release energy (e.g., Huang et al., 2017, Stress drops of induced and tectonic earthquakes in the central United States are indistinguishable, Sci. Adv. 2017; 3 : e1700772). However, the value remains broadly consistent with global averages (e.g., Courboulex et al., 2016).

Given that this was a minor point raised out of curiosity, and that the existing discussion already provides the essential context, we have not modified the manuscript.

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2 Reviewer B:

The authors compare observations of the recordings of "The extremely shallow MW 4.9 2019 Le Teil earthquake, France" with ground motion models. However, even though the models depend strongly on site effects, the authors choose to ignore them and assume an 800 m/s value for the Vs30 parameter, then discuss that the differences are associated with issues like attenuation. Both the answer to this reviewer's observation and the discussion are insufficient in my view.

Site terms are an integral part of all GMM's and hence, assuming a constant value will cause a residual analysis to be wrong.

We understand the reviewer's concern about the simplification of site effects in our residual analysis, and we would like to clarify our methodology and its justification.

We fully acknowledge that assigning a uniform Vs30 value of 800 m/s to all stations is a strong simplification. For this reason, **in the previous version of the paper, we included several sensitivity analyses in the electronic supplements** (also to avoid making the main text too heavy) to demonstrate that we evaluated the implications of this assumption, which we summarize below:

- We presented Figure S5, which illustrates how residuals may be affected by assuming Vs30 = 800 m/s when the actual site condition corresponds instead to a soft site (e.g., Vs30 = 300 m/s) or a hard-rock site (e.g., Vs30 = 2000 m/s). As the reviewer points out, site effects represented through the Vs30 parameter play an important role in GMMs; however the impact varies depending on the GMM used. For example, the CY14 GMM shows relatively moderate sensitivity to Vs30 at high frequencies, even for these extreme cases. In contrast, it exhibits stronger effects at low frequencies for soft sites. The Ko20 GMM, in particular, is generally less sensitive to variations in Vs30 across the frequency range. In the specific case of this model, site terms are introduced only in a secondary step, and the base model coefficients are derived without site-specific information.
  Despite the different sensitivity of GMMs to Vs30, all tested GMMs consistently reproduce the main trends: between-event residuals at low frequencies highlight the influence of surface waves (Figure 5), while within-event residuals reveal systematic contrasts between the northwest and southeast, with negative residuals in the southeast (see Figure 7 and Figures S7 to S10).
- We presented in Figure S11 the within-event residuals obtained **using the 32 stations with measured Vs30 values** and the Ko20 GMM. **The within-event residual patterns obtained are very similar to those obtained using the uniform Vs30 = 800 m/s assumption.** For

instance, at 0.5 Hz, within-event residuals are near-zero or positive in the NW–SE direction and negative in the northeast which is consistent with the expected Rayleigh wave radiation pattern. At 5 Hz, positive residuals are observed in the northwest and negative in the southeast.

- We have strengthened our results in the paper by presenting detailed analysis for three reference rock stations, all with measured Vs30 values above 1400 m/s, located northwest (OCOL), northeast (OGCH), and southeast (IRVG) of the rupture (figure 2). Notably, we directly compared observed and predicted spectra using both measured and fixed Vs30 values (Figure 6). While some differences are observed at high frequencies, the 'S-shape' residual pattern at IRVG (southeast) and the Rayleigh wave-related low-frequency bump at OCOL and IRVG persist regardless of Vs30 input.
- We performed a clustering analysis of residuals (Figure 8), which revealed coherent regional patterns despite the choice of a fixed Vs30 value. Stations surrounding the three rock sites located at different azimuths group into consistent clusters. Additionally, a fourth cluster (Cluster #0) emerges, corresponding to stations with well-known low-frequency site amplification, particularly in sedimentary basins such as Grenoble, Nice, and Annecy, as well as in the Rhône Valley. Assigning a uniform Vs30 value of 800 m/s to all sites facilitated the identification of these stations as a distinct cluster.
- Finally, to address the reviewer's concerns regarding the potential influence of the Vs30 = 800 m/s assumption on attenuation, we examined additional mechanisms that may contribute to the observed high-frequency attenuation in the southeast. We relied on existing literature, we conducted a geological analysis of the region, we highlighted the presence of a low-velocity layer beneath the hypocentre extending southeastward, and we then performed simple numerical simulations to assess how the presence of this layer could affect the seismic ground motion. These simulations showed that the presence of this layer leads to lower high-frequency spectral amplitudes compared to a case without such a layer, which is consistent with our observations of the Le Teil earthquake ground motion records.

Given these precautions and supporting analyses, we are confident in our interpretation of the main ground motion features of the Le Teil event. That said, we have been careful not to overstate our conclusions in the manuscript. We consistently adopted conditional language, including expressions such as "may be" and "could," to reflect the underlying uncertainties.

To respond to the reviewer's comment, particularly the concerns regarding the influence of simplified site assumptions on the residual analysis, we have also made several changes to the main text, carried out additional sensitivity tests and added them to the electronic supplements:

- Lines 321 to 334: **we added a paragraph explaining our choice to present results using a uniform Vs30 assumption of 800 m/s, and in particular, why we preferred this approach over relying on Vs30 values inferred from topographic slope (Wald & Allen, 2007), a method commonly used in GMM development to infer Vs30** (e.g., Lanzano et al., 2019). Prior to writing the paper, we tested the correlation between measured Vs30 values (from geophysical profiles) and Vs30 values derived from topographic slope for the sites in mainland France for which the two values are available (Hollender et al., 2018). We found a very poor correlation, particularly for hard-rock sites (e.g., limestone plateaus with measured Vs30 ~2000 m/s and slope-based Vs30~400 m/s). This analysis is presented in the supplementary material (Figure S4). We also referred to the work of Lemoine et al. (2012), who cautioned against slope proxies for local studies in geological contexts, such as limestone plateaus which is the context of Le Teil region.
- We carried out an additional residual analysis using all stations, assigning measured Vs30 values to the 32 characterised sites and slope-based Vs30 values to the remaining sites. The between-event residual results are presented in Figure S6 and are referenced in the main text (lines 433), while within-event residuals are shown in Figure S12 and referenced in lines 464-468. Using these

new Vs30 values, observed patterns remain consistent with those in Figure 7 using Vs30=800 m/s for all sites.

- Lines 446 to 449: we commented the epsilon values for the three rock sites obtained using Vs30 = 800 m/s and the measured Vs30 values. While the results obtained with different Vs30 values leads to some differences, mainly at high frequencies, the main spectral features, including the 'S-shape' at IRVG and the low high-frequency content, remain consistent.
- Lines 507 to 509: we reformulated the text to improve clarity and we added this sentence : "Assigning a uniform Vs30 value of 800 m/s to all sites facilitated the identification of sites with strong low-frequency amplification as a distinct cluster."
- Lines 517 to 519: we included a reminder that our conclusions are based on a simplified Vs30 assumption, and emphasized that the dominant trends remain robust despite this simplification.
- Lines 778 to 822 (Conclusions): we explicitly expanded on the topic of site effects, underlining the limitations of our analysis related to the choice of a fixed Vs30 = 800 m/s or the use of proxy values, and highlighted the importance of continuing site characterization efforts.

This choice of assigning a uniform Vs30 value of 800 m/s was not made by default, but was carefully evaluated against alternatives and deemed the most consistent approach given the limitations of proxy-based estimates in the regional geological context.

In conclusion, while we acknowledge the limitations inherent to our simplified site condition assumptions, we believe the combination of sensitivity tests, residual analyses, clustering results, and physical interpretations provides a consistent and robust understanding of the main ground motion features observed during the Le Teil earthquake. Throughout the manuscript, we have carefully qualified our interpretations and taken additional steps to clarify the methodological choices and their implications in the revised version.

Point by point responses to Reviewers of the Manuscript: "The extremely shallow M_W 4.9 2019 Le Teil earthquake, France: Main ground motion features and comparison with ground motion models"

Dear Pablo,

We are pleased to submit the third revised version of our manuscript entitled "The extremely shallow M_W 4.9 2019 Le Teil earthquake, France: Main ground motion features and comparison with ground motion models" following your final editorial feedback.

We thank you and the reviewers for the opportunity to make these last adjustments and for the constructive comments regarding the limitations associated with assuming a constant V_{S30} value. In response, we have made the following last modifications:

- **Lines 334–335:** Added the sentence: *"These tests indicate that the various assumptions for the V_{S30} values have no impact on the main conclusions of our study, as explained subsequently."*
- **Lines 511–514:** Added a paragraph clarifying that Section 5 focuses only on Clusters 1 to 3: *"Cluster #0 therefore contains stations strongly affected by local site conditions. These sites are spatially isolated and will not be further discussed in the section "INTERPRETATION AND DISCUSSION". A more detailed understanding of the residuals at these sites would require additional V_{S30} measurements, which is beyond the scope of the present study."*
- **Lines 515–522:** Simplified this part for clarity.
- **Lines 803–807:** Reorganized the conclusion to include a paragraph dedicated to site effects and their limitations: *"Finally, these results still present some limitations, as site conditions are not well constrained and are represented using non-site-specific V_{S30} values (e.g., 800 m/s) in the GMMs. However, several sensitivity tests were performed to support these findings."*

We believe these revisions address the reviewer's and your comments while maintaining the robustness of our main conclusions. The revised manuscript and electronic supplements are attached, with all changes highlighted in red.

Yours sincerely,

Aurore Laurendeau and Coauthors

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1 Reviewer A: Fabian Bonilla

Dear authors,

Thanks for taking into account the reviews. The paper is shorter, more focused and clearer. I have just minor comments:

1. In the data and methods, I suggest to homogeneize the description of the arrays used in this study. This means, please put the number of stations of each array.

2. When discussing the stress drop for this event, the low value is due to the shallow depth? This is just a curiosity, you do not need to edit anything else.

Sincerely,

Fabian Bonilla

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2 Reviewer B

The authors have softened the statements regarding their conclusions. The paper has an evident limitation in assuming that site effects are constant (e.g., a fixed value of Vs30 for all sites), and hence some of the observations made could be shadowed by this simplification. However, the paper provides an initial analysis of this event which could help other researchers interested in this relevant topic improve the analysis using the authors results.

Note that high frequencies would not be affected by Vs30, and hence I propose minor revisions, so that the editor can judge if the language used is appropriate, and the authors can make minor adjustments.