Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

7th Jul 20

Dear Dr Causse,

Please allow me to apologise for the delay in sending a decision on your manuscript titled "Exceptional ground motion for moderate-sized shallow earthquake". It has now been seen by 2 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to
• address these criticisms(that is, either to incorporate the suggestions or provide a compelling argument why the point made by the reviewer is not valid, or relevant to the editorial threshold as outlined below)
AND
• meet our editorial thresholds as outlined below,

then we would be happy to look at a substantially revised manuscript. However, please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

In the following, we list our main editorial concerns:

****Provide compelling evidence for strong ground motions resulting from the shallow, moderate Le Teil earthquake****

****Clearly define the background, motivations and implications of the work and distinguish between your own results and those of other studies****

****Explain your simulations and data analysis techniques in sufficient detail as to enable clear understanding and reproducibility of the results****

In addition, please supply point-by-point responses to all the reviewers' comments.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

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

[link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

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

Best regards,

Joe Aslin

Associate Editor,
Communications Earth & Environment
https://www.nature.com/commsenv/
Twitter: @CommsEarth

EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements

For your information, you can find some guidance regarding format requirements summarized on the following checklist:
Communications Earth & Environment formatting checklist

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This manuscript presents a commendably comprehensive analysis of the ground motions generated by the Mw 4.9 Le Teil earthquake. The analysis is sound, and the results are expected to be of interest to seismologists interested in ground motions and seismic hazard. The presentation, unfortunately, does not do justice to the results, and the problems go beyond sentence-level awkwardness expected from authors who are writing in a non-native language. Especially for a broad journal, the manuscript needs to more clearly lay out the background, motivation, and implications of the study. A reader who is not a specialist will get the impression that near-field recordings of moderately large earthquakes are few in number. The authors need to start by explaining clearly that moderately large earthquakes overwhelmingly occur at seismogenic depths of 6+ km, so recordings at < 6km nearest-fault distances are rare because earthquakes more shallow than 6 km are rare. By that same token, however, the results of this study are immediately relevant only for the small fraction of moderate earthquakes that are especially shallow. It is fair to say that the results provide an indication of the range of possible effects from moderate earthquakes, but is a Mw 4.9 at < 1 km depth a 1-in-a-million event? (Is there any indication it may have been induced?)
Additional minor issues:
1) The first sentence of the abstract is supported by a single reference provided in the introduction. Better to say that earthquakes with magnitudes between 4 and 5.5 can contribute significantly to hazard in low-to-moderate seismicity areas.
2) The plural of millennium is millennia
3) Referencing is thin in many places (induced earthquakes?)
4) The opening of the third paragraph of the introduction is garbled, and needs to be rewritten.
5) Lines 40-42 overstate the case. There have been plenty of detailed investigations of moderate earthquakes, going back to work in the 1990s by Art Frankel and others. The issue again is that very few moderate earthquakes are shallow.
6) Writing is awkward throughout "providing quantitative footprints of the ground acceleration," "Besides, to constrain the rupture duration," etc.
7) On a more substantive note, the authors do not appear to consider the possible effects of attenuation on their corner frequency estimate. Is this a fair assuming, assuming a reasonable Q value and the range of source-receiver distances?
8) (lines 68-74) The reader is left wondering where this information comes from.
9) The authors really need to reference more seminal studies, not just papers published in the last few years -- for example lines 83-84. (Jack Boatwright was doing work on these topics going back to the 1990s).
10) (lines 102). This is fundamentally a modeling study, so while the assumptions and method are sound, the authors need to be careful to note that results are predictions, not observations. (Comparison with field observations is good.)
11) The Discussion and Conclusion section is not well-written -- the 2nd paragraph discusses issues that were not even mentioned in the paper. The first sentence of the first paragraph also comes "out of the blue" in a paper that has focused on ground motions. Again, the authors need to do a better job of explaining the implications of the study.

Reviewer #2 (Remarks to the Author):

Review for “EXCEPTIONAL GROUND MOTION FOR MODERATE-SIZED SHALLOW EARTHQUAKE” by Causse et al.

This paper is focused on the November 11, 2019 Mw 4.9 Le Teil event (France) that caused an unexpected level of damage.
The authors, after having defined/fixed the main six kinematic characteristics of the event, generate a suites of ground acceleration simulations on a virtual array of stations to providing quantitative footprints of the ground acceleration.

The content of the paper and the conclusions/results (ground motion simulations, inferred 1D velocity model, observations of tossed rocks….) are interesting but I think they are not well exposed. The reader can get confused between the results obtained from this work, the results inferred by other works (e.g. slip model derived from the Insar data) and the main goals pursued by the authors. The paper is also not well balanced. Very few words to introduce the work and to explain the simulations while there are a lot of sentences to explain displaced objects. In particular, the authors didn’t stress on their results showing the inferred variability of PGA and PGV values coming from the 2000 simulations. For example, why they didn’t show the variability of PGV and PGA values at the power plants?
For all these reasons, I’m sorry, I suggest a major revision.

Here my list of comments, minor and major.
1) Title. I suggest to add the indefinite article “a” (…for a moderate-sized shallow earthquake)
2) Instead of writing “exceeding 5 m/s2” I would write “exceeding half g”.
3) Check the English for sentence lines 37-40.
4) In general, references are missing. In particular, please add references for the localization of the event at 1km (line 46).
5) In the introduction the authors are missing to introduce the main characteristics of the studied event. Which is the kinematic (trust fault mechanism) and the geometry of the fault? Which are the main collected data for this event? Moreover, introducing their work the authors do not explain what they want to do with their numerical simulations.
6) Figure 1 has not the colorbar to define the slip values projected on map.
7) The authors didn’t introduce the array of virtual stations, in terms of number of stations, relative location respect to the fault. Moreover mentioning Figure 1 the authors do not underline that the temporary stations were installed after the event.
8) Line 68-69: From the text I cannot understand if the author inverted Insar data to obtain the slip distribution of if they infer the slip from other works (again there are not references). Moreover which are the info of Insar data (which satellite, which time interval of the fringes)…
9) How the author fix the geometry of the fault? Is it inferred from previous works? Is it inferred from InSar inversions? Is it inferred from moment tensor solution? Which is the reference of the moment tensor?
10) Line 73-74 are too qualitative.

11) I don’t understand why the authors modify the position of the hypocenter? They should say something about the range of allowed variability.
12) Which is the range of variability allowed for the other kinematic parameters?
13) Because the authors collect the kinematic information from different sources (Insar data for slip distribution, rupture duration from Brune model, rupture velocity from average values of fault dimension, slip velocity acceleration from the literature) did the authors try to fit distant stations with their 2000 models?
14) How the authors can convince the reader that their 2000models for frequency up to 5Hz can be reasonable?
15) The computation of the 1D velocity model using aftershocks is showed in between the generation and explanation of the 2000 models. The reader cannot appreciate this result.
16) The inferred 1D model is a result of the paper. How different is this model respect to previous velocity models of this area? The authors should release the table of Vp and Vs values.
17) Figure S3 panel c: what do the different lines colors represent? They are six while the models are three.
18) How does the PGA distributions change for different frequency ranges?
19) I don’t understand: “The decay of ground acceleration with distance to rupture is fast, except along the EW and SN directions, due to directivity effects”. Are directivity effects along EW and SN directions? Are the authors sure?
20) First sentence of the discussion “The occurrence of the 2019 Mw4.9 Le Teil event shows that friction and stress conditions in the very shallow earth crust (<1 km) can be favorable to moderate-sized earthquakes.” Starting the discussion with this sentence seems to suggest that the rupture was ready to break without human induced activities. Even if the authors have not discussed the cause of the event, they can say that, regardless of whether this event may have been induced, the shallow stress field was still favorable for a potential rupture.
21) I don’t understand the units of measurements for equation (1). Mu is the rigidity and it is in Pa.
22) Figure 4 has not been fully explained. The reader can get lost. The right bottom panel has not a clear explanation.
23) Probably the caption in Figure S4 is wrong. These plots represent PGV not PGA.

Manuscript COMMSENV-20-0191-T: Exceptional ground motion for a moderate-sized superficial earthquake

We are very grateful to the two reviewers for their thorough reviews and comments, which have led to significant changes in the paper presentation. Below we answer to all the reviewer questions and remarks.

Answer to reviewer #1:

Main comment:

A reader who is not a specialist will get the impression that near-field recordings of moderately large earthquakes are few in number. The authors need to start by explaining clearly that moderately large earthquakes overwhelmingly occur at seismogenic depths of 6+ km, so recordings at < 6km nearest-fault distances are rare because earthquakes more shallow than 6 km are rare. By that same token, however, the results of this study are immediately relevant only for the small fraction of moderate earthquakes that are especially shallow. It is fair to say that the results provide an indication of the range of possible effects from moderate earthquakes, but is a Mw 4.9 at < 1 km depth a 1-in-a-million event? (Is there any indication it may have been induced?)

We thank the reviewer for this major comment, which helped us to better clarify the background and implications of our results. We agree with the reviewer that our findings cannot be fairly presented as a major contribution to moderate-sized earthquakes studies in overall, since its unusual shallowness makes it an exceptional event. Consequently, we have reshaped the introduction, so that the article is more focused on the Le teil event itself, the implications for local seismic hazard and the possible high impact of superficial moderate earthquakes in stable continental regions. The main modifications of the introduction are the following:

- *We have clarified that the number of near-fault earthquakes recordings (including all earthquakes, not only shallow or moderate-sized) is still too limited to properly predict near-source features of ground motion (e.g. Chiou et a. 2008), and can then be complemented with numerical ground motion predictions (e.g. Goulet et al. 2015).*

- *We have removed the section regarding the magnitude dependence of exceptional ground motions. The explanations regarding theoretical background of high-frequency seismic radiation have been moved to section "Source analysis and rupture modeling". The references to source modeling of moderate earthquakes have been removed.*

- *Next we have explained that most moderate-sized earthquakes in stable continental regions occur at seismogenic depth larger than about 5 km (Klose & Seeber 2007 propose), making the Le Teil event a rare event.*

- *Finally, we have mentioned that the Le Teil event may have been triggered by rock mass extraction from a large quarry located just above the rupture area, which could explain the shallow location. (see Delouis et al. 2019 and De Novellis et al. 2020 for*

> *ongoing research on the possible causes of Le Teil event; Pomeroy et al. 1976, Seeber et al. 1998, Wiejacz & Rudzinski (2010) and Emanov et al. (2014) for other reverse-style faulting moderate earthquakes; Foulger et al. 2018 for a global review of characteristics of human induced earthquakes, included shallow depth; Grigoli et al. 2018 for the Pohang earthquake, Korea).*

In addition, we have removed the first two sentences of the abstract, which correspond to the very general statement that moderate-sized earthquakes dominate seismic hazard in moderate seismicity areas, and that near-fault recording of such earthquakes are extremely rare. The abstract then starts by introducing the Le Teil event and pointing out its exceptional shallowness.

Finally, the conclusion has also been strongly reshaped.

Additional minor issues:

1) The first sentence of the abstract is supported by a single reference provided in the introduction. Better to say that earthquakes with magnitudes between 4 and 5.5 can contribute significantly to hazard in low-to-moderate seismicity areas.

We have removed the first two sentences of the abstract (see answer to main comment).

2) The plural of millennium is millennia

Corrected.

3) Referencing is thin in many places (induced earthquakes?)

The list of references has been broaden, including in particular references to induced earthquakes, near-fault ground motion prediction (empirical models, numerical simulations, directivity effects) and references to three recently published studies about the Le Teil earthquake (Cornou et al. 2020 is a data paper regarding the postseismic repsonse to the earthquake; Ritz et al. 2020 analyze the surface rupture and causative fault; Mordret el al. 2020 proposed a new technique for source analysis, with an application to two moderate events including Le Teil; De Novellis et al. 2020 analyze the relation between the earthquake triggering and the limestone quarry located above the fault).

4) The opening of the third paragraph of the introduction is garbled, and needs to be rewritten.

This paragraph has been removed from the introduction (see answer to main comment).

5) Lines 40-42 overstate the case. There have been plenty of detailed investigations of moderate earthquakes, going back to work in the 1990s by Art Frankel and others. The issue again is that very few moderate earthquakes are shallow.

These lines have been removed (see answer to main comment).

6) Writing is awkward throughout "providing quantitative footprints of the ground acceleration," "Besides, to constrain the rupture duration," etc.

The word “footprint” has been replaced by “estimates”, and “Besides, to constrain...” has been replaced by “In order to constrain...”.

7) On a more substantive note, the authors do not appear to consider the possible effects of attenuation on their corner frequency estimate. Is this a fair assuming, assuming a reasonable Q value and the range of source-receiver distances?

In the absence of adequate empirical Green’s function, we simply model propagation effects considering the distance and a frequency-independent quality factor Q to model attenuation, considered as a free parameter. An earthquake displacement spectrum is then model with three parameters: seismic moment M_0, corner frequency fc, and Q. In order to estimate fc and M_0, we are then left with two options: (1) estimating M_0, fc and Q for each station and take the average of M_0 and fc; (2) estimating M_0 and fc for a single average normalized spectrum. Option (1) has the advantage of considering station-dependent Q values, which may be a fair assumption given the range of azimuth and distances. Nevertheless, fitting the individual spectra with such a simple model can be difficult and result in strong uncertainty of the fc estimation at several stations, which can bias the average estimates and the variability. Option (2) has the advantage of considering an average spectrum, which can be fairly well fitted by the simple model, but only provides a very gross estimate of the average regional Q value. These 2 approaches were investigated by Ambercombie et al. (2017), who claim that option (2) provides more stable results. Therefore we have considered option (2).

Note that we have checked that option (1) would give Mw=4.84 and fc=0.52 Hz, and hence would yield an average rupture duration of 1.7 s and an average rupture velocity of 2.1 km/s, while option (2) gives Mw=4.85 and fc=0.44 Hz, implying a duration of 2s and rupture velocity of 1.8 km/s. Using option (1) would then result in slightly higher PGA values, and would not change the mains conclusions of the paper.

Finally, note that a similar hypothesis (constant Q value) has been recently used by De Novellis et al. (2020), which was also not published as our study was submitted. We obtain similar values of the average stress drop (we obtain 1 MPa while they obtain 1.3 MPa) and we also infer a bilateral rupture. However, it seems that the authors used a partial dataset of the available seismological data (broadband and strong motion stations), consisting of 22 stations, while we consider 59.

8) (lines 68-74) The reader is left wondering where this information comes from.

This point was also mentioned by Reviewer 2. All the information about the InSAR slip image (source data, data processing, inversion technique, inferred slip image) is described in Cornou et al. (2020). This paper sums up the mobilization of the French post-seismic unit immediately after the Teil event with the deployment of seismological and geodesic stations, geological field surveys and field evaluation of the earthquake intensity. This paper has been recently accepted and was only submitted at the time of writing of our manuscript. Accordingly, we have removed from section “Data” the reference to the French post-seismic group webpage.

9) The authors really need to reference more seminal studies, not just papers published in the last few years -- for example lines 83-84. (Jack Boatwright was doing work on these topics going back to the 1990s).

Lines 83-84 have been removed, so that references to general source studies of moderate earthquakes have been deleted (see answer to main comment). Regarding references to stress drop studies, we have referenced the pioneer study of Kanamori & Anderson 1975. Regarding empirical Green's function studies for source analysis, we have included the seminar work by Mueller 1985.

10) (lines 102). This is fundamentally a modeling study, so while the assumptions and method are sound, the authors need to be careful to note that results are predictions, not observations. (Comparison with field observations is good.)

The expression "numerical simulation" has been replaced by "numerical prediction" in the whole manuscript. In addition, the section "Results" now includes a specific subsection "Numerical prediction of ground acceleration", which is clearly separated from subsection "Near-field observations of displaced object".

11) The Discussion and Conclusion section is not well-written -- the 2nd paragraph discusses issues that were not even mentioned in the paper. The first sentence of the first paragraph also comes "out of the blue" in a paper that has focused on ground motions. Again, the authors need to do a better job of explaining the implications of the study.

We have reshaped the conclusion by doing the following modifications:

- *We have removed the first sentence. The conclusion now starts by pointing out the particularly shallow character of the Le Teil event, and that ground motion and source properties of such moderate sized earthquakes are poorly documented.*

- *We have emphasized the outcomes of our study and the possible high impact of superficial moderate earthquakes (including natural and anthropic earthquakes) on seismic hazard in stable continental regions.*

- *We have removed from the second paragraph the notions of regional shortening and velocity weakening behavior, to better focus on the implications of our results on local seismic hazard.*

Answer to reviewer #2:

General comments:

The content of the paper and the conclusions/results (ground motion simulations, inferred 1D velocity model, observations of tossed rocks….) are interesting but I think they are not well exposed. The reader can get confused between the results obtained from this work, the results inferred by other works (e.g. slip model derived from the Insar data) and the main goals pursued by the authors. The paper is also not well balanced. Very few words to introduce the work and to explain the simulations while there are a lot of sentences to explain displaced objects. In particular, the authors didn't stress on their results showing the inferred variability of PGA and PGV values coming from the 2000 simulations. For example, why they didn't show the variability of PGV and PGA values at the power plants? For all these reasons, I'm sorry, I suggest a major revision.

Additional minor and major comments

1) Title. I suggest to add the indefinite article "a" (…for a moderate-sized shallow earthquake)

Corrected.

2) Instead of writing "exceeding 5 m/s2" I would write "exceeding half g".

We use "5 m/s2" as proposed by Anderson (2010) to define exceptional ground motions. For consistency, we believe that this is then better to keep this formulation.

3) Check the English for sentence lines 37-40.

These lines have been moved to section "Source analysis" (see response to reviewer 1) and replaced by "....on a small fault portion, including local stress drop, strong spatial variations of the rupture velocity (in particular when the rupture front hits the fault edges) and local fault slip acceleration".

4) In general, references are missing. In particular, please add references for the localization of the event at 1km (line 46).

The list of references has been completed (see list of new references below, including induced seismicity, source studies, earthquake depths, empirical and numerical ground motion prediction).

Regarding the earthquake localization, we have referenced the paper recently published by Ritz et al. (2020), who determined the double-couple point source focal mechanism of the earthquake using regional broadband and strong motion records. By testing different focal depths, they obtained the best solution to be at 1 km.

Furthermore, we have referenced the study by Cornou et al. (2020), which was also not yet accepted as the time of submission of our paper. This paper presents the geophysical data (seismological and geodetic) and field observations (geological and intensity estimation) obtained by the French post-seismic unit. The paper also presents the slip model inferred from InSAR data that we use in our study (see answer to question 8).

5) In the introduction the authors are missing to introduce the main characteristics of the studied event. Which is the kinematic (trust fault mechanism) and the geometry of the fault? Which are the main collected data for this event? Moreover, introducing their work the authors do not explain what they want to do with their numerical simulations.

First, we have provided information about the rupture process (reverse-fault focal, rupture size) and referenced the papers by Ritz et al. (2020) and Cornou et al. (2020).

Second, we propose the following paragraph to point out the importance of ground motion records for seismic hazard assessment and the usefulness of numerical ground motion predictions: *"Strong motion records are typically used by the earthquake engineering community to design key structures like power plants, bridges or dams, and to analyze the response of existing buildings for a potential future earthquake. One major issue is that near-fault strong motion recordings are still too scarce to fully understand the physics involved in the highly variable ground motion within a few kilometers from the rupture plane (e.g. Chiou et al. 2008; Pacor et al. 2018). Consequently, ground motion prediction models are not calibrated for near-field motion and are accurate only at fault-plane distances greater than ~10 km (Lanzano et al. 2019). Numerical ground-motion simulation techniques, including physical description of the fault rupture process, have then become popular to investigate near-source characteristics of ground motion (e.g. Goulet et al. 2015).".*

Third, we have mentioned that ongoing studies investigate the possible relation between the shallow location and a local mass rock extraction from quarrying directly above the rupture area (Delouis et al. 2019; De Novellis et al. 2020). A few events triggered by quarries on reverse faults are reported (e.g. Pomeroy et al. 1976, Seeber et al. 1998).

Finally, we have provided more precise information about our simulation strategy (source analysis, 1D velocity model). The simulations provide and estimation of the maximum acceleration and the spatial variability of ground motion that was generated during this exceptionally shallow event. We have simply added the following sentence at the end of the introduction: "Our study provides new insights on seismic hazard due to superficial seismicity in stable continental regions".

6) Figure 1 has not the colorbar to define the slip values projected on map.

A colorbar has been added to Figure 1.

7) The authors didn't introduce the array of virtual stations, in terms of number of stations, relative location respect to the fault. Moreover mentioning Figure 1 the authors do not underline that the temporary stations were installed after the event.

We have included the following sentence in section "Numerical prediction of ground acceleration": "The array is composed of 234 stations with interstation distance of 500 m and is centered on the fault (Fig. 2)."

8) Line 68-69: From the text I cannot understand if the author inverted Insar data to obtain the slip distribution of if they infer the slip from other works (again there are not references). Moreover which are the info of Insar data (which satellite, which time interval of the fringes)…

All the information about the InSAR slip image (source data, data processing, inversion technique, inferred slip image) is available in Cornou et al. (2020). This paper has been recently accepted and was only submitted at the time of writing of our manuscript. Accordingly, we have removed from section "Data" the reference to the French post-seismic group webpage.

9) How the author fix the geometry of the fault? Is it inferred from previous works? Is it inferred from InSar inversions? Is it inferred from moment tensor solution? Which is the reference of the moment tensor?

The fault geometry is based on the InSAR slip distribution (Cornou et al. 2020). The obtained fault mechanism is the same as the one given by the moment tensor inversion of Ritz et al. (2020).

10) Line 73-74 are too qualitative.

*We propose the following modification: "Slip shows a reverse fault mechanism and is characterized by two main slip patches localized between 0 and ~1 km depth on a roughly 4 km*1.5 km fault plane dipping 58°SE, resulting in a magnitude M_w=4.9. Analysis of the farfield seismological recordings gives the same magnitude (moment tensor inversion, Ritz et al. 2020), indicating that the slip obtained from InSAR is primarily seismic."*

11) I don't understand why the authors modify the position of the hypocenter? They should say something about the range of allowed variability.

The available hypocenter position based on inversion of regional seismograms places the hypocenter at about 1 km depth at the fault center with an uncertainty on the epicenter position of about 1 km, as recently demonstrated by Ritz et al. 2020. This location and the associated uncertainty is consistent with the probability distribution that we propose, based on azimuthal variations of the corner frequency. Note that the recently published study by Mordret et al. (2020) suggests a nucleation further southwest. This points out the uncertainty in the hypocenter position of the Le Teil event, primarily due to the absence of near-source observations. There is thus a need to consider uncertainty in the nucleation position. Subsection "Source analysis and rupture modeling" has been completed accordingly.

12) Which is the range of variability allowed for the other kinematic parameters?

All the source parameters are represented by probability density functions, which are defined in Supplementary Information (section "Kinematic rupture modeling"). We have modified the last sentence of subsection "Source analysis and rupture modeling" as: "Finally, we obtain 2000 rupture models representing the uncertainties in the above-defined rupture parameters, represented by probability density functions (Methods, Fig. S3)".

13) Because the authors collect the kinematic information from different sources (Insar data for slip distribution, rupture duration from Brune model, rupture velocity from average values of fault dimension, slip velocity acceleration from the literature) did the authors try to fit distant stations with their 2000 models?

The two closest stations that have recorded the Le Teil event without saturation are located at ~12 km and ~20 km within the Rhone river sedimentary basin, while the Le Teil event occurred out of the valley (Figure 1). The detailed velocity structure of the Rhone valley remains unknown (especially the very shallow layers, that may generate strong high frequency site effects) and the velocity model inferred in our study describes the earth structure at a local scale, in the fault vicinity (see position of the temporary broad band stations on Figure 1). We believe that extrapolating the velocity structure up to far-field stations would lead to biased results. Note that on going research focuses on better characterizing the regional velocity structure, and that such simulations will certainly be carried out in the future. Nevertheless, this is beyond the scope of our study.

14) How the authors can convince the reader that their 2000models for frequency up to 5Hz can be reasonable?

In our kinematic modeling approach, the high-frequency source energy mainly results from local variations of the rupture velocity, local slip velocity and slip heterogeneity, modeled using self-similar spatial distributions. One advantage of our modeling technique is that it ensures that the source time functions are consistent with a Brune's source model (i.e. with basic theory of high-frequency seismic radiation) with a target corner frequency (Figure S3d, Methods). The corner frequency is calibrated based on seismological observations. Second, we claim that the inferred 1D velocity structure is suitable to simulate the near-field wave propagation up to ~5 Hz, since dispersion curves are fitted up to ~5 Hz. Finally, the consistency between the numerical predictions and the in-situ observations seems to indicate that our simulations provide reasonable levels of ground acceleration.

15) The computation of the 1D velocity model using aftershocks is showed in between the generation and explanation of the 2000 models. The reader cannot appreciate this result.

We have split the section "Results" in four different subsections, namely "Source analysis and rupture modeling", "Velocity model", "Numerical prediction of ground acceleration" and "Near-field observations of displaced objects", so as to better emphasize the results on the 1D velocity structure. The geological interpretation, which was presented as supplementary material, has been moved to this subsection. Note that the geological interpretation has been enriched, following discussions with J.F. Ritz. The inferred velocity structure is very consistent with the geological cross-section proposed by Ritz et al. (2020). In order to point out this outcome, we have designed a new figure (now Fig. 2) showing the Vs profile overlaying the geological cross-section from Ritz et al. (2020).

16) The inferred 1D model is a result of the paper. How different is this model respect to previous velocity models of this area? The authors should release the table of Vp and Vs values.

There was no available local velocity model for this area before the Le Teil event. A preliminary model was proposed by Cornou et al. (2020), following the same technique (3-component beamforming method using seismic noise), but with a partial dataset. For this preliminary model, dispersion curves were fitted up to only ~0.5 Hz, while the new results explain dispersion curves up to ~5 Hz. We have released the table of the Vp and Vs values in Supplementary Information.

17) Figure S3 panel c: what do the different lines colors represent? They are six while the models are three.

We showed 6 realizations of source time functions to better illustrate the source variability. The displayed source realizations include the 3 rupture models shown in (a). The caption has then been modified as following: "Example of source time functions (or moment rate functions), including the three rupture models shown in (a)."

18) How does the PGA distributions change for different frequency ranges?

An indication of the ground motion distribution at lower frequency can be provided by the PGV distribution (Figure S4). The comparison between Figures 2 and S4 shows very similar patterns of ground motions, indicating a rather weak frequency-dependency of the ground motion spatial distribution.

19) I don't understand: "The decay of ground acceleration with distance to rupture is fast, except along the EW and SN directions, due to directivity effects". Are directivity effects along EW and SN directions? Are the authors sure?

We agree that this sentence is confusing. The expression "…except along the EW and NS directions, due to directivity effects" has then been removed and we have provided explanations about directivity effects. For a reverse-fault mechanism, directivity effects are expected to be absent along the fault strike direction because the rupture propagates in the direction of a node of the S-wave radiation pattern. On the contrary, such effects are maximized along the up-dip direction (e.g. Somerville et al. 1997, Aagaard et al. 2004). Nevertheless, the weak amount of slip above the hypocenter results in the absence of up-dip directivity effect. Slight directivity effects are then observed at source-receiver azimuths of about -45° and 45°, explaining higher ground acceleration in these directions. Subsection "Numerical prediction of ground acceleration" has been modified accordingly.

20) First sentence of the discussion "The occurrence of the 2019 Mw4.9 Le Teil event shows that friction and stress conditions in the very shallow earth crust (<1 km) can be favorable to moderate-sized earthquakes." Starting the discussion with this sentence seems to suggest that the rupture was ready to break without human induced activities. Even if the authors have not discussed the cause of the event, they can say that, regardless of whether this event may have been induced, the shallow stress field was still favorable for a potential rupture.

The first sentence of the discussion has been removed, as suggested also by reviewer 1. In addition, we have mentioned in the introduction that the earthquake triggering and the very shallow hypocenter may be due to a limestone quarry located just above the rupture area. Accordingly, we have referenced several papers reporting moderate events triggered by quarrying above reverse-faults, as well as the preliminary report on the probable causes of the Le Teil event written by a group of French experts, and the recently published study by De Novellis et al. (2020).

21) I don't understand the units of measurements for equation (1). Mu is the rigidity and it is in Pa.

Mu is the friction coefficient as mentioned in the text. Hence Mu has no unit.

22) Figure 4 has not been fully explained. The reader can get lost. The right bottom panel has not a clear explanation.

We have completed the figure caption to better point out the difference between ground displacement, absolute and relative slab displacement. Note that what can be measured on field is the final relative slab displacement. We have also provided additional information about the meaning and the role of the color scale.

23) Probably the caption in Figure S4 is wrong. These plots represent PGV not PGA.

The caption has been corrected.

Additional modifications

- *The meaning of the term shallow can vary significantly in the seismological community (e.g. Chounet et al. 2018 associate shallow earthquake to depth < 120 km). In order to avoid confusion, we propose to replace the term "shallow event" by "superficial event", leading to modification of the title.*

- *The figure regarding the simulations of funeral slab displacements (Figure 4) has been slightly modified. The level of damage is the cemetery is high (intensity VIII). However this is difficult to realize looking at Fig 4a, which only shows observations that can be used for accurate assessment of the ground acceleration (slab displacements of 1 to 10 cm). Then, we have added a photo illustrating large observed damage in the cemetery.*

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3rd Dec 20

Dear Dr Causse,

Please allow me to apologise for the delay in sending a decision on your manuscript titled "Exceptional ground motion for a moderate-sized superficial earthquake". It has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License) provided you address the remaining concerns in full. In particular, you will need to tone done your connection between the generation of high-frequency, destructive seismic waves and the similarity in kinematic parameters between the Le Teil earthquake and deeper events, and add caveats as appropriate.

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

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Best regards,

Joe Aslin

Associate Editor,
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https://www.nature.com/commsenv/
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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have addressed key concerns, and in some cases (e.g., fc - Q trade-off; reliance on modeling rather than data) have probably done as much as can be done. The revised manuscript is significantly improved. I recommend that the manuscript be published after the authors address two remaining concerns:
1) Given that moderate earthquakes at 1-km depth are rare, the closing sentence of the abstract is not justified.
2) I was remiss for not flagging this in the initial round, but PGA is both a high-frequency measure and an extremal measure. Short-duration, high-frequency spikes might toss rocks around, or knock over small objects, but such ground motions are not necessarily damaging to structures. This issue merits discussion.
Also, some copy-editing is still needed to improve the presentation.

Reviewer #2 (Remarks to the Author):

Review for “Exceptional ground motion for a moderate sized superficial earthquake” by Causse et al.

This paper is focused on the November 11, 2019 Mw 4.9 Le Teil event (France) that caused an unexpected level of damage.
This is my second round of review and I think that the paper has been deeply reviewed by the authors.

The content of the paper and the conclusions/results are now well written and accessible to the readers. I have other comments /suggestions to further improve the paper. I suggest minor revision.

Here my list of minor comments:

Abstract:

1) I don’t understand the meaning of this sentence:

“Based on farfield seismological observations, we demonstrate that the rupture properties are consistent with the ones commonly observed for large deeper earthquakes, implying that the near-surface faulting generated strong high-frequency seismic waves.”

I understand that the retrieved main average kinematic parameters are consistent with larger events but this doesn’t imply the generation of high frequency content. The high-frequency content can have many reasons to exist.

2) I don’t think that the verb “to match” is correctly used in the following sentence.
"we perform numerical predictions of the ground acceleration on a virtual array of near-fault stations, that matches with the locations of independent quantitative estimations from in-situ observations of displaced objects (natural and anthropic)."

I would say “we perform numerical predictions of the ground acceleration on a virtual array of near-fault stations. The inferred variability is in agreement with the locations of independent quantitative estimations from in-situ observations of displaced objects (natural and anthropic) whose presumed minimum acceleration can be inferred”
Or something similar.

3) The sentence “This dramatically changes the perception…” would say that we usually have another perception.

So I would add “This dramatically changes the perception on seismic hazard assessment of the impacts of superficial moderate earthquakes whose occurrence is often neglected”

Introduction:

4) In the introduction the word “frequency” is missing. I think the authors have to add something suggesting the high frequency content that can be produced by superficial events (+ add references).

5) I would change the sentence “The shallow location may be related to quarry extraction directly above the rupture area (De Novellis et al. 2020; Delouis et al. 2019).” With “In the literature the shallow location has been often related to quarry extraction located close to the rupture area”

6) Please define in the introduction the meaning of superficial (< 5km ? o < 3 km)

7) In the introduction the authors should say about the existence of a slip distribution model from Insar data that the authors used as input for the low frequency content and to fix the geometry.

Results:

8) I understand the meaning of “local stress drop” or “local slip acceleration”…but probably it is more correct to write “heterogeneous stress drop”, “heterogeneous/variable slip acceleration”

9) I don’t understand the sentence “Average slip velocity is then 0.5 m/s, and has a maximum value of 0.2 m/s at the ground surface”. I would associate average slip velocity with peak slip velocity. Then the authors can add info on the ground surface.

10) Why the authors do not show any fit to far field data with their 2000 models? Maybe filtering at low frequency … or as an ensemble overlapped with real data.

11) The authors added info on the 2000 models on the Supplementary material. However I suggest to add a sentence on the main text saying which are the rupture parameters modified during the simulations (high frequency of the slip, rupture velocity, hypocenter location, rise time..)

Conclusion:

12) The authors should add a comment to this sentence “Inside the near-source area, the simulated ground motion shows a high level of heterogeneity that is fully related to the rupture characteristics along the fault plane.” Which are the main rupture parameters that allow this variability?

13) Reading this sentence. “The scarcity of observations of Le Teil-event type …” I would expect a comment related to the unlikely dynamic condition (stress field) to obtain an instability. The occurrence of Le Teil-event raises the question of how and why this event occurred.

14) The sentence “The earthquake causative fault , which was not considered as active, is part of a more extended the Cévennes fault system” has to be written in the introduction. This is an important info missing in the introduction.

Supplementary:

Figures S1 and S3 are very interesting and I suggest to include part of these figure in the main text.

In the web I found this abstract

Ampuero, J.-P., Billant, J., Brenguier, F., Cavalié, O., Courboulex, F., Deschamps, A., Delouis, B., Grandin, R., Jolivet, R., Liang, C., Mordret, A., and Oral, E.: The November 11 2019 Le Teil, France M5 earthquake: a triggered event in nuclear country, EGU General Assembly 2020, Online, 4–8 May 2020, EGU2020-18295, https://doi.org/10.5194/egusphere-egu2020-18295, 2020

The authors should cite also this reference when they found in the literature that shallow events are often triggered…

Manuscript COMMSENV-20-0191-T: Exceptional ground motion during the shallow Mw 4.9 2019 Le Teil earthquake, France

Answer to reviewer #1:

The authors have addressed key concerns, and in some cases (e.g., fc - Q trade-off; reliance on modeling rather than data) have probably done as much as can be done. The revised manuscript is significantly improved. I recommend that the manuscript be published after the authors address two remaining concerns:

1) Given that moderate earthquakes at 1-km depth are rare, the closing sentence of the abstract is not justified.

The closing sentence has been removed.

2) I was remiss for not flagging this in the initial round, but PGA is both a high-frequency measure and an extremal measure. Short-duration, high-frequency spikes might toss rocks around, or knock over small objects, but such ground motions are not necessarily damaging to structures. This issue merits discussion.

As mentioned in the manuscript, "It is known that PGA for small earthquakes can be strong without causing damage, because it may be controlled by energetic frequencies that are much higher than the typical resonance frequencies of civil structures (>20 Hz). However our simulations result also in large values of the Peak Ground Velocity (PGV) for frequencies below 5 Hz (exceeding locally 1 m/s, Fig. S4). Moreover, the ratio PGA/PGV, which equals about 2π multiplied by the dominant frequency, has values ranging between 10 and 15 s^{-1}*, implying a dominant frequency around 2 Hz and hence accelerations prone to damage structures (ref.[3])." This shows that the large PGA do not corresponds to high-frequency, short duration spikes. This is also clear looking at the large observed damage in the near-fault area. Note that Intensity reached VII (significant damage) in a hamlet located within the area where upthrown stones have been reported. We have simply added the following sentence at the end of section "Numerical prediction of ground acceleration": "…hence accelerations prone to damage structures (ref.[3]), in agreement with the observed Intensity of VIII in the fault vicinity."*

Also, some copy-editing is still needed to improve the presentation.

We have checked the whole manuscript.

Answer to reviewer #2:

Abstract:

1) I don't understand the meaning of this sentence:

"Based on farfield seismological observations, we demonstrate that the rupture properties are consistent with the ones commonly observed for large deeper earthquakes, implying that the near-surface faulting generated strong high-frequency seismic waves." I understand that the retrieved main average kinematic parameters are consistent with larger events but this doesn't imply the generation of high frequency content. The high-frequency content can have many reasons to exist.

The sentence has been replaced by: "Here we use far-field seismological observations to demonstrate that the rupture properties are consistent with those commonly observed for large deeper earthquakes."

2) I don't think that the verb "to match" is correctly used in the following sentence. "we perform numerical predictions of the ground acceleration on a virtual array of near-fault stations, that matches with the locations of independent quantitative estimations from in-situ observations of displaced objects (natural and anthropic)." I would say "we perform numerical predictions of the ground acceleration on a virtual array of near-fault stations. The inferred variability is in agreement with the locations of independent quantitative estimations from in-situ observations of displaced objects (natural and anthropic) whose presumed minimum acceleration can be inferred". Or something similar.

The sentence has been replaced by: "In the absence of strong motion sensors in the fault vicinity, we perform numerical predictions of the ground acceleration on a virtual array of near-fault stations. These predictions are in agreement with independent quantitative estimations of ground acceleration from in-situ observations of displaced objects."

3) The sentence "This dramatically changes the perception…" would say that we usually have another perception. So I would add "This dramatically changes the perception on seismic hazard assessment of the impacts of superficial moderate earthquakes whose occurrence is often neglected".

This sentence has been removed.

Introduction:

4) In the introduction the word "frequency" is missing. I think the authors have to add something suggesting the high frequency content that can be produced by superficial events (+ add references).

To the best of our knowledge, we do not know studies regarding the high-frequency content of such particular events. In the first version of the paper, we proposed a paragraph on the different mechanisms involved in the high-frequency radiation. As suggested by the reviewers, we moved this paragraph to section "Source analysis and rupture modeling". We suggest to let the introduction as it is.

5) I would change the sentence "The shallow location may be related to quarry extraction directly above the rupture area (De Novellis et al. 2020; Delouis et al. 2019)." With "In the literature the shallow location has been often related to quarry extraction located close to the rupture area".

We believe that this is important to distinguish between studies regarding the Le Teil event and previous studies on the effect of rock quarries on earthquake triggering. Note that there are only a few studies regarding effects of quarries (in comparison with seismicity induced by other human activities, such as dams or geothermal activities, …). We propose to let this sentence as it is.

6) Please define in the introduction the meaning of superficial (< 5km ? o < 3 km).

We have defined superficial event as depth<3 km in the introduction.

7) In the introduction the authors should say about the existence of a slip distribution model from Insar data that the authors used as input for the low frequency content and to fix the geometry.

We have added the following sentence in the Introduction: Rupture models are calibrated based on the Interferometric SAR slip model proposed by [6]

Results:

8) I understand the meaning of "local stress drop" or "local slip acceleration"…but probably it is more correct to write "heterogeneous stress drop", "heterogeneous/variable slip acceleration"

We have used the terms local for the sake of consistency with the term "local processes".

9) I don't understand the sentence "Average slip velocity is then 0.5 m/s, and has a maximum value of 0.2 m/s at the ground surface". I would associate average slip velocity with peak slip velocity. Then the authors can add info on the ground surface.

We agree that this is unclear. The value of 0.2 m/s at the ground surface is not an essential information. We propose: "The average slip velocity over the fault is then 0.5 m/s, as commonly reported from near-fault seismological data (e.g. ref. [30,35]*) and observations during fault scarp formation (e.g. ref.* [36-37]*).*

10) Why the authors do not show any fit to far field data with their 2000 models? Maybe filtering at low frequency … or as an ensemble overlapped with real data.

As mentioned in the answers to the first round of reviews, our paper is focused on the high-frequency and near-source ground motion. In order to calibrate our source model, the comparison with farfield data has been simply performed in the frequency domain. The comparison with time series will be performed once a robust regional model will be available.

11) The authors added info on the 2000 models on the Supplementary material. However I suggest to add a sentence on the main text saying which are the rupture parameters modified during the simulations (high frequency of the slip, rupture velocity, hypocenter location, rise time..)

All information about the Methods is now available in the main manuscript, in the Methods sections.

Conclusion:

12) The authors should add a comment to this sentence "Inside the near-source area, the simulated ground motion shows a high level of heterogeneity that is fully related to the

rupture characteristics along the fault plane." Which are the main rupture parameters that allow this variability?

The near-source spatial variability of ground acceleration is essentially controlled by the spatial distribution of slip and rupture velocity, and the position of hypocenter. This has been mentioned.

13) Reading this sentence. "The scarcity of observations of Le Teil-event type …" I would expect a comment related to the unlikely dynamic condition (stress field) to obtain an instability. The occurrence of Le Teil-event raises the question of how and why this event occurred.

We believe that the scarcity of such observations is a combination of the scarcity of such superficial events and the scarcity of near-fault strong motion observations in moderate seismicity areas. The origin of the earthquake has been discussed in the Introduction section (possible effect the quarry) and is discussed in detail in Ritz et al. (2020) (tectonic and geological setting) and DeNovellis et al. (2020) (impact of the quarry).

14) The sentence "The earthquake causative fault , which was not considered as active, is part of a more extended the Cévennes fault system" has to be written in the introduction. This is an important info missing in the introduction.

This sentence has been added in Introduction.

Supplementary:

Figures S1 and S3 are very interesting and I suggest to include part of these figure in the main text.

We believe that this is better to keep these Figures as Supplementary Figures, because they pertains "details" about the source analysis.

In the web I found this abstract

Ampuero, J.-P., Billant, J., Brenguier, F., Cavalié, O., Courboulex, F., Deschamps, A., Delouis, B., Grandin, R., Jolivet, R., Liang, C., Mordret, A., and Oral, E.: The November 11 2019 Le Teil, France M5 earthquake: a triggered event in nuclear country, EGU General Assembly 2020, Online, 4–8 May 2020, EGU2020-18295, https://doi.org/10.5194/egusphere-egu2020-18295, 2020.

The authors should cite also this reference when they found in the literature that shallow events are often triggered…

We have included this reference in the reference list (citation in Introduction).