Dear Editor,

We would like to sincerely thank you and the anonymous Reviewer for your careful and constructive reviews. We have modified the manuscript according to all requirements listed in the revisions, improving main text, figures and supplementary text.

With regard to the most important modifications made:

The whole text has been revised according to the precise comments provided by the Editor and the Reviewer.

We provided additional details about the spectral inversion methodology, including how it accounts for site effects and which are the main assumptions in the probabilistic framework that we used.

We modified Title, Abstract, Figure 1, Figure 3 and Figure S1, and added a new Figure S3.

With these corrections, we hope that the manuscript is now well shaped to be published as Fast Report in Seismica.

Hereinafter, we answer point-by-point to the Editor’s and Reviewer’s comments.

Editor's Comments

First, the sections can be summarily organized as "Data and Method", and "Results and Discussion" - This is a suggestion. The authors may or may not take it.

We follow the Editor's suggestion and define "Data and Method" section. We would like to keep separate Results and Discussion.

About the title: perhaps - the following would be more concise.

"Source characterization of the 20th May 2024 Campi Flegrei caldera earthquake through a joint source-propagation probabilistic inversion"

We modified the title as suggested. We added the magnitude information, since on that day several earthquakes of moderate magnitude occurred.

Abstract: The conclusion given below is confusing as to how the source characterization in the present study can indicate possible future larger magnitude earthquakes in the region.

"The source characterization obtained in this study suggests that, for future earthquakes in the analyzed hypocentral region, it cannot be ruled out an increase in magnitude of 1.2 ± 0.3 points compared to the main event that occurred on May 20th."

Perhaps, a very short explanation is missing here.

We modified the sentence (L 20-22), specifying stress drop instead of source characterization, and the possible rupture size assumed in this study.

Also, "1.2 ± 0.3 units" would be better than "1.2 ± 0.3 points"

Modified as suggested.

Abstract: The following conclusion is also unclear.

"A systematic application of the proposed methodology to the seismicity observed in the caldera in recent years may help to estimate the expected ground motion if larger events were to occur."

What is the proposed methodology? If that is one applied for the source characterization (of past earthquakes), how does that help in estimating expected ground motions in case of larger future events?

We modified the sentence (L 23-26), specifying source characterization methodology. Moment magnitude and corner frequency estimations from a set of events, both single-station and event estimates, should help constrain ground motion modeling parameters. We briefly discussed that in the Discussion section.

Line 32: Would a seismicity map using data in Figure S1, presented as one of the subplots in Figure 1, be useful? The time-frequency plot can be provided as an inset there.

We modified Figure S1, adding a panel with the seismicity map since 2019. The map shows only the events with Md > 3 to avoid an unclear representation with too many events.

Line 43: perhaps, "... the source of the M_D 4.4 earthquake by inverting displacement amplitude spectra"

_D means subscript D

Modified as suggested.

Figure 1 caption: Instead of "As for panel a,", perhaps, 'As in a.,'.

Also, "INGV event-ID 38759141, location-ID 127958121" can be provided in the section Data Availability as "The INGV event-ID and location-ID of the event (investigated in the present study) are 38759141 and 127958121, respectively.

Modified as suggested.

Line 73: "Baltay et al., 2024" insead of "A. Baltay et al., 2024"

Modified as suggested.

Method: How do the authors deal with the site responses, especially when the analysis involves using waveform data in a wide frequency range? This is an important question since a lack of treatment for site responses will produce incorrect results.

The *Q'* parameter of the forward operator that we use accounts for both anelastic attenuation quality factor Q and site-dependent attenuation term k0. This is now specified in the main text (L 81 – 83) and in Supplementary Text S1 (L 30 – 38), where we also added eq. S2 following a subsequent comment by the Editor.

Method: Do the authors analyze all the components (i.e., vertical and horizontal components) of the ground motion data?

We invert the two horizontal components for S-wave inversion (Text S1, L 29-30), and the vertical component for P-wave inversion (now specified in Text S1, L 47).

Figure 2. Caption: Instead of "Station POZA", "The analysis shown here is at the station POZA (Figure 1)." It would be good to indicate this station on one of the maps shown in Figure 1 (perhaps, in Figure 1a).

We modified the caption and Figure 1a as suggested.

Figure 2. Caption: What is the noise spectrum being depicted here?

The noise spectrum is estimated from a signal time window starting from the origin time, with the same length of the inverted S-wave. This is now specified in the caption.

Figure 2. Caption: "dots", Instead of "circles"

Modified as suggested.

Figure 2b does not serve any purpose since this information is depicted in Figure 2c.

We agree that Figure 2b is just the integral of the PDFs shown in Figure 2c. If the Editor agrees, we would like to keep panel b because we believe it could be easier to interpret than the 2d heatmaps of panel c alone.

Figure 3. "Stations and hypocentral distances, see Key. INGV event-ID 38759141." This is unclear. Is there an online site to look up the event? If so, perhaps this information can be provided in the data availability section.

We modified the sentence to make it clearer (L 126 – 127). We removed the INGV event-ID, which has been moved to Data availability section following a previous comment by the Editor.

The plots and also, listing of station and hypocenter would be perhaps more discernible if they were sorted according to the hypocentral distance (or corner frequency) and also, if the distances were given in km instead of in meters.

We modified Figure 3 as suggested, now stations are sorted according to corner frequency. We moved the distances information to the results table (distances are now given in km).

Line 120: perhaps, "... not allow proper isolation of the P-phase …"

Modified as suggested.

Line 122: Perhaps, Eq. S1 can be reproduced here and referred to.

Modified as suggested.

Line 132: The following is not clear.

"We define as epistemic uncertainty (due to input variable uncertainty) for Mw and fc the difference between the two estimates corresponding to the considered ~15% change in vS".

We modified the sentence (L 148 – 150) to make it clearer.

Line 139-143: This paragraph would be better placed in the Method section. What exactly is the advantage of having PDF as a solution? How does the method account for the between-parameter correlations? Would the between-parameter correlations be observed from the inversion, rather than applied as a constraint in the inversion?

We moved the paragraph to the Data and Method section, as suggested.

We see several advantages in having a PDF solution: the mean event result is obtained weighting each single-station solution for the corresponding single station uncertainty; the single-station uncertainty accounts for between-parameter correlations by definition, as it comes from the integration of the joint PDF; the existing correlation between parameters is mainly driven by the modelization uncertainties, therefore by the spectral model that is commonly used to describe the observed earthquake displacement amplitude spectra (for instance, this was shown by Supino et al. (2019), please see Figures 3 and 5 therein). We believe this suggests that a joint PDF solution is preferable to a scalar solution (the topic has been recently addressed by some authors, for instance Abercrombie 2021 (doi.org/10.1098/rsta.2020.013) and Trugman 2022 (doi.org/10.1029/2021JB023526)); the PDF allows to verify that the solution is constrained, while a deterministic minimizer alone would always provide a scalar solution even in the case the observed spectrum could not be adequately modeled by the assumed forward operator (please see also the answer to Editor's comment on L 176-188).

Line 146-155: This paragraph can go (after some editing) in the Introduction section.

If the Editor agrees, we would like to keep the paragraph at the beginning of the Discussion. We believe it would be important to the reader, who most likely does not expect such a high PGA associated with a Mw 3.7 event.

Line 156: "...provide constraints on", instead of "... provide further constraints about"

Modified as suggested.

Line 157-160: Perhaps, the following:

"For the earthquake, we estimate a source radius $r = k \cdot v S / f c$ = 400 ± 70 m where we have the estimated corner frequency fc = 1.11 ± 0.19 Hz (e.g., Hanks & Wyss 1972), and a rupture velocity vR = 0.9 vS and the constant k = 0.26 for a circular rupture model (e.g., Kaneko and Shearer, 2014)."

We modified the sentence following the suggestion, with some further rewording (L 170 – 173).

Line 166-168: Perhaps,

"Recently, Danesi et al. (2024) relocated the events (since 2005) in the study area, and showed that a ~3 km long structure could exist below the Solfatara crater. The depth of which is comparable with that of the event investigated in the present study."

Modified as suggested.

Line 168-171: Perhaps,

"If we consider a 3 km length based on Danesi et al. (2024) as the maximum possible rupture dimension (1.5 km as the source radius for a circular rupture), then the estimated magnitude corresponds to Mw 4.9 ± 0.3."

Modified as suggested.

Line 171: "units", instead of "points".

Modified as suggested.

Line 171: The assessment of a maximum magnitude earthquake here cannot be argued as a result of the analysis of the 20 May event. One can always get that estimation without having to analyze an event.

It is now better specified that the moment magnitude estimate comes from the stress drop estimated in this study (L 181 - 184).

Line 172: delta_Mw would suffice. The superscript UP is not needed.

Modified as suggested.

Line 172-173: "…that produced the maximum PGA of 3.58 m s -2 on May, 20th 2024." Authors can avoid repeating the same information.

Modifed as suggested.

Line 173-174: Perhaps., "Compared to other parameters, the uncertainty with estimated stress drop would be the largest".

We wanted to specify that the uncertainty on Delta(Mw) is obtained by propagating the uncertainty of the stress drop, and taking the maximum of the two uncertainties corresponding to the two limits of the stress drop interval. This is most likely not necessary to specify, we remove the sentence.

Line 176-188: A rewriting of these two paragraphs would be useful. A suggestion follows:

"Concerning the observed ground shaking levels (Figure 1), we highlight the following: (1) the stations at a similar epicentral distance (but in different directions, i.e., west and east from the epicenter) have different PGA; and (2) the spatial distribution of the estimated corner frequencies are remarkably consistent with the observed PGA. These observations collaborate a need to account for source-directivity in the ground motion modeling, even for moderate magnitude events (e.g., Colavitti et al., 2022; Jayaram & Baker, 2010; Pacor et al., 2016). Therefore, rigorous and systematic source characterization is warranted for the recent events in the Campi Flegrei caldera, which in turn will enable better characterization of future large events in the region, and support ground motion modeling of those events."

We modified the paragraph following the suggestion, with some further rewording (L 190 – 204).

Line 176-188: One may ask whether site amplifications could have played a role in the observed variations of PGA.

This possibility is now discussed in the main text (L 198 - 201). We believe this is unlikely. In the case of frequency-independent site amplification, this should emerge in the spatial variability of Mw single-station values, which however do not correlate with the observed PGA distribution. In the case of strong frequency-dependent site amplification, this should emerge as peaks in the spectra or clear deviation from Brune-type spectra, which however we do not observe (Figure 3). Also, in that case the PDF solution of the inverse problem should be unconstrained (e.g., Figure 10 in Supino et al., 2019), while we obtain well constrained PDF solutions.

Line 176-188: What would be the current challenges in accomplishing systematic source characterizations of the recent earthquakes in the study region? This is a valid question as it seeks to understand why such a study has not been yet carried out.

We believe that the present study shows that it is possible to accomplish such systematic source characterizations, and we are willing to work on that in the very near future starting from this manuscript. To our knowledge, volcanic observatories usually do not routinely estimate moment magnitude or corner frequency, so we are not entirely surprised that such a study has not yet been carried out. A possible challenge concerns earthquakes of small magnitude (e.g., M < 2), since in that case both the signal-to-noise ratio and the number of available stations with a clear S-wave arrival are expected to decrease.

Data Availability: The table in the following link can be included in the supplementary material or even as a Table in the main manuscript.

https://drive.google.com/file/).

This google drive link was intended to be just for revisions, sorry we didn't specify that.

If possible, we would like to use a data repository (Harvard Dataverse) to upload the results file and share it in the Data Availability section. This would also help us more easily comply with INGV data policy.

Perhaps, the remaining text can be:

The seismological time series are accessible at EIDA website (https://eida.ingv.it, last accessed ????) and at the DPC-RAN website (https://ran.protezionecivile., last accessed ?????). The INGV catalog information can be found online at https://terremoti.ingv.it/(last accessed, ????).

Please replace ???? with the last accessed Month and Year.

Modified as suggested.

References: I could not find citations for the following.

Baltay, A. S., Hanks, T. C., & Beroza, G. C. (2013). Stable Stress-Drop Measurements and their Variability: Implications for Ground-Motion Prediction. Bulletin of the Seismological Society of America, 103(1), 211–222. https://doi.org/10.1785/

This reference was wrongly included, we removed it.

Danesi, S., Pino, N. A., Carlino, S., & Kilburn, C. R. J. (2024). Evolution in unrest processes at Campi Flegrei caldera as inferred from local seismicity. Earth and Planetary Science Letters, 626, 118530. https://doi.org/10.1016/j

L 179, 182, 185

Eshelby, J. D. (1957). The determination of the elastic field of an ellipsoidal inclusion, and related problems. Proceedings of the Royal Society of London. Series A. Mathematical and Physical Sciences, 241(1226), 376–396.

L 174, stress drop formula

Oth, A., Miyake, H., & Bindi, D. (2017). On the relation of earthquake stress drop and ground motion variability. Journal of Geophysical Research: Solid Earth, 122(7), 5474–5492. https://doi.org/10.1002/

This reference was wrongly included, we removed it.

Supplementary Information

Line 27-29: perhaps, "The values assigned to the parameters are similar to those considered by Petrosino et al. (2008) for the study region."

Modified as suggested.

Line 34: Perhaps, Eq (4) of Supino et al. (2019) can be reproduced here.

Modified as suggested.

Line 44: "... the earthquake.", instead of "... the analyzed earthquake."

Modified as suggested.

A general note on the supplementary material: There is not much here. One can just explain what is observed in Figure S2 without any illustration. Perhaps, it could be a good idea to incorporate the supplementary information into the main text and do away with the supplementary material.

If possible, we would like to keep the supplementary material.

Reviewer:

Comments to the authors:

Review of the paper "Source characterization of the MD 4.4 earthquake occurred on the 20th of May 2024 at Campi Flegrei caldera (Italy) with a joint source-propagation probabilistic inversion"

The submitted manuscript, authored by Supino et al., presents the source characterisation of the 2024 Md 4.4 earthquake at Camp Flegrei caldera in Italy. This characterisation is based on the Brune model, for which the authors retrieved four different parameters: the seismic moment, the corner frequency, gamma, and the inelastic attenuation Q. The inversion method used a Bayesian framework, for which the space of plausible models is explored. The authors conclude that the earthquake had a magnitude of Mw=3.7 +/- 0.04. They then analyse the implications of the inverted parameters on some source characteristics, such as the stress drop and the source radius. Furthermore, they compare this earthquake with recent studies of the region. Based on these results, they suggest that a larger earthquake could potentially be expected in the zone. The aforementioned results are critical, as the PGA calculated for the close stations is particularly large.

The paper is well written and raises the question of whether this earthquake could act as a proxy for future major earthquakes, and which ground motion could be expected. Below I suggest a few points the authors should consider in order to better support their conclusions. Once all of these points have been considered, I believe the paper can be accepted for publication in Seismica.

#+++++++ Bayesian methodology++++++

Even though it is a fast report, it would be beneficial to include more details on the methodology based on the work of Supino et al. (2019). This could be incorporated into the main text or the supplementary information (SI). Some examples of the additional information that could be included are the a priori PDF use in the study, the uncertainties taken into account, and whether they are similar with those presented in Supino et al. (2019).

We modified the Supplementary Text S1 (L 30 - 42) following the suggestion, specifying additional information on the probabilistic framework and referring more precisely to the previous work of Supino et al. (2019). We also added eq. S2 in Text S1, following a comment by the Editor.

#+++++++ Hypocenter location++++++

The authors have reached several conclusions based on the hypocenter location provided by the INGV-OV (text S1). At the same time, they argue that from the equation S1, the most influencing parameter is Vs. However, if we examine the velocity model they cited (Tramelli et al., 2021), we find that an increase of 0.6 km at depth will drastically change the Vs from 1685 ms-1 to 3089 ms-1, which in turn would change the calculated magnitude. While I do not anticipate a complete reanalysis with a new location, it is crucial to address the uncertainty in the hypocenter location, given the evidence in the litterature indicating that a poorly constrained depth can impact subsequent analysis (e.g., Di Stefano et al., 2006; Tsai et al., 2011). I believe it would be beneficial to review the work of Di Stefano et al. (2006), which analyzed and identified differences in depth regarding the Italian network. Unfortunately, I am unaware of any significant changes to the Italian Seismic Network's operational location system since 2006.

We agree that hypocenter location and velocity model influence the final Mw estimate, and this has been now better specified in the main text (L 55 - 57). However, regarding the possible increase of

0.6 km in depth and subsequent Vs change from 1685 m s-1 to 3089 m s-1, we believe that such location would not allow anymore to assume for Vs the value of the corresponding velocity model depth layer. With that choice and a depth slightly larger than 3 km, we would assign to the seismic waves the velocity of a layer that goes from 3 km to 10 km depth, so that would be slightly crossed by the seismic waves. In that case, we believe that it would be necessary the alternative choice described in the present study (L 142 – 145), assuming the Vs value obtained from the average of the Vs corresponding to the depth layers from the hypocenter to the free surface, weighted with the thickness of the layers actually crossed.

This is consistent with what we observe for events occurring at Campi Flegrei caldera and located just below 3 km depth. We show here one example (Figure R1), for the event 35963161 (https://terremoti.ingv.it/event/35963161) located at 3.35 km depth (please note that by default the online depth information is approximated to an integer (3 km), but the decimal digits can be obtained for instance interrogating the FDSN INGV web-service). We show the histogram of the S-wave velocities estimated for each station (where a S-pick is possible) as the ratio between hypocentral_distance and travel_time.

The mean and median (magenta vertical line and dashed line, respectively) of the estimated Vs are consistent with the averaged velocity value (1638 m s-1, green vertical dashed line), but are significantly different from the velocity value of the depth layer from 3 km to 10 km (3089 m s-1, red vertical line).

Therefore, we believe that the Vs interval described in the manuscript (1437 – 1685 m s-1) and the corresponding estimated Mw epistemic uncertainty would be valid even if the location error would be of the order of ~1 km.

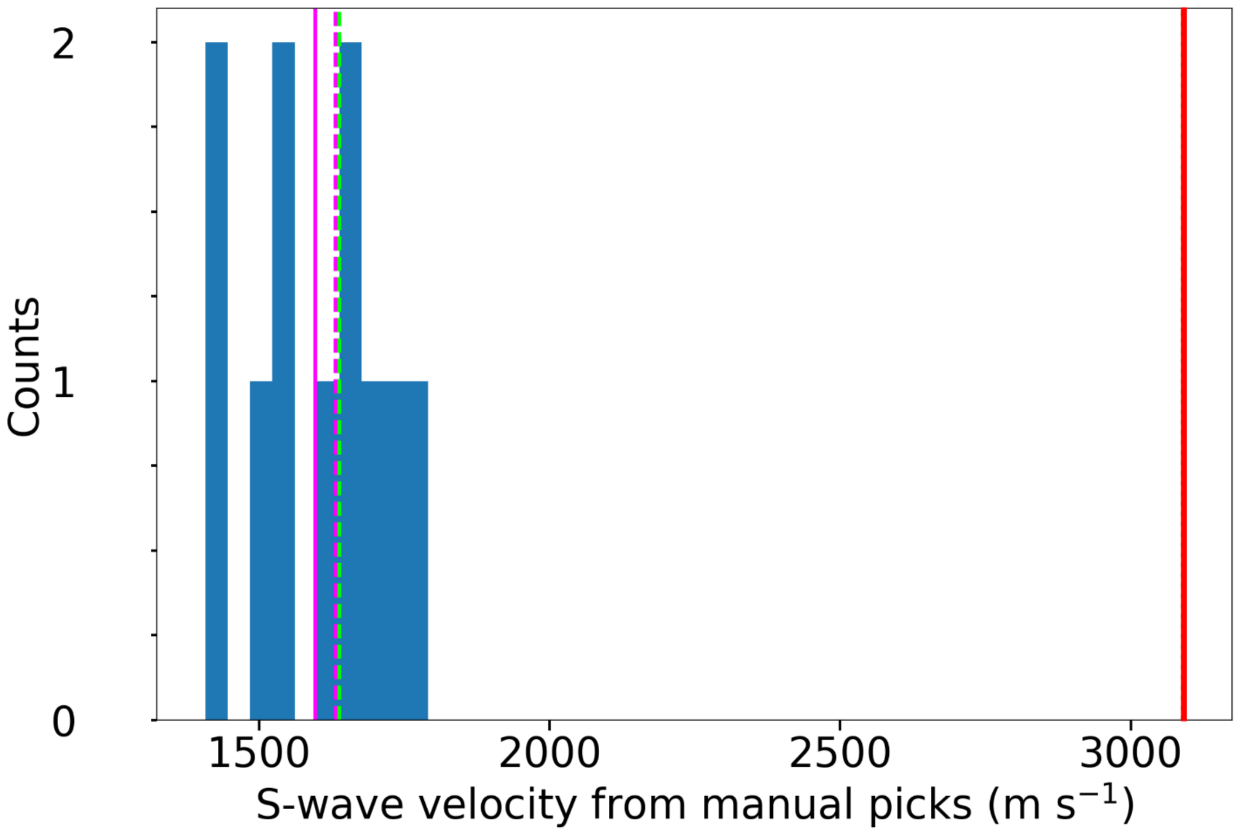

Figure R1.

#+++++++ Difference between Mw and Md ++++++

As the authors indicate in lines 136–138, it may be beneficial to acknowledge and describe the potential differences between the various magnitudes (e.g., the work of Werner and Sornette, 2008, or the work of Drouet et al., 2011).

We modified the main text following the suggestion (L 152 – 156). We would like to leave the discussion of Md-Mw scaling to a separate study, in which a set of events will be analyzed, and possible causes of such differences can be investigated.

#+++++++ Modeling of the rupture model ++++++

Given the interdependence between parameters (i.e., the source radius depends on the corner frequency, and the stress drop depends on the source radius itself), it would be beneficial to include a figure in the supplement to illustrate the parameters' variation regarding the associated uncertainties for each parameter. For instance, it would be nice to include a figure with the Brune and Madariaga end-member models and the corresponding results, similar to the Figure 1 (a) of Abercrombie (2021).

We added the Supplementary Figure suggested by the Reviewer to the SI (Figure S3) and modified the main text accordingly (L 176).

#+++++++ Increase in magnitude ++++++

Given the importance of seismic hazard assessment, a reliable estimation of an increase in magnitude is of significant relevance. However, this parameter relies on the estimation of the magnitude, which I address in one of my previous points.

Please see our answer to the previous Reviewer's comment regarding hypocenter location.

#+++++++ PGA ++++++

The authors demonstrate a correlation between the corner frequency and the observed PGA. While the difference between east and west stations is analyzed, to the best of my knowledge, there is no reference to previous work relative to GMPE or other studies comparing PGA in Italy, particularly with regard to site effects contributions. This could explain the difference in corner frequency at different sites, given that I would not expect a significant directivity effect for an earthquake of Mw3.7. A promising starting point for GMPE could be the work of Bindi et al. (2010, 2011) or the work of Lanzano et al. (2011). Other examples of site effects in different tectonic environments could be the work of Bradley et al. (2018) and Singh et al. (2020).

We modified the main text referencing previous work about GMPE in Italy by Bindi et al. (L 195 - 198) and discussing site amplification as a possible cause of the fc spatial variability (L 198 – 201). We tend to disagree with the Reviewer's comment on the unexpected significant directivity effect. For instance, the work by Pacor et al. 2016 referenced in the discussion (L 198) shows directivity effects for a set of Mw = 3.4 – 4.0 earthquakes (in Italy).

#+++++++ Bibliography ++++++

Bindi, D., Luzi, L., Massa, M. et al. Horizontal and vertical ground motion prediction equations derived from the Italian Accelerometric Archive (ITACA). Bull Earthquake Eng 8, 1209–1230 (2010). https://doi.org/10.1007/

Bindi, D., Pacor, F., Luzi, L. et al. Ground motion prediction equations derived from the Italian strong motion database. Bull Earthquake Eng 9, 1899–1920 (2011). https://doi.org/10.1007/

Brendon A. Bradley, Liam M. Wotherspoon, Anna E. Kaiser, Brady R. Cox, Seokho Jeong; Influence of Site Effects on Observed Ground Motions in the Wellington Region from the 7.8 Kaikōura, New Zealand, Earthquake. Bulletin of the Seismological Society of America 2018;; 108 (3B): 1722–1735. doi: https://doi.org/10.1785/

R. Di Stefano, F. Aldersons, E. Kissling, P. Baccheschi, C. Chiarabba, D. Giardini, Automatic seismic phase picking and consistent observation error assessment: application to the Italian seismicity, Geophysical Journal International, Volume 165, Issue 1, April 2006, Pages 121–134, https://doi.org/10.1111/j.

Drouet, S., Marie-Paule Bouin, Fabrice Cotton, New moment magnitude scale, evidence of stress drop magnitude scaling and stochastic ground motion model for the French West Indies, Geophysical Journal International, Volume 187, Issue 3, December 2011, Pages 1625–1644, https://doi.org/10.1111/j.

Giovanni Lanzano, Lucia Luzi, Francesca Pacor, Chiara Felicetta, Rodolfo Puglia, Sara Sgobba, Maria D'Amico; A Revised Ground-Motion Prediction Model for Shallow Crustal Earthquakes in Italy. Bulletin of the Seismological Society of America 2019;; 109 (2): 525–540. doi: https://doi.org/10.1785/

Shri Krishna Singh, Luis Quintanar-Robles, Danny Arroyo, Victor Manuel Cruz-Atienza, Victor Hugo Espíndola, Delia I. Bello-Segura, Mario Ordaz; Lessons from a Small Local Earthquake (3.2) That Produced the Highest Acceleration Ever Recorded in Mexico City. Seismological Research Letters 2020;; 91 (6): 3391–3406. doi: https://doi.org/10.1785/

Tsai, V. C., G. P. Hayes, and Z. Duputel (2011), Constraints on the long-period moment-dip tradeoff for the Tohoku earthquake, Geophys. Res. Lett., 38, L00G17, doi:10.1029/2011GL049129.

Werner, M. J., and D. Sornette (2008), Magnitude uncertainties impact seismic rate estimates, forecasts, and predictability experiments, J. Geophys. Res., 113, B08302, doi:10.1029/2007JB005427.

Some further quick suggestions:

Need to provide **Author contributions.**
For an example, please look up a recent seismica article.
We added the following Author contributions section before Data Availability section:

Conceptualization, Formal Analysis and Methodology: Mariano Supino; *Investigation*: Mariano Supino, Laura Scognamiglio, Lauro Chiaraluce, Carlo Doglioni, Andrè Herrero; *Writing – original draft*: Mariano Supino; *Writing – review & editing*: Mariano Supino, Laura Scognamiglio, Lauro Chiaraluce, Carlo Doglioni, Andrè Herrero.

Title:
"Source characterization of the 20th May 2024 M_D 4.4 Campi Flegrei caldera earthquake through a joint source-propagation probabilistic inversion"
Modified as suggested.

Abstract and Main Text:

L12: Either 20th May, 2024 or May 20, 2024
Also,
" …. an earthquake of magnitude M_D 4.4 nucleated …"
Modified as suggested.

L20-L23: "The estimated stress drop suggests that future earthquakes in the hypocentral region, considering a possible rupture length of 3 km suggested by previous studies, can have magnitude increased by 1.2 ± 0.3 units with respect to the 20th May event."
Modified as suggested.

L23-26: "A systematic source characterization of the recent seismicity in the caldera would help in estimating the expected ground motion from future large-magnitude events."
Modified as suggested.

L33: Pointing to a supplementary Figure in the first paragraph of the introduction is not a good idea. I would still suggest moving Figure S1 to Figure 1 as another subplot. It complicates reading the article quite early.

If the authors do not want to move FIgure S1 to Figure 1, replacing "Figure S1" with the reference to the original report would be better.
We removed Figure S1 and referenced the original INGV-OV report.

Data and Method
First paragraph:
"For the analysis, we adopt the hypocenter location estimated by INGV-OV. The events in the region are routinely located using a 1D-velocity model (Orsi et al., 2004; Tramelli et al., 2021). It is noted that the uncertainty in the hypocenter location will impact the estimation of moment magnitude. We invert data from 23 seismic stations (Figure 1) from the seismic monitoring network deployed by INGV-OV, and the Accelerometric National Network (RAN) deployed by the Department of Italian Civil Protection (DPC). All data are publicly available through web

services or web pages (see Data Availability section). We use acceleration records for all the stations, except for one station CSMN for which only velocity records were available.”
Modified as suggested. We added the following (in green):
… It is noted that the uncertainty in the hypocenter location and velocity model …

L57 “... moment magnitude.”
Modified as suggested.

To avoid frequent reference to the supplementary material.

L74-77: “The forward operator along with the pre-processing and processing parameters used in the analysis closely follow Supino et al. (2019), and are also described in the Supplementary material (Text S1).”
Modified as suggested.

L77: Perhaps, a line about the site amplifications can be added here:

“Since average estimates of the source parameters are obtained using data from a large number of stations, we expect that possible site amplification effects are somewhat mitigated.”
We added the following sentence after the next period, which describes the model parameters (L 88 – 91):

We do not consider a site amplification term in the forward operator, since the inversion is performed on a single event while a set of events is needed to constrain site amplifications. Since average estimates of the source parameters are obtained using data from a large number of stations, we expect that possible site amplification effects are somewhat mitigated.

Note: I do not concur that the approach will eliminate the effects of site amplifications. Perhaps, a quick note about the site/soil conditions of the stations will be useful (perhaps, after L62). If possible, it would be also helpful if the authors can include a statement that future study will rigorously investigate the impact of site amplifications in the source characterization. There are approaches such as Generalized Inversion Technique that implement joint inversion of source, path and site (see https://www.ecgs.lu/analyzing-earthquake-ground-motions-insights-into-source-characteristics-seismic-attenuation-and-site-amplification/).
We included such statement in the Discussion section, where - following a subsequent comment by the Editor - we discuss future studies including a set of events (L 173 – 179).

L82: “ … Theoretical Green’s Function (TGF) modeling the wave propagation …” is not clear. Additionally, “(TGF)” is not needed here as it is not used elsewhere. Furthermore, do we need a direct reference here? Let us avoid frequency reference to the Supplementary material - make reading difficult.
We removed that part of the sentence and the reference to the Supplementary material.

Figure 2: Does the noise spectrum look like noise? Anyways, what is the purpose of analyzing the noise spectra? Also, how does one get noise spectra if it is estimated using the signal time window starting from the origin time, with the same length of the inverted S-wave? My apologies for not understanding the analysis here.

The noise spectrum is usually included in such figures to directly show for which frequencies the noise can affect the inverted spectrum (S- or P-wave spectrum). Here, the signal-to-noise ratio is large (e.g., SNR > 3) everywhere in the frequency domain, but this is not always the case. The noise spectrum is obtained as previously described assuming that the noise is stationary. This is a common assumption too. If the Editor would be interested, we can provide references for what has been just described.

L105: "Since the event had a shallow depth and has a high spatial resolution as recorded by 23 seismic stations, we were able to generate a dataset to describe the source (Table 1)."
We modified the sentence as follows:

Our analysis is characterized by a high spatial resolution thanks to 23 seismic stations located at a hypocentral distance of less than 9 km. This produced a dataset for the event source parameters (Table 1) that is rarely obtained for earthquakes of similar magnitude at such short distances.

L108-109: perhaps, "... the entire inverted frequency band.", instead of " the entire inverted frequency band (see Supplementary text S1)."
Modified as suggested.

L112: perhaps, " … (as shown in Figure 2c)."
Modified as suggested.

Figure 3 caption: The last two lines can read as follow:
"Colors used to plot the spectra and vertical bars are according to the inverted stations. The listed stations (in the legend) are sorted in ascending order according to estimated corner frequency values".
Modified as suggested.

L129: How does the standard deviation of Mw become 0.04 when estimates of that at individual stations range from 0.06 to 0.18? Likewise, there is some confusion about the standard deviations of the other parameters.
Please note that 0.04 is the standard error, that is the standard deviation divided by square root of the sample size ($\sqrt{23}$ in this case). The standard error is also called standard error of the mean, and is the statistical error usually associated to the mean value of a sample.

L136- 137. "We point out that the M_w value depends on the modeling assumptions as described in the following equation (also, see Supplementary Text S1)."
Modified as suggested.

L138: "(1)" instead of "(S1)"
Modified as suggested.

L139: "Among the parameters, …"
Modified as suggested.

L141: " …1D-velocity model used to locate the earthquake." instead of " …1D-velocity model currently used by INGV-OV to locate the earthquakes in the Campi Flegrei caldera (Orsi et al., 2004; Tramelli et al., 2021).".
Modified as suggested.

L146-147: "We note that a similar percentage change in the other parameters would produce a variation in Mw of less than 0.05 units."
Modified as suggested.

L159-168: If this information is important to the reader, it should be placed right in the "Introduction" section.
We moved the period in the Introduction section (L 41 – 49).

L176-178: "... 0.21), that are commonly considered in literature as the two end-members for possible values that k, and therefore, of r and $\Delta\sigma$ (e.g., Kaneko & Shearer 2014, also see Figure S3)."
Modified as suggested, with a small modification:

k = 0.21), that are commonly considered in literature as the two end-members for possible values that k, and therefore r and $\Delta\sigma$, could assume (e.g., Kaneko & Shearer 2014, also see Figure S3).

The following response may be incorporated in the Discussion section.

"The present study demonstrates the feasibility of systematic source characterizations, and supports near future studies on that line. To the best of our knowledge, volcanic observatories do not routinely estimate moment magnitudes. A possible challenge concerns earthquakes of small magnitude (e.g., M < 2), since in that case both the signal-to-noise ratio and the number of available stations with a clear S-wave arrival are expected to decrease."
Modified as suggested, with a small modification. We also added here a comment following a previous suggestion by the Editor, about the fact the future studies including a set of events will account for site amplification in the modeling.
This period has been added at the beginning of the Discussion (L 173 – 179).

Data Availability:
I do not see any advantage in having a small table in an online file when that table can be very well provided in the main text or in the supplementary material. It would not be necessary to use a data repository to store such a small table (24 rows and 7 columns). Let me reproduce a rough version here. I am still not sure why the authors would prefer to provide this information as a dataset on a repository.

What is missing here is estimates of k_0

We added the table in the main text as suggested. The table includes Q' estimates, and derived Q values if k_0 ~ 0. We believe it is important to provide the reader with these Q values as a simple reference to the anelastic attenuation resulting from the analysis.

Table 1. The source parameters estimated at the stations. The parameters are moment magnitude M_W, corner frequency f_c, high-frequency decay exponent γ, and anelastic attenuation quality factor Q.

Station	**Mw**	**std(Mw)**	**fc(Hz)**	**std(fc)**	γ	**std(γ)**	**Q**	**std(Q)**
BAIP	3.41	0.11	2.1	0.6	2.2	0.3	130	20
BAN	4	0.2	0.44	0.2	1.56	0.16	67	7
CBAC	3.79	0.15	0.8	0.3	1.97	0.18	180	40
CFMN	3.47	0.07	3.5	0.5	3.1	0.3	280	130
CMIS	3.53	0.09	2.1	0.4	2.9	0.3	126	19
CMSN	3.71	0.14	0.8	0.2	2.21	0.18	107	17
COLB	3.79	0.11	1.1	0.2	2.2	0.11	400	200
CPOZ	3.92	0.08	2	0.3	3.1	0.2	110	30
CSOB	3.62	0.09	1.6	0.4	2.1	0.2	120	30
MPCD	3.44	0.12	1.6	0.5	2.1	0.3	370	130
NAAG	3.8	0.16	0.9	0.3	1.9	0.2	54	7
NABA	3.78	0.16	1.1	0.4	1.9	0.3	110	30
NACO	3.87	0.18	0.6	0.2	1.84	0.17	84	10
NAFG	4	0.2	0.38	0.18	1.66	0.16	95	13
NAP	3.9	0.2	0.5	0.2	1.57	0.18	140	20
NAPI	3.71	0.15	0.8	0.2	2.33	0.2	150	30
POZA	3.89	0.09	1.6	0.4	2.1	0.2	66	9
POZB	3.66	0.06	2.9	0.4	2.7	0.3	95	17
POZL	3.9	0.2	0.5	0.3	1.25	0.16	66	5
POZM	3.72	0.14	0.9	0.4	1.52	0.18	53	6
POZS	3.82	0.08	2.7	0.6	2.2	0.3	54	9
POZT	3.77	0.06	2.6	0.2	3.8	0.2	140	40
POZU	3.69	0.07	2.5	0.4	2.5	0.3	110	20

Supplementary Material

Perhaps, “Supplementary Material” instead of “Supplementary Information”.

The title needs to be updated.

Modified as suggested.