Cell-Based Spectral Decomposition of Strong Ground Motion Data for Seismic Source and Attenuation Characterization in Italy

Dino Bindi [1], **Giovanni Lanzano** [2], **Sara Sgobba** [2], **Francesca Pacor** [2]

[1]Helmholtz Centre for Geosciences GFZ, Potsdam, Germany, [2]Istituto Nazionale di Geofisica e Vulcanologia INGV, Milan, Italy

Reviewer A

We would like to thank the reviewer for their comments and suggestions, which helped us improve the manuscript. Our answers to the questions raised and details of the modifications applied are provided below.

Q1. Here, in this paragraph in introduction, I would strongly suggested to authors to enrich the bibliography. Suggested bibliography is provided below and is related to some characteristic studies for several regions/countries/states world wide.I suggest this for the following reasons: In my opinion this study is on the new direction of adopting the new era of GITs and non-ergodic GMPEs, based on cell-based spectral decomposition and which is mainly implemented during the last 5 years. In my opinion the evolution on this direction is crucial to further understand issues related to the parameters that control the ground motion (Seismic Source, Path Attenuation, Site effects), reducing the uncertainties in GMPEs, making more realistic ground motion simulation, making more precise estimations of Mw, etc. I believe that enriching the already satisfied in number bibliography here in introduction, can help in navigating the seismological community into the last 5 year related studies and the future readers to follow the historically evolution of this innovative, in my opinion, step in engineering seismology. Suggested bibliography [· · ·]

A1. We thank the reviewer for their comments on the possible impacts of our study and for the suggested references. As our study does not focus on developing non-ergodic GMPEs, we have only added the two of the most recent references, i.e. Liu et al., (2025); Babič et al., (2026). These references exemplify the most recent developments. An exhaustive review of the historical developments of the non-ergodic GMPEs is provided by Lavrentiadis et al., (2023), already cited in the manuscript.

Q2. A modification of this sentence regarding the location of the reference study is suggested like: "...of a previous study

(Bindi and Kotha 2020) to resolve the inherent trade-off among the terms.

A2. We applied the modification.

Q3. What seismic efficiency means? Please define it at least with an example in a parenthesis.

A3. We added " and Savage-Wood efficiency (Savage and Wood, 1971), the latter computed as the ratio of apparent stress to the stress drop". This parameter is defined in the **Source parameters** paragraph, equation (9), alongside all the other considered quantities.

Q4. Is the selection of the S-waves based on some specific study (report or paper)? If yes, please provide it

A4. As stated in the beginning of the paragraph [In this study, we analyse ITACAext, a strong motion flatfile compiled for Italy (Lanzano et al., 2025)], the data set was prepared by Lanzano et al., (2025), who provide all the details about the processing. In our manuscript, we summarized the processing steps that we consider important for the readers, so they do not need to search for this information in another article. We added the following: "Details about the data processing and flatfile compilation are provided by Lanzano et al. (2025)".

Q5. I suggest to modify it to Figure 1a,b, if you prefer.

A5. Done.

Q6. A. Since you refer to the spectral Amplitude, FASij, I suggest to modify the word "convolution" to the word "product". The word "convolution" refers to the specific mathematical process, in time domain, between the three terms which "compose" the seismic motion. B. I suggest to modify it to just "product" instead of convolution product (related to the previous comment).

A6. Done

Q7. Please provide a more detail explanation of the G function, or simply refer to a study at which the function is provided and analyzed. If this study is the one mentioned directly below at the next sentence (i.e. the "Bindi et al 2017) I suggest to re-provide this study also to this sentence so that a reader to be sure about the use of the G function in this study.

A7. We followed the suggestion and moved the Bindi et al. (2017) reference.

Q8. I suggest you to present the corresponding subplots of Figure 2 for some characteristic frequencies (e.g. 0.3, 0.6, 1, 3 and 6 Hz) to an Appendix, or at least only the map of differences i.e. the corresponding to Figure 2c, since you already present the results for some frequencies in Figure 3 for the intersection point approach.

A8. We have added a new Figure (Figure 3 in the revised manuscript) which presents a summary of the comparison performed for other frequencies through a Taylor diagram. The caption of the new Figure 3 is "Taylor diagram (Taylor,

2001) computed by considering the intersection (reference model) and the mid-point maps for different frequencies (color points). The reference model corresponds to the point with coordinates (x, y) = (1, 0). The Pearson correlation coefficient is related to the azimuthal angle. The normalized standard deviation of the mid-point solution is given by the radial distance from the origin. The normalized standard deviation of the intersection model is 1 by construction. The centered root-mean-squared error is proportional to the distance from the reference point on the x-axis". The manuscript has been modified accordingly.

Q9, caption figure 7 (now Figure 8).A. I suggest to refer here to Eq. [7], if this is the relation used to depict these three lines. It can help the future readers to easily follow the "content" of this figure. B. I suggest you to refer to Eq.[9].

A9. Done

Q10. Please modify it to: "This is in..."

A10. Done

Q11. "agree will" ? I guess you mean "agree well." Please correct it.

A11. Done

CELL-BASED SPECTRAL DECOMPOSITION OF STRONG GROUND MOTION DATA FOR SEISMIC SOURCE AND ATTENUATION CHARACTERIZATION IN ITALY

DINO BINDI [1], **GIOVANNI LANZANO** [2], **SARA SGOBBA** [2], **FRANCESCA PACOR** [2]

[1]Helmholtz Centre for Geosciences GFZ, Potsdam, Germany, [2]Istituto Nazionale di Geofisica e Vulcanologia INGV, Milan, Italy

Reviewer B

We would like to thank the reviewer for their helpful comments and suggestions, which have enabled us to improve the manuscript. It is always a pleasure to receive such detailed revisions, particularly when they highlight unclear or inaccurate statements. Our responses to the raised questions and details of the applied modifications are provided below.

Q1. Line 17: "above 5.5"; above 5 is mentioned in the text (l. 246). Please, address the inconsistency. Also check references to threshold 5 in line 249 and 250 with respect to the red segments in Figure 7c.

We have modified the number in lines 17 and 246 to 4.6, which corresponds to $M_0 = 10^{16}$ Nm. Regarding Figure 7c of the original submission, we introduced a breakpoint at a magnitude of 5.6 in order to compare the average θ_k over the two different intervals. This was done to demonstrate that, when considering only the largest events, the average θ_k was consistent with the assumed value of -4.3 in Kanamori (1977). Therefore, the breakpoint was not intended to be located at the aforementioned threshold for the increase of $\Delta\sigma$ with M_0. We have revised Figure 7c to show the average θ_k computed over different ranges (i.e., $M_w > 5.2,\ 5.4,\ 5.6,$ and 5.8), to more clearly demonstrate the purpose of the analysis. The revised version is also shown in Figure 1 of this letter.

Q2. line 17-18: "pronounced spatial heterogeneity"; This is a strong statement, which is not supported by maximum residuals +/-1.5 MPa.

The statement at line 17-18 concerns the stress drop $\Delta\sigma$, which varies by about one order of magnitude, and not stress drop residuals.

Q3. Data: It is not clear if there was any additional processing of the ITACAext waveforms.

We have analysed the flatfile as disseminated by Lanzano et al. (2025), without applying additional signal pro-

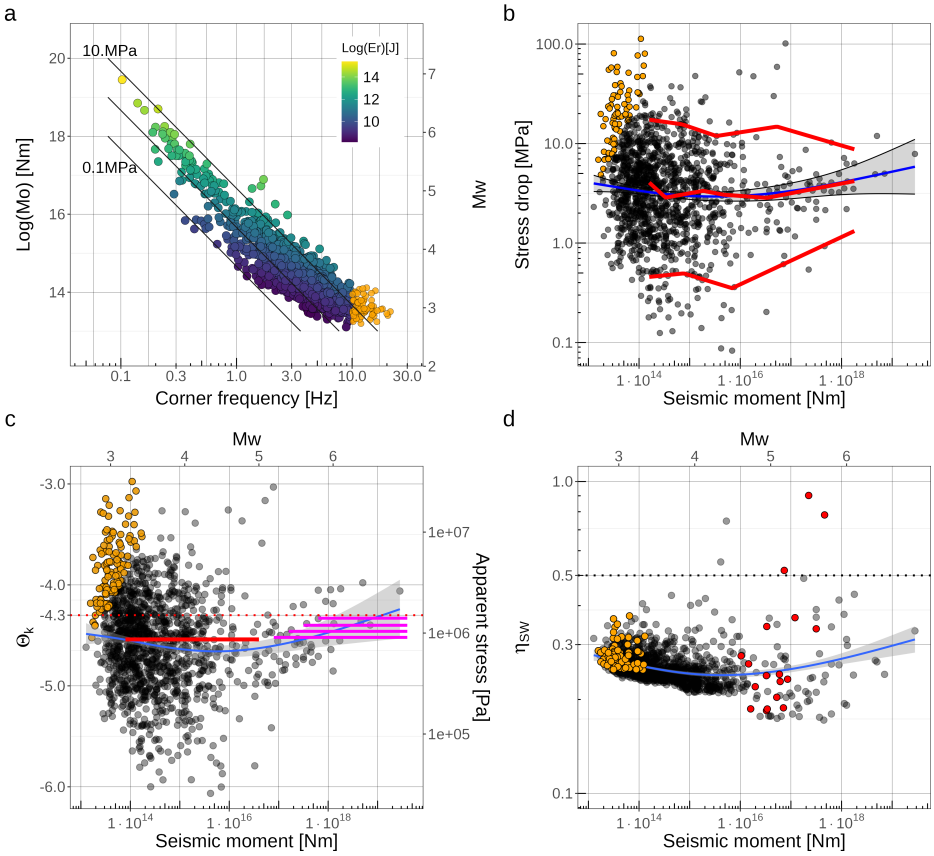


Figure 1 Revised version of Figure 7. In panel c, the magenta segments illustrate the average θ_k computed across various magnitude intervals, as indicated by the segment lengths.

cessing (apart from composing the horizontal components and applying selection criteria). Rather than referring the reader to Lanzano et al. (2025) for all details of the processing, we have summarized part of the processing workflow, focusing on those steps that we consider to be of primary interest to readers, particularly in relation to the spectral decomposition approach. This ensures that all readers are aware of the key decisions made, and those who wish to know more can consult the original paper.

Q4. Sections 3.1 and 4.1: I believe that the comparison of the mid-point and the intersection approaches adds more confusion than value to the scientific outcome of this paper. The authors did very well to test the two different approaches, but they should present just the one that they finally adopted, at least in the main text. I suggest totally removing the mid-point approach or keep it in an electronic supplement.

We believe that readers will be interested in a comparison of the results achieved using two of the most widely used cell-grid strategies. The paragraph is relatively short, but following Reviewer A's suggestion, it now provides a more comprehensive comparison across a larger number of frequencies. Therefore, we would like to keep this analysis in the main body of the manuscript.

Q5. Line 189: n1 is actually very close to 1 with the exception of the 19.9Hz where it increases notably by 20%

The increase of $n1$ for frequencies between 10 and 20 Hz is not sharp but gradual, as shown in the Figure 2. We

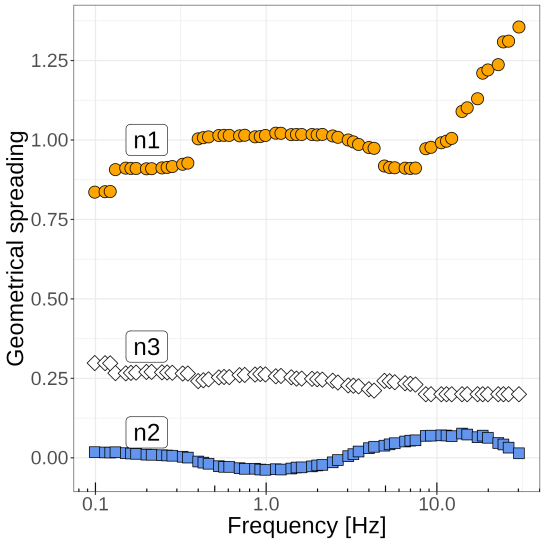


Figure 2 Dependency of the obtained geometrical spreading $n1$, $n2$ and $n3$ on frequency.

added to the text a comment about the $n1$ variability.

Q6. Figures 4 and 5: The authors chose to focus on the 19.90 Hz. However, considering that the focus of the work is the physical interpretation of any resulting spatial variability, a task that gets harder as frequency increases, and even that in Brunelli et al. (2023) "high frequencies" are 2.5-14Hz, it does not seem justified that the authors did not choose a more central frequency as representative for showing their results. Especially for attenuation, the non-representative character of the 19.90 Hz is also implied by the fact that this is the only demonstrated result with an increase in n1 of the order of 20% and as the authors very well highlight, tradeoffs between anelastic attenuation and the geometrical spreading are expected.

We selected 19.90 Hz because the impact of anelastic attenuation is expected to be more evident at high frequencies, and because the solution remains stable at this frequency. As shown in Figure 3 of the original submission (now Figure 4), which displays maps for different frequencies. As can be seen, the attenuation patterns at 10.62 and 19.90 Hz are similar in shape, with differences only in amplitude. To check the impact of selecting 19.90 Hz, we also prepared Figure 4 of the original submission (now Figure 5) also for 15 Hz, which is reported here as Figure 3. No significant differences that could affect on the conclusions of the manuscript are observed. Therefore, we will stick with the original decision to show the results for 19.90 Hz.

Q7.Lines 216-219 and Figure 5: A general good agreement of the α results with the zonation of Brunelli et al (2023) has already been made in Figures 3 and 4. Figures 5e,f do not show this any more clearly and this is still an overinterpretation of the α spatial distribution. I suggest to remove 5e,f.

We would like to keep Figure 5e,f because they allow us (and the readers) to visually compare the automatic zonation driven by the α coefficients with the Brunelli's zonation when a large number of zones are permitted. This will support future analyses comparing more spatially resolved $\delta P2P$ fields with GIT results.

Q8. Figure 6: For consistency and clarity, consider formally labeling the panels of this figure with standard letter tags

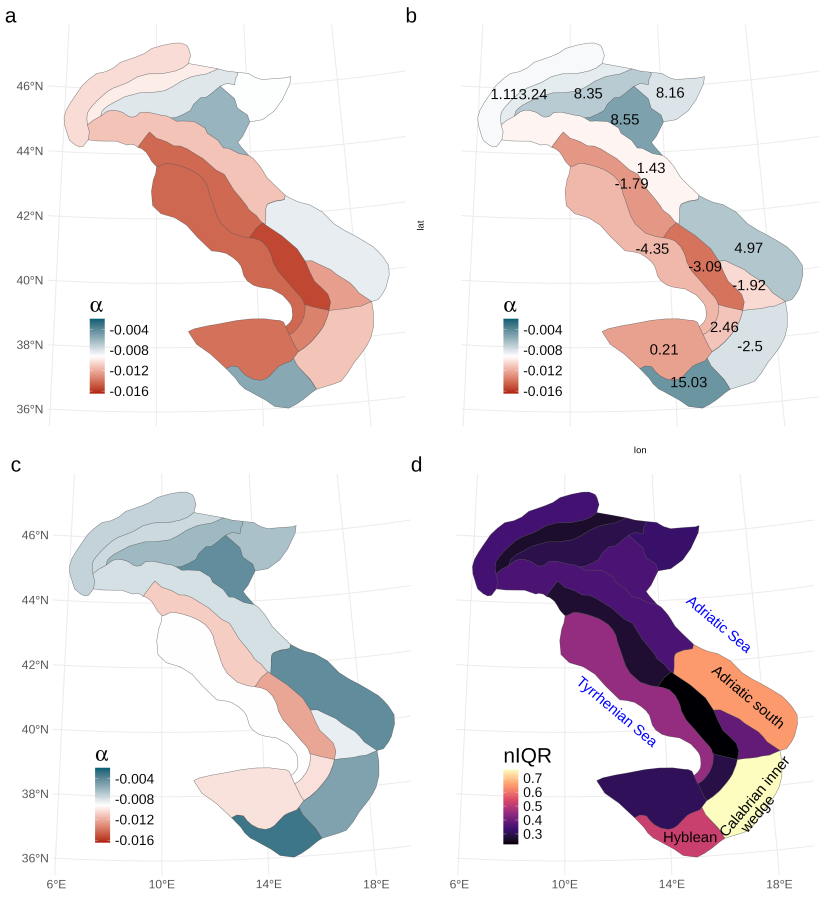


Figure 3 The same as shown in Figure 4 of the original submission but here considering 15 Hz.

rather than positional terms (left/right), and ensure these letter tags are explicitly cited in the body text whenever specific panels are discussed (l. 221; l. 230; l.237). Except from the three spectra used as constraints, selected other spectra (Aquila, Amatrice, Irpinia and Friuli) are colored but no explanation or specific discussion is provided in the text.

We modified the Figure and its caption as suggested.

Q9. 241: Please, explain why the number is smaller than the number of analyzed earthquakes (2044 in l.90).

We thank the reviewer for highlighting this inconsistency, which enabled us to adjust the initial numbers and provide further details about the decisions taken. We began the analysis by considering 2044 earthquakes labeled as SAC in the flat file. This number was reduced to 1405 once we applied a threshold of at least three recordings per station and per event. This data set was used for the spectral decomposition and to prepare Figure 1, for example. After fitting the Brune model to the obtained non-parametric source spectra, the number decreased to 1346 due to the quality-of-fit selections applied.

Q10. Figure 7: An observation in Figure 7 not discussed by the authors is the systematic change of behavior at 1 10^{16} *Nm, e.g., in panel (a) stress drop < 1MPa is practically limited – with very few exceptions - below this value; in panels (c) and (d) this is most probably where the increasing trend initiates (and to me not at magnitude 5 or 5.5) and in panel (d) only above this value the efficiency drops below 0.2. On the other hand, the sharpness of the boundary in panel (d) implies something rather computational/artificial and not physical. Still, I find this systematic change of behavior interesting to discuss and as*

a reader, I would like to see a couple of comments on this by the authors.

A clear feature of our dataset is the disappearance of very low stress-drop values (~ 1 MPa) for events with $M_0 > 10^{16}$ Nm. The extension of the stress drop distribution towards lower values for small-to-moderate events compared to the distribution for large events is a feature observed in many studies (e.g., see Abercrombie (2021)). Nevertheless, we agree with the reviewer that this observation may reflect both methodological limitations and a physical change in rupture behavior. We have expanded the discussion section accordingly and updated Figure 7 (corresponding to Figure 8 in the revised manuscript). In particular:

- From a methodological perspective, stress drop derived from corner frequency scales as proportional to $M_0 f_c^3$. Therefore, for increasing seismic moment, very low stress drops correspond to progressively lower corner frequencies, which may approach the lower limit of the usable spectral band of data and become more difficult to resolve. In addition, dynamic rupture simulations show that stress drop inferred from a single-corner-frequency Brune model does not necessarily correspond to the true static stress drop on the fault and may show much larger variability because is also affected by rupture complexity (Gallovič and Valentová, 2020).

- A physical contribution cannot, however, be excluded. Small and moderate-to-large earthquakes involve very different amounts of slip, and observations and dynamic models suggest that the dominant weakening and energy-dissipation processes may change with increasing slip (Viesca and Garagash, 2015; Gabriel et al., 2024). In this framework, ruptures occurring under very low driving stress may be more likely to arrest before reaching larger dimensions, so that very low stress-drop events would preferentially remain small.

In conclusion, although we interpret the observed lower bound cautiously as it may result from spectral-resolution limits and is based on limited data, it is also consistent with physical constraints on rupture growth which could result in a magnitude-dependent minimum stress drop for large events.

Q11. Figure 10: Please, complete the legend by removing the text in the brackets at the end and adding the description of panels d, e, f.

Done

Q12. Lines 436-437: Please add references for all data-contributing networks to ensure proper attribution, despite the already extensive reference list.

Since our starting point is the flatfile prepared and disseminated by Lanzano et al. (2025), and not the original raw data, we would prefer to avoid adding several tens of citations already available in the original article. However, like the reviewer, we believe that attribution is important. We have therefore indicated in the Data and Resources section where the proper citations of the networks can be found. This information is not strictly necessary for this study, since we started from a derived product.

Q13. Lines 438-440: I trust that the authors will release the decomposition results as described. However, I don't think that this tactic should be accepted by Seismica in future submissions and this is possibly something to be added to the instructions

to authors. It is understandable that the authors do not want to openly distribute their results prior acceptance, but several repositories – including Zenodo that the authors wish to use – provide the possibility to set a repository to restricted access. The reviewers should have access to all results linked to a submission.

We confirm that the results of the decomposition (source and site spectra, attenuation spectral model and source parameters) will be disseminated via Zenodo. Once published, the already reserved DOI will be 10.5281/zenodo.22734257. Regarding the policy, we do not believe it should be mandatory for authors to share the link during the revision process in all cases. It should be up to the authors to decide whether the electronic results are essential for the revision process or not (as we judged in our case). Furthermore, if reviewers need electronic files for the review, they can always ask the editorial board to forward their request to the authors.

Q14. The authors state in their introduction that one of the main targets is the "physical interpretation and validation" of path and source-related terms". Although the conclusive discussion adequately addresses the physical causes of the spatial variability of attenuation, the stress drop spatial variability is not discussed. The authors should provide some comments on this, as well.

The interpretation of the spatial variability of the stress drop results (as derived from seismic moment and corner frequency) is always difficult for several reasons:
1) Large datasets (e.g. from several studies in California) show that spatial stress-drop patterns can be coherent over extremely variable scales, ranging from a few kilometres to entire fault systems (studies from Shearer, Trugman, Hardebeck, Abercrombie, Hauksson, among many others). Therefore, stress drop is not uniform across a fault system, earthquake sequence or crustal volume. Stress drop, apparent stress and seismic efficiency vary according to fault strength, effective stress, structural complexity, depth, lithology, stress orientation, rupture geometry and the local stage of the seismic cycle. A physical interpretation should therefore combine source parameters with focal mechanisms, fault geometry, depth, geodetic deformation and independent information on fluids or fault slip. However, this wide-spectrum analysis is well beyond the scope of our study.
2) Some general patterns can be identified in previous studies. For example, high stress drops commonly occur near locked or strongly loaded fault patches, geometric intersections, rupture barriers, strong host rocks, and complex fault segments. Low stress drops are often associated with creeping or damaged regions, fluid-pressurized volumes, shallow low-strength crust, or areas affected by aseismic deformation. Moreover, depth and faulting style can modify these patterns through effective normal stress and the ambient stress field. But when detailed analysis are performed for specific fault systems using high resolution data sets, then the stress drop show a variability at a finer scale. For example, the San Gorgonio Pass provides an example of spatially organized heterogeneity. High stress drops occur in a structurally complex region between major strands of the San Andreas system, while values are lower away from the pass and tend to decrease with increasing geological slip rate (Goebel et al., 2015). The result supports a connection between stress-drop heterogeneity and distributed deformation, fault geometry, host-rock composition, and the maturity of slip zones rather than being controlled by a single factor. In Italy, several studies have obtained the largest stress drops for earthquakes in the eastern sector of the Alps, which is characterised by a thrusting mecha-

nism. When dense local datasets for this area were analysed, e.g. (Cataldi et al., 2026), it was indeed found that stress drops were spatially heterogeneous, reflecting the presence of a fault system composed of segments with different mechanical characteristics. Another example is the Irpinia fault system, where the 1980 M6.9 earthquake occurred. We obtained the highest stress drop for this earthquake compared to other mainshocks that occurred over normal faulting systems in the central and southern Apennines. For the Irpinia fault system, analysis of a dense local dataset collected by a near-fault observatory also suggested the presence of adjacent faults characterised by different levels of locking and creeping (Picozzi et al., 2021).

3) Another difficulty arises from the fact that there is no universally accepted mainshock–aftershock relationship. Stress drop can vary substantially during individual sequences. For example, the 2016–2017 Central Italy sequence shows an increased stress drop following the largest earthquakes, as well as concentrations of high-stress-drop events around structurally complex fault zones. However, a detailed study of the L'Aquila nucleation volume found no systematic difference between foreshocks and aftershocks. When small events in California were analysed to identify spatial patterns that could be used to predict those of large events, no satisfactory coherence was found (Hardebeck, 2020).

4) Taking the previous points into account, it can be concluded that a detailed interpretation of the spatial variability of stress drops would require a joint analysis of several studies performed using dense networks. Unfortunately, stress drop estimates are affected by large uncertainties relating to data availability and quality (e.g. limited bandwidth, signal-to-noise ratio at high frequencies and network coverage at short distances), as well as to model assumptions, particularly those used to isolate the source spectra from attenuation and site effects. Even when the precision of stress drop estimates within a study is good, their accuracy could be very low, e.g. (Bindi et al., 2023; Pennington et al., 2021). Absolute stress drop values are difficult to compare between different studies, even when they are performed in the same area (SCEC benchmark, (Abercrombie et al., 2025)). The non-uniqueness of the inverse problem solved to obtain the source spectra, and consequently the source parameters, and the significant influence of model assumptions on the final results make it difficult to compare the results from different studies.

Therefore, given the aim of the study and the efforts required for a detailed interpretation of stress drop spatial variability at the Italian scale, we decided to include only a few general considerations on stress drop spatial variability in the manuscript, considering also the conclusions from previous studies.

Q15. Lines 395-397: This is demonstrated in only very few cases. The authors should at least re-assure the reader that the comparison was made to a much larger number of sites.

We can confirm that the statement is based on a much larger number of checks. We have modified the last line of the Site amplification results section.

List of typos

We corrected them all. We appreciate the reviewer putting so much care into reviewing our manuscript. Thanks.

References

R. E. Abercrombie. Resolution and uncertainties in estimates of earthquake stress drop and energy release. *Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences*, 379(2196), 2021. doi: https://doi.org/10.1098/rsta.2020.0131.

R. E. Abercrombie, A. Baltay, S. Chu, T. Taira, D. Bindi, O. S. Boyd, X. Chen, E. S. Cochran, E. Devin, D. Dreger, W. Ellsworth, W. Fan, R. M. Harrington, Y. Huang, K. B. Kemna, M. Liu, A. Oth, G. A. Parker, C. Pennington, M. Picozzi, C. J. Ruhl, P. Shearer, D. Spallarossa, D. Trugman, I. Vandevert, Q. Wu, C. Yoon, E. Yu, G. C. Beroza, T. Eulenfeld, T. Knudson, K. Mayeda, P. Morasca, J. S. Neely, J. Roman-Nieves, C. Satriano, M. Supino, W. R. Walter, R. Archuleta, G. M. Atkinson, G. Calderoni, C. Ji, H. Yang, and J. Zhang. Overview of the SCEC/USGS Community Stress Drop Validation Study Using the 2019 Ridgecrest Earthquake Sequence. *Bulletin of the Seismological Society of America*, 115(3): 734–759, 2025. doi: 10.1785/0120240158.

D. Bindi, D. Spallarossa, M. Picozzi, A. Oth, P. Morasca, and K. Mayeda. The community stress-drop validation study—part II: Uncertainties of the source parameters and stress drop analysis. *Seismological Society of America*, 94(4):1992–2002, 2023.

L. Cataldi, M. Picozzi, M. D'Amico, P. Morasca, D. Bindi, V. Poggi, G. Costa, A. Viganò, and D. Spallarossa. Stress drop and seismic efficiency distributions over the Adriatic indenter (European Southeastern Alps). *Journal of Geophysical Research - Solid Earth, accepted*, 2026.

A.-A. Gabriel, D. I. Garagash, K. H. Palgunadi, and P. M. Mai. Fault size–dependent fracture energy explains multiscale seismicity and cascading earthquakes. *Science*, 385(6707):eadj9587, 2024. doi: 10.1126/science.adj9587.

F. Gallovič and L. Valentová. Earthquake stress drops from dynamic rupture simulations constrained by observed ground motions. *Geophysical Research Letters*, 47(4):e2019GL085880, 2020. doi: https://doi.org/10.1029/2019GL085880.

T. Goebel, E. Hauksson, P. Shearer, and J. Ampuero. Stress-drop heterogeneity within tectonically complex regions: a case study of san gorgonio pass, southern california. *Geophysical Journal International*, 202(1):514–528, 2015. doi: 10.1093/gji/ggv160.

J. L. Hardebeck. Are the stress drops of small earthquakes good predictors of the stress drops of moderate-to-large earthquakes? *Journal of Geophysical Research: Solid Earth*, 125(3):e2019JB018831, 2020. doi: https://doi.org/10.1029/2019JB018831.

H. Kanamori. The energy release in great earthquakes. *Journal of Geophysical Research*, 82(20):2981–2987, 1977. doi: 10.1029/JB082I020P02981.

G. Lanzano, L. Vitrano, D. Bindi, C. Felicetta, S. Sgobba, L. Luzi, and F. Pacor. Itacaext 2.0: A high-quality parametric table of strong-to-weak motion recordings for seismological and engineering research in italy. *Seismological Research Letters*, 96(5):3295–3316, 2025. ISSN 0895-0695. URL https://doi.org/10.1785/0220240309.

C. N. Pennington, X. Chen, R. E. Abercrombie, and Q. Wu. Cross Validation of Stress Drop Estimates and Interpretations for the 2011 Prague, OK, Earthquake Sequence Using Multiple Methods. *Journal of Geophysical Research: Solid Earth*, 126(3):e2020JB020888, 2021. doi: https://doi.org/10.1029/2020JB020888.

M. Picozzi, D. Bindi, G. Festa, F. Cotton, A. Scala, and N. D'Agostino. Spatiotemporal Evolution of Microseismicity Seismic Source Properties at the Irpinia Near-Fault Observatory, Southern Italy. *Bulletin of the Seismological Society of America*, 112(1):226–242, 2021. ISSN 0037-1106. doi: 10.1785/0120210064.

R. C. Viesca and D. I. Garagash. Ubiquitous weakening of faults due to thermal pressurization. *Nature Geosci*, pages 875–879, 2015. doi: ttps://doi.org/10.1038/ngeo2554.

Cell-Based Spectral Decomposition of Strong Ground Motion Data for Seismic Source and Attenuation Characterization in Italy

Dino Bindi [1], **Giovanni Lanzano** [2], **Sara Sgobba** [2], **Francesca Pacor** [2]

[1]Helmholtz Centre for Geosciences GFZ, Potsdam, Germany, [2]Istituto Nazionale di Geofisica e Vulcanologia INGV, Milan, Italy

Reviewer B

We would like to thank you and the reviewer for the additional corrections to our manuscript that we have implemented. Once again, we would like to express our appreciation for the care that the reviewer put into checking our manuscript.

Comments/Questions

The magnitude threshold remains "Above magnitude 5" in the Conclusions. Please harmonize (4.6 everywhere else).

We harmonized the magnitude values.

257 states a median of 3.39MPa, but Conclusions state a median of 3.64. θ_k values in the Conclusions also differ from those in section 6. Please, harmonize these values, as well.

We harmonized the θ_k values.

The reduction from the 2044 SAC events of Lanzano et al. (2025) to the 1405 finally used events could be stated explicitly in the Data section for the readers.

Done.

227: Figures 5e,f –> Figure 6e,f. Caption of Figure 7: "2009 Norcia" –> "2016 Norcia".

Thanks for spotting the mistakes, we corrected them.

About the network references (Comment 12 of my first-round review): I understand the authors' argument that they start from a derived product, but I still believe it is important to acknowledge the networks that constantly struggle to demonstrate

the usability of their data. I maintain that they should be cited via their FDSN DOIs, but I leave the final decision to the Editor.

While we disagree with the reviewer's suggestion that any study using products derived from analysed measurements in earlier studies should refer back to the original sources, which is often impractical, we were happy to provide full citations of the registered networks considered in the previous study when compiling the selected portion of the flat file. We would expect KPIs for data services to be connected to the number of accesses and volumes downloaded. Citations of direct data usage should always be mandatory for any publication. Whether to cite the services when using derived products from earlier studies should, however, be a decision taken by the authors on a case-by-case basis.

241-244: Please check the k0 values. I suspect they should be 0.011 and 0.020.

Thanks for spotting the typos, now corrected.

Please correct everywhere in the text and the References' list: Atik –> Al Atik.

Done

Indices i/j are still inconsistent: e.g., i for event at l.103, but for station at l.114 and l.117.

Thanks for spotting the typos, now corrected.

117: along of the segment –> of the segment. 169 : follow –> follows. 401: limitations –> limitation. 413: Apulien –> Apulian.

Done

Please rephrase at l.421: "how coherent stress-drop patterns can be over extremely variable scales" e.g., "how stress-drop patterns can be coherent over extremely variable scales".

Done

Caption of Figure 8: corner a –> a corner

Done