Solid Earth Discussions

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Comment on se-2021-88

Anonymous Referee #2

Referee comment on "A functional tool to explore the reliability of micro-earthquake focal mechanism solutions for seismotectonic purposes" by Guido Maria Adinolfi et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-88-RC2, 2021

Manuscript by Guido Maria Adinolfi et al. attempts to provide methodology on how to explore the reliability of focal mechanisms inverted from events with generally small magnitudes. Using empirical data from Southern Italy they explore influence of different path- and network-geometry related parameters on stability of focal mechanism estimates.

From technical side, the manuscript by Guido Maria Adinolfi et al. is logically and well written. The introduction section providing general overview of moment tensor/focal mechanism inversion methodologies and related pitfalls could provide more complete overview of existing methods. The amplitude inversions are entirely missing, and the overview lacks of discussion of methods that accounts for some of the problems listed in the manuscript as factors contributing to inversion reliability. The methodology side discussing focal mechanism inversion and sampling/modelling/reliability assessment procedure could be in my opinion shortened, as vast majority of the text related to the focal mechanism inversion were already published in references manuscript. At the same time, the description of modelling procedure could be presented more clearly. The results and discussion parts are clear and to the point, as well as conclusion section (with major remarks regarding scientific content following below). Regarding the graphical presentation, the major issue I have is the use of colormaps that are not suitable for color-blind people. I made a reference in the detailed comments and ask the authors to replace the jet colormap with selected perceptually uniform one. Apart from the, the figures are clear and to the point.

As to the scientific content I have one major comment that I believed should be addressed somehow, or the Authors should consider reviewing the scope of the manuscript in the abstract and main text. The point is that the proposed parameters (and their range) tested in the simulation are very focused on the actual case study and they hardly allow to use this potentially interesting outcomes anywhere else in the other than conceptual way. As such, the manuscript presents the way the Authors dealt (nicely) with the reliability problem for THEIR case scenario, but in my opinion they failed a bit to provide a more general framework that could be actually useful for a broader audience. For example, the influence of magnitude on reliability of focal mechanisms is discussed by considering events with M1 M2 and M3. It is fine as such, but the magnitude is in fact a very indirect proxy influencing the quality of focal mechanism. The magnitude drives i.a. amplitude of ground motions including first pulse signal-to-noise ratio, but it is also a function of distance/depth and some other factors that ultimately drives whether the particular polarity is actually detected or not. So in my opinion, more general approach would be to prospect how the number of polarities that are well spread on the focal sphere (see next point of my argumentation) would affect the reliability of focal mechanism. Another example: The Authors present that outside of seismic network the reliability of focal mechanisms is decreasing dramatically. This is of course not a big surprise, but again Authors could in my opinion do better and more general here. Instead of barely presenting these outcomes in a form of maps showing strong deviations on edges of seismic network, they could actually test using their empirical data how the uniformity (i.e. azimuthal or takeoff-gap or both together!) of the focal sphere coverage affects the reliability of focal mechanism estimation. This could be achieved by cherry-picking the stations to enhance/limit the coverage of the focal sphere. My experience from the small scale seismicity is that the uniformity of coverage and number of stations are ultimately key factors for reliable solutions. Using these factors would make the presented case more generic.

To summarize, I like overall the idea and empirical approach. However, I approached the manuscript with strong expectations of seeing a more generic approach to the problem of testing of reliability of focal mechanism. At the moment, the manuscript is more a case study, providing at the same time some conceptual framework, 'food for thoughts' as I would say, for the readers. I do believe the Authors could enhance its attractiveness/usability/generatily by discussing 'simulation' data using more generic range of parameters/variables, as proposed, but not limited to, to the ones discussed above.

Detailed comments.

P2 L64-66. I note here there is missing overview paper by Bentz at al. [1] that discusses peculiarities of moment tensor inversion for small events while using full waveform and amplitude approaches.

P2 L64-66. I would update paper with some newer references for the full-waveform inversion, for example Kiwi tools is missing, to name a few.

P3 L67-83. This part tackles problem of focal mechanism inversion from very general perspective. However, the description is at the moment not complete and must be updated. For example, P-wave amplitude inversion methods which are very popular in the magnitude range of concern in this study are not mentioned. Example of methods and software packages include PCA approach [2], source-function deconvolution (papers from Jan Sileny et al. from Prague). Moreover, as this and later part of introduction discuss external factors influencing quality of MT/focal mechanism inversion, there are already existing methods addressing these problems that could be referred to as well (e.g. [3], to name a few!). Please provide up-to-date description of the state-of-the-art in the field then.

P3 L109-111. PCA approach is good solution here and should be mentioned [2] in my opinion.

Introduction part as a whole: You are not mentioning explicitly the potential biases related to double-couple model assumption. Many small earthquakes are in fact not following double-couple especially in case of induced seismicity (that you incoroporate in your manuscript in discussion). I believe this potential modelling bias should be mentioned as another source of errors with appropriate referencing.

L101-113 In general, some of your parameters can be consider a "quality of coverage of hypocenter with seismic stations with sufficient signal-to-noise ratio". I feel this part is a bit too much of the text overall.

Method:

L131-133. Any references? What magnitude range? This overall sounds to me a bit bold statements, or maybe that refers to larger magnitude events (?). My experience that is largely related to analysis of focal mechanisms of very small events is that S/P amplitude ratios are actually very hard to constrain manually.

L140 and following. Do we really need this very detailed description of BISTROP code? I think you could easily skip the very first part by referring to key input and outputs of the code and focus more on last part, which is typically less familiar to the reader. It is quite unclear actually how the bayesian framework was use with respect to quality criteria parameters you used (kagan angle and others).

L264-267. I agree empirical approach pursued here is better, but in general case approaches could be used. See [4-5] and some nice references therein that cover problems of detection using empirical data as well.

L278. I believe the "synthetic" word might cause a confusion, as it suggests that you generate or model something from scratch. In fact, as I understand you resample your empirical polarity data to create "new" events taking into account different constraints posed beforehand, at least this is how I understand it. All in all, maybe revise this part to make it more understandable (bullet points? scheme? chart?)

L296. It is actually not clear (or I missed that) where this Gaussian noise is applied (to the P/S ratios?), and if the latter is the case, how it is actually applied (formula?)

Figure 1. No axes labels!

Figure 3. Maybe some scale just to give impression how big the area is. Axis labels missing, e.g. "grid points along latitude"

Figures 4,5,6... Axes' labels missing. As you want to optimize the space (which is fine), why don't you just mention them in caption what is the horizontal and vertical direction.

Figures 4,5,6... please DO replace 'jet' colormap with ANY color-blind friendly colormap in all plots. For a general guidance, this paper is a treasure: https://www.nature.com/articles/s41467-020-19160-7

L335 Kagan angle cannot be negative (?)

L378 Typo

L377 'is less' or 'is smaller' ?

References

[1]Bentz, Stephan, P. Martínez-Garzón, G. Kwiatek, M. Bohnhoff, and J. Renner (2018). Sensitivity of Full Moment Tensors to Data Preprocessing and Inversion Parameters: A Case Study from the Salton Sea Geothermal Field. Bull. Seismol. Soc. Am. 108, 588–603, doi 10.1785/0120170203

[2]Vavrycuk, V., P. Adamova, J. Doubravová, and H. Jakoubková (2017). Moment Tensor Inversion Based on the Principal Component Analysis of Waveforms: Method and Application to Microearthquakes in West Bohemia, Czech Republic. Seismological Research Letters 88, 1303–1315, doi 10.1785/0220170027

[3]Kwiatek, G., P. Martínez-Garzón, and M. Bohnhoff (2016). HybridMT: A MATLAB Software Package for Seismic Moment Tensor Inversion and Refinement. Seismol. Res. Lett.

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[6]Stierle, E., M. Bohnhoff, and V. VavryÄ□uk (2014). Resolution of non-double-couple components in the seismic moment tensor using regional networks—II: application to aftershocks of the 1999 Mw 7.4 Izmit earthquake. Geophys. J. Int. 196, 1878–1888, doi 10.1093/gji/ggt503

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