Dear Dr. Meng Wei, the Handling Editor at Seismica

We sincerely thank you and the two anonymous reviewers who took the time to provide many useful comments on our paper. Based on the reviewers' comments, we revised the text to improve clarity and readability, substantially enhancing the scientific style and English in several sections with the assistance of AI. We also added the requested supplementary calculations, updated the figures with larger fonts and improved color scales, and incorporated Table 1. Table 1 provides uncertainty quantification of non-DC components of all 25 processed events. Below, we address the comments in detail, marking our response in bold.

Reviewer A:

This paper describes a method to invert seismic waveform data for full (six degrees of freedom) moment tensor. The method is a slight modification to a previously published method, in which the authors incorporate a station weighting scheme to account for data/model uncertainty. The method is then applied to a the 2025 Anydros earthquake "swarm". I will provide specific comments on the uploaded and marked-up PDF attached to this review.

Some general comments:

1) The paper feels a bit like two separate papers. The first half is the description of the new method and the second is the use of the method to interpret a geologic scenario.

Our intention was indeed to present both the method and its application. However, we had mistakenly opened the introduction with a paragraph providing a geological overview. This paragraph has now been correctly relocated to the application section.

2) Although point 1 (above) in itself isn't a deal-breaker (or even that unusual), I feel that given the emphasis of the authors' new-ish method of incorporating/estimating uncertainty isn't being used in the interpretations. Uncertainty estimation in moment tensor estimation is (in my view) woefully unaccounted for in many, many (MANY) papers, and any research addressing a viable, efficient, *defensible and data-based* this should be applauded.

Thanks for your support. Yes, it is particularly alarming when uncertainty is ignored in agency reports. This was one of the main stimuli of our paper.

3) (=2 continued) In this regard, I think that this contribution is worthwhile. However, it seems that the uncertainty is completely ignored in the analysis/interpretation of the geologic scenario. I'd encourage the authors to incorporate, or tie in, a more complete assessment of the uncertainty into the geologic interpretations.

This was easy. Already before the initial submission of this paper, we calculated all 25 events in the same way as the two presented “examples”. Accordingly, one-sigma confidence intervals of ISO, CLVD, and depth were already available for all events. These were not included in the initial submission because they were part of our earlier extended abstract presentation. Now, we present the uncertainties in a newly added table (Table 1). In this way, the non-DC uncertainty in the application is fully quantified, as requested.

4) I'm afraid I had a really difficult time understanding how the authors' method actually works. I found the description of the method confusing and poorly organized, with many of the key concepts (e.g. NBEST, NPER) not well explained. Can these terms/concepts be better explained? A more rigorous and thorough explaination of the station weighting term (W) would be helpful as well. From what I can tell, the term W is drawn from a distribution, Monte Carlo style? Is this correct? And what exactly is meant by the term 'position' (is it depth?). Please see the marked up PDF for details of my concerns.

The comments you provided in the PDF effectively guided our revision. We addressed more than 90% of them, which resulted in an almost complete rewrite of the manuscript, though without any change to the underlying scientific results.

Regardless of my concerns, I think the authors are on the right track with this: accounting for and estimating uncertainty in moment tensor inversions. If the text is cleaned up, better organized, and with many more details included, I think this paper would make a worthwhile contribution to the literature.

Recommendation: Resubmit for Review

Reviewer B:

The manuscript presents an extension of the (free, open source) ISOLA software for MT inversion, now including a bootstrapping approach used to analyze uncertainties. After a description of the methodology, the authors present detailed example inversions for two earthquakes of the 2025 Anydros crisis, focusing on the uncertainty estimates of the non-DC components. Subsequently, they apply the method to 25 events in the sequence and describe the observed rupture mechanisms, including crack-opening/closing components.

ISOLA is a well-established software, used for routine MT inversions in many places. I highly value the presented extension and hope that many more researchers and catalog providers will, in the future, include the uncertainties in their moment tensor bulletins!

The manuscript is well-structured, well-written, and suitable for publication in Seismica. I only have a few minor, mostly very minor, comments, questions or suggestions:

- I think the application you show uses only time-domain waveform fitting, right? Is the bootstrapping also available for other input types? I think ISOLA can also combine waveforms with first motions or envelopes, correct? If so, are the same weights used for all input types at each station?

Unfortunately, not. The current ISOLA BaBoo code version inverts only waveforms.

- Is there, in addition to the bootstrap chains with the random weights on the station, one "global" chain in which all stations are weighted the same? A comparison of the mean or median solution with the "global best" solution could also help provide a first glance at stability.

Our practice is that before any run of BaBoo, we always perform a standard inversion without random station weights. Thus, in the first example (event No 19), we have now added a comparison of Baboo confidence intervals of ISO and CLVD with the values obtained when weighting all stations equally, saying: "Finally, to provide a first glance at stability, we compare the resulting one-sigma confidence intervals of ISO% (34, 52) and CLVD% (23, 47) in this example (Fig. 4) with a test in which, instead of BaBoo, we weighted all stations equally and did not analyze uncertainty. In such a test with full MT we obtained ISO=44% and CLVD=48%. "

- It may be an issue with our printer, but the color scale in Figs 1-2 is difficult to see, especially the yellow.

We provided new figures with larger fonts and better color scales.

For the comparison of the event with the solution obtained with Grond (Fig 5), I was wondering how the DC compares? Maybe you can mention the Kagan angle?).

Kagan angle deviation of the DC part of MT between Fig. 5a (GROND) and the median MT of Fig. 4a is 13°. This information was included in the caption of Fig. 5. However, this information has a limited value because the DC part varies within BaBoo perturbations; then, the Kagan angle can be (on average) about 30°. We do not discuss the variability of the DC component in greater detail, as doing so would require substantial additional material and lengthen the manuscript.

The inset in Fig 7a is very, very small. But there is sufficient space to the sides, so maybe move next to the Figure?

We submit a better version of Fig. 7 and modify the caption accordingly.

- In Fig. 7A, it would improve visibility if you used a different color for the nodal lines on the beachballs. Especially for the inverse "fried eggs" types, it could help. Or have the nodal lines in black and a dark-gray fill color?

Nodal lines in Fig. 7 were not plotted in the first submission. They are also not plotted in the present revision because the DC part of MTs would require additional explanations, lengthening the manuscript.

- In the discussion, you (correctly) point out the issues with constraining the inversion to a deviatoric MT. However, the nearly zero CLVD component in the GCMT solution is intriguing. This raises several technical questions: 1) Are your double-couple (DC) component and that of GCMT equivalent, and can this be quantified? 2) If you constrain your inversion to a deviatoric MT, does it also yield a similarly small CLVD? 3) To what extent does the variance reduction (VR) or waveform misfit improve when using a full moment tensor (MT) inversion versus a deviatoric inversion?

This discussion is about Event no. 1 of Table 1. Our strike/dip/rake = 235°/39°/-81°. GCMT provides 246°/40°/-72°, so their Kagan deviation is very small, just 7°. As requested, we also calculated (non-BaBoo) MTs in three modes: Full MT: ISO% = 31, CLVD%= 7, VR=0.73, deviatoric MT: ISO% = 0, CLVD%= -20, VR=0.71, and DC-constrained MT: ISO% = 0, CLVD%= 0, VR=0.71. Analogously to (deviatoric) GCMT, our deviatoric MT also yields low CLVD, i.e., large DC%=80% at nearly the same waveform fit. While full MT cannot be preferred based on its VR value, as in many other similar situations, full MT is generally preferable, because when an event contains an ISO component, the deviatoric assumption is not valid, so CLVD% and DC% are biased.

A one-sentence summary of this has been included in the Discussion: "When we calculate deviatoric MT for event No. 1, similarly to GCMT, we also obtain a large DC% = 80 at nearly the same variance reduction as that of full MT. "

Inspired by this comment, we also made a similar comparison for Example 1 (event No. 19). Full MT: ISO% = 44, CLVD%= 48; Deviatoric MT: ISO% = 0, CLVD%= 12. Again, the deviatoric solution is considerably biased. This test was not added to the text to keep it simple.

- In my opinion, the discussion of the two hypotheses for the driving mechanism of the sequence is a bit long (and unnecessary), given that with the 25 MTs, you do not add any conclusive information to the debate. Note also that the recently published Lomax et al. (Science) report that changes in failure stress suggest that the unrest was caused by pump-like magma intrusions into newly opened dikes 12 kilometers below the seafloor, in good general agreement with Isken et al. Therefore, in my opinion, the wording "The hypothesis of a dike intrusion cannot be completely ruled out…" (line 428) is a bit strong. Having said this, it is of course fully up to the authors to ignore

The remark is good and should not be ignored. We accept a dike as a possible triggering mechanism of seismicity, but this phenomenon does not explain non-DC components. We modified the sentence as follows: "Analogously, Karakostas et al. (2025) and Lomax et al. (2025) proposed a thin vertical tensile source (a horizontally propagating dike) and its Coulomb stress effect as a possible trigger of the massive seismicity beneath Anydros. Anyway, a physics-based model explaining the non-DC MT components near Anydros is still lacking, and the mechanisms by which individual normal-fault ruptures might incorporate an opening component remain unresolved."
In the Abstract, we did this modification: "We preliminarily interpret the observed moment tenors as pointing to a fluid-assisted rupture process in a complex network of tectonic faults, likely triggered by a dike emplacement."

Recommendation: Accept Submission
