

Response to Reviewers: Plourde and Gosselin (2026), Seismica, v1

Thanks to all the reviewers for the insightful and constructive comments. Please note line numbers throughout this response letter refer to the updated (not track-changes) version of the paper.

NRCan Internal Reviewer

Ramin Mohammad Hosseini Dokht, Geological Survey of Canada - Pacific

I do not have any major comments. I just have a minor note regarding Section 3 (Effect of bounds on the RVDM): it may be worth explicitly mentioning that kurtosis is a measure of the peakedness of a distribution and remains around 3 for a normal distribution.

We have added a sentence (line 141) "Kurtosis measures the prominence of outliers in a distribution, where normal distributions have values ~ 3 , higher values indicate more extreme outliers, and vice versa.", and made a few other small changes in the first paragraph of this section to accommodate that addition.

Additionally, the section could conclude more explicitly that the RVDM is effectively a small-angle approximation and remains reliable for most seismological applications. After all, focal mechanism uncertainties are rarely large enough to approach the 120-deg limit. As shown in Figure 2b, for uncertainties larger than ~ 30 deg, the tails of the distribution begin to interact with the physical 120 deg boundary, which leads to an underestimation of uncertainty (as illustrated in Figure 2a).

We have added the following short paragraph to the end of this section (line 155). This test highlights that the RVDM is a small-angle approximation and will describe the FM PPD most effectively when $\sigma \lesssim 30^\circ$. In the following section, we examine PPD misfits for synthetic FM inversion using P-wave first-motion data. We find that, for P-wave first-motion FMs at least, uncertainties $> 30^\circ$ are actually quite common. One main argument we make is that, even in these cases, using the full RVDM is still a big improvement vs. using an even simpler metric.

Reviewer A

This paper introduces the rotation vector distribution matrix (RVDM) for representing focal mechanism (FM) uncertainty in terms of a covariance matrix of the three components of a rotation vector acting on the best-fit FM. Synthetic catalogs are used to test the RVDM methodology, both to test how well the RVDM represents the focal mechanism uncertainty, and how the FM uncertainty representation affects the stress tensors obtained from focal mechanism inversion. The method is then applied to FMs and stress inversions for real earthquakes.

The RVDM seems like a reasonable, compact way to represent the FM uncertainty. However, I didn't find that the reported tests made a very strong case that it's a significant improvement over a simpler representation of the FM uncertainty with a single rotation angle, at least for stress inversion applications.

We suggest that the results in Section 5, specifically Figures 5a and 6 and Table S1, demonstrate conclusively that the RVDM leads to a statistically significant improvement over simpler representations of FM uncertainty w.r.t. to stress inversions, and that the first inversion example of real data from Yukon (in Figure 8) is an illustrative example. In response to your third point below, which raises an important issue that we had not previously addressed, we discuss the specific way it reduces stress uncertainties vs. the UNI model.

1. The examples in Figure 3 show that the reported FM uncertainty (sigma) from the RVDM method is much larger than the actual FM error (Kagan angle). This is just a single event, but it suggests that either the inversion method or the RVDM method may overstate the FM uncertainty. Please show sigma versus Kagan angle for the full synthetic dataset to see if this is the case.

The example in Figure 3 has “artificially” low error/uncertainty ratios because (for this figure only) we have ensured all polarities are correct, but still assigned them uncertainty. We have added a note of this to the figure caption (page 10) : "All first-motions are consistent with the FM in these preliminary examples, but they are still assigned uncertainty (this contributes to high σ to θ_K ratios)".

We have added a new SI Figure S3 examining θ_K vs. σ (misfit vs. uncertainty) for the full synthetic dataset from Section 4.2. As we now state in Section S3, we find that σ is "...slightly over-reported relative to noise levels in the input data and that synthetic first-motions are correct slightly too frequently, but we consider them to fit reasonably closely. As described in Section 2.1.1, difficulty achieving this balance (or, more generally, selecting a noise amplitude σ_i) is a drawback of the (Pugh *et al.*, 2016) Bayesian formulation (Eq. S3)." When we first made this figure with the results from our initial submission, the θ_K distribution was further below the the 1:1 line; this older plot is included here for comparison. Observing this discrepancy prompted us to improve our scheme to assign noise to first-motions (described in Section S2.1.1). This change (along with a correction to the station distribution, relating to your point #5), required updating Figures 3–6, as well as SI figures. It did not change anything in the application of our methods to the Yukon dataset.

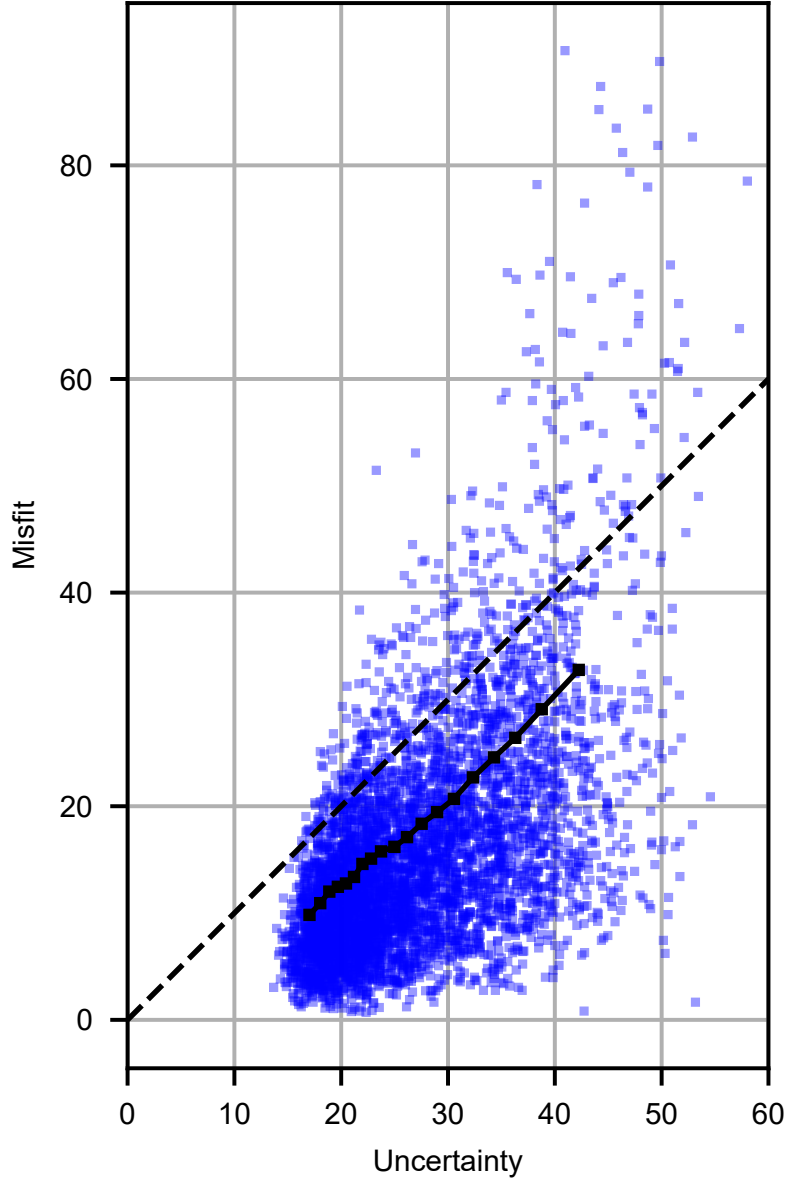


Figure 1. Version of Figure S3 based on past error formulation, but with only the 68.3-percentile shown.

2. The synthetic stress inversion tests show no significant improvement in the accuracy of the stress inversion when RVDM is used, compared to a simpler representation of the FM uncertainty. This is only shown in the supplement. A discussion of this result should be added to the main text, and Figure S2 should be moved to the main text.

We now include what was Figure S2 as Figure 5b in the main text and have added the following discussion to the text in that section (line 307).

A second set of box-and-whisker plots in Figure 5b examine stress misfits vs. the true, input stress tensors, defined as:

$$\epsilon_{\tau} = 1 - \tau_{\mathbf{5}} \cdot \tau_{\mathbf{5}}^{\text{true}}. \quad (1)$$

We find that ϵ_τ improves substantially with increasing N_{sta} (increasing data quality) but that, unlike with ϕ_τ , only modest improvements are observed with increasing N_{FM} . Furthermore, while $\epsilon_\tau^{\text{RVDM}}$ and $\epsilon_\tau^{\text{PPD}}$ are always lower than $\epsilon_\tau^{\text{UNI}}$, the differences are small and never statistically significant. We believe this is because ϵ_τ depends only on the best-fit τ_{ij} and not its entire PPD. In many inversion schemes, including the Vavryčuk (2014) algorithm, misfit would not depend on the uncertainty estimates at all because the best-fit result is computed directly from the “noise-free” data, rather than from the ensemble of Monte Carlo samples. Our algorithm considers a weighted mean of Monte Carlo results to compute the best-fit τ_{ij} , so the FM-uncertainty model provided can affect ϵ_τ , but we find it affects the uncertainty ϕ_τ much more than the best-fit τ_{ij} and its misfit.

3. The synthetic stress inversion tests demonstrate a decrease in the stress tensor uncertainty when the RVDM representation is used. However, a decrease in the stress tensor uncertainty is only a desirable result if either the stress tensor accuracy has improved (which it doesn’t appear to have), or if the smaller uncertainty is a better representation of the actual accuracy. Please test how well the uncertainty found with RVDM matches the accuracy. For example, compute the percentage of synthetic test for which the true stress tensor falls within the X% confidence regions of the inversions. This percentage should be close to X% if the uncertainty is a good representation of the inversion accuracy.

You raise a excellent point that has led us to re-frame how we interpret the results of Section 5.1. We agree that if two datasets, e.g. sets of UNI and RVDM stress tensors for a given N_{FM} and N_{sta} , have different uncertainty distributions but similar misfit distributions, something is amiss—likely that uncertainty for at least one distribution is poorly characterized. We have added the following paragraph to Section 5.1 (line 318):

Considered together, the ϕ_τ and ϵ_τ results indicate that using the RVDM model or the PPD of FM-uncertainty reduces ϕ_τ (vs. the using the UNI model) primarily because ϕ_τ^{UNI} overestimates stress-uncertainty by providing the inversion FM realizations that are not actually probable and, due to greater propensity of rotations about the N-axis, are more likely to indicate a substantially different stress tensor. This issue is most prominent when the FM PPD misfit ϵ_{UNI} is high, generally at low N_{sta} . Supplementary Section S4 discusses this further and compares the distributions of ϵ_τ and ϕ_τ for each uncertainty model.

We have also added a new supplementary figure comparing the stress uncertainties (reported at the 95% C.I.) and misfits for the UNI, RVDM, and PPD datasets. The scaling is not a perfect fit—the total percentages of $\epsilon_\tau > \phi_\tau$ for the UNI, RVDM, and PPD datasets are 6.9%, 13.1%, and 13.3% respectively, whereas they should be 5%. In our previous results, with too few incorrect first-motions, we see the same shift between UNI and RVDM/PPD data, but misfits were lower in general (old version shown here). We have added the following text to Section S4 (paragraph 3): In Figure S3 we found that uncertainties on synthetic FMs may be slightly overestimated, which would lead to overestimated ϕ_τ , opposite to the discrepancy we observe here. We cannot rule out that there may be an issue in the stress inversion algorithm that leads to underestimated ϕ_τ ; however, we hypothesize that the ϵ_τ distribution is outlier-prone due to binary nature of first-motion data and the potentially compounding effects of incorrect nodal plane selection. If we ignore this (potential) overall bias to lower uncertainties and compare the the three datasets, it could be interpreted that use of the UNI model overestimates uncertainty. We suggest that the UNI model must, in fact, contribute to overestimated ϕ_τ because it provides the stress inversion FMs that are not actually probable and, due to having a greater likelihood of rotation about the N-axis, are more likely to have opposing stress implications.

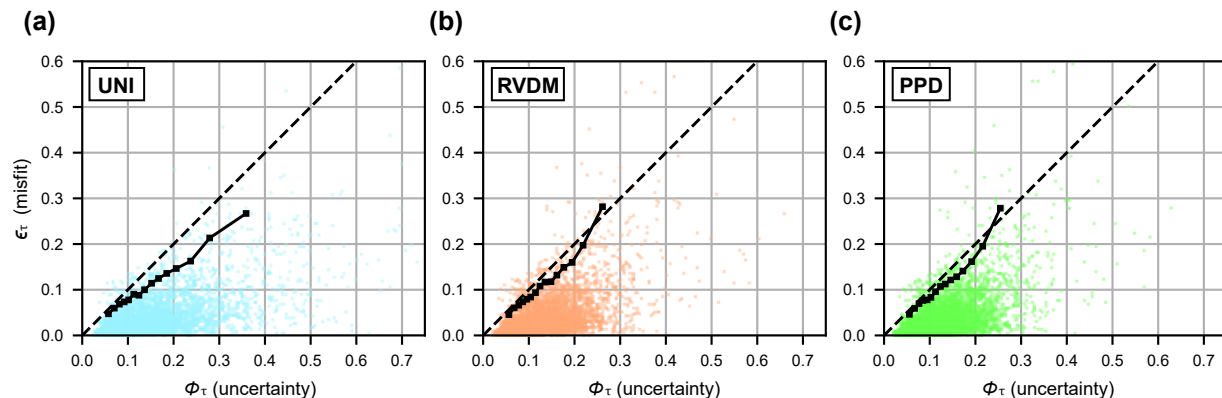


Figure 2. Like Figure S4, but with the old synthetic results that had too few incorrect first-motions, relative to the reported uncertainties.

4. The stress inversion accuracy and uncertainty are quantified using a 5-component representation of the stress tensor. However, focal mechanism stress inversions can return only 4 independent stress tensor components, as the isotropic and differential stress magnitudes can't be determined from geometric information alone. Please define the 5-component representation of the stress tensor and explain why it is used. Please clearly explain whether this representation contains either redundant information (non-independent stress components) or additional information (assumptions about stress magnitude), and how this may affect the accuracy and uncertainty metrics. Indeed, the magnitude of the stress tensor is arbitrarily constrained to 1 in our inversion and this was not mentioned before. We now describe the algorithm as outputting a "best-fit deviatoric, *unit-norm* stress tensor" (line 240) and rather than referring to five *independent* stress-tensor elements we (more carefully) say "A vector of the five stress elements in the linearized inversions $[\tau_{11}, \tau_{22}, \tau_{12}, \tau_{13}, \tau_{23}]$ is denoted τ_5 " (line 242).

We also now say earlier on "The method is built on the linear system of stress, fault normals, and slip vectors formulated by *Michael* (1984)." We hope these changes satisfy your concerns.

5. For the synthetics, the source-station distances are said to be drawn from a uniform distribution (L193, L265). A uniform distribution of distances would bunch the stations up near the event in an unrealistic way. The stations should be uniformly distributed in 2D on the earth's surface.

You have made a good point. Having simulated station locations be uniformly distributed on the 2D surface would be, in general, more realistic. We have updated the synthetics accordingly and those sentences now read: "... 2) assigning a maximum epicentral distance $d_{\max}$ from which to draw station positions (from a uniform distribution on the 2D Earth surface), set at..." (line 201) and then "36 stations positions were drawn from a disk of uniform distribution with radius $< d_{\max}$, where 5 sets..." (line 279). To compensate for this change we also lowered the five $d_{\max}$ to 20, 40, 60, 80, and 100 km. These changes affect all the synthetic results (Figures 3–6, and S1–S4).

6. Please show the Gosselin et al stress inversion results on Figure 7. That would make it easier to visualize the stress field described on L331-334.

We have added a Figure 7b plotting the stress inversion results of *Gosselin et al. (2024)*, highlighting those that we examine in Figure 8.

7. The FMs in the Yukon example have very large uncertainty, with median sigma of 45° or more in the examples in Figure 8. It appears that these uncertainties may be too large to be accurately represented by the RVDM method, as Figure 2 shows that sigma starts to saturate above about 40° . Please address this.

In section 5.1 and in the Conclusions section, we now highlight how the RVDM actually provides a great improvement over the UNI model when σ are high, despite the wrap-around effects. In the Conclusions (line 432) we now discuss in more detail:

"Despite the limitation of bounded disorientations illustrated in Section 3, with both synthetic and real data we found that implementing the RVDM is most useful, meaning it makes a bigger improvement to stress-inversion uncertainty compared to retaining only uniform error angles, when FM uncertainties are relatively high ($\sigma > 30^\circ$) and PPD truncation issues are present. This is possible because C_{ij}^{UNI} fits the PPD even more poorly ($\epsilon_{UNI} > \epsilon_{RVDM}$) at high σ . Retaining the full FM PPD is likely to have the most benefit when σ is high, such that truncation issues are present and ϵ is high, but the improvement we observe in stress uncertainties, versus use of the RVDM, is generally low."

Please also see our response to Reviewer B's point 3, which relates closely to this comment.

8. In the comparison with the Gosselin et al stress inversion results, 20% of the bins have a difference that is larger than the 95%-confidence region for the RVDM stress inversion. This suggests that the uncertainties computed using the RVDM may be too small. Please test, on the synthetics, how well the uncertainty represents the inversion accuracy (see point 3).

- As we explain at line 362 "We also omit FMs from the International Seismological Centre (ISC) catalog (*Lentas et al.*, 2019) for which we do not have similar measures of their PPDs, but were used by *Gosselin et al.* (2024)." This makes a precise comparison between the two results a bit dubious. We would also need to consider the Gosselin et al. PPDs to make a robust statement on how improbable the difference we observe is. We have expanded this explanation somewhat in the text (line 395): "However, we are not accounting for the uncertainty represented by the stress PPD from *Gosselin et al.* (2024) results in this simplified significance test (only the best-fit solution is considered), and some differences in stress results are not surprising given the differences between the inversions from *Gosselin et al.* (2024) and those presented in this work. Most notably, *Gosselin et al.* (2024) included additional FM data from the ISC, used a different nodal-plane selection metric, and did not consider FM weighting." Ultimately, the most useful comparisons are drawn between the PPD and RVDM inversions performed as part of this work.
- However, we do now perform the test on the synthetics as you recommended (Figure S4), and describe those results in response to your point 3 and in Section S4. The synthetic results also have too many high ϵ_τ , but not 20% like for the Gosselin et al. dataset.
- Looking into the details of this point, and generating Figure 7b, prompted us to notice a bug in how the best-fit axes from Gosselin et al. were reported in the previous version of the manuscript—rather than the true best-fit axes, we had accidentally drawn a random realization from the *Gosselin et al.* (2024) PPD results. We have corrected this and updated Figure 8, 9b, and the set of full stress-inversion figures in the SI.

Reviewer B

Review of "Describing the uncertainty of earthquake focal mechanisms", submitted by Alexandre P. Plourde and Jeremy M. Gosselin for publication in *Seismica*.

The authors introduce the Rotation Vector Distribution Matrix (RVDM), a 3×3 matrix description of earthquake focal-mechanism (FM) uncertainty, and show that it can retain directional information that is lost in scalar rotation-angle descriptions (here, the scalar Kagan angle). The core idea is compelling and practically important: a compact, anisotropic description of FM uncertainty is more informative than a single uncertainty angle and easier to propagate than a full posterior probability distribution (since it is more

compact). I find the manuscript to be clearly written, well organized and presented, and I really appreciate the author sharing the whole code distribution to replicate their results.

My main concern is not with the practical motivation of the RVDM, but with its geometrical framing. The manuscript presents the RVDM as a covariance matrix of rotation vectors, but does not clearly acknowledge that focal mechanisms do not live in ordinary Euclidean rotation-vector space. Much of the work presented here is based on the Kagan angle, i.e. the shortest rotation between two focal mechanisms. This is a useful and widely used measure, but it is not a neutral representation of distance in moment-tensor space. The space of double-couple mechanisms is built from 3D rotations, but is complicated by the symmetries of the source. Several rotations can represent equivalent focal-mechanism orientations. The shortest-rotation convention therefore introduces nontrivial boundaries and discontinuities in the coordinate system used to describe the posterior. These geometrical complexities affect how we can interpret probability, uniformity, and uncertainty estimates, and should be discussed more explicitly.

It seems to me (but that might be my interpretation!) that the RVDM works well as a local tangent-approximation of the posterior, rather than a general representation of the full posterior. Its start to get complicated when we get to the bounds defined by the rotation space defined over the Kagan angle. Clarifying this point would make the method easier to interpret and would help readers understand when the RVDM is expected to be reliable. I do think connecting your development to the well established geometrical properties of the moment tensor space would make it easier to grasp.

I therefore recommend major revision focused on the following points.

1 - The manuscript refers several times to “FM-space,” including a grid that “fully samples FM-space.” I think this needs a clearer definition. Is the grid constructed in strike–dip–rake coordinates, or in a rotation-based representation of focal-mechanism orientation? How are equivalent descriptions of the same focal mechanism handled, such as the two nodal planes or rotations that leave the same principal-axis frame unchanged? This matters because the RVDM is computed from rotation vectors, but the rotation between two focal mechanisms is not unique until a shortest-rotation convention, such as the Kagan angle, is imposed. A short clarification of what space is being sampled, how equivalent mechanisms are treated, and what “uniform” means in that space would make the method easier to interpret.

We appreciate this comment. We construct a grid in strike-dip-rake coordinates that we believe samples all possible FMs as evenly as possible. Requiring integer numbers of samples for both strike/rake and $\cos(\text{dip})$ makes it impossible (to our knowledge) for the grid to be completely perfect, but we attempt to make it as close as possible. We have updated this part of the text (line 161) to read:

In addition to a centroid (best-fit) FM, the code returns the PPD across a grid of FMs that fully samples FM-space. The FM grid uniformly samples $\text{strike} \in [0^\circ, 360^\circ)$, $\cos(\text{dip}) \in [0, 1]$, and $\text{rake} \in [-90^\circ, 90^\circ]$, selecting only right-lateral rakes to avoid unnecessarily sampling both FM nodal planes. We sample strike and rake at $\sim 4^\circ$ increments and $\cos(\text{dip})$ at its equivalent in radians ($\sim 4^\circ \cdot \frac{\pi}{180^\circ}$), resulting in a grid of 66,270 FMs.

2 - Much of the method relies on the Kagan angle, or shortest rotation between focal mechanisms. This is a useful and widely used measure, but it is not the only possible notion of distance between mechanisms. Tape & Tape (2012), for example, show that two double-couple mechanisms can have the same Kagan/principal-axis angle from a reference mechanism while being quite different as beachballs or moment tensors (I think a good summary of this can be seen in their figures 14 and 15, where it is shown that the Kagan angle is not very informative). This does not necessarily mean that the Kagan angle is inappropriate here, but it does mean that the metric choice is not neutral. And for that I think the authors should clearly explain why this is the right metric for describing FM uncertainty and propagating it into stress inversion. This would also be interesting to compare results against the Matrix-angle based moment tensor uncertainty quantification developed in Tape & Tape (2016)

- Thank you for pointing us to the [Tape and Tape \(2016\)](#) reference which was very natural to add into the introduction within numbered point 3 (line 52): “[Tape and Tape \(2016\)](#) introduced the matrix-angle MT confidence metric, computed as the arccosine of that dot product. This provides a robust measure of similarity in the FM radiation pattern, although only as a scalar metric.”
- A detailed comparison to the [Tape and Tape \(2016\)](#) uncertainty quantification using matrix angles would be quite involved, so we hope that you consider it acceptable to leave this to future works. However, we have included a new supplementary figure that could be produced using the synthetic FM results from Section 4.2. Supplementary Section S3 (and Figure S2) compare the matrix-angle and Kagan-angle misfits of input and recovered FMs. The results highlight that rotational misfits are lowest about the N-axis, and higher about the P/T axes. We state in that section (paragraph 2): “This result relates to what [Tape and Tape \(2012\)](#) illustrate in their Figures 14 and 15, which is that θ_K is not always a good measure of the difference between two FM (double-couple) moment tensors.”
- We do refer to scalar Kagan/disorientation angles in multiple ways, including reporting overall FM uncertainties with a “standard” Kagan angle. However, the RVDM is fundamentally built on rotation *vectors* and its purpose (vs. using only a single angle) is very much related to your point here. Rotations of equal angle about different axes affect the FM radiation pattern to different degrees (as shown in [Tape and Tape, 2012](#) Figure 14), this is why rotational uncertainty (in the RVDM) about the N axis is typically lower than about the P or T axes. We see this tendency in Figure S2.
- Re. whether the Kagan angle is the right metric for describing FM uncertainty, we now describe this choice in Section 2 (line 100): Note that there are always multiple ways to rotate from one FM to another, including about different axes and by adding any number of 180° increments. The disorientation is not the only possible metric to describe the difference of two FMs (there is e.g. the matrix angle, [Tape and Tape, 2012](#)), but disorientation retains an intuitive connection to physical rotation and the rotation-vector construction allows the RVDM to describe distinct rotational uncertainties about different axes. Also, in the new Supplementary Section S3, we conclude with: “We are not aware of how the matrix angle concept might be extended to form a compact description of an FM PPD analogous to how rotation vectors (of scale θ_K) can construct the RVDM.”

3 - My reading is that the RVDM works best as a local approximation of the posterior around a reference focal mechanism. For compact and relatively simple posteriors, this is likely appropriate and very useful. However, for broad, skewed, or multimodal posteriors, a single covariance matrix may become difficult to interpret. The results around Figure 2 already suggest this: the RVDM behaves well for relatively compact distributions, but starts to deviate as uncertainty grows. I think this should be discussed as a geometrical limitation of the representation, as I am not entirely sure that the bounded-Gaussian tail is the right explanation in this case. When uncertainties become large, the shortest-rotation convention can change which equivalent rotation is used to represent a given mechanism, and I wonder if it can alter the recovered covariance. Evaluating and stating this more clearly would help define the regime in which the RVDM is expected to be meaningful.

- Thank you for this feedback. We have adopted your wording of “skewed, or multimodal” PPDs and updated a sentence in our Conclusions (line 444) to read “Some forms of data, such as absolute-value amplitude ratios (e.g. $|P/SV|$) and various forms of waveform fitting, may introduce symmetries causing skewed and/or multimodal PPDs. Some sort of clustering algorithm, similar to what [Hardebeck and Shearer \(2002\)](#) and [Skoumal et al. \(2024\)](#) use in HASH, may make it possible to produce a meaningful RVDM from such a PPD, but retaining the full PPD becomes more worthwhile in these cases.” We also added the final sentence to more explicitly say that retaining the full PPD is useful in those cases.

- We agree that a “bounded-Gaussian tail” is not a complete explanation of the issues caused by disorientation-bounds—as you describe, the shortest-rotation convention can remap larger rotations to other axes. We do not explicitly use that phrase, but to clarify we have added the sentence “*These disorientation have effectively been mapped to an incorrect rotation axis that is not represented in the underlying RVDM.*” (line 150) to the interpretation in Section 3.
- We do expect that the issue you describe exists at high σ , i.e. that the shortest-rotation convention maps rotations to “incorrect” axes and effectively contaminates the RVDM. However, other than the *Gosselin et al. (2024)* study, tectonic stress inversion studies have (to our knowledge) only ever used a simpler assumptions on FM uncertainty (UNI or similar) that also implicitly rely on Gaussian distributions with the same bound constraints. We argue that use of the RVDM is an easy improvement on these simpler approaches, even when σ is high. In the second paragraph of the Conclusions (line 436), we now state “*Retaining the full FM PPD is likely to have the most benefit when σ is high, such that truncation issues are present and ϵ is high, but the improvement we observe in stress uncertainties, versus use of the RVDM, is generally low.*”
- In summary, we suggest the RVDM is meaningful across all our results. Misfits vs. the full PPD reach ~ 0.1 for the highest- σ synthetic cases (Figure 4b), and the benefit of retaining the full PPD is highest in those cases, but that doesn’t make fitting an RVDM futile or meaningless for those PPDs.

Minor comments: The manuscript mixes spelling conventions, for example “modelled” in Figure 3 but “modeled” elsewhere, and “epicentre” but also “center.” Please choose one spelling convention and apply it consistently. I have also found a couple of typos that should be corrected:

- l. 79: could in principle describes -> describe
- l. 114: A overall -> An overall
- Caption figure 1: relative to the the rotation vectors -> remove extra "the"
- Table 1: recovered and input FM -> recovered
- l. 242, 367, 368, caption figure 7: also remove extra "the" (several "the the" in the text)
- l. 367: and the those of -> and those of
- l. 428: purpose of the this study -> purpose of this study Please do a thorough typo and grammar check are there might be more.

Thank you for finding all these! We have corrected all those typos (and several more) and gone with "epicenter" and "modeled" consistently.

I think the paper can be a valuable proposition, but its main contribution should be framed more carefully. The RVDM might be a very useful compact summary of focal-mechanism uncertainty when the posterior is sufficiently local. It should not be presented as a general substitute for the full posterior distribution without a clearer discussion of the geometry, as the uncertainty metric is largely dependent on the geometry of the considered solution space. Addressing this point would make the manuscript much stronger and would help readers use the method appropriately.

Thank you for this valuable feedback. We believe that, by addressing your comments, we have framed our work appropriately.

Sincerely,
Julien Thurin

Tape, W. and Tape, C., 2012. Angle between principal axis triples. *Geophysical Journal International*, 191(2), pp.813-831.

Tape, W. and Tape, C., 2016. A confidence parameter for seismic moment tensors. *Geophysical Journal International*, 205(2), pp.938-953.

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