We apologize for the delay in the submission of the revised manuscript, which was caused the application of a new approach that resolves two problems identified by the reviewers. This new approach yields essentially the same results, namely that the non-double-couple (NDC) components in seismic moment tensors are, in many cases, artifacts of the inversion process.

We now perturb Earth model PREM for the generation of synthetic seismograms and use the unperturbed model for the inversion rather than using a different Earth model for the generation of synthetic seismograms. This new methodology reflects the practice of global moment tensor catalogs which all use PREM for their inversion and is thus a more realistic approach for our experiment.

In addition, we have identified the software bug responsible for our not reproducing the double-couple (DC) component during the inversion, as mentioned by reviewer A, and resolved the problem with our current approach.

Below, we address the reviewers' comments in detail.

REVIEWER A:

The manuscript assesses whether non-double couple components of moment tensors are an artefact of inversion processes using less-than-perfect velocity structures, with implications for whether the NDC components can be believed, or should be interpreted in terms of the mechanism of faulting. The manuscript is well written, but unfortunately, I find it to be scientifically unconvincing in its current state. I detail below three major issues, along with several minor points. The first two of these issues will, in my view, need resolving before the conclusions of this manuscript are robust.

We have addressed the major points raised by reviewer A.

Major Comments:

1) Figure 6 encapsulates my major problem with this study. Of the 12 inversion results shown in Figure 6, by visual inspection I'd say that only one (AK135-PREM) comes anywhere close to recovering the real orientation of the input moment tensor - many of them are completely different orientations from the input MT (e.g, CPac-NRus, NRus-AK135, which separately recover completely opposite polarity sub-horizontal/sub-vertical fault planes, for an input strike-slip mechanism...). Forget the NDC components – your inversions are simply not recovering anything sensible. I'd assume that the PDF's from the Bayesian inversions (which are never shown) are extremely broad, and the moment tensors essentially unconstrained. From this, I'd say this study is instead showing that you need to have either decent data coverage (see my next point) or a good idea of the velocity structure, (and ideally both) to accurately recover a moment tensor. But I'm not sure you're really able to conclude anything about the source of the NDC components as things stand – they are far from the biggest issue in the inversion results shown. This isn't to say that the conclusions of the paper are wrong (on the contrary, I suspect they may well be right), but I don't believe they have been demonstrated on the evidence presented.

We agree that the moment tensor results presented in the previous version of our manuscript did not reproduce the fault geometry (DC component) of the original moment tensor. We investigated this issue and found that Mineos software did not yield the proper amplitude of the horizontal components of synthetic seismograms for Earth models with ocean layers. Our new approach to generating synthetic seismograms resolves the DC component, as discussed in the text.

2) My second major issue centres on data coverage. I have no idea why you've restricted yourselves to only 10 stations – all I can think of is computational cost, but that seems unlikely at such low frequencies. Either way, using only 10 stations is completely unrepresentative of what would be used in reality, let alone for an earthquake with SNR's of 10. The gCMT catalog, for example, now requires a minimum of 100 stations with high-quality waveforms for events to make it into the catalog. Even if the stations are equally distributed, you've got azimuth gaps of 36° – at best. I imagine that in reality you'll have azimuth gaps of 2-3 times this for most of the earthquakes studied, leading to very poorly-constraint MT's (on Fig 5, there is an azimuth gap of almost 90°). I suspect this feeds in substantially to the first problem raised, but further raises into question whether the results you show are in any way representative of observational catalogs. Additionally, limiting yourselves to 100s periods and longer is a bit out of date – again, most moment tensor catalogue now used intermediate period surface waves, and in many cases long period body waves (also now commonly included in the gCMT). Given the period-dependent depth sensitivity of surface-waves, this will also have a massive impact on how much the different velocity structures actually impact on the synthetics/Greens' functions calculated.

We now use 50 stations for each inversion - the maximum possible by BayesISOLA. This guarantees good azimuthal coverage for every earthquake while making it possible to carry out 2500 moment tensor inversions.

We scale the earthquakes in our experiment to a moment magnitude of 7.0 so periods of 100-500s should be appropriate for the inversion. The accuracy with which the DC component of moment tensors is reproduced confirms this choice of frequencies.

3) Lastly, in terms of the final conclusions of the paper, even if the above problems are resolved, it seems to me me that, at the moment, the authors only test half of the problem – they dismiss NDC components, on the basis that inverting DC synthetics using a different velocity structure introduces NDC components, but do not ask if some NDC components could still be real. I would be interested to see the results from a similar set of tests, but run on synthetics with a known NDC component, to see if this simply adds to the artificial NDC component introduced by the differing velocity structures (and potentially discountable), or still results in a similar level NDC component.

We agree that this experiment could be expanded to check whether existing NDC components are altered during the inversion of synthetic seismograms generated for a different Earth model than used during the inversion. However, we view this question as best addressed in another paper and so have decided to limit our experiment to DC moment tensors.

Moderate comments:

Lines 49-54: I think you need to mention the impact of noise here.

We excluded noise from the synthetic seismograms to focus entirely on the influence of Earth structure.

Lines 80-82: Fairly minor, but these percentages add up to 100%, to one decimal place. This implies that your classification scheme identifies essentially no oblique faulting – in which case, I would clarify this.

We now include earthquakes with oblique-faulting mechanisms.

Line 92: An artifact of the inversion, or *one introduced by mapping the added noise into the moment tensor*.

We have changed the language of the entire paragraph.

Lines 93-103: This paragraph seems like a fairly pointless polemic on the benefits of “toy models”, and I think it fairly unnecessary. I would cut it.

We have deleted the paragraph.

Line 114: Somewhere in this paragraph, you need to say (as you do later) that you use each of the four velocity models to calculate synthetics for the same earthquake, then inverting all four sets of synthetics with the three other sets of velocity models (so 16 recovered MT’s for each earthquake, if I’ve understood correctly.

This is now unneeded because our approach has been changed.

Line 124: The sentence is incomplete?

The sentence included a formula, but appeared incomplete because it was on the next page.

Line 132: I’m not really sure what the point of using a Bayesian framework is if you’re just reporting the best-fit MT?

This inversion guarantees that the moment tensor provides the best fit between “observed” and synthetic seismograms, and minimizes computation time.

Figure 6: Would be improved by showing all 16 possible inversion results, rather than just the 12 where velocity models changes. This would complement Section 3.2

The change in methodology no longer yields additional inversion results than shown in figure 6 (now 5).

Minor Comments:

Line 15: I’d suggest “The distribution of NDC components varies …”

We believe this sentence is clear as is.

Line 47: I think this should be “ratio of the smallest and largest absolute eignevalues”

The sentence is correct as is.

Line 163 "…values…"

This text is no longer in the manuscript.

REVIEWER B:

This paper is addressing the problem of non-double couple (NDC) components in moment tensor (MT) inversion procedures. The authors focus on global MT inversion, create synthetic data, for double couple sources in one crustal model, add noise, and invert them in another, thus NDC appear. Their results compare well with observed NDC distribution in real data inversions, from global MT catalogues; thus, the paper concludes that the NDC are artifacts of the inversion procedure (mainly inadequacy of crustal model). Although the idea that NDC appear due to "wrong" crustal models is not new in moment tensor inversion studies, I find the paper interesting since it is based on a simple methodology and clear results.

Thank you!

I believe that the paper is interesting for the Seismica audience and can be accepted for publication after minor revision.

I have a few general comments that could be useful for the authors, and I have made some notes on the PDF file, also.

1). I understand the selection of AK135 and PREM but why use regional models and not some other global model?

The choice was based on the availability of Earth models for the Mineos software format. However, our new approach is only based on Earth model PREM that is used by all global moment tensor catalogs in their inversion.

2). Is it possible to quantify the difference between crustal models? Figure 3 is not very helpful, what I would like to see is large differences in the models reflected in large differences in results, Fig.6 helps, but this is not so straightforward.

Our new approach quantifies these differences. We use perturbations of 1, 3, 5, and 10% in the elastic structure of the Earth model, and 25, 50, and 75% in the anelastic structure. These uncertainties cover the ranges reported by Karaoğlu and Romanowicz (2018), and Dalton and Ekström (2006).

3). What about DC part? in some cases even DC is not retrieved correctly (Fig,6), could you add some discussion on this? To me it looks as a larger "danger" for interpreting the results later.

As noted above, our new approach resolves the DC component correctly.

4). Is it possible to add a statistic test to quantify better the similarity of real NDC distribution and the one calculated by the experiment?

In the experiment, the real source has no NDC component, so all NDC components resulting from the inversions are spurious. Those resulting NDC components show no correlation to the original NDC components of the moment tensors in the GCMT catalog. This is expected because the perturbation of Earth models is random and thus results in NDC components with an average size, but arbitrary polarity.

Review Questions

Is the paper of value and interest to a significant position of the potential readers of Seismica? Yes

Is the study timely and of current interest? Yes

Is the manuscript clear and easy to follow? Yes

Is the manuscript's title adequate and accurate? Yes

Is the abstract adequate? Yes

Are the methods appropriate and described in sufficient detail to be transparent and reproducible? Yes

Are the conclusions adequate and supported by the data? Yes

Is the paper unnecessarily long? Does it include too many materials that can be found in other sources? Size of paper is ok.

Is the paper significantly different to those already published by this author(s) or any other paper in this field of study? Yes

If the study disagrees significantly with the current academic consensus, is there a substantial case? If not, what would be required to make their case credible? Not applicable.

If the paper includes tables or figures, what do they add to the paper? Do they aid understanding, or are they superfluous? Tables and Figures are in general ok (I have comments in PDF). Authors could remove Fig.1 if they agree I think it is not adding much.

We addressed the issues raised by reviewer B in the annotated manuscript. We mention the effect of the Earth model on the seismic waveforms of smaller earthquakes for which higher frequencies are used in the inversion and thus are expected to have larger NDC components. We have deleted the paragraph about the toy models. Issues related to the influence of noise are no longer contained in the manuscript.

REVIEWER C:

I am in favor of publishing the study as it shines a light on something, that many moment tensor inversion studies are somewhat aware of, but also somewhat neglect. However, in the attached review pdf and annotated paper, some questions should be addressed and improvements to enhance the study in my opinion.

Thank you! We have addressed the semantic issues raised by reviewer C in the annotated manuscript.

The issues related to the signal-to-noise ratio are no longer present in the manuscript.

We have added a detailed description of the synthetic seismograms and now mention their record lengths.

We use the full trace for inversion because synthetic seismograms are zero wherever there is no signal, thus not affecting the inversion.

Rösler et al 2022b

Manuscript: Apparent Non-Double-Couple Components as Artifacts of Moment Tensor Inversion Date: September 29, 2022

Brief Summary

In a previous paper (Roesler et al. 2022) it was identified that a pervasive, uncorrelated, non- double-couple (NDC) component is found throughout various moment tensor (MT) catalogs, tectonic environments, and magnitude ranges, which suggests the NDC component may be introduced by the inversion procedure.

The present study aims to quantitatively back this suggestion by first modeling "observed" data using a true double-couple (DC) moment tensor and one Earth model, and subsequently using a different Earth model for forward modeling synthetic data, and inverting a six component moment tensor. Although not shown the authors claim that the procedure has been verified by retreiving the same MT if Earth model one and two are the same. If the Earth models are different the inverted moment tensors show the suspected NDC components across multiple "model 1 → model 2" permutations. The authors also tests the effect of noise on the inverted moment tensors and conclude that compared to structural effects, noise contributes negligibly to the NDC in the inverted source parameters. It has to be noted that inversion for the location is omitted to ensure the effect of the inversion on the moment tensor is isolated.

The inversions are performed using 60-minute (??) seismograms "filtered" between 100s and 500s from station locations of the IDA deployments with 10 degrees minimum epicentral distance from the source in question, and the inversions are performed using BayesISOLA.

Finally, the authors conclude that using an Earth model that is not the true model introduces a significant NDC component during inversion and that using a model that is closer to the true model would significantly reduce this effect.

1 Review

I think the manuscript systematically addresses an issue that the MT inversion community is generally aware of, and that is the trade-off between inverted source parameters from a mismatch between the true Earth model and the Earth model used for forward modeling. To my knowledge, no one prior to the authors of the present study has attempted to quantitatively show the true extent of this trade-off. Hence, I believe the study is worth publishing; in particular, since this manuscript in company with the previous publication (Rösler et al., 2022a) shines a light on something that is not often discussed.

Thank you!

I do have some remarks, and questions to the authors, that could contribute to the improvement of the study, which I delineate below. The points are in the manuscripts "chronological order".

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1. The title the authors chose may be misleading. It sounds like moment tensor inversion inherently introduces an NDC component, but that would mean that an inversion from e.g. PREM to PREM would also result in NDC components. As the authors show and conclude, it is indeed a mismatch between the true Earth model and the Earth model used for forward modeling that introduces the NDC component during inversion.

The reviewer is correct in principle. However, in practice all moment tensor inversions use an Earth model that simplifies real earth structure and thus – as we show – introduces spurious NDC components. Because other reviewers were comfortable with it, we are inclined to keep it.

2. I don't believe Figure 1 truly shows what the authors want to show. See suggestions in the Figure section below.

3. Does volcanic regime automatically mean extensional? It seems slightly more complex than that (see Rodríguez-Cardozo et al. 2021)

Not always. However, we don't see how this is related to our paper.

4. The authors introduce the problem by mentioning that smaller earthquakes are the main concern and that smaller earthquakes are more prone to uncorrelated NDC components (from the previous study). However, when setting up the problem the authors indicate that they would like to prevent any sort of magnitude bias by only considering events of magnitude 7 and up. This seems strange to me. In a synthetic setting, all events down to M_W 5.5 should be detectable worldwide. Are the eigenfunctions a constraint here as well (I'm not a normal mode expert)? Otherwise, I would suggest doing the workflow in a loop and using the same event locations simply changing the event moment magnitude to [5, 5.5,6,6.5,7] in the end this is a synthetic test. This would shine a light on the NDC components in terms of magnitude and – most importantly – performance in the presence of noise. Is the effect the same for all magnitudes? Does the NDC component naturally increase with event size? And with a decrease in event magnitude, does the effect of the noise play a larger role in contributing to the NDC instead of the "bad" Earth model?

As mentioned in the comments made to reviewer B, we limit our experiment to earthquakes of one magnitude and mention the effect of Earth structure on smaller earthquakes.

5. Although the anecdote is quite nice, I don't believe the toy problem paragraph adds to the manuscript.

We have deleted the paragraph.

6. I'm quite puzzled seeing no change in SNR with increasing epicentral distance. Even though surface waves attenuate much less than body waves, there should at least be a trend.

   On the same note, I did not quite understand what was correlated. Was it simply epicentral distance and SNR? Then, -0.16 does indeed show a slight negative correlation.

We were equally puzzled. To avoid this issue and keep our manuscript focus on the influence of Earth structure, we have removed the influence of noise from our experiment.

7. The study is focused on the effects of the inversion on moment tensor parameters. What I think is lacking here is a bit more detail in terms of the inverse problem. I understand that the inverse is reviewed in a prior paper. However, an objective function to at least see what we are minimizing would greatly help the reader's understanding. What does BayesISOLA optimize? Is it just the plain waveform misfit? Windowing involved? All components? just Z? How long are the traces?

Yes, the inversion minimizes the misfit between "observed" and synthetic waveforms by adjusting the moment tensor elements. We have clarified the paragraph and refer to Vackář et al. (2017) for details about this procedure.

8. In section 3.2, the authors mention how they used periods below 100s even though earlier it was mentioned that only periods above 100s are used.

This misunderstanding arose because we mention periods of >50s and >75s, which includes periods >100s. Section 3.2 was replaced and will thus no longer cause misunderstandings.

9. Line 200-201. Is this true? ... I may have to brush up on my seismology basics. The wavefield for laterally varying earth structures is very different from laterally homogenous earth structures. Are you not comparing two laterally homogenous Earth models in all cases? I'd be interested in a reference here, or an elaboration.

This is correct, we are comparing seismic waveforms generated for two different laterally homogenous Earth models. The differences in them reflect differences due to different Earth models, whether they are laterally homogeneous or not.

10. Sawade et al. (2022) support your claim as the distribution of DC to non-DC components is narrowed if however only ever so slightly!

This paper was published after we submitted ours. We have included it in our references.

11. Other manuscripts that are worth including here in the discussion and conclusion

• Hjorleifsdottir et al.(2010) deserves a mention here I think, they try to show something similar yet different.

• Valentine & Woodhouse (2010) – Combined source and structure inversion, for true improvement in both source and velocity model

• Valentine & Trampert (2012) – Uncertainties in MT inversion, a general paper that talks about similar things that are discussed here

We included Hjörleifsdóttir and Ekström (2010) in our manuscript. However, Valentine & Woodhouse (2010) show how tomographic models are based on corrected moment tensor solutions, not the opposite, and Valentine & Trampert (2012) study the uncertainties in moment tensor solutions in general without discussing that NDC components can be due to those uncertainties.

2 Figures

2.1 Figure 1

Figure 1 does not really add to the paper. A statistical overview over the GCMT catalog would be much more elusive. The point the authors are trying to make in the text are that - paraphrased "a lot of NDC events are visible on the plate boundaries." From the looks of the map the Mid- Atlantic ridge looks mostly like an outlier rather than the norm; compare, e.g., ridge in the Southern Ocean almost all events are close to DC.

It is possible that what the authors are trying to show could be better illustrated by creating a 2D histogram with event locations of the first 50 km, weighted by the epsilon and divide by bin count; that provides a sense of local average. The STD may also be more interesting than the average (which should be close 0 ...). The average ε or |ε| and the respective standard deviation could be indicated by color bars.

Because no other reviewer or the editor have objected to this figure, we prefer to keep it in its current form.

2.2 Figure 2

If one creates a 2D histogram of the epsilon values, as mentioned above, Figure 1 and 2 may be combined. The beachballs of figure 2/table 1 could simply be plotted on top. I think that figure would be more impact full.

If you decide on doing this consider using lines to connect the beach balls and their actual location so that the 2d histogram stays in clear view.

The histogram of the NDC values in the GCMT catalog is included in figure 7. Figure 2 is an illustration of the earthquakes and seismic stations used in this study.

2.3 Figure 4

I understand that the authors want to underline their choice of general noise-level, but I don’t think this plot adds a lot to the main text.

This figure is no longer included in the manuscript.

2.4 Figure 5

This figure is great as it provides a neat overview of a single inversion. I have two minor suggestions here: For comparison on what waveforms are inverted and the result. It would be more illustrative to plot the ’observed’ and ’synthetic’ data on top of each other, and maybe inverted waveform on top. Common approaches are coloring observed in black, synthetic in red, and inverted in blue. Then below one can still plot the noise in a subplot. Then color code the beach balls accordingly. I think size ratio between map and waveform should be adjusted since – although station geometry is important – the most important thing is the waveform. If all traces of a single component are plotted in a single plot, one may plot all three components of a station in a single plot.

The "observed" and synthetic waveforms are now in one single plot on the bottom of the figure. Noise is no longer part of our manuscript.

2.5 Figure 6

Consider adding a second subfigure with "model 1 + noise → model 1".

The effect of noise is no longer part of our manuscript, as it has been discussed more widely in other studies.

2.6 Figure 7

The caption could be a bit more detailed, what is counted in theses histograms? All permutations of events and Figure 6? Although most people are aware what σ, μ, M stand for, it is better to explicitly say what the variables indicate.

We hope to have solved these issues with the new design of figure 7.

3 Other Notes

ε Tape and Tape have various studies (Lune diagram) showing that a more natural quantity to show DC vs. CLDV is https://sites.google.com/alaska.edu/carltape/home/research/beachball_ gallery?authuser=0 [Carl Tape's website - beachball gallery]] (Second figure on the page, also in Tape and Tape 2012a, Fig. 11). The epsilon value is however traditional. Also note, that the distributions will also look more or less the same. I just wanted to make the authors aware.

We agree that the Tape formulation provides additional insights, but use the ε formulation because it is reported in global catalogs and primarily used in the literature and thus allows comparison of our analysis results to reported ones.

Response to suggestions made by the editor

Major, general comments

1) A point that was raised by the reviewers is that the effect of unknown/unmodelled heterogeneity on generating artificially high non-double couple components is already quite well-known from previous studies. Although the papers of Hjorleifsdottir & Ekstrom (2010) and Sawade et al. (2022) are very briefly mentioned in the Discussion, given their strong relevance to your conclusions, they are not discussed nearly enough. Moreover, these studies should be first described in the Introduction; therefore, at present, the overview in the literature appears quite incomplete. These relevant papers help to provide an accurate framing and context for your new study and to generate/refine your motivations and objectives. In addition to the above studies, I found many other studies that appear to show the effect of 3-D structure on the retrieval of double-couple components – please see these papers below. A number of these should ideally be cited.

- Hejrani, B., Tkalčić, H., & Fichtner, A. (2017). Centroid moment tensor catalogue using a 3-D continental scale Earth model: Application to earthquakes in Papua New Guinea and the Solomon Islands. *Journal of Geophysical Research: Solid Earth*, *122*(7), 5517-5543.
- Zhu, L. and Zhou, X., 2016. Seismic moment tensor inversion using 3D velocity model and its application to the 2013 Lushan earthquake sequence. *Physics and Chemistry of the Earth, Parts A/B/C*, *95*, pp.10-18.
- Wang, X. and Zhan, Z., 2020. Moving from 1-D to 3-D velocity model: automated waveform-based earthquake moment tensor inversion in the Los Angeles region. *Geophysical Journal International*,*220*(1), pp.218-234.
- Ferreira, A.M.G., Weston, J. and Funning, G.J., 2011. Global compilation of interferometric synthetic aperture radar earthquake source models: 2. Effects of 3-D Earth structure. *Journal of Geophysical Research: Solid Earth*, *116*(B8).
- Henry, C., Woodhouse, J.H. and Das, S., 2002. Stability of earthquake moment tensor inversions: effect of the double-couple constraint. *Tectonophysics*, *356*(1-3), pp.115-124.
- Liu, Q., Polet, J., Komatitsch, D. and Tromp, J., 2004. Spectral-Element Moment Tensor Inversions for Earthquakes in Southern California. *Bulletin of the Seismological Society of America*, *94*(5), pp.1748-1761.
- Jechumtálová, Z. and Bulant, P., 2014. Effects of 1-D versus 3-D velocity models on moment tensor inversion in the Dobrá Voda area in the Little Carpathians region, Slovakia. *Journal of seismology*, *18*, pp.511-531.
- Hingee, M., Tkalčić, H., Fichtner, A. and Sambridge, M., 2011. Seismic moment tensor inversion using a 3-D structural model: applications for the Australian region. *Geophysical Journal International*, *184*(2), pp.949-964.
- Covellone, B.M. and Savage, B., 2012. A quantitative comparison between 1D and 3D source inversion methodologies: Application to the Middle East. *Bulletin of the Seismological Society of America*, *102*(5), pp.2189-2199.

Thank you very much for pointing us to these papers. We have added a paragraph (lines 76-85) to show previous work focusing on this issue and their importance to our paper. In lines 86-89, we now show how our study builds on previous work and clarify the motivation for our study. We have included all but one reference (the LIDAR paper), and added more.

2) As also raised originally by Reviewer B, I find that the manuscript still does not contain enough details about your moment tensor inversion method. I would like to see more details rather than just referencing Vackar et al. (2017). For example, even though the Green's functions period is stated, what frequencies are used for the actual inversion?

Also, because the main motivation of the study is to compare with solutions from the GCMT catalogue, I feel that it is important to explicitly state the difference between the GCMT inversion method and your approach. Do these method differences affect how you directly compare your results to GCMT solutions? Also, how is centroid timing and position solved for, or do you assume the GCMT centroid position? Is any distance-weighting applied in the inversion?

We have expanded the description of our inversion procedure in lines 148-166. Additionally, we have expanded on how we generate synthetic seismograms in lines 110-116.

3) In the Conclusions, I would like to see a short summary of how your results might impact and could better help future analyses of moment tensors by the seismological community. Are non-double-couple mechanisms being routinely misinterpreted by seismologists, particularly given the average NDC of 20% in global catalogues? In MT catalogues using 1-D models, what threshold of NDC% should we start to trust and make physical interpretations about source mechanics. Are these artificial NDC components likely affecting full-waveform tomography approaches?

We have expanded our conclusions in lines 229-242, and in line 247, to address this issue.

Specific comments

- L39-49 – A minor point, but I find the equations and much of the detailed theoretical background about moment tensors in the Introduction largely superfluous. Many of these equations can be found in most seismological textbooks, so I'm not sure that they are needed here.

We show the definitions we use because differences among the various commonly used definitions lead to different results for the NDC component, scalar moment, and other quantities.

- L71-72 – this statement about NDC components in volcanic environments needs some reference(s).

We have added four references in lines 72-73.

- L79-92 - How were the 25 earthquakes chosen? Was a specific strategy/filtering used to select them? Or were they chosen randomly? Please be clear about this in the text.

We have added text in lines 101-102.

- L187-188: Vavrycuk (2002) and Horalek et al. (2002) appear to both interpret their low-double-couple earthquakes as due to tensile faulting. So are you suggesting that their physical-based interpretation is incorrect?

We don't consider that their interpretation is incorrect. We have clarified this in lines 217-218.

- Figure 1: I find it difficult to interpret anything useful from this figure because there are so many focal mechanisms plotted (one of the reviewers also raised this point).

As a start, I suggest colouring beachballs by their double-couple percentage so the reader can help visualise how common low-double-couple earthquakes are. Please see also some other suggestions from Reviewer B for presenting this information.

We have color-coded the beachballs to show the size of their NDC components.

- Figure 4 & 5: I suggest combining these figures into a single figure with a panel (c).

Although both figures are related to the mid-Atlantic ridge earthquake in 1994, we consider it best to leave them separated because of practical reasons during typesetting, which more difficult for a figure that covers an entire page.

- Figure 6: Please state in the caption the meaning of the greek letters above each subplot .

Done.

- Figure 7: The yellow text on white background is very difficult to see.

Done.