Dear Editor Robert Skoumal:

Thank you very much for your comments and those from the two reviewers about the submitted paper “Moment tensor solutions and stress regime along the Northern Caribbean plate boundary off southeastern Cuba”. We consider these very pertinent and constructive, and all of them were considered in the new version of the paper I am sending you.

In the response-to-reviewers letter, you will see that all the comments have been considered and answered, and also that in the Acknowledgements section of the manuscript, we recognize the work of the reviewers and yours.

I am attaching a pdf file with some highlighted changes due to some problems with my text editor, but all of the recommendations have been considered in the new version, including yours in the last email.

Please don't hesitate to contact me if you have any questions or comments about this new version.

All the best.

O´Leary

Answer to the Reviewer A:

Thank you very much for all your comments, suggestions and opinions about the manuscript, including the smaller technical issues in the manuscript PDF file, we consider these very appropriate and helpful. All of them have been take into account in the new version and we consider that they have improved the manuscript into its present form.

We are answering below each one of your comments, and you may see also where the changes were done in the new version.

> *The fact that focal mechanisms are deviatoric (not full moment tensors, i.e., assuming the isotropic part vanishes) needs to be more emphasized. I suggest putting the word "deviatoric" in the headers of all tables. Then it will be clear that "non-DC" in this paper is just CLVD.*

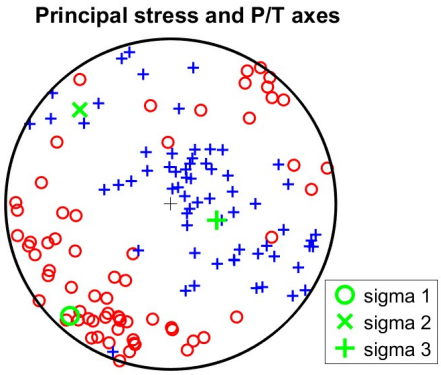
R:/ The headers of all tables and in lines 28, 168 and 396 it was emphasized.

> *The part on non-DC components in Lines 241-252 would need a modification. The simplest possible solution is to remove this part from the paper, and consequently remove the last sentence of the Abstract and Conclusions. The reason for this suggestion is that ISOLA calculations of moment tensors were not supplemented by uncertainty assessment. Perhaps the authors used an older code version where the uncertainty was not included. While strike, dip, rake, and moment are relatively stable parameters, DC% is not, so a single best-fit value of the DC% is of a very limited physical meaning (Zahradnik and Sokos, SRL 2025). Although new ISOLA (version 2024 and later) enables uncertainty assessment, I do not recommend this to be done as additional work in this paper; the non-DC analysis would need a specific investigation, which is beyond the scope of this paper. Another reason for suggesting removal is that the literature overview on non-DC in Lines 241-252 would need a considerable extension and in-depth analysis. If the authors are interested in continuing their non-DC research (later, beyond this paper), I am ready to cooperate.*

R:/ Lines 241-252 were changed, in the following form: In the new database, 13 moment tensor solutions have significant non-DC component (> 40%). This feature is also present is some of the gCMT solutions. Large non-DC components may be associated with artifacts of the inversion process (Rösler et al., 2024), limited azimuthal coverage of seismic stations (Kumar et al., 2015), or natural causes such as fault complexity (Yue and Lay, 2020) and volumetric changes in the source area linked to fluid-triggered seismicity (Vavryčuk, 2002). However, current evidence does not allow a definitive distinction between these possibilities, and resolving such causation is beyond the scope of this study. Nevertheless, the presence of large non-DC components does not affect the seismotectonic interpretations presented herein regarding the focal mechanism classification.

Lastly, I have problems with Lines 359-399, the section on the overall stress regime. Similarly to the authors, I can hardly understand why broadly variable focal mechanisms result in Fig. 7a, in a purely thrust regime. Based on important and correct Fig. 4 and Fig. 5, I think that stress variation across the region is so high that we cannot process it as a whole. Thus, I suggest the removal of this chapter. It will not make any basic change to the result of the paper and tectonic discussion, which is well made for separate provinces in the previous sections. If authors want to continue on that topic, perhaps they should put more attention to one output from Vavrycuk's StressInverse code, I mean the plots "P_T_axes.png". If this plot shows a very large spread of the axes, it is an indication that the main assumption of the stress analysis, i.e., homogeneity of stress, is violated, so the method cannot be used.

R:/ Yes, this is one of the main questions of the paper. The same behavior has been observed in others important transform faults. In fact, as you mentioned, the P_T_axes.png mentioned have spread of the axes, but most of them seems to be consistent with the proposed thrust regime. As supplementary material, we are including all the output information from Vavrycuk's StressInverse code.



In addition, the uncertainties values associated with the azimuth and plunge of the principal stress axes were included, and the fact that their relatively low values do not significantly affect the classification of the stress regime based on the plunge ranges.

All technical issues and those related to a better understanding of the text in the manuscript PDF file were considered.

Later, we would like also to continue with the non-DC research, and we will appreciate your cooperation for that.

All the best for you.

O´Leary

Answer to the Reviewer B:

Thank you for your comments, suggestions, and observations on the manuscript. We found them highly appropriate and helpful, all have been considered in the revised version and have improved the manuscript.

Below we respond to each comment and indicate where the changes were made:

In general, the manuscript is written clearly, with some room to improve the readability. The figures/tables and SI may be revised to be more informative.

R:/ SI information was enriched and the tables and figures improved according to your recommendations.

1. *[waveform fittings]Since the authors are using SI and this work highly relies on the fitting quality of the waveforms of limited stations, I would suggest the authors include the waveforms into the SI.*

R:/ In the SI were included the waveform fittings as well as other relevant information.

2. [narrow bands and high SNR?]Related to 1, I noticed that relatively narrow frequency bands (e.g., 0.04-0.06Hz,0.05-0.07Hz) were used--a potential "trick" to improve the viriance reduction and simplify fitting for such inversions. However, the variance reduction (VR) values remain relatively low (many of the events just above 50). Therefore, I highly encourage the authors show the fittings.

R:/ Yes, VR is lower than 50% for 3 earthquakes, and they were exceptionally accepted on the database, as described in the text (lines 238-242).

2. *[nonDC]Related to 2, when the VRs are low (say below ~80), I won't try to interprete the non-double-couple components. I think it is reasonable for the authors to resolve the deviatoric moment tensors only.*
   To validate the necessity of the CLVD components, the authors could use the same Green's Functions to forward model the corresponding waveforms with best-DC moment tensors, and see if the VR is significantly lower that the deviatoric solutions.
   Otherwise, I would soften or even remove sentences like line 42.

R:/ Yes, the interpretation of the non-double-couple components and the comments associated with this theme were modified in the new version of the paper.

[Figures &Tables]

1. I feel Table 1 that shows instument types is less important and may be moved to SI.

R:/ Yes, it was moved as Supplementary information #1.

2. *Suggest to add "largest station azimuth gap" to both Table 2&3. Or even better, include the P-wave 1st motions and/or the station distributions on the resolved moment tensors. This would give a better idea of the low-hemosphere coverage than the distances and CN(count of components).*

R:/ In the new version as Supplementary Information 2 and 3, for each earthquake, the P-wave 1st motion showed in the low-hemisphere and the station distribution are included.

3. *I find figure 6 hard to understand with the view angle and "seismicity profile"(blue elllipses). Specifically, where are the beachballs located? Please either improve the projections and use a 3-D structure for the faults, or stay with 2D depth profile. The depth color-coding is redundant since this is plotted along depth aready (may be use time to colorcode or keep it same as Figure2?). Colorbars should also be included (i.e., tompography and beachballs).*

R:/ Figure 6 was adjusted to present the information more clearly.

[minor]

1. *line 31: better to use "historical" or another word for "16 additional ones" (at first I thought the authors were only able to recover 14 events and have to use 16 from gCMT due to potential failure)*

R:/ Corrected.

2. *line 174: less than 70% are inconsistent? do you mean consistent?*

R:/ In the new version this sentence written more clearly.

3. *line 136: use a table or figure for the velocity model is better than text.*

R:/ A table was used and included as Annex 1.

4. *line 235: WPD or WDP? In adidition, is "Oriente Deep Zone (ODZ)" the same as "Oriente Deep", where exactly is the "Oriente fault"(L289)*

R:/ Corrected the WPD, the Oriente Deep Zone and the explanation about the Oriente fault system (L65-68). In the new version in figure 1, the Oriente fault system appears more clearly.

5. *line 253-257: what is the point for commenting on the magnitudes(and Figure 3)? I find this part wavy/redundant. More responsible analysis (fitting and*

calibriting between Mw and ML? error ranges for the magnitude calculation?) need to be conducted.

R:/ The paragraph was rewritten to avoid redundancy and improve clarity. Its main purpose is to demonstrate that the new Mw values are consistent with the theoretically expected. New studies about the relationships between Mw and other magnitudes for the SSNC are currently in process.

6. line 322: how many is "the majority"? (XX out of 45?)

R:/ In the new version this sentence written more clearly.

*7. It is hard to read/follow with the current indexing of all events and notations (c and *) frequently used in pages 11&13&15 etc. I have open Figure 2 on the side to follow the disucsison here. Please consider improve.*

R:/ We acknowledge the point, but this indexing is not easily improved given the needed to differentiate the three database.

Thank you for your careful and constructive comments, which have helped us to improve the manuscript in its present form.

Best regards.

O´Leary