Point to point response to Reviewer 1

original comments in black, our response in green

We thank the editor and the two reviewers for their help and constructive criticism. We have considered their suggestions and updated the text and figures. Please find below the detailed point-to-point responses.

On the data and methodology

Comment 1:
It is unfortunate that the authors are very sparse in describing the processing applied to the raw data: Who? When? How? A brief expansion on this would help readers familiar with seismic-reflection interpretation understand the scope of the work.

Response 1:
We include the details of this as much as possible from the report provided by Rhein Petroleum. The data were acquired for re-exploration of hydrocarbons in URG in the 2010s. It is now available for research purposes. At TU Darmstadt, it has been re-interpreted for various purposes, such as geothermal prospectivity and storage potential.
Please refer to line no. 169 – 176.

Comment 2:
Another criticism regarding the data aspect is the lack of detail in the README files, which is supposed to describe the content of the GitHub repositories.

Response 2:
We now provide clearer README files, which show how to read the data- and script-files.

Comment 3:
The text is well-written, and the structure is sound. However, some figures are too small. For example, the map illustrating faults at the UHY horizon should occupy at least half a page. Additionally, I believe it would have been wiser to present cross-sections perpendicular to the faults (NNW and WNW) to better illustrate their geometry and relationships to surrounding layers at depth. The choice of oblique sections is debatable.

Response 3:
We have resized the UHY map and provide new cross-sections that are perpendicular to the faults (as the main figure). However, we now place the oblique sections in the Appendix.
Refer to Figure 3 and Figure A1.

Comment 4:
One aspect that needs revision: In Section 3.2, the authors describe the subsidence history of the horizons from the most recent to the oldest in a completely counterintuitive manner.

Response 4:
We changed the order from oldest to youngest sequence.
Please refer to line no. 296 – 318.

Comment 5:
The reference list and citations are coherent, except that there are duplicates in the reference list (e.g., Cardozo and Behrmann; Schumacher).

Response 5:
Duplicate references were removed. The references were thoroughly checked and updated.

On the substance
The results are interesting and deserve to be highlighted. However, some interpretations of the results are, in their current form, debatable or require clarification. For this reason, I recommend major revisions
Here are the main comments:

Comment 6:
Introduction
The authors focus on distant geological times, particularly the late Miocene (5 Ma) for the most recent level analyzed, compared to the period of interest for "active tectonics" (Quaternary) and current seismicity. Therefore, the claim about the importance of this work for understanding hazards needs to be better justified, either in this section or later in the discussion.

Response 6:
We have now written about the active tectonics and its implications. We have added a paragraph justifying why studying fault activity in this area (distant geological times) is important to study current hazards.
We now introduce a new section that explains the structural elements and the implications associated with rift systems with a multiphase tectonic history.
Please refer to line no. 46 – 50.

Comment 7:
The "Expansion Index" (EI)
The index calculated to estimate the active and syn-sedimentary nature of faults (EI, Expansion Index) does not, in my opinion, bear an appropriate name. It is more of a "relative subsidence index" than an "expansion" index, which would be preferable to use in the early stages of the work. It allows for the comparison of vertical elements. Moreover, the authors are convinced that this relative subsidence is associated, post-rifting, with oblique movements (along the faults). If the authors wish to retain this term, they must justify it with bibliographic references that already use it.

Response 7:
We want to keep this terminology, because the terms are well used in the literature. We added supporting references and further literature where it has been used.
Please refer to line no. 225 – 243.

Comment 8:
Section 3.3: Fault Throw Analysis
I do not share the authors' interpretation of the commented figures.
While I agree with the authors that there is a notable change in the slip pattern since the rifting phase (pre-24 Ma) and the subsequent phase (post-24 Ma), I estimate based on Figure 5 that the "southward migration of displacement maxima in post-rift sequences" is an unfounded claim. In Figures 5a to 5c, the peaks are stable in their position for the green to orange markers (CBS to JT1 top). In Figures 5f and 5g, the migrations of the two bumps on each profile change direction between pre- and post-rift, but not uniformly southward, as the authors suggest. This point needs to be clarified, discussed, and conclusions potentially revised.

However, I believe the authors overlook an interesting point: There is an evolution between the pre- and post-rift phases. The complex displacement pattern (camel-back shape for 1A, 2B, 1D) decreases over time, eventually forming only a bell-shaped pattern.

Response 8:
We now address RC1's comment by highlighting the evolution from a complex "camel-back" (segmented) shape in the rifting phase to a simpler "bell-shaped" (linked) pattern in recent times (transtensional phase).
We also now individually describe the fault characteristics.
Please refer to line no. 321 – 348.

Comment 9:
Section 4: Discussion
This section needs revision if my interpretation of Figures 5 and 6 is correct. I do not question the existence of a tectonic change around 24 Ma, which is documented in the literature and supported by the data presented in this work. However, I am not convinced by the authors' arguments (correlation between subsidence rates on the two fault families and changes in tectonic stresses).
4.1 Fault Propagation
Even if the authors' conclusion is sound, they must clearly explain why a potential southward migration of displacement (which is not demonstrated for 1A and 1B!) would indicate a shift from an extensional regime to a transtensional one.

Comment 10
4.2 Fault Interaction
According to the authors, the WNW transverse faults become truly active after CBS (post-rift phase). How, then, do they explain why 2A no longer impacts 1D (Figure 5f)? How do they explain Figure 5f, which shows an EI profile almost aligned with the value of 1?
The authors state that the change in tectonic regime after CBS (post-rifting phase) alters the activity of the transverse faults 2A and 2B, which then show stronger subsidence. However, in Figures 6m and 6n, this is not clearly reflected in an increase in the "normalized EI," especially for 2A.

Response 9 and 10:
We agree with RC1 comments, and we have now recognised that there is no southward migration. This has been removed from the text. Section 4.1 and 4.2 are now merged to Section 4.1, discussing the observation (sections 3.1, 3.2, and 3.3) and what it implies.
Please refer to line no. 357 – 406.

Response 10:
We now explain also how Rupelian clay affects the behaviour of the faults.
Please refer to line no. 268-282, 366-379.

Comment 11:
4.3.1
The authors declare that the maximum horizontal stress field was oriented NNE-SSW during the rifting phase. What is the argument in their data, or which reference do they refer to?

Response 11:
Additional references were added and explained with regards to our findings.

Please refer to line no. 424 – 431

Comment 12:
4.3.2
It would be helpful to remind readers of the justification for the stress rotation proposed by Schumacher. As I understand it, this is based on a comparison between subsidence zone geometries (data provided by this author, Schumacher) and old microtectonic data (Villemin and Bergerat, 1987). While the latter data are undisputed, it should be noted that the polyphase interpretations at the time were not chronologically constrained.

The authors indicate a zig-zag structural pattern for 1E at the intersection with 2B during the CBS period as a marker of this stress state change. Cartographically, the zig-zag I see is on 1D at the intersection with 2A. Can the authors confirm their statement and explain how this is a marker of the stress state change?

Regarding the discussion of the evolution of 1E's geometry over time and depth, I suggest the authors illustrate their argument with an appropriate seismic section.

Lines 309–312: It is difficult to reconcile the "vertical displacement acceleration since the Aquitanian (UHY)" with what was described earlier. Which faults are being referred to in particular? Are these the ones described in Mair et al. (2025), which show 35 m of throw in the Quaternary?

Response 12:
We added mentioned references, also we explained our findings in the discussion section in more systematic order to avoid confusion.

Please refer to line no. 434-448.

Comment 13:
4.4 Influence of Fault Activity on Sedimentation Patterns (previously)
This point should, in my opinion, be addressed first in the discussion.

Response 13:
Some of the observations and related discussions were repeated in this section. We decided to delete this section.

Comment 14:
4.5 The URG as a Natural Laboratory for Oblique Rifting (previously)
I am not a specialist in "basin analysis and tectonics (at long-term scales)," so I cannot comment on whether the area is emblematic of oblique rifts. I wonder whether the bibliography is up to date.

Response 14:
Some of the observations and related discussions were repeated in this section. Also, section 1.4 defines terminology, which brought consistency to the discussion. We decided to delete this section.

Comment 15:
4.6 (look more into references)
The authors could refer to active fault systems where earthquake clusters migrate over time from one segment to another within the same fault system, such as in the Central Apennines. This could help explain such apparent discrepancies.

Additionally, regarding the recent (Quaternary) activity of the faults on the eastern border of the URG, the authors could refer to recent observations by Peña-Castellnou et al., where a horizontal slip rate was derived.

Response 15:
We agree to reviewers' comment and updated this section. We tried to explain our observations by referring to the suggested reference.

Please refer to line no. 452 – 467.

Comment 16:
Conclusion
This section provides a good summary of the paper's findings. However, I highlight two specific points:

First, the authors begin this section with a major theme that has barely been developed earlier: the decoupling of deformation on either side of the Rupelian (RT). The paper presents results and analyses that largely focus on what happens after the Rupelian, during the Late Oligocene and Miocene. This point is not clearly addressed before Section 4.4 in the discussion, nor is it mentioned in Section 3.2, which deals with the tectonic control of sedimentation. I suggest this theme be further developed in the core of the paper and placed in its proper context, as already suggested above.

Second, the authors do not provide a clear perspective on the methods that could be deployed to obtain observables of the strike-slip component of the faults described in the basin or those in its vicinity. For example, new microtectonic studies coupled with U-Pb dating of syn-tectonic calcite?

Response 16:
We agree with the reviewers' comment and have included an explanation of why we think that material behaviour can influence fault propagation.

Please refer to line no. 137 – 142, 268-282, 366-379, 481-483.