Dear editor,

In the article “Networks of geometrically coherent faults accommodate Alpine tectonic inversion offshore SW Iberia”, the author presents a beautiful and excellent quality set of reflection seismic images of the Alentejo offshore basin, in SW Iberia. He proceeds to use his interpretation of the seismic profiles to propose an idea of coherent and systematic inversion of a fault network in a passive margin and the influence of crustal heterogeneity on this pattern. This a valuable idea that could be tested on other margins (including some nearby ones!) with potentially very significant implications. He further raises the prospect of the analysis highlighting potential seismic risks for this region, which is a highly valuable contribution to risk mitigation. The article is very well written, and is very easy to follow. I do however, have some significant issues with the interpretation of the seismic profiles presented, as the fact that they are in the time domain has been mostly ignored (or so it seems to me) and the interpretation could be driven significantly differently if these lines were interpreted in depth. In that sense, I think the author needs to make a much better case to support/argue for his interpretation and to discuss what uncertainties may underlie it.

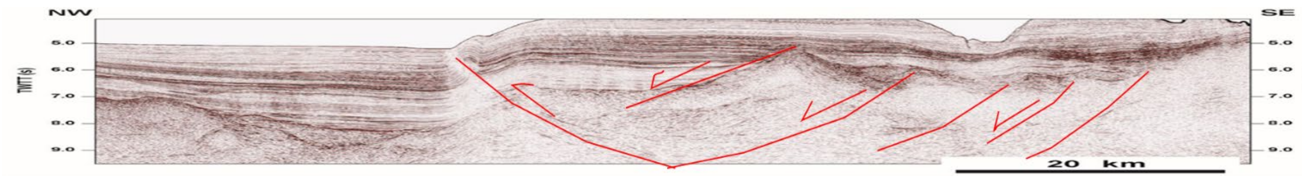
best regards,

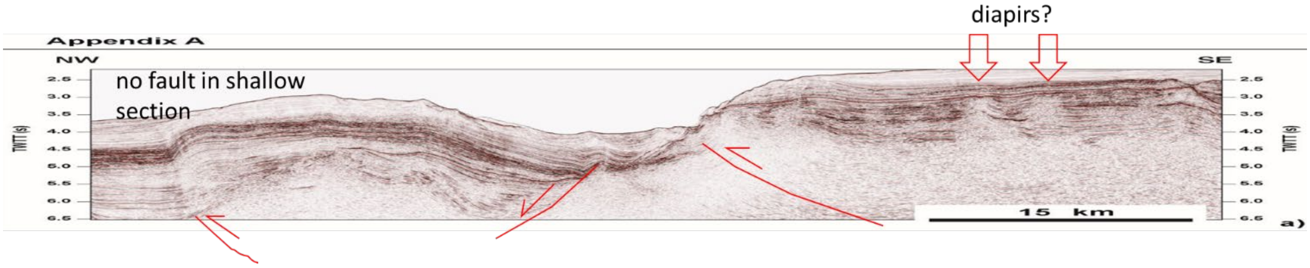
Oscar Fernandez

Major issues:

Vertical exaggeration is major on the seismic profiles. It is misleading not to include even an estimate of what this could be. On Fig. 5, for instance, assuming a high velocity of 4000 m/s for the whole line, we would have a vertical exaggeration of 3.3x (Fig.4 and Fig. 7 have around 1.7-1.8x exaggeration with that same velocity; App. A1 is 3.1x exaggerated; App. A2 is a whooping 4.7x exaggerated if not more!). Naturally, vertical exaggeration depends on the velocity and since shallow velocities can be very low, then the shallower portions (water velocity 1500m/s) are highly exaggerated and the deeper ones are possibly not as exaggerated as calculated above (basement velocity can be 5-6000 m/s or higher). Petrel and other software tools provide excellent options for depth conversion. I strongly encourage the author to provide even a first-pass depth conversion (water at 1500 m/s would make a HUGE difference in how the uplift-related bathymetry is perceived) and in its defect an estimate of vertical exaggeration. Just for the sake of demonstrating the importance of not looking at refelction seismic only in the time domain, an illustration of the lines in Fig 5 and Appendix A1 are shown below, scaled very approximately so that every second TWTT is equal to 2 km (4km/s seismic velocity). I think the difference in how one could interpret these lines is clear. Of course, the author has traced these structures across multiple lines for consistency, and has even uncovered internal coherence in his interpretation, but I find a much better case needs to be done to convince the reader that the interpretation is actually correct. Sorry Tiago for being a pain with this: I think we really need to be extremely careful with seismic! No ill intention meant, but I would really expect to see a better clarification of why you have picked faults the way you have, and what uncertainties there may be in the interpretation and what impact (if any, of course) this would have on the rest of the analysis.

The same applies to the analysis of throw and heave (Section 6). You are combining different magnitudes (time and true distance). It is well worth clarifying this and stating any possible caveats.





On the line in Fig. 7, the depth in time goes down to 11s TWTT. This depth is huge! Most likely, some of the reflectors you are seeing could even be Moho!!! I know the Estremadura spur is likely not in isostatic balance (held up by far field stresses, which only makes things worse, actually), but in isostatic balance, your Moho reflections tend to come in at depths of 9-11s TWTT!! See the paper of Warner (1987, https://doi.org/10.1111/j.1365-246X.1987.tb06651.x). I encourage you to have a look at this line again and see if there is something even more spectacular that you may have missed!!! :)

Fault 3: I understand this becomes a key structure in your analysis. I have, however, a question as to whether this is really a single fault, and as to whether it really is an extensional fault. On Fig. 4 you clearly identify it as a SE dipping thrust. Why do you not interpret it the same way on Appendix A1? I think it would make much more sense! In any case, if you interpret the fault to change from extensional (transtensional?) to thrust along strike, then you shouldn't really interpret it as being linked. If you observe the throw map for this fault (Fig. 8a, 9a) it seems clear to me that it could actually be formed by three independent segments. As you are working on 2D seismic, I would like to see a better argumentation in favor of this being a single, long (linked-up?) fault.

I am sorely missing a structural map (i.e. isobath or isochron map) of a pre-uplift horizon. The map in Fig. 6 is misleading because the fault cutoffs shown are not referenced to any specific interpretation horizon. The Base of Miocene is a likely candidate. The reason that I ask this is because the structural map is a very quick way of visually assessing the relative throw on each fault. I think this would be a major positive contribution to the article – this helps remove ambiguity on your fault interpretation in a very significant way.

There is a major argumentation flaw (in my opinion) in applying some of the criteria in section 3.2 further down. In Section 5.2 you talk about folded strata and you previously mentioned in section 3.2 that stratal thickness is an important criterion in distinguishing post-rift from syn-rift fault activity. However, your seismic images are shown in <u>time</u>, with partially extreme vertical exaggeration (the values I calculated above are low range values for your sediments, that likely have seismic velocities lower than 4km/s and are therefore more vertically exaggerated). You can <u>only</u> use this criterion if you somehow attempt to display these same structures in <u>depth</u>. In my view, as this is, it is not admissible (sorry for the harshness, no ill intention).

Salt tectonics is prevalent both to the north and to the south (Pena dos Reis et al. 2017, https://doi.org/10.1016/B978-0-12-809417-4.00015-X; Cascao et al. 2023, http://dx.doi.org/10.1306/08072221100; Matias et al. 2011, http://dx.doi.org/10.1306/01271110032; Ramos et al. 2017a, https://doi.org/10.1016/j.marpetgeo.2017.09.028) but is not interpreted here. I understand it has been previously stated that this basin is peculiar because of this absence of salt tectonism. However, on the seismic data provided, there are isolated features that may be diapirs based on their post-rift and pre-uplift growth strata and short wavelength. Most evident may be the structure under the text "blind thrust" above Fault 25 on Fig. 4b. It is somewhat reminiscent of the wavelength of structures in the westernmost Algarve basin (cf. Fig. 12 in Ramos et al. 2017a). Furthermore, even if the Dagorda salt was not a player in syn-rift salt tectonics, it would most likely have acted as a detachment during shortening. Indeed, your "thrust-and-fold-belt geometries" are somewhat reminiscent of fold belts with a well expressed detachment.

In terms of terminology, I understand the use uplift and advection for rock masses, but when you are talking of displacement on faults, the terms that are normally used are throw and heave. Is there a reason not to use these? Furthermore, when you give values for throw and heave, you should refer them to the specific marker/horizon/stratigraphic interval for which you have measured the offset.

Minor comments:

line 50 – I understand it lies "around the corner" but a comparison to the well documented example of Ramos et al. 2017b (https://doi.org/10.1002/2016TC004262) could be interesting

line 91 – "in what ways do tectonic"

line 92 – "and how do they interact"

line 101 – please check the recent publication of Amigo Marx et al. 2022 (https://doi.org/10.1111/ter.12595)

line 146 – there is an event of re-activation of salt structures in the Algarve basin in the Early Cretaceous and related sedimentary record (Ramos et al. 2016, https://doi.org/10.1007/s00531-015-1280-1) that likely relates to the regional picture (note that

line 155 – inversion in the Algarve extends beyond the Miocene (Ramos et al. 2016, 2017b)

line 183 – He et al. missing year

line 187-193 – slight repetition

line 197-198 – beds are not really deposited horizontally on continental margins (the syn-uplift units on your own seismic betray you) ;) I think it is worth stating it is near-horizontal (my personal battle against assuming pre-deformation = horizontal)

line 217 – the faults on Fig. 6 look like they are NE-SW striking, not NW-SE. Can you please check this is correct? Also, if your faults indeed strike NW-SE then sections in Fig. 4 and Fig. 5 would be parallel to your fault, which I think is not what you interpret?

line 219 – "on seismic data"

line 228-29 – NW-dipping normal faults reactivated as SE-dipping reverse faults is a pretty paradoxical statement... I understand it, but I think it would be useful to discuss this style of inversion (compare to the very similar style of inversion described by Ramos et al. 2017b for the Guadalquivir and Portimao Banks in Algarve). As in the example of Algarve, I think this speaks for an inherited basement fabric and could speak for both basins sitting on a similar SPZ-style basement (with inherited Variscan thrust fabric that re-activates as opposed to inversion of normal faults). (?)

line 267 – to speak of 100s of meters, you should provide a vertical scale in meters.

Fig. 6 – can you please check Appendix B and Fig. 4 are correctly placed here? There is a line labelled SF2 which seems to be App B, and App B seems to be Fig. 4? If Fig. 4 is not on this map, please locate it.

Fig. 6 – Fault 3 on Fig. 4 is a SE-dipping thrust, but on Fig. 6 it is represented as a NW-dipping thrust/inverted fault.

Section 6 – the analysis here is mixing depth in TWT milliseconds with horizontal distance in meters. This requires some very careful clarification.

line 320 – provide a Fig. reference for faults 3,7,11. (Fig. 5?)

line 329 – sorry, not sure where you see that the intrusions drive inversion? Did you really explain it before? Wasn't clear to me at least. OK, is it what you later say in line 340? Then you need a reference here – it is not something you are demonstrating with your data.

line 340 and further – if igneous bodies are so important in your inversion history, it is worthwhile including a seismic line that shows these relationships

lines 357-358 – deformation is really mostly accommodated by the folds on the left part of Fig. 7. The thrusts that you interpret linking to the extensional faults are really minor. They don't really accommodate much shortening... furthermore, I am not fully convinced that what you are seeing at the base of the thrusts is really syn-rift strata and not some form of deep crustal reflectors.

line 390 – I don't fully follow this argument. Thermal subsidence is well known for passive margins. I do not know that it applies to inversion setting (!?) nor to intrusions (whose thermal-related dilation potential is probable limited).

Fig. 8 – you place your throw and heave bars always on the western side of the faults. However, I find this misleading and I think it would be much more accurate to place them always in the hangingwall or always in the footwall, or any other placement that makes kinematic sense (block up vs block down). But placing them always west seems to indicate these are the upthrown or downthrown blocks consistently, which is not the case.

Fig. 9 – you should NOT plot depth vs time domain units. This is not valid. Please revise, provide an approximate conversion factor or something similar before you attempt this.

Fig. 9, 10 – it is not clear what your horizontal axis is. It needs to be clarified and made evident in a location map. I assume you are referring to dip seismic profiles? Are they parallel to your assumed shortening direction? If not, then you cannot really add up displacements... You need to state these assumptions better.

line 430 – this is an interesting thought. However, shortening should be accommodated along strike equally. What happens to the shortening that is not expressed around the intrusions? Is this internal strain or is it being transferred elsewhere?

lines 459-463 – this is very interesting point and perhaps is worth bringing into the discussion. After all, this provides a mechanism to accommodate the type of dynamic topography implied by the lithospheric folding model (Cloetingh et al. 2002, https://doi.org/10.1029/2001TC901031)

line 465 – I do not see that the authors have documented intrusion-related uplift... Sounds like a conjecture? Please check this is adequately argumented.