Dear Editors:

On behalf of the authors, I am pleased to submit the revised manuscript “Discontinuous transtensional rupture during the Mw 7.2 1995 Gulf of Aqaba earthquake” for consideration in Seismica. We have applied minor revisions to the material. The revised manuscript includes one new figure and many changes to the text. The latter are highlighted in blue font in the markup-version of the manuscript. Major edits entail:

- updated Figures: 1, 3, S13
- uploaded InSAR data to Zenodo and citation in the section "Data and code availability"
- extended discussions

Below you will find all the comments and suggestions made by the editor and reviewers and our responses, as well as notable changes we have made to the manuscript.

Sincerely,
Hannes Vasyura-Bathke

1 Editor Comments

1. I am pleased to say that I have now received two peer-review reports for your manuscript. Both reviewers are supportive of your work being published in a Research Article format in Seismica. They also suggest that some revisions are needed before publication. In particular, I agree with reviewers A that more information need to be provided about the InSAR data used (which satellite, what time spam and what is the possible contribution of post-seismic deformation).

   Response: The technical information on the SAR data was already previously included in Tab. S1 and, secondary sources of deformation were discussed in detail in section 4.3. The reviewer might have missed these. So we refer to that section again in the sec. 2.2.1. when describing the data.

   Changes: Added to sec. 2.2.1: "The influence of possible secondary sources of deformation on estimated parameters is discussed in sec. 4.3. ."

2. Regarding reviewer's B comment about the code availability, although we encourage authors to share codes that can be executed to reproduce the results, this is not required. However, more details should be given in the "data and code availability" section to explain which code was used for which step (in particular the author should detail which code is used for the backprojection, which one for the finite-fault inversion). A statement about the availability of the InSAR data used is also missing from that paragraph.

   Response: We uploaded the InSAR data to zenodo and we updated the "Data and code availability" section.

   Changes: "The post-processed InSAR data are available for download at https://zenodo.org/records/10462416 (Vasyura-Bathke et al., 2024). Bayesian inferences and multi-array teleseismic backprojection were performed using the Bayesian Earthquake Analysis Tool (BEAT) (https://github.com/hvasbath/beat) (Vasyura-Bathke et al., 2019, 2020) and Palantiri (https://github.com/braunfuss/Palantiri) (Steinberg, 2021; Steinberg et al., 2022), respectively."

2 Reviewer 1 Comments

2.1 Major comments

1. GCMT solution of the 1995 earthquake has 13% CLVD, which might be due to uncertainty, but it could also reflect the mixed faulting (strike-slip and normal) revealed by the authors. Perhaps the authors could calculate the moment tensor of their finite fault solution and compare the amount of non-DC part with GCMT.

   Response: The moment tensor of our finite fault solution shows 99 and 1% of DC (Mw 7.19) and CLVD (Mw 5.9) components, respectively. In our understanding that is not particularly noteworthy and we refrain from mentioning that in the manuscript.

   Changes: No changes.

2. In my experience (see Gallovič et al., 2015, sec. 5, doi:10.1002/2014JB011650), the spatial discretization of the kinematic finite fault must follow the maximum modeled frequency (see also references therein; in particular Sekiguchi et al., 2002, doi:10.1046/j.1365-246X.2002.01669.x). In the last step, the authors of the present manuscript model waveforms up to 0.5Hz. Considering the slowest velocity in the model to be the rupture speed, the maximum frequency corresponds to 6km minimum wavelength. The spatial sampling should be a fraction of the wavelength, i.e., much less than 5km, as considered by the authors. Otherwise, the sub-faults act as point sources instead of elements of the integral of the representation theorem, leading to ringing artifacts in the synthetics. Note that the moment rate functions shown by the authors feature strong peaks that might be related to this point. The authors should check whether this issue does not bias their finite-fault inversion.

Response: In general, the reviewer is right that spatial aliasing in the Greens functions is indeed an issue if the fault patches are not sampled dense enough. However, we discretize each fault patch with several moment tensors in order to simulate a finite extent of a rectangular fault patch of a spatial sampling interval of 1km in depth and distance directions (see sec.2.2. "... we calculate Green's functions (GFs) with 1 km spacing ...") (Heimann et al., 2019) to make sure that aliasing is not an issue. The works that the reviewer is referring to demonstrate case studies at local and regional distances where this issue is of larger importance. In this work, we use seismic stations at teleseismic distances of $> 3000km$ distance where our assumed spatial sampling interval for Green's Functions is sufficient.

Changes: No changes.

3. In Sec. 2.2.1, the authors mention the long time span of the interferograms. They should discuss that there might have been a contribution from (mainly shallow) postseismic slip along the considered causative faults. In other words, the co-seismic slip from the teleseismic data inversion does not necessarily have to be the same as what is suggested by the InSAR data. There are real-world examples of such discrepancies, such as the 2004 Parkfield or 2014 Napa earthquakes.

   Response: The reviewer might have missed section 4.3. where we have discussed several secondary (non co-seismic) sources of deformation that also covers the mentioned requested post-seismic slip.

   Changes: Such that readers can find that section easier we refer to that section earlier in the manuscript discussing the interferograms. "The influence of such secondary sources of deformation on estimated parameters is discussed in sec. 4.3."

4. The explanation of how fault segments are anchored in space (Sec. 2.2.3) is confusing. The authors refer to centers of mapped surface ruptures of the earthquakes but do not show them exactly. Are they the tiny black broken lines close to the label "Elat Deep" in the inset of Fig.1? Moreover, the authors explain that the top centers of the fault are fixed at that point. However, especially in the constant slip inversion, this sounds like too strong constraint because only the fault size is searched for, and thus the rectangular slip patch could not "move" spatially during the inversion. The authors should clarify this issue.

   Response: Indeed, in an earlier stage of the work we allowed the location of the fault segments to be free as well, however, due to the complexity of the earthquake this did result in physically non-plausible fault geometries that were not consistent with regional tectonics and geology. Thus, we followed the assumption that fault offsets at surface mark the location of the upper edge of the fault, which is a common assumption in the literature of earthquake source studies (e.g. Jónsson et al., 2002; Minson et al., 2013). In the uniform slip inversion we did not only search for fault size but also for the upper edge depth as well as strike and dip to allow for some flexibility of the fault geometry to be explored. All parameters that were estimated were listed in sec. 2.2.3 "We estimated geometry and fault-slip kinematics of rectangular fault segments considering the following parameters for each fault segment: depth, length, width, slip, strike-, dip- and rake-angles.", while the marginals of parameters that were searched for are shown in Figures S2/S3 and S8-S10. Still in order to be able to account for some of the theory error that may be caused by this constraint we allow for the hierarchical error scalings. "We also estimated a noise-scaling parameter for each dataset residual to account for data and theory errors (Vasyura-Bathke et al., 2020, 2021)." We agree that it was unclear, which mapped surface ruptures we were referring to and tried to highlight these in the new version of the manuscript.

   Changes: We highlight the mapped surface ruptures with an arrow in Figs. 1 and 3. We edited the text: "The top-center points of the fault segments have been fixed to be located on the mapped faults and surface ruptures of the earthquake (Ribot et al., 2021; Lefevre, 2018) (i.e. faults annotated AgF, AnF and mapped ruptures in Fig. 1)."

5. In Sec. 4.2.2, the authors estimate rupture velocity from backprojections (lines 392 and 415) without showing any figure. It would be instructive to show the classical semblance plot with distance along the Gulf on the x-axis and time on the y-axis, with several lines marking different rupture velocities.

Response: Thank you for that comment!

Changes: We included the suggested plot as subplot to Fig. 3c. The caption is extended by ”c) Ensemble of multi-array semblances projected along the axis of the gulf (strike of 18° towards North) referenced with respect to the earliest semblance between -8 and -6 s. Dashed black lines show theoretical rupture velocities while shaded grey lines show averaged velocities for the whole ensemble of semblances.”

6. I suggest using active tense throughout the manuscript to make the text more appealing and reading livelier.

Response: To the best of our understanding we mostly use active tense in the manuscript. In sec. 3.1. on the backprojection results we prefer to keep that in passive voice as the earthquake already occurred.

Changes: No changes.

2.2 Minor comments

1. Fig. 1, caption: I suggest adding “of the 1995 event” to the first sentence before the brackets. For a while, I was confused about whether the beachballs were aftershocks or what.

   Response: Done.

2. Fig. 1, caption: I suggest changing “causing postseismic deformation” to “contributing to postseismic deformation” because later, the authors comment on other possible causes, including an aseismic movement.

   Response: Done.

3. Line 135: “be be” should be corrected.

   Response: Thank you for spotting that!

4. Line 138: Typo in “propgation”.

   Response: Corrected.

5. Line 144: “after-shock” should be changed to “aftershock”.

   Response: Corrected.

6. Eq. (1): There are missing brackets around the residuals $(d_{obs} - d_{syn})$.

   Response: Corrected.

7. Line 230: The explanation of how the semblance maps from the backprojections are used to constrain the seismic-data inversion should be better clarified. Is it used as a prior? Of what? Onset points or nucleation times of the segments only?

   Response: We clarify that the prior of the nucleation times was constrained.

   Changes: The edited sentence reads now: ”We constrained the prior for the rupture-nucleation times for each fault segment based on a-priori information from the back-projection semblance maps.”

8. Line 288: The authors state that the three segments significantly improve the data fit without quantification. Later, they use the Bayesian information criterion (BIC) in a similar respect (line 342). Perhaps the same quantification can be performed here to support the statement.

   Response: The improvements refer to the improvements in variance reductions (VRs) that were quantified in the sentences before that finalizing sentence. As the inference at this stage was done using geodetic and seismic data jointly a BIC calculated by only using seismic data would be erroneous. We modified the sentence to clarify that we are referring to the improvements in variance reductions.

Changes: The sentence now reads: "These improvements in VRs in comparison to the two-segments setup are significant and support ..."

9. Line 314: Term "unilateral rupture towards in down-dip direction" sounds odd.

   Response: Corrected.

   Changes: Now reads: "unilateral rupture towards the down-dip direction"

10. Line 412: The semblance maps from the synthetic experiment are spatially smaller than those from the real data. Perhaps the authors could add a comment on the cause of it.

    Response: We have observed this as well for other cases where we have employed the backprojection method and where we have compared it with synthetics (Daout et al., 2020; Steinberg et al., 2022; Hicks et al., 2020). The relatively simple kinematic model is only capable of producing sharp start and stop phases, while in reality coherent energy will also be radiated due to fault geometrical changes as well as changes in rupture velocity. The strongest influence to synthetic backprojection is the assumed source depth. For the backprojection method applied here, all semblance gets mapped onto a fixed depth, projecting the semblance from other (true) source depths onto the semblance map of this fixed depth.

    Changes: We added to sec. 4.2.2.: "The simple kinematic model used to calculate the synthetics produces sharper start and stop phases than actually observed (Steinberg et al., 2022), as the modelled fault ends abruptly, whereas in nature there would be a tapering. Furthermore, the synthetic semblance maps produce much sharper semblance patches in comparison to the real data backprojection. The reason for this is due to the lack of noise in the synthetic backprojection and, the fact that semblance is mapped onto a single fixed depth, which causes a blurred semblance in the real data backprojection."

11. Line 440: Could a smaller segment connecting the two faults facilitate the apparent rupture jump? Such a segment could be simply unresolvable by the employed data.

    Response: This is certainly a possible explanation and we agree that an "apparent jump" of the rupture is also plausible. However, it would have to be a fault segment that is small and with a smooth rupture process, without distinct changes in geometry or rupture velocity, as to not promote the occurrence of significant start and stop phases itself.

    Changes: We added: "Another possibility is that a small, unresolved normal-fault segment acted as an intermediary and facilitated an apparent jump of the rupture. However, this would require that the rupture process on this small normal-fault segment was very smooth and did not emit any start or stop phases that could be resolved through backprojection using the available data."

12. Tab. S1: An explanation here (or elsewhere) of T343off shown in the figures with InSAR data is missing.

    Response: We added a sentence to the caption of Tab. S1.

    Changes: "The data named 'T343off' shown in SAR data misfit figures (S5d, S12d, S16d, S21d) are amplitude offsets derived from the data of track T343."

13. Fig. S1b and S1c: The traveltime corrections in the backprojections seem very coherent. I would expect them to be rather random due to 3D heterogeneities in the Earth. Perhaps the authors should discuss what can be their cause.

    Response: The Fig. S1b shows the estimated traveltime corrections for the backprojection, while Fig. S1c shows the traveltime correction for the P-phase in the kinematic fault inversion and Fig. S1d for the S-phase. Traveltime corrections at the used low frequencies are mostly accounting for the larger scale local/regional inconsistency at the source in comparison to the used 1-D velocity model. Therefore, the coherent trend of traveltime corrections reflect in this case the interaction of the seismic phases with the trend of the regional geology of the broader region around the Gulf of Aqaba, such as possibly the Red Sea in the south leading to apparent slower phase velocities for the backprojection. Other possible 3D path effects further from the source, at the used frequencies and distances, lead to second order differences with the available global 1D velocity model used (as can be observed in all three figures).

Changes: No changes.

14. Fig. S3: Reference to the caption of Fig. S2 should be used instead of S8.

    Response: Corrected.

15. Supplementary page 8 (and at several other locations): The repeating occurrence of the Figure number at the beginning of the caption continuation should be removed.

    Response: Done.

16. Fig. S13: Some superfluous symbols are in the middle of the figure.

    Response: Good catch!

    Changes: We updated the figure S13.

17. Fig. S16: Reference to the caption of Fig. S11 should be used instead of S15.

    Response: Corrected.

18. Fig. S19: "s-waves" should be corrected to "S-waves".

    Response: Corrected.

19. Fig. S21: Reference to the caption of Fig. S11 should be used instead of S20.

    Response: Corrected.

20. Fig. S22: A dot is missing at the end of the caption.

    Response: Added the dot.

3 Reviewer 2 Comments

3.1 Major comments

1. Discussion section 4.2.1 is somewhat unsatisfactory. There is a list of possible explanations given for the artifacts in the results, but no discussion of which if any is more likely, what data might resolve the issue, or if other studies shed any light.

    Response: Sources in theory error in geophysical inverse problems are cumulative and being able to distinguish between them is considered a holy grail in seismology. So each of the listed causes of theory error are likely present, but they cannot be quantified with the available data. We added that regional seismic data could likely help resolving the rupture evolution in larger detail.

    Changes: We added to sec. 4.2.1.: "To improve this, the kinematic evolution of the rupture could be better resolved by utilising regional seismic data, which would likely reduce bias and artifacts in the estimated parameters."

2. One of the most obvious of these possible artifacts is the area of clear semblance in the far northern area of the Gulf, which the authors suggest is an artifact but do not provide sufficient discussion of why that may be, or whether it could relate to fault activity etc. There is a paragraph about this in Section 4.2.1, but some reference to it should come earlier in the paper (near Fig. 3 where the semblance maps are shown). More discussion would be appropriate here as to what interpretations are most likely given other studies, what data are needed, etc.

Response: We added an earlier explicit mention and also offer a evaluation of the source of mismapping - likely depth in our opinion. In 1995 there is not sufficient array data available to carry out a full 3D backprojection to fully resolve this question. Therefore, we have to speculate based on experiences from other studies (Steinberg et al., 2022; Daout et al., 2020). Specifically, we think that this last semblance patch to the north is due to missmapping of depth phases, possibly sP phases, which are relatively large for strike slip earthquakes. We added this interpretation to the discussion.

Changes: We added to section 3.1: "The high-frequency energy radiation at 10-12 s is possibly an artefact of the processing and discussed in detail in sec. 4.2.2." We added in section 4.2.2: "No other study reported rupture of a fault segment this far north of the EF, therefore, a mismapping of the semblance seems likely. Previous studies have shown that the choice of grid depth and the deviation of the source depth from the grid depth have a strong effect on the location of the mapped semblance (Steinberg et al., 2022; Daout et al., 2020), so this seems to be the likely cause." as well as: "Furthermore, the depth phase separation is challenging, particularly for strike-slip earthquakes that generate relatively large sP phases."

3. I am not sure that the authors' statement on data and code availability is up to the Seismica standard. My understanding was that codes needed to provided to reproduce the results shown in the paper. I defer to the editor on this one.

   Response: As requested we are now more specific about which code was used for which aspect of the presented work. We also uploaded the InSAR data to Zenodo. Please also see our answer to editor comment 2.

3.2 Minor comments

1. Abstract, line 30: "...should be considered when conducting ....

   Response: Corrected.

2. Introduction, lines 50-51: do you mean that the Gulf is similar to the Dead Sea fault? Clarify what is similar.

   Response: The slip rate is similar to that of the Dead Sea fault. We edited the sentence.

   Changes: The sentence reads now: "Geodetic observations show that the current left-lateral interseismic motion in the gulf is similar to that of the Dead Sea fault, with a small amount of opening across the gulf (ArRajehi et al., 2010; Li et al., 2021; Castro-Perdomo et al., 2022; Viltres et al., 2022)."

3. Line 79: "...are not sensitive..." This is my own pet peeve, but probably it's better to say "...are not very sensitive..." or something like that. InSAR is sensitive to N-S deformation, just not very much.

   Response: Done.

   Changes: Added "very".

4. Last paragraph of Section 1, lines 96-97: I would put the part starting "We here derive ..." at the beginning of the sentence.

   Response: We think that makes the sentence unclear and more difficult to follow.

   Changes: No changes.

5. Section 2.1, line 113: delete the 't' in 'not'

   Response: Corrected.

6. Figure 5: "Colored histograms show estimated rupture nucleation times of each faultsegment" I was not sure what this means / refers to. Is it referring to part b? I would clarify or remove.

Response: These refer to subfigure a).

Changes: We edited to "Colored histograms (next to the moment-rate) show ..."

7. Line 361: "Aragonese deep" should deep be capitalized?

   Response: Thank you! We capitalized all occurrences of "deep" referring to the sedimentary basins.

8. Section 4.3, line 451: re-write to "... magnitudes $Mw \geq 3.9$..." (i.e. put the Mw and inequality sign before the number)

   Response: Done.

9. Section 4.4, line 464: "simulation" should be "simulating" I think

   Response: Corrected.

10. Section 4.4, line 468: "Khrepy et al" should be inside the parenthesis.

    Response: Corrected.

11. Section 4.4, line 470: change to "derived using topographic slope as a proxy" or something like that

    Response: Done.

    Changes: The sentence reads now: "We considered site effects through the shear-wave velocity in the thirty meters below Earth surface, Vs30, derived from the topographic slope as a proxy (Wald and Allen, 2007)."

References

Daout, S., Steinberg, A., Isken, M. P., Heimann, S., and Sudhaus, H. (2020). Illuminating the Spatio-Temporal Evolution of the 2008–2009 Qaidam Earthquake Sequence with the Joint Use of Insar Time Series and Teleseismic Data. *Remote Sensing*, 12(17):2850.

Heimann, S., Vasyura-Bathke, H., Sudhaus, H., Isken, M. P., Kriegerowski, M., Steinberg, A., and Dahm, T. (2019). A Python framework for efficient use of pre-computed Green's functions in seismological and other physical forward and inverse source problems. *Solid Earth*, 10(6):1921–1935.

Hicks, S. P., Okuwaki, R., Steinberg, A., Rychert, C. A., Harmon, N., Abercrombie, R. E., Bogiatzis, P., Schlaphorst, D., Zahradnik, J., Kendall, J.-M., and Others (2020). Back-propagating supershear rupture in the 2016 M w 7.1 Romanche transform fault earthquake. *Nature Geoscience*, 13(9):647–653.

Jónsson, S., Zebker, H., Segall, P., and Amelung, F. (2002). Fault slip distribution of the 1999 Mw 7.1 Hector Mine, California, earthquake, estimated from satellite radar and GPS measurments. *Bull. Seismol. Soc. Am.*, 92(4):1377–1389.

Minson, S. E., Simons, M., and Beck, J. L. (2013). Bayesian inversion for finite fault earthquake source models I-theory and algorithm. *Geophys, J. Int.*, 194(3):1701–1726.

Steinberg, A., Sudhaus, H., and Krüger, F. (2022). Using teleseismic backprojection and InSAR to obtain segmentation information for large earthquakes: a case study of the 2016 M w 6.6 Muji earthquake. *Geophys, J. Int.*, 232(3):1482–1502.