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Review Report

Heckenbach et al., 3D Interaction of Tectonics and Surface Processes Explains Fault Network Evolution of the Dead Sea Fault, TEKTONIKA, 2024.

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1st Round of Revisions

Decision Letter

Dear Esther L. Heckenbach, Sascha Brune, Anne C. Glerum, Roi Granot, Yariv Hamiel, Stephan V. Sobolev, Derek Neuharth:

We have reached a decision regarding your submission to тектоniкa, "3D Interaction of Tectonics and Surface Processes explains Fault Network Evolution of the Dead Sea Fault". The manuscript was handled by Associate Editor, Guillaume Duclaux, and received two reviews. Guillaume nicely summarises the reviewers opinions and I find very little to add. I thank you for submitting to Tektonika and and am grateful to the reviewers for their expert aid.

Our decision is: Revisions Required (please see below for details)

Kind regards,

Craig Magee (Executive Editor)

Associate Editor letter:

We have now reached a decision regarding your submission to тектоniкa, "3D Interaction of Tectonics and Surface Processes explains Fault Network Evolution of the Dead Sea Fault".

Our decision is to request minor revisions to the manuscript.

This decision was reached based on two thorough and constructive reviews, which both conclude that this work is an important contribution on a timely topic, but some edits are required.

In order to enhance the manuscript's accessibility and impact please carefully address the two main comments of the reviewers : i) while the models are described as generic, there is some ambiguity regarding their generalisability. The text oscillates between efforts to make the models resemble natural examples of the Dead Sea Fault system and preserving their generic character, leading to potential inconsistencies. Clarifying the hybrid nature of the models and emphasizing their advantages for proving concepts could address this issue. ii) While the overall writing is good and the illustrations are very well drafted, there are instances of technical jargon and unclear wording that hinder comprehension to non numerical modelers. A thorough round of polishing to address these issues would enhance the clarity of the manuscript.

We encourage you to carefully address all of their comments, documenting all changes made, and we look forward to reviewing a revised version soon.

Comments by Reviewer A (Attile Balazs) and author response

We thank the reviewers for taking the time to provide detailed and constructive feedback on our manuscript and believe that the manuscript greatly improved with it. Below, we display the reviewer's comments in black and our answers in blue. New text is highlighted in bold.

At the beginning of the manuscript, it is stated that the joint modelling of releasing and restraining bends lead to a better understanding of the system and separately simulating a releasing or restraining zone. Can such a statement be done without comparing this with a model with only a releasing or only a restraining zone?

We have now made clearer the advantages of a model setup that includes both bends in a periodic setup in the Introduction, starting in line 89:
"In this study, we utilize geodynamic models to investigate the evolution of strike-slip faults that include both a releasing and a restraining bend ***in a periodic setup. Compared to previous studies featuring only one kind of bend, this has the advantage that possible interactions are included and fault kinematics do not need to be prescribed in the boundary conditions but can evolve according to the accumulation of stress and strain.****"*

Ln. 31-32: there are several other continental transforms, so please write: for example, or for instance. It would be also better to add other references too. There are several relevant ones discussed in the review by Sengör 2019 (Chapter 1 - Franz Lotze and the Origin of the Idea of Transform Faulting in Central Europe)

Thank you for this suggestion. We changed the wording and added references to each of the faults mentioned.

"*Besides the Dead Sea Fault* ***(Dubertret, 1932),*** *other prominent* ***examples of*** *continental transform faults are the San Andreas Fault of California* ***(Lawson, 1895; Gilbert et al., 1907)****, the East and the North Anatolian Fault in Turkey* ***(Ketin, 1948; Arpat, 1972; Seymen and Aydin, 1972)****, the Alpine Fault of New Zealand* ***(Wellman, 1952)****, and the Altyn Tagh Fault in Tibet* ***(Tapponnier and Molnar, 1977)****."*

In the manuscript it is not always indicated if the authors mean surface heat flow, basement heat flow, sediment blanketing corrected heat flow, etc. Therefore, please specify this at the beginning of the manuscript.

Good that you spotted this inconsistency. Since we are only writing about surface heat flow, we have now replaced all occurrences of "*heat flow*" with "***surface heat flow***" throughout the manuscript. Since this should make it clear everywhere, we did not add an additional explanatory sentence.

Ln. 53: the authors mention ca. 8-12 km sedimentary thickness. Is it in line with the reference model results? It would be also useful to plot a subsidence and a separate sediment accumulation history plot and compare this with previous backstripped data from the basin.

We compare sediment thickness from model results and observational data in Fig. 4. The reference model shows very good agreement. To better link this existing figure to the corresponding text passage in the results section 3.2, we added another reference to the figure in line 360:
"Our reference model does not only closely fit the width, height, and slopes of the basin bounding topography, but also matches the observed maximum sediment thickness of 8-12 km ***(compare blue lines and blue shaded area in Fig. 4d)*** *(Ten Brink et al., 1993; Garfunkel and Ben-Avraham, 1996; Götze et al., 2007; Mohsen et al., 2011)."*

I am a bit concerned about the 100 km depth of the model domain. What is the lower thermal boundary condition? A constant heat flux? If it is a constant temperature, than during thinning of the crust and lithosphere, it would lead to model artifacts, their role could be tested by using a deeper model or a heat flux boundary condition. Please clarify this.

We use constant temperature bottom boundary conditions. We tested the impact of model depth and prescribed boundary temperature but it did not change our conclusions, since only secondary details changed, but model results like fault outlines, stress regime distribution, heat flow pattern, topographical features and asymmetries remained unchanged . We now also included this in the manuscript: For the boundary temperatures, we added a sentence in the Model setup section:

"The initial temperature distribution is based on a steady-state 1D conductive continental geotherm in the lithosphere and on an adiabat below the lithosphere (see Fig. 2c and Supplementary Table S1). ***The top surface is fixed to 273 K and the bottom to 1615 K. Varying the depth of the lower model boundary (80 km and 110 km instead of 100 km) showed no major influence on modelling results."***

The authors write (even in the title) that they simulate the evolution of the Dead Sea Fault, later it is mentioned that they used a thicker lithosphere than the region would require to "preserve the generic character of the model". This can be done, but please also show, either in the main text or in a supplementary a model with lithospheric thicknesses similar to the studied region. What is the role of using a thinner lithosphere? In general, many transform have similarly thin lithosphere as the DSF, so I am not entirely sure, why a thicker one is more generic.

Thank you for pointing this out. We have added the following explanation to the manuscript:

"The chosen model lithosphere of 100 km is slightly thicker than the current lithosphere along the DSF (Mohsen et al., 2011) to preserve the generic character of the models. ***This thickness, however, coincides with the lithosphere structure at the onset of DSF formation, prior to a hypothesized delamination event (Petrunin et al., 2012)"***

Rheology: what is the role of other strain localization mechanisms, such as grain size evolution?

We do not account explicitly for grain size evolution, but in the brittle domain this process is parametrized through our strain-dependent softening formulation. We have updated the rheology paragraph and model limitations paragraph accordingly:
"The evolution of fault systems is furthermore influenced by frictional strain softening over the plastic strain interval of 0--1 (Fig. 2b), reducing the internal angle of friction from 30° to 7.5° ***for strained material, which mimics small-scale processes like chemical alteration of fault gauge, pore pressure changes due to fluid flow along faults, and grain size reduction in frictional shear zones.****"*
"We therefore did not include the variations in crustal and lithosphere thicknesses observed in our focus area (Gotze et al., 2007; Mohsen et al., 2011; Smit et al., 2008) nor melting, magmatism, ***grain size evolution****, salt tectonics, and potential changes in velocity and thermal boundary conditions [...]."*

Ln. no. 158-168: What drives the initial distribution of the (thermal) weak zones? In nature, they were present prior to the onset of strike-slip faulting or evolved during the early stages of evolution. I think this is the most relevant parameter during the evolution of releasing and restraining bends and therefore, some additional justification for these values would be preferred.

We use thermal weak zones through thinning of the initial lithosphere because they affect the onset of localization but not the latter stages. Once the model has localized into distinct strike-slip domains, the initial seeds do not affect the model evolution anymore, which is why we prefer this approach over introducing weak seeds via compositional fields. We have added the following sentence in the paragraph on Fault localization within the Model setup section:
"[..] all faults form self-consistently according to the prevailing stress field. ***We have varied the amplitude of the perturbation by a factor of 2 and 0.5, which slightly affects the timing of localization, but this choice does not affect the conclusions."***
The choice of initial perturbation offset and overlap values was already justified in the previous version of the manuscript.

Have you considered lacustrine or marine pelagic sedimentation (as a source term)?

Yes, but we finally did not include those sources for reasons of simplicity. We explicitly specify this point in Section 2.1:

"Then, equations describing river incision (extended stream power law), hillslope diffusion and marine sediment transport are solved. The intensity of their combined effect is described as surface process efficiency throughout this text. ***All sediments in the basin derive from the model domain and we do not include additional sources like pelagic sedimentation."***

Please also mention elasticity in the Limitations.

This is indeed a good place to mention the lack of elasticity, which was beforehand only mentioned in the Discussion. We now added a sentence in the limitations: ***"Furthermore, the models lack elastic deformation and evolve under a visco-plastic rheology only."***

Furthermore, the region was affected by magmatism and salt tectonics. It is wise, that these are not included in these models, as it would make the study too complex, but they should be mentioned.

Magmatism was mentioned already in the Limitations section, we have now added salt tectonics: "*[...] nor melting, magmatism, grain size evolution,* ***salt tectonics,*** *and potential changes in velocity and thermal boundary conditions [...]"*

Ln. 218: Accomodation space: with respect to which base level? It would be better to write about basement subsidence and uplift values

We feel that the concept of accommodation space relative to the local sea level fits best to describe how much volume is available for sedimentation. In sedimentology and sequence stratigraphy, accommodation space refers to the volume available for sediment deposition that is not only dependent on changes in the basement depth, but also on changes in sea level, and tectonics, and (previous) sediment input. E.g. see chapter 4.1 from Coe et al., 2003 "The sedimentary Record of Sea-Level Change" (https://www.cambridge.org/gb/files/7113/6682/0319/1535_200388.pdf).

While we already use basin floor subsidence in l. 218 (paragraph "Phase II – Strong vertical motion"), we additionally included this link to accommodation space in the paragraph "Sediments control longevity of pull-apart basin."
"showing how sediments create their own accommodation space (Bond et al., 1988, Mahattanachai et al., 2021, Neuharth et al., 2022) ***by inducing basement subsidence*** *in this spatially restricted continental setting."*

Ln. 264: What are the water depth variations along-strike the basin? Are the modelled values in line with constraints from the DS basin?

The Dead Sea as well as the modeled water body are very shallow in terms of water depth. In this study we rather compare the depth of the crustal basement. Here, the along-strike variations of the modeled and observed basin do fit reasonably well with 1) the greatest depth of the sediment-filled basin being located north of the geometrical center, 2) the Northern and Southern tip being very shallow and extending further North and South with respect to the border faults, and 3) the presence of a fault that separates a medium deep Southern part from a deeper central and Northern part of the basin. However, for the argumentation of the paper, we believe that it is sufficient to compare greatest basin depth instead of including these second order features.

Ln. 307: It would be great to better discuss the role of sediment blanketing in the DSF evolution and in the models. In general, in many cases only mentioning heat flows are anyhow misleading, as it only represents the sum of many effects. It would be better to show a few temperature profiles at different times and locations.

We agree that heat flow values represent the sum of many effects. Thermal blanketing is one of those, another is hydrothermal circulation, which we explicitly discuss in the revised version: *"However, taking accurate measurements of surface heat flow in the DSB environment is challenging (Oryan et al., 2019),* ***for example because they are affected by hydrothermal circulation*** *[…]"*

Ln. 314-316: this sentence is not clear.

Thanks for pointing this out. We have tried to improve the clarity of our statement and it now reads like this:
"*The low surface heat flow* ***aligns with observations of*** *deep seismicity* ***which is equally*** *hinting at a brittle and comparably cold lower crust (Aldersons et al., 2003)."*

Ln. 357: additional references could be added here, both based on observations and based on other model results

We have added an observational reference (**Mahattanachai et al., 2021)**, and a modeling study **(Neuharth et al., 2022)**.

The authors write at several places the evolution of strain partitioning. This could be better clarified: do you mean the differences between oblique slip motions versus dip slip motions by pure normal and strike slip faults? Did the authors calculated this variation as a function of sediment loading and erosional unloading in the releasing and restraining zone, resp.?

Thanks for pointing this out. We added a definition of our use of strain partitioning in lines 84-87:
"***Throughout this study, strain partitioning refers to the process where initially a single fault accommodates the full oblique motion, but experiences differentiation with ongoing tectonic***

deformation such that in the end one set of faults hosts pure strike-slip motion and another set hosts pure dip-slip motion."

Strain partitioning in our models has been inferred based on visualised patterns of velocity, stress and strain rate.

Comments by Reviewer B (Joao Duarte) and author response

Main Comments:

1. It is mentioned that the models are generic. But in a certain way they are hybrid, which is great. But could you maybe clarify where the models are indeed generalizable? In the text, you go back and forth between "I will correct this aspect to make the models resemble the natural example" and then "I choose this parameter to preserve the generic character of the models". This makes the manuscript somewhat inconsistent giving the impression that there are a few arbitrary choices. I understand that you want your model to resemble a natural example (it is in the title) but keeping a generic character at the same time. But the way it is done, it seems too forced in places, and you end up halfway. Maybe the way out of this is to assume that these are hybrid models. Hybrid models have their advantages and are great for proofs of concept.

Thank you for this suggestion. It is true that we were not completely consistent and we now tried to make our intentions more clear by modifying the relevant passages of the text.
In the abstract:
*"**With much of our model setup being kept generic,** our results provide templates for the evolution of fault bends worldwide."*
At the end of the Introduction:
*"Our target region is the Dead Sea Fault**, but the models are kept generic to a great extent.** The results are **hence** transferable to other fault bends worldwide. "*
And at the end of the Methods in the limitation paragraph:
*"To derive results that are generally applicable to the dynamics of restraining and releasing bends, **but comparable to the DSF area,** we decided to employ hybrid models. **The first order geologic setting hence corresponds to the DSF area, while second order features are deliberately left out to keep a generic character of the setup.**"*
Within the results part "Present-day surface heat flow":
*"The fit of absolute values increases when considering the uncertainty range of available data points (shown in red in Fig. 4e), and the remaining differences may be attributed to the **partly** generic character of our models."*
In the conclusions:
*"Despite their **partly** generic nature, our 3D models of a restraining and a releasing bend match a multitude of observables from the Dead Sea Fault System."*

2. The text is in general well written but is hard to follow in places with technical jargon that is not explained and some unclear wording. I have given a few examples below, but I ignored a lot of other ones (I didn't want to be too picky) but an overall round of polishing, checking for these, would benefit the manuscript. The science is good, but the communication can slightly improve. This will help make the paper more accessible and, therefore, more impactful. Note that I stopped being too strict

with this around the start of the methods section, otherwise, I could not advance, but please be critical when polishing the manuscript.
Several examples are provided below.
We carefully went through the text again and tried to make it more accessible to non-modelers. For example, we explained the solver strategy:
"For each material (e.g., upper crust) and plastic strain, an additional advection equation is solved (continuous field method). Nonlinearities in the rheology (Glerum et al.,2018) are iterated out using a Newton solver scheme ***(Fraters et al. 2019). A Newton solver is a powerful tool used in solving mathematical equations that depend on their own solutions, for example when the viscosity depends on the strain rate and pressure. This approach starts with an initial guess of the solution and then refines it through a series of calculations until a solution is found that meets a certain level of accuracy (tolerance)."***
and slightly modified the rheology paragraph:
"The evolution of fault systems is furthermore influenced by frictional strain softening over the plastic strain interval of 0--1 (Fig. 2b), ***linearly*** *reducing the internal angle of friction from 30° to 7.5°* ***for strained material****."*
"To better visualize deformation and fault offsets, we included ***two initially horizontal layers*** *of 120,000 passive particles at an initial depth of 8* ***and*** *10 km.* ***By plotting their locations through the evolution of the model, these can be used*** *as deformation markers."*

Specific Comments:
- Line 42: the restraining bend has more topography, as expected, but it is also possible to clearly see a central (along-strike) depression. Can you make a short comment on this? Here or elsewhere in the manuscript.

Thanks for pointing this out. We were also very intrigued by this structure in the beginning of the project, but then forgot to mention it in the manuscript. We now added a little statement at the end of the paragraph:
"*The southern part of the DSF accommodates several narrow pull-apart basins resulting from releasing bends with thick sedimentary infill (Garfunkel et al. 2001) contrasting the high elevations of the Lebanese mountains within the restraining bend in the north.* ***The central topographic depression that divides the mountains is called the Beqaa valley. It is situated in a region characterized by inherited structures (e.g. Gomez et al. 2007, Fedorik et al., 2022) and is believed to have originated as a syncline (Lateef 2007, and references therein), which is, however, beyond the scope of this modeling study."***

- Line 83: I think you want to say "long-distance connection" instead of "far-field connection".

Thank you for this suggestion, we gladly changed the sentence accordingly.

- Line 87-93: This paragraph about ASPECT is rather technical and only a numerical modeler willunderstand it. It is ok, but could you, maybe, slightly edit it to make it more user-friendly? I think a lot of non-modeler colleagues like to understand how these codes work but are put off by the jargon such as "extended Boussinesq approximation with an infinite Prandtl number". Can you, in a couple of sentences, explain what this means? It will increase impact.

Thank you for this suggestion, this is indeed a very good idea. We added the following explanations and hope that this makes the methods more accessible to non-modelers:
"***The classical Boussinesq approximation implies incompressibility of the involved materials (Rayleigh, 1916) allowing for a more efficient modelling strategy. In this case it is assumed that density variations are so small enough that they can be neglected everywhere except for buoyancy (right-hand-side term in Eq. S1) . The extended Boussinesq approximation additionally includes the processes of adiabatic heating and shear heating when computing the temperature evolution (the two right-most terms in Eq. S3), but neglects the associated volume and density changes (van Zelst et al., 2022). We employ this approximation here as it is well justified in models of lithosphere deformation where the relatively shallow modelling domain implies only minor adiabatic temperature changes. A high Prandtl number indicates that momentum transport and with it convection dominates over heat diffusion, which means that the temperature distribution has an important effect on the viscosity of the modeled rocks. Assuming an infinite Prandtl number means that the viscous resistance to inertia is so high that inertia can be ignored in the governing equations.***"

- Lines 94-103 The following paragraph is better, but still could benefit from some re-writing. There are a couple of unclear statements. For example, it is not fully clear to me what is the "velocity of the mesh surface"? (Line 100).

We have now rewritten the paragraph and hope that it is now easier to understand.
"*After a prescribed number of FastScape time steps, the updated surface topography is given back to ASPECT.* ***ASPECT's mesh is then modified by computing the velocity of the mesh surface such that it captures the update in topography over the current timestep. To avoid distorted cells, the velocity of the mesh interior is distributed smoothly across the model domain*** *by solving a Laplace equation (see Rose et al. 2017). Subsequently, ASPECT solves the governing equations on the updated mesh.* "

- Line 104: Here you use altitude. In other places, you use topography. Is there any difference? If not, stick to one.

We have unified our terminology.

- Line 104-110: This explanation is also a bit hard to follow for someone that never worked with this. Could you try to improve it?

The paragraph is now changed to:
"***Normally, FastScape uses the user-set sea level as the reference height for all erosional processes such that after an infinite amount of time, FastScape's set of equations would level all non-marine topography to this height in the absence of tectonic uplift. However****, to account* ***for the smaller water bodies that often form at releasing bends in general, and in particular because the surface of the Dead Sea is below global sea level,*** *we need to discriminate between local and global sea level* ***and their effect on fluvial erosion****. We therefore introduce a new parameter, the regional erosional base level, in the ASPECT-FastScape coupling. The new parameter minimizes mass loss and assures that only a small lacustrine area at the releasing bend evolves, while the surroundings remain as* ***elevated*** *topography. The specific version of ASPECT and FastScape used in this paper can be found at https://zenodo.org/doi/10.5281/zenodo.10405076."*

- Line 113: Not clear what "process efficiencies" is. Please clarify/specify.

We now better introduced this terms in the paragraph describing FastScape and its coupling to ASPECT. Now, it says in line 129:
"Then, equations describing river incision (extended stream power law), hillslope diffusion and marine sediment transport are solved. ***The intensity of their combined effect is described as surface process efficiency throughout this study."***

- Line 121: It is not clear why this correction is needed and what its impact is, especially when you start by advocating that these are generic models. Can you clarify/expand on this? The same for the following sentence starting on line 123. It is hard to follow what these choices mean. I had to read these sentences a few times, and still, end up not getting them. This should be avoided. Getting stuck makes the manuscript hard to read. As an alternative, a simple sentence, after a technical sentence, starting with "This means that…" helps a lot.

Thank you for mentioning this. We have now rephrased and simplified this paragraph to the following.
*"The local FastScape sea level that determines sediment accommodation space is set to be 500 m lower than the regional erosional base level****, as the former*** *represents the Dead Sea* ***or similar water-filled pull-apart basins,*** *while the latter represents global sea level."*

- Line 126: Clarify what "material layers" are or use user-friendly language. This is the language of a numerical modeller, but not all the readers will be numerical modellers. Note that this is not a problem for me. It is just a friendly suggestion.

We have replaced material layers by ***initially horizontal geologic units***. Thanks for spotting this!

- Line 128: How thin [is the asthenosphere]?

With the model domain and the lithosphere both being 100 km deep, the asthenosphere is indeed only present as a unit where the lithosphere is thinned compared to its reference initial thickness. In the rest of the model it only covers the bottom nodes. We corrected this in the manuscript.
"The initial model setup consists of five initially horizontal geologic units: a sedimentary cover ***(top nodes only)*** *on top of a 20 km thick upper and a 13 km thick lower crust that are underlain by 67 km of lithospheric mantle* ***and a thin layer of asthenosphere*** *(Fig. 2a).* ***The asthenosphere only covers the bottom nodes, but increases in volume where the lithosphere is thinned.****"*

- Line 128: Not sure "Unperturbed" is the best wording here. At least is not clear.

We do agree that this was not a very clear description. We have now replaced the word with "***Initial reference*** *crustal layer thicknesses and all densities are chosen according to [...]"*

- Line 138:
a) Again, it is impossible for a non-modeller to understand what this means: "The ASPECT model setup is periodic along-strike". I know what it means, but I also know that no non-modeller will get it. I think this is the major problem with this paper. It is written only for (relatively advanced) numerical modellers. This can be a choice, but I think it would be a pity. For example, this journal has a wide spread of readers.
b) Note that it is not evident why a periodic boundary allows "for self-consistent localization of strike-slip faults without prescribing their width and fault-scale kinematics". This link is not evident because it is not direct. It is not the boundary condition that allows for this. The boundary condition allows for more accumulation of strain, and, therefore, a better localization of strain and the formation of a fault. This is the kind of thing that needs to be explained. There are many more examples like this in the text.

Thank you for this suggestion. To make the text more accessible we modified this paragraph to: *"The ASPECT model setup is periodic along-strike,* ***which means that the opposing model boundaries in the x-direction are continuously connected in terms of all model variables like velocity, temperature, pressure, and composition. Hence, material can flow out through one boundary and in through the other without leaving the model domain. This allows for a more*** *self-consistent localization of the strike-slip faults* ***since*** *their width and fault-scale kinematics* ***do not need to be prescribed, but can evolve according to the stress field and strain accumulation."***

- Line 141: Can you clarify what the top boundary is? "Coupling with the FastScape code" is not clear.

We have better linked this sentence to the further explanations given above in the Methods section and rewritten it as:

"[...] and the top is controlled ***by the equations describing surface processes*** *through coupling with the FastScape code* ***as described in Sec. 2.1****."*

- Line 145: You write: "strike-slip boundary velocity" I don't think it is correct to use "strike-slip" for a boundary velocity condition. "Strike-slip" is used for movements a long a fault or a plate boundary, not a volume (for that we usually write "shear", but also does not work here). It is better to explicitly say something like "each boundary moves in an opposite direction with an x velocity". Anyway, it might be a stylistic choice. But if you change this, make sure to change it along the text.

We changed all occurrences to ***total boundary velocities in the x-direction*** and this specific sentence to:

"We use a value of 5 mm/yr for the total boundary velocity in the x-direction*, which is in the range of the proposed* ***strike-slip rates*** *for the DSF in the Dead Sea area of 3.8–7 mm/yr (Le Beon et al. 2008, Hamiel et al. 2019, Hamiel et al. 2021).* ***In the model setup, the total boundary velocity is divided in half such that the two fault-parallel boundaries are each prescribed a velocity of 2.5 mm/yr in opposite directions."***

- Line 180: Please explain what "Kf" is here.

Kf stands for river incision rate, which on a more general level is one of the parameters that represents surface process efficiency. The higher the incision rate, the more erosion of the continental areas. Its meaning is explained in Section 2.2 Model setup – parameter testing, lines 236-245.

- Line 209: This is an important part, in which the results are described. However, I feel the start of the results section is a bit mixed with interpretation. For example, in the sentence that starts in this line, it is said that "difference exists between" geological structures, without first having described the structures objectively. It is always a good idea to first describe the results objectively, and then point out the differences, details, implications, etc.

Thank you for pointing this out. We have now restructured this part of the text. See section 3.1.

Figures:

Fig. 1:

- I missed a better tectonic context showing the DSF in the context of the Nubia-Sinai-Arabia

Plates. The inset in Fig. 1a is really small. It could also help a reader who is not familiar with the region and increase the overall broader impact. This is just a suggestion. The figure looks quite good already.

Thanks for that suggestion. We have been trying alternative figure layouts with a larger tectonic map. None of them looked as good as the original version, which is why we finally kept it.

- The interpretation on the maps shown in a) and in c) do not quite match. Small editing could maybe avoid some inconsistencies.

We **modified the figures** accordingly and also noticed the same issue when comparing **1a) and c) to 4a)**. Now all three maps include the same main features.

- The caption needs a bit of polishing, for example, it should be “topographic map” instead of “topographical map”

Thank you for pointing this out. We now employ a more consistent use of ***“topographic”*** for topographic map, topographic low, etc and ***“topographical”*** for topographical features throughout the manuscript. While including this, we also went through and corrected the occurrences of “*geologic”* and “*geological”*.

- I also got confused for having a sentence starting with “Be”. Maybe write “Be:” or “Be —”.

Good point, thank you. It now writes as follows:
“The mean of several measurement points is provided by Be (Ben-avraham et al., 1978), while *Or (Oryan et al., 2019) values show their measurement and the correction range in brackets* ***and*** *Sc (Schutz et al., 2014) provide an uncertainty range.”*

The other figures look very good! But please double-check the other captions for these kinds of minor problems.
Thanks for noticing. We have now carefully gone through all the figure captions and indeed found a few things to polish here and there.

We also slightly edited Figure 6 by adding missing arrows.

Acceptance letter

Dear Esther L. Heckenbach, Sascha Brune, Anne C. Glerum, Roi Granot, Yariv Hamiel, Stephan V. Sobolev, Derek Neuharth:

We have reached a decision regarding your submission to тектоnіка, “3D interaction of Tectonics and Surface Processes explains Fault Network Evolution of the Dead Sea Fault”. We are pleased to inform you that your revised manuscript has been accepted for publication.

We appreciate the thorough revisions you have made in response to the comments from peer review. You have adequately addressed all of the concerns raised, and the additional analyses and clarifications have significantly improved the quality of the manuscript.

Your contribution represents an important advancement in understanding the interplay between tectonic processes and surface dynamics in three dimensions along continental transform fault systems. We believe the findings and interpretations presented will be of great interest to the readership of Tektonika.

Your manuscript will now proceed into the production process for copy-editing and typesetting. Thank you for your patience throughout the peer review process. We look forward to publishing your work in Tektonika.

Sincerely,

Craig Magee and Guillaume Duclaux