Dear Meng (Matt) Wei, Lingchao He, Bridget Smith-Konter:

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, "A model of the earthquake cycle along the Gofar oceanic transform faults". Thank you once again for submitting your work to Seismica.

While both reviewers agree that your study is a valuable and timely contribution, one reviewer in particular has concerns about missing description of some key model setups and the conclusion that your model can reproduce the earthquake pattern when the cycles are prescribed based on the earthquake catalog. Both reviewers also provided other detailed comments for your consideration which I hope can help you improve the manuscript during the revision.

When you are ready to resubmit the revised version of your manuscript, please upload:

- A 'cleaned' version of the revised manuscript, without any markup/changes highlighted.
- A pdf version of the revised manuscript clearly highlighting changes/markup/edits.
- A 'response-to-reviewers' letter that shows your response to each of the reviewers' points, together with a summary of the resulting changes made to the manuscript.

Once I have read your revised manuscript and rebuttal, I will then decide whether the manuscript either needs to be sent to reviewers again, requires further minor changes, or can be accepted.

If you deem it appropriate, please check that the revised version of your manuscript recognises the work of the reviewers in the Acknowledgements section.

Please note that Seismica does not have any strict deadlines for submitting revisions, but naturally, it is likely to be in your best interest to submit these fairly promptly, and please let me know of any expected delays.

I wish you the best with working on the revisions. Please don't hesitate to contact me with any questions or comments about your submission, or if you have any feedback about your experience with Seismica.

Kind regards,

Yen Joe Tan
The Chinese University of Hong Kong
yenjoe.tan@seismica.org

Thank you very much. We have prepared a revised version as you suggested. We felt that the manuscript has been significantly improved. We really appreciate the reviewers and your help in this process.

Reviewer B:

Wei et al. present a first-order model of seismic cycles along the Gofar transform fault, tracking 3D time-dependent deformation and stress transfer in an elastic layer above a viscoelastic half-space. The short recurrence intervals of moderate earthquakes on the Gofar OTF make this a great natural laboratory to evaluate event sequences in a natural setting. The conclusions drawn from the observations and model analysis seem well founded and supported by the presented material, despite the lack of more near-field observations during the observation period that would help better illuminate details of the seismic and aseismic slip history. The paper also assesses how future seafloor geodetic observations could further enhance what can be learned about this model system. I consider this a valuable component of this paper. The manuscript is clearly written and organized, and the figures are of high quality. I recommend publication in Seismica following rather minor revisions. Below are some detailed suggestions I hope the authors can consider in their revisions.

We appreciated the comments.

Minor comments by line number:

Line 77: That is probably not a proper citation. Can you find a more suitable reference?

Yes, we replaced the website with the Global Seismic Hazard Map as a citation.

Line 129: Do you mean Fig. 1b, which features events outlined by dashed circles.

Yes, we meant Figure 1b. We re-arranged the figure but forgot to update the citation. Thank you for catching this. It was corrected.

Line 164: Maybe specify if you use the empirical equation for strike-slip events in WC1994 and what the assumed aspect ratio is? Does this seem accurate for those events where detailed aftershock data provide complementary constraints on the rupture extent?

We added more detail about this: "We estimated earthquake rupture length at depth using the empirical equation for strike-slip events from Wells and Coppersmith (1994). Although this method is derived from continental earthquakes, this approach is consistent with the well-recorded 2008 M6.0 earthquake on the Gofar OTF and the 2015 M7.1 earthquake on Charlie-Gibbs OTF (Shi et al., 2022)."

There is no specific aspect ratio in the approach (Wells and Coppersmith, 1994). Usually, they assume events with a ratio of horizontal to vertical slip > 2:1 as strike slip events.

Line 170: Elaborate that the described "pixel domain" is for map view?

Great idea. We revised as “The 2D map-view domain of the Gofar model space is 1024x1024 pixels…”. We also added the following at the end of the paragraph: “3D results can be obtained with repeating calculations at different depths. ”

Line 171: Can you more clearly illustrate what the line of symmetry in the "mirror approach" is. I couldn't quite follow this description.

We added “The line of symmetry is on the top boundary, parallel to the fault traces.”

Line 179: Can you add a few words explaining that gray fault areas slip aseismically (I think).

We added “The gray areas are creeping faults, which only slip aseismically.”

Table 1: Slip is aseismic between h and H?

Correct. We modified the physical meaning of h and H so that this is clear.

Line 200: “assume asperities to be creeping “ ◊ Should it be "assume barriers to be creeping"?

Yes, corrected.

Line 227-229: I wonder if Thatcher and Pollitz (2008) or similar recent compilation (Pollitz, 2019) would be a better reference for range of H in earthquake cycle studies. JS2004 is a really fine paper, but not really a study with particularly good constraints on the elastic layer thickness.

Not many papers report H. Both papers mentioned above focuses on viscosity. So we decided to keep JS2004 here but added the following “and Smith and Sandwell (2006) estimate an elastic layer thickness of more than 60 km in California using GNSS data.”.

Line 229-230: Can you briefly elaborate on the argument why simulation of multiple earthquake cycles supports the use of a greater H?

We deleted this sentence. This statement seems not useful and probably not accurate.

Line 238-246: It may not be necessary to list all these older studies, given that there are compilations that report on many more prior results. It is also not clear (as you note later) that all the California studies have much relevance for the Gofar OTF environment, for which we simply don't have many constraints. Maybe Iceland studies are slightly more relevant (e.g., Sigmundsson et al., 1997 on Krafla deformation and more recent ice unloading estimates; e.g., Pagli et al., 2007).

This is really a good idea. We added several references for studies in Iceland and cited the values that they suggested for both H and viscosity. We also mentioned the difference in oceanic and continental lithospheric structure and suggested Iceland being more applicable for Gofar.

Line 257: "Resembles" for "assembles"?

Yes, corrected.

Line 269: “We can estimate average stress thresholds"? Is it correct that the forecast times are based on the average values listed in Table 2?

Yes, we modified the sentence to “According to this model, we can estimate stress thresholds for the next large earthquakes on these segments and forecast future events using these thresholds (Table 2).”

Line 271: Forecasted future rupture times? Can you provide formal uncertainties for these forecasts in Table 2? Presumably this could be based on the standard deviation of the failure stresses.

This is a great idea. We added uncertainty in Table 2 and added captions below the table to explain how they were calculated.

Line 283: Wording seems a bit off. What about F3e was very large? Or is it meant to say "the rupture that initiated in G3d and G3e was very large"?

Yes, that is what we meant. We added “that” in the sentence to avoid the confusion. The sentence is now: “One possible scenario is that the rupture that initiated in G3d and G3e was very large, and it propagated through the barrier patch and triggered slip on G3h and G3i.”

Table 2 and discussion in lines 402-414: Good to make clear again in caption and text that these stress values are specific to 4 km depth and presumably vary quite strongly for shallower and deeper portions of the asperities.

We added “at 4 km depth” to the title of Table 2 and in the text. We also added a sentence stating: “The 2-3 MPa threshold that we observed is evaluated at 4 km depth, and we note that this value varies with depth strongly for shallower and deeper portions of the asperities. While the average Coulomb stress for the asperities, which is physically equivalent to stress drop, might be half of the value, this is still within the range observed by Allmann and Shearer (2009) and close to the average.”

Lines 300-312: Would it be useful to discuss time- vs. slip-predictable aspects of this system (e.g., Rubinstein et al., 2012 JGR; Weldon et al., 2004 GSA Today).

Yes, it is a good idea. We added two paragraphs in Discussion with a section title of “Time- and slip-predictable models”.

Line 338: Presumably at large enough distances, the fault-parallel long-term rates should always reach plate rate, right? That might be good to make clear here.

Yes, we revised the sentences to: "The basic features are very similar. However, the fault parallel is noticeably smaller in the far field for thinner models, even though at large enough distances, the fault-parallel long-term rates will reach plate rate. The vertical velocity is much higher in the far field for thinner models, and the fault perpendicular velocity does not change much with H."

Line 385: Earthquake swarms and foreshocks?

Corrected.

Line 405: Does Seismica allow for citing in-prep papers?

No, so we deleted this reference.

Lines 434-439: Maybe add that in addition to being effective for assessing (not accessing) the locking state, acoustic ranging would also be valuable for capturing postseismic afterslip and transient slow slip events.

Revised to "This technique is effective for accessing the locking state of segments and capturing shallow afterslip and slow slip events. However, it can only measure distances between transponders a few kilometers apart, due to the downward bending nature of acoustic wave propagation near the ocean bottom."

Line 473; and FOR map-related plotting ...?

Added "for"

Roland Bürgmann

Recommendation: Revisions Required

Reviewer F:

Review of manuscript: "A model of the earthquake cycle along the Gofar oceanic transform faults" by Wei et al.

In this work, the authors build a model for earthquake cycle along the Gofar OTF.

Using information from relocated earthquakes, they prescribe six asperities along the fault as initial setup for the model. Along with reasonable estimations of other parameters, they use earthquake catalog to determine the timing of the earthquakes in their model. They also did careful sensitivity test on less well constrained plate thickness and viscosity. Finally, their model also generates surface topography predictions.

Overall, I find this paper a timely contribution that can be of wide interests to our community. It is well written and easy to follow. However, as every paper has weaknesses, below I describe some of my concerns with more specific points listed afterwards.

We really appreciate the reviewer's effort and support.

My major concern is that the earthquake cycles are not predicted by the model but are prescribed with known earthquake catalog (L259). This undermines the conclusion that the model can reproduce the earthquake pattern (L32,33).

We agree. We changed the sentence into "Our model simulates the earthquake pattern on this fault for the past 30 years."

Another concern is that, even though they cited related references, some key model methods are not explicitly described in detailed. For example, what is the boundary condition of the model should be better illustrated, how the 'stress loading rate' and 'back-slip method' (L257) are used in these models should be clearly described. The equation for calculating the key model output 'Coulomb stress' should also be stated. In addition, it is understandable that the authors have been mainly referencing continental strike-slip models since the code Maxwell was developed for continental cases. However, the boundary conditions for OTFs may be very different than that of continental ones. The far-field plate pull and near-field ridge push may influence the stress state significantly, but are not considered and discussed in the current paper.

The stress loading rate and back-slip method is well known. The Maxwell method was not developed by this paper. Therefore, we felt that the citations are enough for the purpose of this manuscript. We did add the equation for the Coulomb stress in Section 3.1. We added the boundary condition in the 4^{th} paragraph section 2.2 and discussed the unrealistic aspect of it in Discussion. We also added two sentences with respect to the fact that we omitted the compression or extension state of OTFs in our model in Discussion.

Finally, the figure illustrations and some annotations require some further work for clarity.

We agree.

Below are specific points that hopefully will be helpful for the authors.

L70: ‘much simple’ may be controversial, see Gong and Fan 22 Plain Language Summary.

We deleted “much” to make it less controversial. The revised sentence is “the geometry and thermal structure of OTFs are simpler than a continental system (Roland et al., 2010)”, which we think is true based on existing data.

L73: ‘share fundamental physics’, with fluid interactions and magmatism, some physics governing OTFs may not overlap with their continental counterparts.

We revised the sentence to “OTFs share some fundamental physics with their continental counterparts”.

L80: ‘inter-‘, most previous work use ‘intra-‘

Changed.

L83-85: annotate the mentioned ‘three large earthquakes’ in Fig 1?

Thanks for the suggestion. Since the timing of the earthquakes are included, it is very easy to find them in the figure. We felt like that annotate them in the figure will complicate the figure, making it harder to see other important annotations that we made. We would prefer to not annotate them.

L99-100: “results show ... 2-3MPa”, these are important results, please describe them in more details in the methods, results sections for reader to better understand how you got those numbers.

We added several sentences in Section 3.1 to describe how we calculated this:

“Coulomb stress (σ_f) can be used to evaluate a fault’s stressing behavior throughout the earthquake cycle. In this study, we calculate Coulomb stress accumulation at seismogenic depths as $\sigma_f = \tau - \mu_f \sigma_n$, where τ is the shear stress, μ_f is the effective coefficient of friction, and σ_n is the effective normal stress. Because stress varies as a function of observation depth within the seismogenic zone, we calculate the representative stress at 4 km depth, which is 1/2 of the local locking depth, following King et al. (1994). Stress calculations are performed on a fault-segment by fault-segment basis, thus only the local fault contributes to the final stress result. Restraining bends have higher normal stress and lower rates of Coulomb stress accumulation; releasing bends have lower normal stress and higher rates of Coulomb stress accumulation. For the Gofar system, the largest angular deviation of a local strike-slip segment from the average slip direction is very small, thus the normal stress contribution to the total Coulomb stress calculation is generally less than 10%. Therefore, the exact value of the effective coefficient of friction is not a crucial parameter. Here, we set μ_f to be 0.6.”

We also have advanced this discussion in Section 4: "The 2-3 MPa threshold that we observed is evaluated at 4 km depth, and we note that this value varies with depth strongly for shallower and deeper portions of the asperities. While the average Coulomb stress for the asperities, which is physically equivalent to stress drop, might be half of the value, this is still within the range observed by Allmann and Shearer (2009) and close to the average."

L110-111: any reference for the assumed R1 Rayleigh wave group velocity?

Yes, we added Nishimura and Forsyth, 1988 to the text and reference list.

L135: Figure 1, Fig1a, add longitude and latitude; align Fig1a and Fig1b so they match in along strike position

Good idea. Modified as suggested.

L140: magenta dashed circles is confusing, not sure which one you mention. Maybe change color?

Changed the magenta dashed circles to black dashed squares.

L146 Maybe describe or illustrate 'screw dislocation' in more detailed since this is an important boundary condition for your model and discuss why it may be suitable for OTFs tectonic environment.

Screw dislocation is not a boundary condition. It is a dislocation model to calculate deformation and stress caused by fault slip. Since this method is developed by co-author Smith-Konter before and described in detail in the reference, we felt that a reference is enough. We did add the boundary condition in the 4th paragraph in section 2.2 and discussed it in Discussion.

L160-163: describe in more detailed how you use the relocated earthquakes to determine the asperities and maybe give an example.

We modified the sentence to be: "The asperities are estimated by comparing rupture lengths and centroid locations of relocated earthquakes (red horizontal line in Figure 3) with the location of the 5-km sub-segments. We assigned a segment as asperity when it overlaps with the rupture of multiple repeating large earthquakes."

I understand that you test effects of finer grid L173, but have you test effects of total number of asperity and size of these 32 sub-segments?

No, we did not. We chose 5 km for simplicity and convenience. Seismic data does not have the resolution to fine tune the asperity size.

Discuss similarity and difference between your setup and Gong and Fan 22's finding:

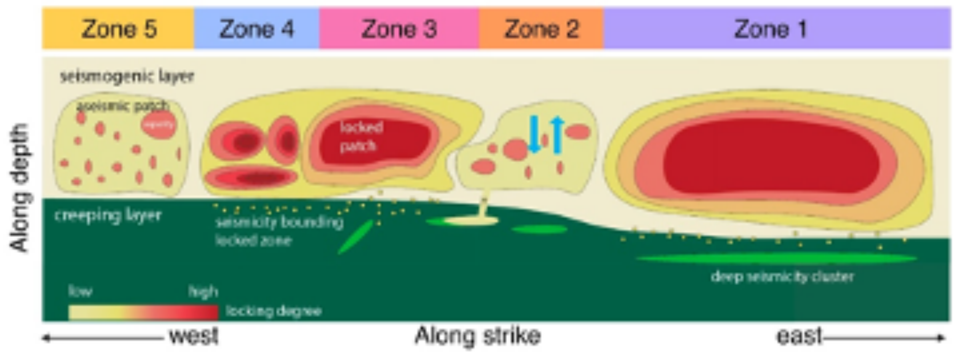
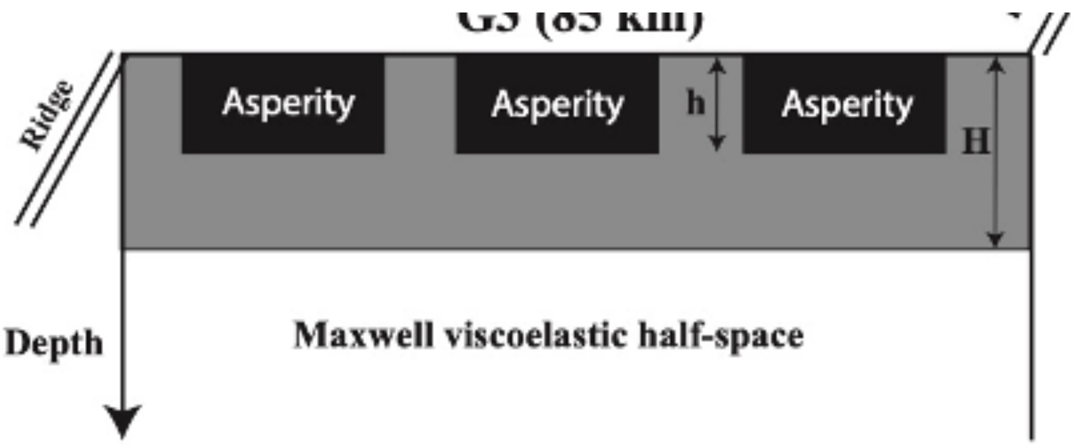


Figure 10. Conceptual model of microseismicity and fault slip modes at the westernmost Gofar transform fault. Irregular shaped patches denote fault patches of various sizes, and their colors correspond to different locking degrees. Zones 1, 3, and 4 are represented as sporadic, locked patches. Zones 2 and 5 are represented as damage zones embedded with small asperities. Microseismicity near the Moho discontinuity is denoted as small yellow stars. Green and yellow ellipses denote deep seismicity clusters. Blue arrows denote intense fluid circulation in Zone 2.



We added this in Discussion: "Gong and Fan (2022) proposed a conceptual model of microseismicity and fault slip modes at the westernmost Gofar transform fault (Figure 10 in Gong and Fan, 2022). In general, their conceptual model is consistent with the asperity model that we proposed for Gofar (Figure 2). where locked patches are separated by barriers with earthquake swarms and slow slip events. However, their model has more details regarding along-strike depth variation and seismicity in the mantle. Also, they only proposed two seismic patches on G3 (zones 3 and 1 in Gong and Fan 2022) instead of three as simulated here. This difference might be caused by different dataset used: we used global seismic data spanning the past 30 years, while Gong and Fan (2022) mostly relied on OBS data between 2008-2009 when the west segments were mostly active. Future data and events should be able to indicate which one is more likely correct."

L171: describe or illustrate 'mirror approach'

We modified the paragraph to "The 2D map-view domain of the Gofar model space is 1024x1024 pixels, and the grid size is 250 m x 250 m. The Maxwell software places a mirror image of the force couple distribution in a mirror grid so that the net moment is zero and the Fourier transformation can be used (Smith and Sandwell, 2003). The line of symmetry is on the north boundary, parallel to the fault traces. Consequently, the effective domain becomes 512x1024 pixels, corresponding to dimensions of 128 x 256 km."

L181 Figure3. I recommend change magenta lines to other colors or patterns. 'q,I,e,a,e,a,I,e,a' on top of the figure are not described and are confusing. Also for Table 2 and Figure 4.

We replaced magenta lines with black lines. We added the complete list of letters on top and added the following in the caption: "The letters on the top are the name of the segments used in the text and following tables and figures. For example, segment a in G1 is G1a."

L199: to be fair, Gong and Fan also observed depth variation along strike.

We changed the sentence to "Gong and Fan (2022) observed much less depth variation."

L257: 'stress loading rate' and its estimation method is first mentioned here but may be more appropriate in Method section.

This is a great idea. We moved these sentences to the method section 2.2 (just above Figure 2).

Figure 6,7,8, maybe change color scale. See:

Crameri, F., Shephard, G. E. & Heron, P. J. The misuse of colour in science communication. Nat. Commun. 11, 5444 (2020).

We updated Figure 6, 7, 8 with a Batlow colormap. I did not know this before. It is eye opening. Thanks.

Recommendation: Resubmit for Review
