In the manuscript, Li et al constructed a 3-D numerical model to simulate the crustal deformation in northeastern Tibet. Their modeling results present fault slip rate along several large strike-slip faults and relatively regional crustal faults; based on the above, they quantitively assessed seismic hazards along several faults. In general, I think the topic and scientific issues the authors discussed are interesting. Nonetheless, based on my research, I think there are several major issues the authors should make clear.

1) Regarding the 3-D model. The main topic of the manuscript is to investigate the 'present-day' fault slip rates. However, in constructing the numerical model, the authors assume low fault friction coefficient, thus allowing faults slip aseismically or continuously in the seismogenic layer. Such a practice is inconsistent with what we usually think of as interseismic fault deformation, because during the interseismic phase, faults are locked in the seismogenic zone and freely slipping below it. However, in this study, the fault slip rates are due to average velocities over several seismic cycles. Therefore, the authors should consider their model either reflects long-term kinematics or update the model by locking the faults in the seismogenic layer.
2) A relevant issue that promotes me to judge the model assumption is the GPS velocity profiles shown in figure 13 and 15. The modeled velocities behave as steps across faults, and therefore show discrepancies with GPS observations. The velocity steps are related to the model, which allows faults slip continuously in response to the far-field loading they experience.
3) A subsequent issue based on the modeling results is the seismic hazards assessment. If the faults are slip continuously in the seismogenic zone, how does elastic strain accumulate? I gauss the authors might mis-interpreted the fault slip rate and fault locking; because in calculating strain budget on the Jinqianghe-Maomaoshan-Laohushan faults, they used the 3.5-4.1 mm/a long-term slip rate as stress loading rate (actually in their model, the faults are freely slipping in the seismogenic layer); whereas in interpreting the seismic potential on the Maxianshan-Zhuanglanghe faults, they regarded zero slip rate as reflective of locked fault zone. The above practices are contradictory.
4) Another major issue is the block deformation. In the abstract, the author state that the Bayan Har and Qaidam blocks are deforming continuously, whereas the Qilian block is more of block-like. The main evidences for this conclusion come from the interpretation of velocity gradient within blocks (section 4.3). I disagree with such interpretations, because crustal blocks are rotating with reference to their Euler poles, the velocity gradient within blocks are likely caused by the block rotations. Therefore, unless the authors separate the block rotational components, the velocity gradient inside the block is misinterpreted.

5) There are also quite a lot of language and/or grammar issues. I would suggest the authors seek help from native speakers or professional services.
6) Based on my above judgements, I suggest a major revision for the manuscript.

There are also lots of language and/or grammar and other minor issues. I just name a few:

1) Line27-28, give references
2) Line34, please explicitly indicate the earthquake locations in figure 1
3) Line36, what is the strength of an earthquake?
4) Line39, the sentence reads quite strange, rephase
5) Line48-49, actually, quite a lot previous studies adopted elastic block models (e.g., Y. Li et al., 2017, 2021), and their results show non-negligible internal deformation
6) Line58, partitioning of deformation modes? What does it mean?
7) Figure1, indicate the time span of earthquakes and the sources, give descriptions of P1-P3
8) Line84-85, cite references
9) Table1, list the references in the table
10) Line125, change critically important to important
11) Line134, fitting misfit in mm/a or cm/a or others?
12) Line135, I am not fully understood, why F2 and F3 are not considered in friction coefficients adjustments?
13) Line136, it seems to me, for F3, friction coefficient from 0.02 to 0.1 is large, why?
14) Line163, Wang et al. (2020) should be Wang and Shen (2020), check the whole manuscript to avoid similar mistakes
15) Line180, see the 5th major comment. I don't agree with this interpretation, velocity gradient within blocks might be related to block rotation as well!
16) Line187, crustal velocity, not crust speed. Check and replace the whole manuscript
17) Line215, older?
18) Line215-217, give reasons for the discrepancies
19) Table3, change table to figure, which shows 1:1 plots of fault slip rates
20) Line297, rephase the sentence
21) Line300, see the 1st and 2nd major comments. It seems to me that the differences are large, especially across faults. The modeled velocities have steps across faults, this should be result from the fact that numerical model does not consider fault locking.
22) Line315-319, I don't think the way of earthquake potential assessment appropriate. First, aseismic creep is not found along the Jinqianghe and Maomaoshan faults, as recent studies show. Second, the seismogenic does not corresponds to 20 km. Check the latest studies (e.g., Y. Li, 2021, JGR) to update your way of calculation.

The above modifications are not sufficient to represent expressive or language problems

in the text, please the author see the revision as an opportunity to carefully re-arrange your language and contents.

Yanchuan Li