Solid Earth Discuss.,
https://doi.org/10.5194/se-2020-55-RC2, 2020

Solid Earth
Discussions

Open Access

Interactive comment on "Seismicity related to the eastern sector of Anatolian escape tectonic: the example of the 24 January 2020 Mw 6.77 Elazığ-Sivrice earthquake" *by* Mohammadreza Jamalreyhani et al.

Anonymous Referee #2

Received and published: 29 June 2020

The present study examines the 24 January 2020 Elazig-Sivrice earthquake by jointly trying to model quasi co-seismic static surface displacements from InSAR and high-frequency co-seismic data from seismological networks at local, regional and teleseismic distances to retrieve source parameters of the mainshock. Furthermore, the authors claim that they estimated moment tensor for 18 fore-/after-shocks with Mw $\geq$ 4.3 based on the modelling of the regional broadband data. The authors declare and highlight that the mainshock partially ruptured a seismic gap. Although the current work examines an important event occurred recently in the region, I do have problems with

Printer-friendly version

Discussion paper

the present manuscript particularly because of some significant principle seismological issues which I have attempted to clarify and to explain those point effectively along the lines detailed below.

The organization of the manuscript and presentation of the data and results need significant improvement with major revisions, clarification and organization to focus it on its most interesting topic, the one announced in the title. Therefore, I, alas, find the current status of the paper very poor and it is NOT scholarly written in many ways.

Briefly, I, first of all, find the present work very weak mainly because of an insufficient of visual materials to support the authors' arguments such as unilateral slip characteristics or so. Secondly, discussion section lacks a decent organization and, thus, it needs more comprehensive assessment and interpretations of the results. Unfortunately, at most part of the text we see superficially obscured questions. For instance, the link between InSAR based models and interpretation on fault segmentation is barely discussed within only two lines of sentences. Furthermore, I found some part of the discussion in which various scenarios are compared based on the aftershock distribution is very misleading due to the inappropriate resolution capability of the aftershock distribution data obtained from the AFAD PDE catalogues. . .

I feel that the manuscript written in a hurry is rather unfocussed and could also clearly benefit from careful editing by a native speaker as the written English needs some brushing up. Overall, this manuscript must have substantial changes in the present form (e.g., improvements in manuscript title, abstract, introduction, organization, layout, re-writing, figures, references, discussion and conclusions etc.) before further consideration for the EGU-Solid Earth as a brand-new submission.

Thus, it is NOT suitable and NOT acceptable for a publication at the Solid Earth journal as it is.

I recommend REJECTION and resubmission to SE and/or to any other journals.

* Please see attached supplementary PDF file containing my anonymous referee report *

Please also note the supplement to this comment:
https://se.copernicus.org/preprints/se-2020-55/se-2020-55-RC2-supplement.pdf

---

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2020-55, 2020.