Review Comments on manuscript submitted to the ***Solid Earth*** by Jamalreyhani et al.
"*Seismicity related to the eastern sector of Anatolian escape tectonic: the example of the 24 January 2020 Mw 6.77 Elazig-Sivrice earthquake*"

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*General Comments:*

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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 ≥ 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 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, refences, 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.

*Scientific Rationale:*

*Abstract:*

The authors state that "*2020 Elazig-Sivrice mainshock shows that the earthquake, with a magnitude Mw 6.77, ruptured at shallow depth (5±2 km) with a left-lateral strike-slip focal mechanism, with a dip angle of 74°±2° and a causative fault plane strike of 242°±1°, which is compatible with the orientation of the EAF at the centroid location*". Why did they accept this solution of the 2020 Elazig earthquake? However, in text, they reported three diverse focal mechanism parameters from inversions with different data set (see the Table 1; strike/dip/rake/depth/Mo/Mw). But, in Abstract, they mixed all parameters. For example, they selected strike and dip angles of FF (InSAR and Strong Motion), but they selected focal depth and Mw from MT (with Regional and Teleseismic data). So, what is the scientific motivation for these choices in describing the focal mechanism parameters of the 2020 Elazig earthquake? Therefore, Abstract needs serious corrections in many ways.

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| This Study | M. T. | Teleseismic | 38.25±0.03 | 38.90±0.08 | 247±6 | 70±6 | -5±6 | 8±2 | - | - | - | 27±1 | - | 1.37E+019 | 6.7±0.2 |
| This Study | M. T. | Regional and Teleseismic | 38.37±0.02 | 39.01±0.06 | 246±3 | 70±9 | -16±9 | 5±2 | - | - | - | 28±1 | - | 1.79E+019 | 6.7±0.1 |
| This Study | F. F. | InSAR and Strong Motion | 38.32±0.01 | 39.0±0.02 | 242±1 | 74±2 | 0±2 | 5.5±1 | 26±5 | 9±1 | 1.6±0.2 | - | 2100±130 | 1.3±0.4E+019 | 6.6±0.1 |

*Methods and Results: general comments*

In general, introduction, method and results sections (e.g., mainshock, seismic sequence, Coulomb failure stress change analysis) are NOT clearly presented and NOT well-written. For example;

**A.** The details of each methodology used in this work are not adequately explained here. The authors only described what they did in this study. No detailed information about the processing steps are provided on the selected algorithms (e.g., Grond, SNAP, Kite, Coulomb 3.3 etc.) There are too many unexplained sections about them making it difficult for the readers. They only directed the readers to the *Pyrocko* webpage for getting the information on each algorithm. However, I think it would be appropriate to present additional details clarifying each method and work devoted, since it is an important part of the article. Furthermore, the authors did not present their results in appropriate ways. The results section contains missing and incomprehensible parts. For example, on page 6, the authors reported that "*We performed a moment tensor inversion for 18 earthquakes (2 foreshocks and 16 aftershocks) with Ml ≥ 4.3. For this purpose, we proceed as for the point source inversion. However, due to the weaker magnitude, we rely on only regional broadband data of the KOERI network (Fig. S3)*". But they did not provide any BWIDC or surface-wave point-source MT results in Fig. S3 which only presents wrapped and unwrapped interferograms spanning the co-seismic of the Elazig earthquake for both ascending (up) and descending (down) directions. The authors should be very careful in numbering and referring to figures…

**B.** Also, the authors provided time domain waveform fits for some selected traces for the best model at teleseismic (P- and SH- waves: tp.p and td.s) and regional (Rayleigh and Love waves: rd.rayleigh and td.love) distances. But they did not summarize the obtained/preferred source parameters (e.g., source mechanism models) from these waveform fits.

Hence, I think the results are generally not displayed properly and satisfactorily presented in Fig. S7. There are too many points to criticise and to question further such as:

1. Did the authors also cross-check P-wave first motion polarities recorded at near-field stations? Did they examine if the P-wave polarities are compatible with the nodal planes or not?

2. The authors compared their focal mechanism solutions with those source parameters reported by other agencies (GCMT, GEOFON, AFAD etc., see Table S1). What are the main differences between these solutions? Did they check the effects of variations in each source parameters on waveform fits? Why do they suggest that their parameters are more reliable than the other solutions? Authors should verify and stipulate additional figures/plots, maps in order to convince readers that their results significantly appropriate and better than the others.

3. How did the authors calculate the uncertainties of each source parameters? Which method was used to determine the amount of uncertainties in the source parameters? There is crucial need to clarify these points (see page 4 lines 110-115).

4. Why did the authors select the frequency band of 0.08-0.20 Hz in the finite source optimization/inversion with near field data? Similarly, the authors modelled entire waveforms and amplitude spectra in the frequency band ranging 0.02-0.05 Hz. How and why did they select these frequencies?

5. Is there any slip distribution/rupture propagation model with the amount of displacements on the fault plane for which the authors favour? How did they estimate the fault length and fault width for this earthquake? They barely provided some waveform fits that are not clearly recognised (see comment E on *Discussions and Conclusions*).

6. They also did not evidently explain that which source parameters (Table 1 or else?) were used in Coulomb stress change analysis. They only mentioned that a homogeneous elastic half-space Earth model and *the causative fault* of the Elazig-Sivrice earthquake are considered as a rectangular dislocation (26 km long 9 km wide with mean slip equal to 1.8 m) in Coulomb stress modelling. What is the **causative fault plane** of this earthquake? The authors should distinctly summarize each earthquake source parameters resolved (e.g., strike/dip/rake angles/seismic moment/depth etc) that they used in Coulomb stress analysis. Furthermore, the authors should explain how they calculated the mean slip of 1.8 meters given in this section too (see Page 6 line 190).

7. How did the authors estimate the rupture duration and rupture velocity? (see page 5 line 135).

**C.** Nonetheless, my biggest concern is primarily on how the authors describe the rupture propagation and its time evolution in the current manuscript. Neither in the text nor in the supplementary material they present convincing material documenting the signature of unilateral propagation that the authors generously claimed. I would expect to see spatio-temporal co-seismic slip behaviour following the inverse modelling of such data set, if any. However, this is a very critical detail regarding the physics of time evolution of this earthquake, and there is NO evidence in this current work to clarify and/or to debate on these diversities of observations and interpretations.

In fact, if there is a proposed model of co-seismic slip distribution based on the inversion of InSAR data set, I have not seen any relevant model result and I am wondering why authors avoided to share these details, if any. The absence of segmentation is only mentioned very briefly referring to the InSAR data modelling in Fig. 2c (Page# 8, Lines#: 245-247, see: "*Some systematic residuals in the near-fault InSAR results of the finite slip modelling (Fig. 2) may point to a slight segmentation, but the overall good data fit in the single-segment finite fault modelling suggests that segmentation is not a first-order feature.*"). But unfortunately, I cannot see any clear elaboration from the interpretation of Fig. 2c. Even it is exceedingly unclear to what these two different InSAR modelling results belong which specific data subset. At this stage answer to this issue is very critical because the reader can have tough times in understanding the link between the rupture process and InSAR data with only available information presented in the current form of the manuscript. It would be nice to see snap-shots and a movie of time evolution on the preferred model of co-seismic slip distribution using InSAR data-set.

**D.** Another surprising issue for me is that why authors did not consider a direct strong motion data analysis obtained from a fairly dense station distribution though this is also questionable issue (?) as the Turkish Government AFAD authority **officially** released the data set on the **16 June 2020**, and not before! Hence, I am NOT contented how the authors attained this data-set which was not available on the days of 24-25 January 2020 and afterwards until the 16th June 2020.

**Thus, I am quite curious how the authors obtained these unreleased strong-motion data which should be clarified and confirmed in writing from the Turkish government authorities. Otherwise, this does NOT grant an equal opportunity on *Data Availability* for international scientists to conduct a research on the current and other relevant earthquakes in the region for global and/or regional mutual interests. Therefore, I consider this current work being NOT an objective piece of scientific conduct, and it is quite unfair to the others interested to study these events further.**

Furthermore, under these conditions, one would investigate the time variation of the pseudo displacement that could be easily extracted from these near-field recordings in relation to the ground motion using these stations located at different azimuths with respect to the mainshock. This, therefore, would give a direct and less-biased information on likely different episodes of the propagation. The authors should revisit these issues to elucidate further. And, it would be healthier to see snap-shots and a movie of time evolution on the preferred model of co-seismic slip distribution based solely on strong-motion data-set.

**E.** Page# 8, Lines#: 243-245, "*Scenario 3 is unlikely, because the depths estimated by earthquake relocation and aftershock centroid MT inversion do not change significantly along the fault. Both scenarios, 1 and 2 are plausible and in contradiction with the observed data*"

I do not think that the AFAD's routine PDE locations have very high resolution that can enable us a precise aftershock distribution to comment on further. It is pretty clear that they are not quite reliable to make such firm conclusions. The biggest problem with their relocated earthquakes stems from the very irrelevant type of 1-D initial seismic velocity structure model used in their localization procedure. Thus, the best option would be to perform relocations based on conventional relative techniques since they can provide much precise values as they do not depend on presumably uncertain knowledge of seismic velocity structure. Relative locations, for instance, HypoDD will better work in keeping track of the spatio-temporal behaviour of the seismicity much consistent. Precise relocation is highly achievable via phase reading data set that is publicly accessible dataset from the AFAD and other regional archives of KOERI or so (*see:* Waldhauser F. and W.L. Ellsworth, 2000. *A double-difference earthquake location algorithm: Method and application to the northern Hayward fault, Bull. Seism. Soc. Am., 90, 1353-1368, 2000;* Waldhauser, F., 2001. *HypoDD: A computer program to compute double-difference earthquake locations, USGS Open File Rep., 01-113, 2001*).

On the other hand, foreshocks can be often described as small event activities in a close proximity to the hypocenter of the mainshock. They may have an essential role in understanding the physics and initiation process of an upcoming event. Two main models have been, so far, proposed to explain the link between foreshocks and the main rupture. These are pre-slip (Ellsworth and Beroza, 1995) and cascade models (e.g. Fukao and Furumoto, 1985). In order to efficiently evaluate whether a series of events can be regarded as the foreshock activity and the proper mechanism involving the type of physical process affecting on the fault plane requires a tedious and critical investigation on extremely precise event localization, spectral analyses for their high resolution source characteristics (e.g. source radius, released energy, etc.), or the amount of stress change they caused. Although, the present work refers to possible foreshock activities at few places in the text, it is hard to see reasonable arguments if these activities can be interpreted within the concept of foreshock classification. There are almost no detailed efforts performed in the present work to elucidate this issue.

*Figures:*

Most of the figures are NOT well prepared for a clear publication quality. They are rather busy, and there must be a way to make them look easier to read. The fonts used on the maps and seismograms (*see* Fig. 2a, b, c; Fig. S7, S11, S13, S14, S15) are too small, hence it is hard to read. There are also many missing sections and references as such in the figure captions. The authors should cautiously arrange/plot the figures and re-write the figure captions without leaving any open questions as far as copyright issues are concerned.

**Fig. 1 and Inset:**

The *Inset* is very confusing as it does not present right geometry of the NAFZ in the Sea of Marmara, nor the extensional features in the western Turkey and the Aegean Sea. There is immense amount of high-quality papers, graduate thesis and extensive geophysical and geological experiments conducted in the Sea of Marmara and the Aegean regions, but I do NOT see them noted in discussions or cited in references leaving many open questions. What are the sources of historical and instrumental earthquake data and GPS velocity vectors plotted in Fig. 1? Any references to add?

**Figs. 2a, b,c; S2 and S4:**

In Fig. 2a, the axis information (i.e., coordinates of the location map) is too small and not readable at all. The readers cannot easily recognise which stations are in the rupture direction and which are in the opposite direction to the rupture zone (see Fig. 2a) as the names/codes of stations are not properly presented on a focal sphere (i.e.: azimuthal coverage), and/or on the map view (see Fig. S2). In addition, the number of seismic stations identified on the map (see Fig. 2a) is much more than the number of waveforms modelled. Explain the main reasons why the other stations are not modelled in the joint inversion. How did you select the modelled stations? What are the criteria? The authors should provide a summary Table specifically to elucidate Fig. 2a, and it can be posted to the supplementary.

Fig. 2b is not helping readers to identify the waveform fits, and details of the methods and software should be briefly summarized in the Supplementary On-Line material to guide those who are not familiar with this code. Besides, the choice of coloured lines and envelope is not helpful, therefore there is a need for further upgrade here. Fig. 2c requires some more details regarding the difference between two different solution. What is the major observation, interpretation and discrepancies between top and bottom InSAR maps in Fig. 2c?

**Figs. 3. and S1:**

The entire Fig. 3 is NOT acceptable at all. It should be seriously revised. The aftershock distribution in this figure does not make any sense due to too much uncertainty inherently existing in the epicentre/hypocentre location data taken from the AFAD PDE data catalogues. I strongly suggest that the imperative data must be revisited by using relative relocations and/or any other conventional methods. The authors should add a proper reference of the velocity model to the caption of Fig. S1, and also give right reference to the AFAD for earthquake locations plotted in Fig. 3. The discussion based on these maps and figures are irrelevant, and they should be removed in text as it does NOT reflect the ground truth until authors improve them with the accurate relocation techniques. How the rupture area (red rectangle) and rupture direction in Fig. 3b are defined, likewise light-yellow coloured region in Fig. 3d, e? Is it simply based on inadequate seismicity map of the AFAD and/or KOERI or else, if any?

**Fig. 4:**

As long as the aftershock distribution data is updated, presenting the vertical extent of the stress change along the fault plane can be physically more meaningful as well. The authors should add a proper reference for active faults plotted in the epicentral area. This figure should be presented in the supplementary along with other Coulomb stress change map in Fig. S6 with additional brief information on CST and up-to-date worthy referencing.

**Table 1:**

Source mechanism solutions are summarized, but I am still not quite sure how the authors claim that the magnitude of mainshock is Mw 6.77. Where is it taken from? How about the errors? How did you calculate them? The authors should provide parameters of their own results in a separate Table.

There is NO introductory information regarding these material as the authors should add a cover page describing individual plots. It looks quite clumsy as a grab bag in its present form.

**Fig. S1:**

What is the source for the 1-D radial velocity models used for calculating near-field Green's functions? Any references? Is Acarel et al. (1996) right one to refer to? According to my recollection, this region is characterized by relatively thick crust compared to the rest of Turkey and Moho depth taken about 31-32 km in this model could be misleading. I highly recommend authors to check recent papers dealing with Anatolian crust based on ambient noise tomography (e.g., Delph et al., 2015), Pn tomography tomography (e.g., Mutlu and Karabulut, et al., 2011), receiver functions (e.g., Vanacore et al., 2013; Karabulut et al., 2019) in order to obtain a reliable 1-D velocity model, among many other tomography studies in 1-D and even furthermore in 3-D FWI studies.

**Fig. S2:**

Here, the distribution of teleseismic stations are plotted without station codes. Thus, it is making even more difficult for readers to identify waveform fits. Besides, there is a huge azimuthal gap in the North and North-East quadrants spanning from Greenland to Kamchatka peninsula? This is quite important especially when the authors speak about directivity of the main fault based on their observation at other data-set they claim that they have along NE-SW striking geometry. How can we see these propagation effects (i.e., doppler-shifting) in waveforms if we do not have stations at these azimuths? And, also, in SW azimuths. The authors should provide complete list of stations for which broadband *P*- waveforms obtained along with complete catalogue information in a Table. Additionally, arcs of latitudes and longitudes should be plotted at each 15$^{o}$ arc-distances or so in order to help readers for an orientation of the nodal planes.

**Fig. S3:**

The more detailed information should be provided in figure caption for wrapped and unwrapped interferograms of the InSAR data presented in Fig. S3.

**Fig. S4:**

This is a confusing map… Firstly, traces and/or outline of main fault zones in Turkey, in the Aegean and nearby countries are NOT precise and misleading as presented in inset of Fig.1. The authors should be very careful in cross-referencing others' data without proper knowledge. This figure ought to be replaced with right one as there are many experts around to give help. Otherwise, copy and paste fashion can be very damaging one, and it looks as if data-base of the main faults of Basili et. al. (2013) is NOT the right resources to make use of it…? Hence, remove this reference and use a decent relevant one on the active faults of Turkey and surroundings. Secondly, what are the sources and which networks of coloured triangles refer to? Green? and Red? triangles stand for which network? Are there misinformation here?

How about AFAD broadband stations as the authors were able to get some of their other type of data-set? Why those local and regional broadband seismic stations of AFAD are not used in joint inversions? Any clarification and/or explanation?

**Fig. S5:**

It would be better to mark the units of the colour scale-bar (s stand for seconds?) given next to the plots?? Above all I find this figure not a helpful one.

**Fig. S6:**

This figure should be presented along with Fig. 3 and with additional brief information on CST and up-to-date new referencing. However, it is not informative without inclusion of the mechanisms of entire clusters with proper seismicity. Otherwise, this can be removed from the manuscript.

**Fig. S7:**

This figure presents time domain waveform fits of selected P- and SH- body waves, and regional surface waves for the mainshock. However, the distribution of teleseismic stations are plotted without station codes in Fig. S2 which makes it difficult to analyse these closely. Thus, it is making even more difficult for readers to identify waveform fits. Besides, there is a huge azimuthal gap in the North and North-East quadrants spanning from Greenland to Kamchatka peninsula? It is NOT proper to present automated figure generations in the Grond software toolbox of Heimann et al. (2018). The authors should refine these graphics to be more relevant for the readers to orient themselves. Furthermore, brief introductory explanations should be provided in order to summarize the main features of the Grond software toolbox. Specifically, filtering is dangerous, and should be clarified properly reasons why. Otherwise, it looks like an output of the computing in a black-box fashion.

**Figs. S8-S13:**

The authors should add colour bars for the misfit values. Also, it would be great to contribute some explanatory information regarding the optimization procedure used throughout in this article. For instance, I am not even sure what type of data you are displaying as misfits? Is this the total misfit obtained from the contribution of different data sets (strong motion, teleseismic, geodetic, etc.)? The detailed clarification is needed as this issue is rather critical.

**Fig. S8:**

Sequence plots of distribution and uncertainties of some parameters are a bit confusing one, and it does not help much with 68% confidence intervals.

**Fig. S9:**

Bootstrap misfit of the optimization is also too technical and does not help the readers much. Therefore, this figure should be removed.

**Fig. S10:**

Yes, I agree that MT decomposition is not well presented, and requires further analyses. Therefore, this figure should be removed.

**Fig. S11:**

Source parameter's scatter plots are not easily readable and does not help the readers much. Therefore, this figure should be removed especially when considered the azimuthal gaps of the broad-band stations used.

**Fig. S12:**

It refers to time domain waveform fits for strong motion data. Again, I do have reservations on this data-set and how the authors were able to get an access these data before the official release date of the **16 June 2020**. So where is the doppler effects and directivity on strong-motion data? Also, it is NOT proper to present automated figure generations in the Grond software toolbox of Heimann et al. (2018). The authors should refine these graphics to be more relevant for the readers to orient themselves. Furthermore, brief introductory explanations should also be provided in order to summarize the main features of the Grond software toolbox. Specifically, filtering is dangerous, and should be clarified properly reasons why. Otherwise, it looks like an output of the computing in a black-box fashion.

**Fig. S13:**

Finite Fault model plots of distribution and uncertainties of some source parameters of FF are a bit confusing one, and this figure does not help much to convince the reader especially when considered the azimuthal gaps of the broad-band stations used. Therefore, this figure should be removed.

**Fig. S14:**

Bootstrap misfit of the optimization for the FF model is also too technical and does not help the readers much. Therefore, this figure should be removed.

**Fig. S15:**

Source parameter's scatter plots for the FF model are not easily readable and does not help the readers much. Therefore, this figure should be removed especially when considered the azimuthal gaps of the broad-band stations used.

**Table S1:**

Moment tensor inversion results of the foreshocks and aftershocks are summarized in Table S1. However, I would like to see individual plots of complete waveform-fits of each earthquake spanning from 4 April 2009? right date? Otherwise, from 27 December 2019 to 19 March 2020. Are they regional moment tensor (RMT) results or else?

How reliable are these mechanisms? How about the error bars in the earthquake mechanisms of both nodal planes? The waveform modelling for earthquake and tsunami source studies is a tedious profession and it takes longer time and careful consideration. Thus, I advise authors to be very careful at this kind of studies.

*References:*

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The authors are using selective limited publications to cite, and some of them are irrelevant ones. Besides, there are very valuable SCI journal papers and Special Issues of WoS Journals as well as the known established society's special publication Books to cover specific questions on neotectonics, seismotectonics and geodynamic evolution of the eastern Mediterranean Sea region and Anatolia ranging from seismology, geodesy, geochemistry to 1-D/3-D teleseismic and local earthquake tomographic studies to orient the authors and the readers. The authors need to invest some further reading sessions on the above topics regarding the Eastern Mediterranean Sea region.

1. Acarel et al. (1996) paper is an irrelevant and poor one and not an objective good quality paper to cite as there are major misleading information included. Are you aware of them? Therefore, the adapted local crustal model is not valid and not reliable one to rely on further.

2. I do not see quite relevance of the below articles besides being case studies.

   I advise removal of one of the below articles?

   -Asayesh, B. M., Hamzeloo, H. and Zafarani, H.: Coulomb stress changes due to main earthquakes in Southeast Iran during 1981 to 2011. J Seismol 23, 135–150, https://doi.org/10.1007/s10950-018-9797-y, 2019.
   -Asayesh, B. M., Zafarani, H., and Tatar, M.: Coulomb stress changes and secondary stress triggering during the 2003 (Mw 6.6) Bam (Iran) earthquake. Tectonophysics, 775, 228304, https://doi.org/10.1016/j.tecto.2019.228304, 2020.

3. The statement in *Lines of 60-64* is not true as Bulut et al. (2012) was not the first to report.

   "*Bulut et al. (2012) characterize the EAF as a left-lateral strike-slip system, involving NE-SW and EW oriented segments which run parallel to the segmented trend of the main fault. Besides the dominant strike-slip mechanisms, Bulut et al. (2012) found evidence for additional thrust faulting on EW trending structures and normal faulting on NS trending secondary faults.*"

   I advise authors carefully to read scholarly written papers on the Anatolian seismotectonics and geodynamics studies in order not to reach such strong conclusions. There are many sentences like these throughout the manuscript as authors are misusing cross-referencing, and therefore not giving the right credit who deserves much in the first place. I repeat here again that the authors need to invest some further reading sessions on the above topics.

4. It looks as if data-base of the main faults of Basili et. al. (2013) is NOT the right resources to make use of it? Subsequently, remove this reference and use a decent relevant one on the active faults of Turkey and surroundings. The neotectonics features of the Anatolia is well studied and established and is widely known. So why to refer to an incomplete data-base?

Basili, R., Kastelic, V., Demircioglu, M. B., Garcia Moreno D., et al.: The European Database of Seismogenic Faults (EDSF) compiled in the framework of the Project SHARE. http://diss.rm.ingv.it/share-edsf/, doi: 10.6092/INGV.ITSHARE- EDSF, 2013.

5. Line 131, the authors report that "*Some surface cracks, rockfalls, landslides, and liquefaction were reported (Lekkas et al., 2020)*".

   Lekkas et al. (2020) did not execute field excursions after the mainshock in the area to map and to report such observations. This is not right referencing, and proves another example of wrong usage of cross-referencing! Check Turkish official report of MTA (2020) at the right web page. Otherwise, one can easily form a paper simply navigating at the virtual space to get information in a copy and paste fashion. This is a serious issue and can be considered as a misconduct as decent piece of scholarly science requires sensitive and careful analyses ever.

   **MTA. (2020).** Preliminary field and evaluation report on 24 January 2020 Sivrice (Elazıg) *Mw 6.8* Earthquake, General Directorate of Mineral Research and Explorations of Turkey (MTA), Ministry of Energy and Natural Resources, Ankara, 48 pages (https:// https://www.mta.gov.tr/).

6. The authors should also consider large aftershocks observed striking NE-SW along the EAFZ before jumping on wrong conclusions with those of Nissen et al. (2019). Thus, what is the direct relevance of the below article in the current study? I would have written a serious comment on the below article, but I do not have much time to invest on this adventure.

   Nissen, E., Ghods, A., Karas.zen, A., Elliott, J. R., Barnhart, W. D., Bergman, E. A., Hayes, G. P., Jamal-Reyhani, M., and et al.: The 12 November 2017 M w 7.3 Ezgeleh-Sarpolzahab (Iran) earthquake and active tectonics of the Lurestan arc. Journal of Geophysical Research: Solid Earth, 124. https://doi.org/10.1029/2018JB016221, 2019.

*Data Availability:*

I am quite curious how the authors obtained the unreleased AFAD's strong-motion data which should be clarified and confirmed in writing from the Turkish government authorities. Otherwise, this does NOT grant an equal opportunity on *Data Availability* for international scientists to conduct a research on the current and other relevant earthquakes in the region for global and/or regional mutual interests. Therefore, I consider this current work being NOT an objective piece of scientific conduct, and it is quite unfair to the others interested to study these events further. Similarly, why did the authors NOT use any waveforms from the local and regional broadband stations operated by the AFAD?

*Software Availability:*

Some of the tools are available for the broad scientific studies, but the details and decent expertise are rather limited. This issue should be enhanced in the text with right referencing, and note as SOM.

*Language:*

I feel that the manuscript is rather unfocussed and could also clearly benefit from careful editing by native speaker as the written English needs some brushing up. I can point out several places where this needs to be done below, but certainly not every occurrence.

Line 206 reads "*The mainshock started to nucleate from the topper part of the fault plane (Fig. 3b)*".

What does "topper" mean? Any good grammar? British/American English or a slang word invented?

*Discussions and Conclusions:*

In addition, I would like to hear the authors' overall comments on the following submitted and accepted articles that I have recently acquired on the dedicated web pages.

**A.** I have just noticed the following accepted article on the 2020 Elazig earthquake, which is on line since 29 March 2020 under URL (https://www.essoar.org/doi/10.1002/essoar.10502613.1), and I wonder how and why authors did not note/comment on this as they claim that they are jointly using many common available data-sets.

**Léa Pousse-Beltran et al. (2020).** The 2020 M w 6.8 Elazıg (Turkey) earthquake reveals rupture behaviour of the East Anatolian Fault, AGU-*Geophysical Research Letters (GRL),* https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2020GL088136, also available at **ESSOAr** | https:/doi.org/10.1002/essoar.10502613.1, First posted online: Sunday **29 Mar 2020**.

Pousse-Beltan et al. (2020) also deal with the 2020 $M_W$ 6.8 earthquake, and its rupture properties by using satellite geodesy and seismology. They mainly investigate the mainshock rupture, postseismic deformation and aftershocks, and relations to previous earthquakes. According to their model, to the ENE the mainshock may have propagated into the rupture zone of the 1874 M ~7.1 Golcuk Golu earthquake, and then stopped in the Lake Hazar basin, considered hosting a major EAF segment boundary. To the WSW the rupture propagated to the WSW at ~2 km/s and halted after ~20 s along a straight, structurally simple section of the Puturge fault segment. Furthermore, their study indicates bilaterally propagating rupture at relatively slow propagation speed from a nucleation point on an abrupt ~10° fault bend. Their model suggests the mainshock rupture with a pronounced shallow slip deficit that is only partially recovered through shallow afterslip and they keep discussing further. However, there is no significant surface rupture observed at distinctive studies already reported.

Hence, outstanding and open questions are:

1. I have further noticed by closely analysing InSAR data that this more complex geometry is NOT necessary to fit the InSAR observations as Pousse-Beltran et al. (2020) accomplished two disconnected fault planes with different dip to fit the InSAR data. So, I wonder what is the opinion and/or explanation of the authors on this matter? Explain it in details as you both use similar type of data-set in order to help reader of wider geological community.

2. What are the major discrepancies among their findings and major results in the present work?

3. How and why do they interpret the overall results by using both seismology and InSAR data?

4. The authors should add through discussion on this article at Discussion/Conclusion sections.

**B.** Recently, Bletery et al. (2020) calculated a coupling map from InSAR and GNSS long-term velocities which suggests regions with slip deficit between 50-80% along the ruptured fault segment. Is there any further discussion and comments on this by the authors?

Bletery, Q., Cavalie, O., Nocquet, J-M., and Ragon, T. (2020). Distribution of interseismic coupling along the North and East Anatolian Faults inferred from InSAR and GPS data, submitted to AGU-*Geophysical Research Letters,* Earth and Space Science Open Archive (https://www.essoar.org/) Published Online: **Thu, 5 Mar 2020**, https://doi.org/10.1002/essoar.10502450.1.

**C.** I have also noticed the following article that refers to the 2020 Elazig earthquake, which is on line since **4 February 2020** under URL (https://eartharxiv.org/8xa7j).

**Jonathan R. Weiss et al. (2020**). High-resolution surface velocities and strain for Anatolia from Sentinel-1 InSAR and GNSS data. EarthArXiv Preprints, https://doi.org/10.31223/osf.io/8xa7j.

Weiss et al. (2020) claims that their "*3D velocity and strain rate fields illuminate deformation patterns dominated by westward motion of Anatolia relative to Eurasia, localized strain accumulation along the North and East Anatolian Faults, and rapid vertical signals associated with anthropogenic activities and to a lesser extent extension across the grabens of western Anatolia*".

I wonder how and why authors did not note/comment on this as they are also using assembled InSAR data-set in the Anatolia. Thus, I would like to hear what is the opinion and/or explanation of Jamalreyhani et al. on this matter? The authors should explain it in details as they both use InSAR data-set in order to help reader of wider geological community.

**D.** I wonder why the authors did not make use of the GNSS observations as they privilege (!) that they are using all the available data-set collected in the Anatolia.

**E.** Furthermore, I would like to see the *Finite-Fault Slip Distributions* on the preferred fault plane mechanism of the authors by using individual data-set, in pairs and with entire data-set that the authors have. For example, local data (the strong motion, AFAD?), and regional seismology data (KOERI, AFAD, GEOFON?) and teleseismic body-wave inversions may recover zones of large slip, while they are combined into a single large zone in the slip distribution by the geodetic inversion (InSAR? or GPS?).

The authors have only provided Finite-Fault slip-inversion jointly using InSAR and a few strong motion data. I am puzzled to see that they have not used available teleseismic and regional data-set? Then, we may continue debating in discussions and making resolved conclusions.

A new figure is needed on *Finite-Fault Slip Distribution* integrating below data-set separately.

(a) Teleseismic body waves (GDSN, FDSN, through IRIS DMC or else)
(b) Local seismic networks (AFAD, KOERI or else?)
(c) Regional seismic waveforms (AFAD, KOERI, GEOFON or else?)
(d) Strong Motion (AFAD, KOERI or else?)
(e) Geodetic (InSAR) (ESA, NASA or ALOS?)
(f) Coulomb (Cautious tidies work should be conducted)
(g) Seismicity (AFAD and/or KOERI?)
(h) Joint inversion with any of the above data-set to compare with each other.
(i) Full Inversion of all the above data-set.

I would like to see grid-space along-strike and along-dip with finite-fault slip distribution on these cells delineated with slip-vectors and displacement values (e.g. D-maximum, D-average), and evolution of seismic moment release as a function of time (i.e., source time function). This must not be too difficult to resolve and to retrieve over the inversion tools as there are much data.

**F.** The authors then can plot map-view of any of the above preferred ones on the morphology in order to compliment neotectonic and seismotectonics maps. Afterwards, we may then continue debating in discussions and making stable conclusions for likely future earthquakes in the region.

In conclusion, I still believe that this manuscript must have substantial major changes in the present form 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.

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