Reviewer 1

For author and editor

The authors present original data dealing with active faulting in Azerbaijan, in the coastal area of the Caspian Sea. The paper exposes new and quality dataset and robust results, then deserves publication in the journal.

I would however recommend some corrections and clarifications before being defintely accepted. They are mainly form modifications and I think that they will significantly improve the readability of the paper. All these requested corrections are available in the attached PDF. Major requests are:

- Reference list is not complete.

- IRSL dating: what about the water content impact of age estimation, in this environment with drastic changes of water level during holocene history

- Figure 1 description needs to be clarified. Confusion of Units naming between figure and text. etc

- A bit of caution could be introduced when dealing with recurrence intervals.

- The conclusion is a bit disappointing; looks like an abstract, not a conclusion.

Please refer to the annotated PDF.

Reviewer 2

For author and editor

This paper presents earthquake geology work on the West Caspian Fault, which is little-studied but potentially significant from a perspective of local seismic hazard and regional tectonics. The relationship between faulting and mud volcanism observed here is also interesting and potentially relevant to other fault zones. The paper presents a large body of work including fault trace mapping, mapping of geomorphic surfaces, measurement of laterally offset features, and paleoseismic trenching. Results include an earthquake chronology and slip rate estimates. Overall, I find the results broadly reasonable and largely well-presented. However, there are a number of internal inconsistencies, chains of logic that lack adequate explanation, and aspects of the discussion that need to be fleshed out. While I don't anticipate these issues have produced fundamental flaws in the primary results of the paper, they nevertheless must be addressed before the paper would be suitable for publication, and the changes required are significant enough that an additional round of review after revision is probably necessary. The figures and text also need a fair amount of polish to correct illegible labels, typos, incorrect or inappropriate figure references, missing or misnumbered section headers, etc. I have summarized my primary comments below; more details can be found in the annotated PDF.

1. Throughout the paper, clarification is necessary for what you consider to be the West Caspian Fault. Are the Shirvan and Alat faults parts of it? Is the unnamed western seismicity alignment part of it? How have these various names been used in the previous literature? At the moment, it appears that the names “West Caspian” and “Shirvan” are used interchangeably. If these refer to the same exact structure, please just pick one and use it consistently throughout the paper. But it perhaps seems more useful (and maybe more consistent with prior work?) to use “West Caspian” to refer to this entire system of parallel faults and “Shirvan” and “Alat” to refer to individual strands within the larger system.
2. The geodetic slip rate estimates in Fig. 1C are described as being taken up along the West Caspian fault, but wouldn’t this motion be distributed across several faults, including the Talesh thrust fault on the map? In the final paragraph of section 2.2 it seems like the role of the Talesh fault ought to be mentioned, but it is not.
3. In the Alat fault system, I see that the general large-scale trend of the volcano lineaments is NNW while the individual mapped fault segments are much shorter WNW-trending lineaments. You should discuss how you think these two trends relate kinematically.
4. Based on the maps in Fig. 10, the trenches are oriented NE-SW, but most of the descriptions are given as though they were E-W oriented. Please adjust throughout the text and the figures.
5. There are references to unit U70 in the text, but on most of the figures the unit I surmise to be U70 is labeled as part of U30. If you are certain U70 and U30 are the same deposit, describe more explicitly that U30 extends broadly and eliminate references to U70.
6. Most of the IRSL ages are very well behaved, in stratigraphic order, but there is one exception to that in Trench 2. The out-of-order age looks like it’s still consistent within uncertainty but you should explicitly mention it.
7. Are all the BP ages before 1950 CE? It’s generally unnecessary and confusing to refer to non-radiocarbon ages as “cal BP” since there was no calibration involved.
8. In section 5.4 (Geochronology), the authors mention two hypotheses to explain inconsistent sample dates: correct IRSL ages with reworked shells, or incorrect IRSL ages with in-situ shells. They say they will discuss these further in the following sections but these discussions never materialize.
9. As section 6.1 is directly interpreting the trench exposures, it should be moved back into section 5. Section 6 should deal exclusively with offset measurements and slip rate.
10. Various inconsistencies regarding event dating: In the text, Event 1 is described as constrained between two sample ages in Trench 1, but in the OxCal model (Fig. 14), it is placed after both of those samples. This is a major inconsistency that needs to be corrected. Also, I'm not sure you can confidently place this event relative to the younger 350-year-old sample. The bracketing sample dates for Event 2 given in the text don’t match the dates given elsewhere. One of these is the out-of-order date, which again needs to be highlighted explicitly. For Event 5, it appears that the OxCal model also uses sample Sal1-7, from elsewhere in U80, as a constraint. Is that justified?
11. Further explanation is required about the structure of the OxCal model. What stratigraphic constraints are included? Since there is no explanation I cannot assess whether it has been done correctly.

12. The description of event evidence focuses almost exclusively on F1 in T1 and F2 in T2. I suggest adding a brief discussion of what you can glean from F1 in Trench 2 and F2 in Trench 1. The latter I would say has evidence for several events, perhaps mostly older, but less well constrained in terms of age. Nevertheless worth mentioning.
13. While the event evidence is generally reasonable, I think it should be reassessed and discussed in light of the hypothesis mentioned later that the fault creeps aseismically. Which deformation is or could be consistent with gradual creep? Is there any that requires sudden, rapid coseismic rupture? I'd say the large fissures of E2 in Trench 1 possibly require seismic rupture but the others don't. E2 is also the most abrupt deformation contrast in Trench 2. I suggest comparing the fault expression seen in your trenches to those seen in trench studies of other creeping faults.
14. Regarding the apparent cluster of events in the last 650 years, I suggest adding at least a brief discussion of whether this could be caused by missing events / hiatus in deposition in the earlier part of the record.
15. Interpretation of 7.5 m offset: firstly, no uncertainty is ever given for this number. Second, I don't quite follow the logic of how the age constraint of the terrace riser offset is assigned. It might help to add interpretations of where the terrace surfaces lie to Fig. 15. Finally, this section seems to be heading towards a slip rate calculation, but then never actually arrives at that point. For the record, it seems reasonable to me to use the 2850 age as a maximum for the age of the riser formation, which yields 2.6 mm/yr, about in the range you'd expect from geodesy.
16. Interpretation of 53 m offset: you calculate a slip rate of 29.4 mm/yr based on the charcoal ages but don't give an uncertainty on that number, nor is it clear how exactly it was calculated from the displacement with uncertainty and two sample age ranges. Please clarify and give an uncertainty. Also, given that this rate is more than an order of magnitude higher than the geodetic rate estimated in Fig. 1, it warrants further discussion. Do you suspect that current geodetic rates are much lower than long-term rates? Do you suspect that the layers containing charcoal could have accumulated on the terrace surface after the riser formed? You later calculate a minimum slip rate of 3.9-4.8 mm/yr using the 11ka-13.5ka age of the LGM highstand, but does that slip rate range take the offset uncertainty into account? Even this minimum slip rate is significantly higher than the calculated geodetic rate, and the discrepancy is not currently discussed or even mentioned. A more comprehensive discussion of which geologic rates are the most likely to be reliable and how they compare with the geodetic rates is required.
17. At the end of section 6.2 you calculate a slip per event of 2-2.4 m. You might want to point out that this is consistent with the smallest drainage offsets you measured.
18. The authors suggest that the fault may slip aseismically due to the lack of historical records of large earthquakes. However, it is unclear how long the historical record is in this region and no citation is given for a pre-instrumental catalog. Please give the source for the historical record and explain its length and likely completeness.
19. It's beyond the scope of your study, but you could mention in the discussion that geodetic methods such as pixel tracking or InSAR could be used in the future to investigate whether the fault is currently creeping.

Major figure comments:

Fig. 1: I suggest making the town names smaller, at least as small as the fault names, since the faults and not the towns are the focus of this paper. Please explain the meaning of the dashed lines in part B.

Fig. 2: Text on inset map in part A is too small to be readable. White box does not accurately show area of main figure. Light grey faults from Zelenin are not really visible on gray DEM; use a different color. "0 m.a.s.l." label does not have a corresponding contour line; I suggest removing it. In part B, I suggest making the mud volcanoes a more distinct color from the earthquakes. Perhaps clarify that you infer the lineaments of mud volcanoes to be fault zones. Is the seismicity lineament labeled #5 the one identified by Gunnels et al.? Or is that #1? Clarify.

Fig. 3: Mapped faults northwest of Alat are difficult to see under the volcano symbols. I suggest putting the fault layer on top of the volcano layer. Make town names smaller; they are not the focus of the paper and are distractingly large. The river is not clearly visible; if you want to label it you need to map it with a line. Light grey faults from Zelenin are barely visible; please darken them a bit. Specify what the dashed lines mean, and should alignment #4 from Fig. 2 be included here?

Fig. 5: Add indication of the locations of views in Figs. 7A and B.

Fig. 6: The text on the strain ellipses is illegible and should be removed. The strain ellipses need to be described and explained in the caption. The labeled "Fig. 6 Salyan Trench Site" should refer to Fig. 10. Indicate where the view shown in Fig. 7F is on Fig. 6.

Fig. 10: It would be helpful to put the trench outlines on part A) as well as on part B), for easier comparison with the mapped terrace units.

Fig. 11: The profile in the upper left needs to be described and explained in the figure caption. The fault and fracture lines are hard to distinguish from some of the unit colors. Consider making them brighter red or black so that they stand out better.

Fig. 12: Please put unit labels on the trench log as you did in Fig. 11. As in Fig. 11, the elevation profile needs description in the figure caption. If it is supposed to show an outline of the trench within a longer profile, there must be an error because the outline is 20 m long but the illustrated trench wall is only 11 m long, as described in the text. The orthomosaic lacks a scale bar.

Fig. 14: Please put the actual sample ages after the names. The names of the two radiocarbon samples don't match Table 2, but given that this model is effectively entirely based on IRSL, since the two radiocarbon ages are 10 kyr older than the oldest luminescence ages and do not affect the earthquake age bounds at all, I suggest removing those two radiocarbon ages from the plot.

Fig. 15: since this figure is discussed before Figs. 13 and 14, perhaps it should be moved up to Fig. 13. I suggest adding the fault zones as schematic vertical red lines between the strat columns, labeled F1, F2, etc. Why is only one of the two charcoal dates from the sample pit shown? Are they from the same stratigraphic level so you keep only the younger

one? Explain. Column labels I assume indicate grain size but they are illegible; please make them legible. Label the sample pit column “sample pit” instead of T3 which it is not called anywhere else.

More minor comments:

- Spelling of some names such as Kopeh Dagh, Shahroud, and Shirvan is inconsistent between text and figures.
- Throughout the paper, numerical ranges should use a single en dash with no spaces, rather than with spaces or a hyphen.
- “highstand” should be one word (currently inconsistent throughout the paper)
- Hyphenation of multi-word adjectives such as “high-velocity” is done inconsistently throughout the paper; should be done consistently.
- I suggest referring to the Qad sample pit consistently as “sample pit” rather than “sample site” as it is currently labeled on several figures.
- Numerous references about the trench site to “Fig. 6” should probably be to Fig. 10.
- The third and fourth authors are not listed as having done anything in the “author contributions.”

Reviewer	Line	Comment	Reply
1	18	Throughout the paper, clarification is necessary for what you consider to be the West Caspian Fault. Are the Shirvan and Alat faults parts of it? Is the unnamed western seismicity alignment part of it? How have these various names been used in the previous literature? Maybe what you mean to say here is that the West Caspian fault comprises several parallel strands.	Ok thanks - I have made an edit to the abstract and figures to hopefully improve the clarity.
2	75	In the map, we barely distinguish the "Active fault - this study" lines from the others. Change their colour?	Changed
1	82/83	What is the meaning of the dashed lines?	Added to the description of the figure.
1	85/86	Wouldn't this be distributed across several faults, including the Talesh fault?	Yes some of the east west shortening. The point here is even with a very loose calculation just using the GNSS data you would expect some right lateral motion.
1	101	Unclear what this means, please clarify. Do you mean that the current geodetically measured shortening rate has been constant since initiation? How do you know that?	Thanks I've removed and reworded slightly.
1	120	First mention of this. What is it? How does it relate to the West Caspian Fa	Changed to West Caspian Fault zone.
1	130	What does this mean?	Productive series is a regional geologic term for the sands. Made change to illustrate. Not hugely important for the text.
2	136	I am not fan of having coloured the beach balls	Made changes to figure
1	136	Not really visible on gray DEM - multiple comments on the figure	Made changes to figure
1	140	Inconsistent spelling with Fig. 1	Made changes to figure
2	142	Do you mean: "Receiver function analysis and seismic reflections studies show, from top to bottom, a low velocity layer 15-20 km thick, then a 10-18 km thick high velocity basaltic-like layer beneath"?	Thanks yes. Made change
1	145	en dash	Changed
2	155	of the SCB basement?	believed to be the paleo-Tethys oceanic crust
1	161	Inconsistent spelling with Fig. 1	changed
2	166	The figure 1 does not reflect this change	ok - I'll try to reflect this with kinematic markers
1	176	What about the Talesh thrust? You don't mention it at all here even though	I've added a sentence that hopefully explains this a bit better.
1	177	Can't open the comment	Hopefully the above clarifies.
1	181	Which of the seismicity trends discussed later is this? #5 or #1? Refer back to this when you discuss Fig. 2.	Addressed this
2	189	Do you refer to Figure 2A with the couple of shallow dipping reverse focal mechanisms ? Note that their epicenters are on the surface fault trace but these EQ are quite deep.	Yes I think this is a bit confusing. I will adjust the text.
2	193	Was this reference level used to decipher the very long term (U. Pleisto) activity of Shirvan Alat fault network?	I reference/introduce sea level here to give a regional geomorphic context. This is then used to provide some level of constraint on the age of a marine terrace, which as sea level dropped was incised. The incision crosses the Shirvan fault which is offset. This provides a proxy for the slip-rate since the latest-Pleistocene (end-Last Glacial Max).
1	210	Various figure comments	Made changes to figure
2	210	Figure comments	Made changes to figure.
2	245	at this stage of the paper, maybe use "deflection" instead?	Done
1	252	Edit for clarity, some repetitive statements?	Thanks done.
2	288	Maybe this is the place to indicate which hypothesis has been done in terms of water content of the sediments: the current one? or another? Considering that the area was under water during the early holocene, we can assume that the water content was higher at that time, and then the date estimated for early holocene sediments could be biased if an hypothesis of (dry) sediment has been applied. I am especially wondering that for the marine sediments of trench 1 (unit 50-60).	Thanks - contacted Ed and Rich tbc.
1	292	Start with a delineation of how you define the Shirvan Fault, the Alat fault, and the West Caspian fault. It sounds like maybe you consider the Shirvan and Alat faults to be components of the broader West Caspian fault system but that is never explicitly laid out.	I've added something and separated out the Shirvan and Alat faults in sections 4.1 and 4.2
2	306	Here, one section title is missing: 4.1 The Shivan Fault Zone?	See above thanks!
2	309	morphology of what?	Made a change to reflect this
1	312	It would be preferable to indicate the extent on Fig. 3 if that is possible.	Made change
2	312	ticks of the reverse fault symbol are too small to be clearly visible	Made change
2	319	however, we find RL deflections/offsets between 5 and 20 m for a series o	Made change
1	317 -328	Various text edits made	I have accepted these changes
2	317 -328	Various text edits made	I have accepted these changes
1	329	What about this linear scarp? Is it a shoreline? Explain.	Yes shoreline - edited the figure to reflect this - most likely 35 ka given elevation.
1	349	Various comments on the figure.	I have refined the figure and removed the illustrative strain ellipses.
2	349	This map is very interesting. In my opinion, it calls for an additional level of description/analysis. Could you show and describe : - secondary faults (I imagine there are based on the scheme to the right) - the basement stratification - the NW-SE to NS ridges in the plain; their origin? - marine/fluvial terraces? a possible origin/age? It also could be nice to indicate where are the sites shown on Fig. 7	Thanks. I have added a description on the secondary faults. The basement stratification is broadly Oligocene muds and sands. I'm not sure this adds much to the text, but would be interesting to fully investigate in a separate study. The ridges are vertical beds with softer sediment eroded out to create ridges. Marine/fluvial deposits are detailed later in the study.

1	358	did you mean WNW-ESE? That's what it looks like in figs. 8-9.	Yes thanks!
2	367	could you roughly underline the contours of the volcano and mud flow? It's not clear whether the fault crosses across (as suggested in the text line 361)	Thanks - I'll add some direction lines to the figure - tricky to do the contours without a high res DEM, but the scarp crosscut the topography. It's a very hard area to work in, the surfaces are just a series of mudflows with little to distinguish them.
2	367	View to the north, right?	Yes - NNW - I'll add this to the figure.
1	374	I see that the general large-scale trend of the volcano lineaments is NNW while the individual mapped fault segments are much shorter WNW-trending lineaments. You should discuss how you think these two trends relate kinematically.	I'll add some wording to the discussion. But not sure I fully agree. I see a NNW trend on the Shirvan fault, this trend then seems to change to NW on the Alat and is WNW in places (where the fault probably bends). I don't know about the kinematics and can only speculate, but I see a correlation between where there is some fault trace and the mud volcano ediface. The literature would suggest that over-pressured mud mobilises through fault and fracture networks to the surface. Seismic reflection data is tricky due to the amount of fluids here and I think MT would be worth doing to better image the fluids thereby illuminating the fault orientation.
2	369	do you mean that the fault cuts the mud flows? or this is deflection?	Yes the fault seems to cross cut mud flows.
2	402	marine or lacustrine ?	Marine - thanks.
2	410	length? direction? height ?	0.2 m height, NNW, ~ 25 m long. Added to the text
1	423	missing words: "the terrace sediment is composed...?"	Yes thanks - added.
2	444	any explanation ?	Looked like a burn layer
2	464	I dont really understand this sentence. Is the scarp only 6 m long?	I've reworded - basically the 0.5 m scarp stretches the width of the Qaf surface (~6m).
2	468	wide-aperture scarp height seems to be larger on figure 11	Not sure - measured at 0.5 m.
2	473	deposition environment?	Was not able to determine this - they look significantly older than the other sediments, and no visible clasts or fossils to determine. It is also quite deformed in and around the two fault zones.
2	487	Not visible on the log fig 11. Is it the layer labeled U30 on top of U65 between F1 and F2?	Yes - I'll edit the figure.
2	503	The caption of this rich figure could be a bit upgraded. Plus: 1) what about the fault F2 that reaches the ground surface? E1? 2) the drawing of the bottom of the trench around F2 shoud be improved. We infer an inversion of stratigraphy. What does it mean? 3) there seems to be a confusion between U30 (between F1 and F2) in this figure and U70 in the text. then what about the continuation of U30 east of F2? this is quite confusing.	Ok I have upgraded the caption. 1) F2 reaches the surface - I'll add a marker for E1. 2) The trench log is accurate. The clays and blue sands are mixed - deformed and not sure about an interpretation without excavating deeper. 3) Apologies - yes U70 should be U30.
1	519	do you mean "group 2" that you defined above?	Yes
2	519	what is this?	group 2
1	552	You should at least mention the out-of-order age you got in Trench 2. It looks like it's probably within uncertainty but needs to be discussed.	Ok added
1	571	The discussion of these two different hypotheses never seemed to materializ	Ok added to slip-rate section and EQ recurrence intervals
2	573	Could we explain this with higher water content in the past (early holocene), not accounted for in the calculation?	Need to check this with Ed
1	577	did you mean Fig. 15? Since it is discussed first, maybe Fig. 15 should be	Removed figure 15 from the paper as I think it confuses. I have deleted this paragraph
1	584	This subsection should be moved into the previous section 5, as it is directly interpreting the trench exposures.	Moved subsection to 5.5.
1	594	In your OxCal model in Fig. 14, you actually place Event 1 after both of these sample ages, obtaining an event age of post-412 BP. This is a major inconsistency that needs to be corrected. Also, I'm not sure you can confidently place this event relative to the 350-year-old sample.	Thanks - yes looks like there is a problem here with the age model. I think we can place it confidently after 560 BP though. This is consistent in both trenches.
2	613	Do you think that lateral component could explain the change in apparent throw?	Fair point thanks. I have adjusted the text - we also have a capped rupture here.
1	635	It appears that your OxCal model also uses sample Sal1-7, from elsewhere in U80, as a constraint on Event 5. Is that justified?	We have sampled the same unit either side of the fault (Sal1-11 and Sal1-7). I have used the two ages of U80 to bracket the event. Both ages are close and show good agreement with our interpretations.
1	641	Elsewhere you refer to this as U70.	Sorry about this I have made a change and didn't update the text from a previous draft. U70 is interpreted as U30. Sediments are the same.
2	641	lines 488-489. there is the description of U70 instead of U30	Sorry about this I have made a change and didn't update the text from a previous draft. U70 is interpreted as U30. Sediments are the same.
1	654	I suggest adding a brief discussion of what you can glean from F1 in Trench 2 and F2 in Trench 1. The latter I would say has evidence for several events, perhaps mostly older, but less well constrained in terms of age. Nevertheless worth mentioning.	I have added some wording to cover the older events
1	655	I think this evidence should be reassessed and discussed in light of your later hypothesis that the fault creeps aseismically. Which deformation is or could be consistent with gradual creep? Is there any that requires sudden, rapid coseismic rupture? I'd say the large fissures of E2 in Trench 1 possibly require seismic rupture but the others don't. E2 is also the most abrupt deformation contrast in Trench 2.	E1 and E4 could be aseismic deformation - there is no smoking gun for coseismic rupture.

1	663	You should add at least a brief discussion of whether this discrepancy could be caused by missing events / hiatus in deposition.	Added some wording.
2	664	I think that this is true if the sedimentation recording is considered rather complete with a resolved stratigraphy. I would then be more cautious...	Added wording.
1	673	ection 6, which is about measured displacements and slip rate, should sta	reorganised
2	673	Maybe here you can introduce a new section: 6.1 Slip during single events	reorganised
1	679	There is a logical leap here that I don't quite follow, and Fig. 15 does not help. Why have you decided that U30 represents the age of the Qt3 terrace? Why not include U80, which you described as part of the same fluvial package as U30? Perhaps you should add your interpretations of where the terrace surfaces lie onto Fig. 15.	added a bit more wording - I've removed fig 15.
1	680	not sure using the out-of-sequence age by itself, ignoring the other two which are younger, is the best practice here. Add more discussion of your reasoning.	changed
1	684	And... are you going to calculate a slip rate from that? I thought that's where this was going, but then the thread just drops. For the record, it seems reasonable to me to use the 2850 age as a maximum for the age of the riser formation, which yields 2.6 mm/yr, about in the range you'd expect from geodesy.	Thanks for this I've added some text.
1	691	and other displacements between 5-10 m mentioned above?	Added wording.
1	702	insert space: 29.4 mm/yr. More importantly, how exactly was this number calculated and what is the resultant uncertainty? You give a displacement with uncertainty and two date ranges, but it's not clear how you calculate the rate from that collection of values. Even more importantly, this rate is an order of magnitude higher than the geodetic rate you calculated in Figure 1. This discrepancy warrants substantially more discussion-- do you suspect that current geodetic rates are much lower than long-term rates? Do you suspect that the layers containing charcoal could have accumulated on the terrace surface after the riser formed?	Yes exactly sorry I should have explained. Yes it's way out from the regional rates pretty much everywhere. Therefore, the charcoal has either been transported to the site or this is a human fire.
1	719	This seems to be derived using the 53 m measurement with the age range above, but what about the uncertainty on the 53 m measurement? Shouldn't that factor in? Also, even this minimum rate is significantly higher than the calculated geodetic rate. Further discussion is warranted.	I have added in the uncertainty - and adjusted the slip-rate range accordingly. Yes it's worthwhile noting here that the geodetic rate is a an estimate and more geodetic work is needed in the area.
1	723	Did you want to mention that this is consistent with the smallest drainage offsets that you measured?	Yes thanks I've added it into the section.
1	728	So this sentence suggests you don't consider the Alat fault and Kura basin fault to be part of the WCF, but in the next sentence you refer to the Shirvan fault zone. Are West Caspian and Shirvan fault completely interchangeable names? If so, just pick one and use it consistently throughout the paper. But perhaps it would make more sense for all of these strands to be collectively referred to as the West Caspian fault system, with the Shirvan fault being one strand of the WCF.	Thanks apologies for the confusion - yes West Caspian Fault zone consists of the Shirvan and Alat faults and possibly others.
1	737	No, this is a minimum slip rate, since in your interpretation the terraces formed after the highstand. You need to discuss the potential reasons why the minimum geologic rate is double the geodetic rate!	~Yes thanks

Second Round of Review

Editorial Comments

Based on reviews I have received, your manuscript may be suitable for publication after some revisions. The same reviewers as for your original submission has reviewed the revision, and both see much improvement. Reviewer A has some suggestions to improve the conclusions, but Reviewer B has more extensive comments. The comments from Reviewer B are thorough and constructive, and I think they will further improve the paper. The paper is quite long and hard to follow in places, and I think Reviewer B's comments and annotated pdf will be of great help.

Please note the annotated pdf from Reviewer B is large and I will provide it separately.

I don't have much to add to the comprehensive review from Reviewer B. I think it is important to remind you though that the revision should attempt as best as possible to address reviewer concerns while keeping in mind the journal guidelines. In general, we do not want more than 10 figures and tables, and you have a lot more. The paper is also on the long side for Seismica. I realise both of these points are in the nature of paleoseismic papers that need to document trenches. However, for example, Figures 1 to 3 provide context and could be reorganised into two or even just one multi-panel figure. The tables could potentially be a supplement? They are important to provide, but are they essential for a reader to follow the argument? Could some of the other figures also be merged or made supplementary? We can allow a couple of extra figures, but note that all extra figures and tables will take extra time in copyedit. On top of that, I would urge you to try shorten the paper, but I think the comments from Reviewer B should help focus this effort.

Ake Fagereng

Reviewer A

For author and editor

The revised version submitted by the authors addresses the comments raised during the first round of review. I find it fully relevant and suitable for publication.

However, I still believe the conclusion could be enhanced. While the results and their significance in relation to the initial research question are well-summarized, it would benefit from including additional perspectives and/or unresolved questions. For example, building on what particularly piqued my interest in this excellent work—the relationship between active faults, mud volcanoes, and aseismic slip—I suggest exploring the following:

1. What insights do the global examples cited in the discussion provide regarding these interactions? Has aseismic slip been documented at the surface in the study area?
2. How could this question be addressed here? Given the slip rates of ~5 mm/year, could an InSAR analysis using Sentinel data provide some answers?

Including these elements would strengthen the conclusion by offering future research directions and stimulating further scientific inquiry.

Reviewer B

For author and editor

This paper has been substantially improved since the first version I reviewed, and many of my comments and concerns have been addressed. However, several have been addressed incompletely or not at all, and some of the revisions have created inconsistencies within the paper or necessitated further explanation. Therefore, significant additional revisions are still required before this paper would be suitable for publication. I summarize my major comments below; details can be found in the annotated PDF.

1. In the stratigraphy descriptions, designating the formerly-labeled U70 as part of U30 necessitated more rewording than the authors had done. I've made some suggestions to eliminate repetition and improve clarity in that section. Another stratigraphy question I have is how U80 was correlated across F1. How is that layer distinct from the rest of Group 4? Seems like it's pretty similar (laminated silts). If that correlation is done just based on age, that's not a good reason-- different units can have the same age. This becomes relevant later because dates from different parts of the unit are used to constrain earthquake ages, which should not be done if the similar ages are the only basis of the unit correlation in the first place.
2. Are the IRSL "BP" ages before 1950, or before the date of laboratory analysis? Verify and clarify.
3. The promised discussion about whether the IRSL ages or 10kyrs older radiocarbon ages are correct still hasn't fully materialized: at least in the context of the earthquakes it seems like the radiocarbon samples are ignored, and in the slip rate section the inferences taken from those ages are not clear and certainly are not presented as competing hypotheses. My suggestion is just to support hypothesis 1 from the beginning, i.e. the IRSL ages are correct and the two shells are reworked (super common in fluctuating lake level environments.)
4. Section 5.5 should start with a brief introduction to your OxCal model with some explanation that you chose to use only dates from Trench 1, since correlations of units and events between the two trenches are somewhat uncertain. Since you have removed Fig. 15, there should also be a statement about your assumptions that the three youngest events are the same in both trenches (no missing or misinterpreted events). It seems reasonable given the dates but you have to note that they can't be correlated unequivocally.
5. The descriptions of date brackets and resulting ages for events 1-3 need to be updated now that the OxCal model in Fig. 14 no longer includes the Trench 2 samples. The T1 and T2 ages can be combined, but you need to explain further how you did this combination, including stating the assumption that the evidence in both trenches represents the same event. Also, currently, the dates given for Event 1 in Fig. 14 seem to bear no relation to what is discussed in the text.
6. Lines 688-712: there are inconsistences in how many events are certain earthquakes (four often stated, but three listed), which has implications for the stated recurrence interval. Likewise, there are inconsistencies throughout the paper, starting here, about the time period with the recent cluster (is it 2 events or 3? 500, 600, or 650 years?) In addition to correcting those, I suggest a substantial reorganization of this paragraph for clarity, along the lines of: "Events 2, 3, and 6 provide the most compelling evidence for coseismic rupture, given the [list evidence such as large fissures]. These features are unlikely to have formed otherwise. Events 1 and 4 could have formed

due to aseismic slip processes rather than earthquakes: [list evidence]. Events 5 and 7 are even less confidently identified as earthquakes: [describe]."

7. In section 6.1, it looks like some words / sentences are missing, but as stated there is still no explanation for why U30 is related to Qt3. The age of the 7.5 m offset is still not explained / justified. It's hard to tell the relationship between Trench 1 and the geomorphic units on Fig. 10, which is why I recommend plotting the trench outlines on Fig. 10A.
8. Lines 743-749: This paragraph is a bit disorganized and the numbers of offsets in various categories don't seem to match Fig. 15. I suggest reorganizing along the following lines: "From our field and remote observations, we identified X offsets below 5 m and Y offsets between 5 and 10 m (including the 7.5-meter displacement at the Salyan trench site). We infer that the offsets below 5 m occurred in the MRE and the 5–10 m offsets represent at least two events."
9. Late Pleistocene slip rate in Section 6.2 and discussion/conclusions: It's unclear what constraints on the Caspian Sea level are being argued for in lines 773-776. Given the large mismatch with the geodetic rate, I think you have to consider the possibility that the slip rate you calculate from the 53-m offset is incorrect. Perhaps the landscape wasn't completely reset during the LGM highstand, or perhaps more likely, the riser was always somewhat undulatory (this can be seen to the southwest of the measured offset on Fig. 10A-B, actually), and the 53-meter measurement overestimates the true offset. Considering the other faults such as the Talesh thrust (not to mention all the other WCF strands) makes the problem worse rather than better. Why do you not mention the slip rate you get from the 7.5-meter offset in the discussion/concusions? That one seems better constrained and more in line with the geodetic estimate.
10. In Section 6.3, since you're talking about how to divide up slip, I suggest using the estimate that assumes little to no aseismic slip. Something like "Assuming the paleoseismic events represent earthquakes rather than aseismic slip, 5 to 6 earthquakes occurred in the last 3000 years." Then calculate slip per event from that, and end with "However, because a substantial portion of this slip may have accumulated aseismically, we cannot draw robust conclusions about coseismic slip-per-event."
11. In the discussion, it would be good to state when historical records become reliable enough in this region to exclude M6+ earthquakes.
12. Suggested some reorganization and streamlining for clarity in the Conclusions.
13. Figures 1, 2, and 3 have been substantially improved, but some undesirable and possibly unintentional changes were also made, such as the disappearance of the yellow box in Fig. 1A, the conversion of all fault motion line symbols to dashed black lines in Fig. 2A, the absence of a seismicity legend on Fig. 2B, and the disappearance of previously mapped faults from Fig. 3.
14. While somewhat improved from the original submission, there are still numerous typos, text fragments, and minor grammatical errors (particularly regarding hyphenation) throughout the text. I have tried to mark these comprehensively on the annotated PDF.

REVIEW RESPONSE (Round 2)

Reviewer B:

This paper has been substantially improved since the first version I reviewed, and many of my comments and concerns have been addressed. However, several have been addressed incompletely or not at all, and some of the revisions have created inconsistencies within the paper or necessitated further explanation. Therefore, significant additional revisions are still required before this paper would be suitable for publication. I summarize my major comments below; details can be found in the annotated PDF.

1. In the stratigraphy descriptions, designating the formerly-labeled U70 as part of U30 necessitated more rewording than the authors had done. I've made some suggestions to eliminate repetition and improve clarity in that section. Another stratigraphy question I have is how U80 was correlated across F1. How is that layer distinct from the rest of Group 4? Seems like it's pretty similar (laminated silts). If that correlation is done just based on age, that's not a good reason-- different units can have the same age. This becomes relevant later because dates from different parts of the unit are used to constrain earthquake ages, which should not be done if the similar ages are the only basis of the unit correlation in the first place.

   NM: Thanks. No this was logged as the same material in the field. I was very happy to that the ages either side of the fault came back similar! Group 4 I agreed is a bit confusing on reflection. I have moved U80 and U100 into group 3 as these are interpreted to be alluvial/fluvial in environment. The laminated nature we interpret as overbank flood plain deposition. Group 4is a collection of silts that we interpret as windblown and possibly some colluvium. However, it is almost impossible to distinguish colluvial material as all the units are so fine (including the source rock). For U80, this unit contains fine sand and silt and we see these sands either side of F1. Our interpretation is that this unit an alluvial deposit that has then been faulted. There is a distinct change in bed thickness to the southwest across the fault in this unit which is consistent with the edge (thinner) part of the alluvial deposit being right-laterally offset next to the thicker more central part of the alluvium.

2. Are the IRSL “BP” ages before 1950, or before the date of laboratory analysis? Verify and clarify.

   BP ages before 1950 – clarified in the text.

3. The promised discussion about whether the IRSL ages or 10kyrs older radiocarbon ages are correct still hasn’t fully materialized: at least in the context of the earthquakes it seems like the radiocarbon samples are ignored, and in the slip rate section the inferences taken from those ages are not clear and certainly are not presented as competing hypotheses. My suggestion is just to support hypothesis 1 from the

beginning, i.e. the IRSL ages are correct and the two shells are reworked (super common in fluctuating lake level environments.)

Thanks. I've gone with your suggestion i.e. IRSL are correct and shells are reworked.

4. Section 5.5 should start with a brief introduction to your OxCal model with some explanation that you chose to use only dates from Trench 1, since correlations of units and events between the two trenches are somewhat uncertain. Since you have removed Fig. 15, there should also be a statement about your assumptions that the three youngest events are the same in both trenches (no missing or misinterpreted events). It seems reasonable given the dates but you have to note that they can't be correlated unequivocally.

   Yes I've put in an introduction to the OxCal, we utilize both sets of dates, but given the inversion in trench 2 and the good stratigraphic order in trench 1 we prioritize trench 1 as a fairer representation over a longer time period (~5k yrs). If we assume Qt1 is around 500 yrs old, from the three dates this gives 2-3 events in 500 years. In trench 1 we have 2 events within ~600 yrs, so roughly speaking the events in both trenches are in the same ballpark but it's not a home run.

5. The descriptions of date brackets and resulting ages for events 1-3 need to be updated now that the OxCal model in Fig. 14 no longer includes the Trench 2 samples. The T1 and T2 ages can be combined, but you need to explain further how you did this combination, including stating the assumption that the evidence in both trenches represents the same event. Also, currently, the dates given for Event 1 in Fig. 14 seem to bear no relation to what is discussed in the text.

   I've made some adjustments to the descriptions that do not combine Trenches 1 and 2 for the probable ages. I comment that trench 2 does not correlate but the ages are close with the exception of E3.

6. Lines 688-712: there are inconsistences in how many events are certain earthquakes (four often stated, but three listed), which has implications for the stated recurrence interval. Likewise, there are inconsistencies throughout the paper, starting here, about the time period with the recent cluster (is it 2 events or 3? 500, 600, or 650 years?) In addition to correcting those, I suggest a substantial reorganization of this paragraph for clarity, along the lines of: "Events 2, 3, and 6 provide the most compelling evidence for coseismic rupture, given the [list evidence such as large fissures]. These features are unlikely to have formed otherwise. Events 1 and 4 could have formed due to aseismic slip processes rather than earthquakes: [list evidence]. Events 5 and 7 are even less confidently identified as earthquakes: [describe]."

   Added the suggested text and made changes to the recurrence interval so that it consistent as 500-1000 years. I have also removed text to make this paragraph clearer.

7. In section 6.1, it looks like some words / sentences are missing, but as stated there is still no explanation for why U30 is related to Qt3. The age of the 7.5 m offset is still not explained / justified. It's hard to tell the relationship between Trench 1 and the geomorphic units on Fig. 10, which is why I recommend plotting the trench outlines on Fig. 10A.

   Thanks, I've made changes to this section and the figure as requested.

8. Lines 743-749: This paragraph is a bit disorganized and the numbers of offsets in various categories don't seem to match Fig. 15. I suggest reorganizing along the following lines: "From our field and remote observations, we identified X offsets below 5 m and Y offsets between 5 and 10 m (including the 7.5-meter displacement at the Salyan trench site). We infer that the offsets below 5 m occurred in the MRE and the 5–10 m offsets represent at least two events."

   Made the changes thanks for the helpful suggestion.

9. Late Pleistocene slip rate in Section 6.2 and discussion/conclusions: It's unclear what constraints on the Caspian Sea level are being argued for in lines 773-776. Given the large mismatch with the geodetic rate, I think you have to consider the possibility that the slip rate you calculate from the 53-m offset is incorrect. Perhaps the landscape wasn't completely reset during the LGM highstand, or perhaps more likely, the riser was always somewhat undulatory (this can be seen to the southwest of the measured offset on Fig. 10A-B, actually), and the 53-meter measurement overestimates the true offset. Considering the other faults such as the Talesh thrust (not to mention all the other WCF strands) makes the problem worse rather than better. Why do you not mention the slip rate you get from the 7.5-meter offset in the discussion/concusions? That one seems better constrained and more in line with the geodetic estimate.

   Thanks, good points. It's hard to see a model where a riser forms prior to sea level dropping. I take the material point though and the riser could be eroded (fluvial) and exaggerating the offset. It is also possible that slip is variable through time. It is worth noting that the geodetic estimates are far from robust. I have added to the text to reflect this.

10. In Section 6.3, since you're talking about how to divide up slip, I suggest using the estimate that assumes little to no aseismic slip. Something like "Assuming the paleoseismic events represent earthquakes rather than aseismic slip, 5 to 6 earthquakes occurred in the last 3000 years." Then calculate slip per event from that, and end with "However, because a substantial portion of this slip may have accumulated aseismically, we cannot draw robust conclusions about coseismic slip-per-event."

    Thanks made the change and added in a recurrence interval for the 7.5 m offset riser.

11. In the discussion, it would be good to state when historical records become reliable enough in this region to exclude M6+ earthquakes.

    Thanks yes, unfortunately I'm not best placed to make this determination outside when instrumental data is more widely available. So, I don't want to comment on this until other documents/archeological data becomes available in the future.

12. Suggested some reorganization and streamlining for clarity in the Conclusions.

    Made the changes as per your helpful comments in the pdf, thanks.

13. Figures 1, 2, and 3 have been substantially improved, but some undesirable and possibly unintentional changes were also made, such as the disappearance of the yellow box in Fig. 1A, the conversion of all fault motion line symbols to dashed black lines in Fig. 2A, the absence of a seismicity legend on Fig. 2B, and the disappearance of previously mapped faults from Fig. 3.

    Made the changes and moved fig 3 to supplementary document fig S1. In fig S1 I have not included the Zelenin et al., 2022 fault map – purely for simplicity. It is on fig 2.

14. While somewhat improved from the original submission, there are still numerous typos, text fragments, and minor grammatical errors (particularly regarding hyphenation) throughout the text. I have tried to mark these comprehensively on the annotated PDF.

    Thanks for this. I have made the changes marked in the pdf.

Recommendation: Revisions Required