Reviewer A (Formal Review for Authors (shown to authors)):

Review Comments on "A Rare Earthquake in the Upper Mantle of Subcontinental Lithospheric Mantle of North America by Benedict Fitton, Timothy J. Craig, and Sebastian Rost".

The authors re-examined the 2025 Mw 4.1 earthquake that occurred in northern Utah. Using regional and teleseismic recordings, they reassessed its source location and focal mechanism. Their analysis confirms that the hypocenter lies below the local Moho, within the continental mantle. The inferred focal mechanism is consistent with reported mantle seismicity but contrasts with the crustal stress field. They conclude that mantle seismicity in this region is unlikely to influence crustal geodynamics.

The study is sound and clearly presented. The manuscript is close to being acceptable as is.

A few nagging concerns below - I believe that it would not be difficult for the authors to address them or provide clarifications.

1) Does the inconsistency between the mantle event's focal mechanism and the crustal stress field constitute definitive evidence that shallow crustal geodynamics do not influence mantle seismicity (Abstract, Line 9-10)?

We agree that the observed inconsistency between the mantle event focal mechanism and the crustal stress field does not constitute definitive evidence that shallow crustal geodynamics do not influence mantle seismicity. We have therefore revised the wording throughout the manuscript to avoid overly definitive interpretations and to better reflect the uncertainty associated with this relationship.

Actions taken:

- *Revised the discussion of the stress relationship in Section 3 "The focal mechanism we recover for the Utah event is not consistent with this stress regime: a thrust fault with a strike of 118°implies a compressional principal stress oriented NNE-SSW. Hutchings et al. (2025) argued that the Mw 4.7 2013 Wind River and the composite solution for the Ml 3.2 2019 Granger (Figure 1), Wyoming mantle earthquakes have focal mechanisms which reflect the same stress field as in the crust. We do not observe this in the case of the Mw 4.1 2025 Utah earthquake~~, indicating that the stress between mantle and crust may not be as well-coupled south of the Eastern Snake River Plain.~~. This difference in principal stress orientation may reflect heterogeneity or a transition within the mantle stress field, potentially associated with craton-edge dynamics (Koper et al., 2026), or some degree of crust-mantle decoupling)*
- *Softened language in the abstract: "is not reflecting shallow crustal geodynamics" to "may not reflect shallow crustal geodynamics"*

2) Is it possible to investigate or make any inference about the stress drop associated with this event? One possibility is that mantle earthquakes exhibit relatively long-period source characteristics, including slower rupture velocities and longer rise times.

We agree that the investigating the stress drop and further source characteristics of the event could provide additional insight into the rupture process. However, we consider this analysis to be beyond the scope of the present study, more suited to a full paper than a short report.

3) The data can be explained without an isotropic or CLVD component, but it may not imply a lack thereof. Likewise, the absence of an associated earthquake cluster or aftershock sequence may not negate magmatic impregnation of the lithosphere. I might be wrong here.

We find that the source is adequately represented by a double-couple mechanism. This is supported by the preferred USGS moment tensor solution, which indicates a 99% double-couple component. Although introducing additional degrees of freedom could improve the fit, the close agreement with the USGS solution and the quality of the fit achieved with the double-couple model suggest that a more complex source representation is unnecessary.

Action taken

- *Expanded double-couple assumption explanation: "In both cases, the source is well described by a purely double-couple mechanism, without requiring an isotropic or CLVD component; for the 2025 Utah earthquake, this is consistent with the favoured USGS solution provided by the University of Utah, which finds a 99% double-couple source (USGS, 2017)."*

Reviewer B (Formal Review for Authors (shown to authors)):

The submitted manuscript; " A rare earthquake in the upper mantle of subcontinental lithospheric mantle of North America", authored by Benedict Fitton, Timothy J. Craig, and Sebastian Rost is presenting source process analyses for a Mw 4.1 earthquake on 2025-09-10 occurred beneath northern Utah. The authors utilise regional seismic records to perform a probabilistic source inversion for the focal mechanism, focal depth, and magnitude, and they further evaluate the source depth through teleseismic depth-phase analysis using the teleseismic records. They obtain a centroid depth of approximately 52 km, which they argue places the event firmly in the sub-Moho continental lithospheric mantle. The recovered focal mechanism shows oblique thrust faulting (strike 118°, dip 60°, rake 50°), implying NNE-SSW maximum compression. The authors argue that this orientation is inconsistent with the known E-W extensional crustal stress field, and they interpret this as evidence for stress decoupling between the crust and the mantle. They also argue against fluid-related triggering mechanisms based on the absence of an aftershock sequence and the relatively large distance from the Yellowstone hotspot.

The methodologies are solid: the probabilistic regional waveform inversion using BEAT provides well-resolved posterior distributions for the source parameters, and the teleseismic depth-phase analysis with array beamforming and vespagrams offers an independent constraint on focal depth that is largely consistent with the regional waveform solution. The combined approach is a genuine strength of the study and goes well beyond routine catalogue-based analysis. The mechanism remains an open question, but I concur with the authors that the application of probabilistic source analyses with independent depth verification, as demonstrated here, provides a fundamental basis for untangling the enigmatic mantle seismicity in this region and beyond. I only have a few minor issues to be noted here, which I hope will be useful for improving the manuscript.

There seems to be a recently published study of the same earthquake by Koper et al. (2026, https://doi.org/10.1785/0320260006). One point I believe worth a comment is that Koper et al. report a focal depth in a range of 60–70 km, which differs from the present study. Both studies agree that the event is sub-Moho, so the substantive conclusion should be unchanged, but a brief acknowledgement of the discrepancy and a sentence or two on possible reasons (e.g., differences in station distribution, frequency band, or velocity model) would be valuable for future readers comparing the two solutions. Relatedly, it would be worth noting whether the very close station UU.VNL2 (~13 km from the epicentre), which provides strong P-S differential time constraint on depth, was included in the regional inversion (or perhaps it didn't satisfy the authors' screening condition?); if not, a short comment on why would help readers interpret the depth estimate.

We thank the reviewer for drawing our attention to the recent study by Koper et al. (2026). We agree that a comparison is valuable, particularly given the difference in inferred focal depth (~52 km in this study versus ~60–70 km in Koper et al., 2026), although we note that in both cases, the earthquake remains in the mantle. We have revised the manuscript to acknowledge this discrepancy and briefly discuss potential contributing factors, including differences in data selection, station distribution, and velocity models. The station UU.VNL2 was not included in our regional inversion.

Despite the difference in depth estimates, both studies independently place the earthquake beneath the Moho, and the focal mechanisms are consistent. We now highlight this agreement by reporting the small Kagan angle between the solutions.

Action taken

- *Added comparison with Koper et al. 2026 in the Discussion "Koper et al. (2026) reported a focal depth of ~60–70 km for the same event. This discrepancy may reflect differences in data selection, station distribution, and velocity models. Our depth estimate is supported by teleseismic beamforming, which provides additional constraints on depth-sensitive phases that were not exploited in Koper et al. (2026). Theoretical radiation patterns indicate that the dominant depth phase is sP rather than pP, and the alignment between observed and predicted sP arrivals supports a shallower depth in our preferred range. Both studies place the earthquake sub-Moho, and the inferred focal mechanisms are highly consistent (Kagan angle = 7.5°; Tape and Tape, 2012)."*

Engagement with the mechanism proposed by Koper et al. (2026) would also strengthen the discussion. Their hypothesis is that edge-driven or regional-scale mantle convection produces increased strain rates near the Wyoming craton boundary, making either conventional brittle failure or thermal runaway feasible at relatively high pressure-temperature conditions, with possible additional contribution from fluids inferred from high electrical conductivity around the craton edge (e.g., Bedrosian and Frost, 2023, https://doi.org/10.1130/B36417.1). This is directly relevant to the present discussion of triggering mechanisms. I appreciate the authors' restraint in not advocating strongly for any single mechanism, which I think is intellectually honest given the data, and I do not think the manuscript needs to expand the discussion substantially. However, a brief mention of this hypothesis alongside the alternatives the authors already consider (Mohr-Coulomb failure in cold lithosphere, dehydration embrittlement, shear heating, fluid migration) would round out the discussion.

We thank the reviewer for this suggestion and agree that the hypothesis proposed by Koper et al. (2026), together with the observations of elevated electrical conductivity reported by Bedrosian and Frost (2023), provides relevant context for interpreting potential triggering mechanisms for mantle seismicity near the Wyoming craton boundary. While we aim to keep the discussion concise *and focused on constraining the event depth, we agree that briefly acknowledging these proposed mechanisms strengthens the broader tectonic interpretation without overextending the scope of the report.*

Action taken:

- *Added reference to the hypothesis proposed by Koper et al. (2026) (See response to reviewer A question 1)*
- *Added reference to Bedrosian and Frost (2023) in the discussion section* *"In contrast, Zhao et al. (2024) propose that ~~ascending fluids derived from the remnant subducted Farallon slab may have induced the 2013 Wind River earthquake~~. the 2013 Wind River earthquake may have been induced by ascending fluids derived from the remnant subducted Farallon slab, consistent with high electrical conductivity observed near the edge of the Wyoming Craton (Bedrosian and Frost, 2023)."*

The authors interpret the NNE-SSW compression implied by the focal mechanism as evidence for stress decoupling between the crust and mantle. This is a reasonable reading, but Koper et al. argue, on the basis of six continental mantle earthquake (CME) focal mechanisms in the region, that the mantle stress field itself may be heterogeneous, with NE-SW compression in northeastern Utah/southwestern Wyoming possibly transitioning to extension to the northeast, driven by ~4 cm/yr ENE asthenospheric flow impinging on the cratonic edge. This alternative reading is worth acknowledging briefly, since the same focal mechanism can be interpreted either as evidence for crust-mantle decoupling or as a sample from a regionally coherent but spatially varying mantle stress field driven by craton-edge dynamics.

We agree that the NNE-SSW compression implied by the focal mechanism could reflect heterogeneity or a transition within the mantle stress field, rather than solely indicating crust–mantle decoupling. We have revised the discussion to acknowledge that the observed stress orientation could also be consistent with a spatially varying mantle stress field associated with craton-edge dynamics, as proposed by Koper et al. (2026).

Action taken:

- *Added discussion acknowledging that the difference in principal stress orientation may reflect heterogeneity or a transition within the mantle stress field and reference to craton-edge dynamics proposed by Koper et al. (2026) (See response to reviewer A question 1)*

A few additional pieces of related literature would also be worth incorporating. Woo and Chen (2025, https://doi.org/10.1785/0320250032) recently identified six additional CMEs in the Wyoming craton region during 2007–2010, expanding the regional catalogue. Hutchings et al. (2025) report an ML 1.4 event on 15 December 2020 at ~55.8 km depth located only ~7 km from the 2025 epicentre, which is potentially relevant to the framing of the 2025 event as "isolated"?

We have now incorporated reference to the recent work of Woo and Chen (2025), which expands the catalogue of continental mantle earthquakes beneath the Wyoming Craton. We also acknowledge the ML 1.4 mantle earthquake reported by Hutchings et al. (2025), located close to the 2025 Utah event, and have revised the discussion to avoid overstating the isolated nature of the 2025 earthquake.

Action taken:

- *Added reference to Woo and Chen (2025) in the introduction.*
- *Added acknowledgement of the nearby ML 1.4 event identified by Woo and Chen (2025) "Recent work by Woo and Chen (2025) identified additional small mantle earthquakes beneath the Wyoming Craton, including an ML 1.4 event in 2020 located close to the 2025 Mw 4.1 Utah earthquake"*

The "double Moho" possibility, in which a high-velocity layer at the base of the crust could create an apparent shallower Moho with the true Moho deeper, is briefly discussed by Koper et al. for this source region (they find no strong evidence for it from nearby receiver functions). Given that the present study's depth of ~52 km is closer to the depths at which this scenario could matter, a brief mention would help close out this potential concern. Second, the assertion that the absence of aftershock activity argues against a fluid-driven mechanism (l.172–173) is reasonable, but Koper et al. note a small impulsive arrival ~1.8 s after the first P at VNL2 that they tentatively interpret as a possible early aftershock (although propagation effects cannot be excluded). The authors may wish to briefly acknowledge this observation.

We agree that the possibility of a "double Moho" structure is relevant in principle; however, we follow the interpretation of Koper et al. (2026), who found no strong receiver-function evidence supporting such a structure beneath this region. We therefore do not feel that further discussion of this possibility is necessary within the scope of the present study.

The label on Figure 1A reads "Mw 4.1, 10/09/2026" but should be 2025?

"who's" in the abstract something intended? (not "whose"?)

"too hot to failure" -> "too hot to fail" ?

"best studies" -> "best studied" ?

We have corrected the typos identified by the reviewer