Authors' Response

We would like to express our gratitude to the reviewers and the editor for their constructive and helpful feedback. We have revised the paper to incorporate the suggestions. In line with this, we also prepared this document to itemize and compile our responses and actions on the comments of the reviewers and editor. We hope that the revisions we prepared are acceptable and will be published in the NHESS.

Reviewer 1

- **What is the probability that the earthquake might have been larger than Mw6.6?**

Response: Our preferred estimate of Mw6.6 is equal to the instrumental moment magnitude. However, given the rupture length estimates and the lack of data in the offshore sections, the downward counterfactual approach can suggest a larger geodetic moment magnitude. Estimating such can be undertaken upon the availability of detailed information at depth of the fault plane and the submerged portions. As such, we recognized and acknowledged the downward counterfactual thinking in-text as follows:

> "The Mw estimates from the maximum and average displacements range between Mw6.5 to Mw6.7, and closely fit the instrumental 6.6 moment magnitude. All rupture length estimates returned high Mw values between Mw6.7 to Mw7.0 which agrees with the 2003 earthquake observations, wherein the fault rupture is longer than expected relative to the magnitude. However, a downward counterfactual approach (Mignan and Woo, 2018; Woo 2019) given the fault uncertainties at depth, suggests the probability of the larger Mw estimates. Nevertheless, comparing the individually calculated Mw values show that the moment magnitude estimates from maximum displacement provide the closest fit with instrumentally determined moment. The Mw6.6 estimate from the inferred maximum displacement is equivalent to a seismic moment of 1.2×10^{19} N-m."

- **Suppose that a stochastic model of the Masbate earthquake were to be undertaken, out of the possible scenarios generated, would any have approached Mw7? In other words, how surprising would such an outcome have been in August 2020?**

Response: The highest Mw estimate derived from the rupture lengths is indeed Mw7.0. However, it is essential to consider the available historical seismicity spanning from 1800 to the present (SEASEE, 1985; Besana and Ando, 2005). The record indicates that significant seismic events produced by the Masbate segment have been moderate in magnitude and have not reached Mw7.0. This observation is likely attributed to the presence of interseismic creep, which enables a constant release of stress along the segment. The Masbate segment acts as a relay (Bacolcol, 2003), which transfers stress onto the other segments of the Philippine Fault. Consequently, we regard the Mw7.0 estimate as implausible in light of this information. Furthermore, there is a concurrence among the instrumental measurements of various monitoring agencies.

Reviewer 2

- **Field data - There are few details on the field offset measurements compared to the detailed description of the InSAR and optical processing, and no uncertainties are plotted. How were field measurements made? How are uncertainties determined? Please show uncertainties in the figures that include field data points. The field measurements follow the optical measurements extremely closely. This is surprising because field measurements tend to be more variable than optical measurements, and because the field measurements were made two years after the earthquake. The authors suggest that geomorphic change can explain this, but geomorphic change is likely to make field measurements more variable, not less. Furthermore, optical image correlation includes far-field distributed deformation where it exists, so it is expected that optical measurements might be larger than field measurements in places. What else can explain why the field measurements are so similar to the optical measurements? Finally, the table of the field measurements is not referenced in the main text.**

Response: We appreciate the reviewer's comments. However, we stand by our findings and the most that we could add are the field measurement uncertainties from the inherent fuzziness of offset features coupled with probable human errors. We reevaluated our available data and provided the range of measurements for closely-spaced measurements and compared our measurements with the earlier ones. We cannot alter neither our field measurements nor the image correlation results. As for the variability of slip distribution, there are areas where variability is present and was discussed in the manuscript. In addition, we calculated a coefficient of variability (Cv) = 0.36 (following the approach of Reitman et al., 2021), which shows that our data is well within the global range. Lastly, per reviewer #3, our "*remote sensing results accurately capture ground displacements and provide greater along-strike coverage. The similarity between the image correlation results and field measurements is a good point rather than a bad point, and emphasizes the strength and usefulness of the method.*"

- **Post-seismic deformation - The values calculated for post-seismic deformation (min/avg/max, Mw, etc) are all minimums because the post-seismic insar time series begins two days after the mainshock. This point is not made clear until late in the discussion. It should be made clear in the Data and Methods section, and every place one of those values is discussed it must be stated that it is a minimum, including in the abstract and conclusion.**

Response: Thank you very much for pointing this out. We have clarified that the values are lower bound estimates of post-seismic deformation.

- **Supplementary Material – The supplement is detailed and contains a lot of relevant information, but it is not referenced in the main text. Add references to the supplement where appropriate throughout the main text.**

Response: Thank you. The supplements are cross-referenced in-text in the revised paper.

- **Diverging colorbars (red-white-blue) – Many figures use the diverging red-white-blue colorbar, which is a great choice for visualization of diverging data such as optical pixel correlation and unwrapped interferograms. However, many of these figures have asymmetric scales, and it's unclear if 0 is white. The purpose of a diverging colorbar is to highlight a break in data (such as from negative to positive in pixel correlation results), and making that break within one side (not at white) negates that purpose. Make sure that all diverging colorbars have white = 0.**

Response: This is acknowledged. The color scales of all figures in question have been modified to a sequential scale.

For the specific comments, our responses are itemized below. Minor changes that do not need an exposition are marked with a check (✔) for simplicity.

Lines 3-4 – State average displacement in addition to max for optical and insar results. ✔

Line 10 – Present tense "demonstrates" ✔

Line 12 – Vague. What are the insights gained from this work for seismic hazard in Masbate?

Response: The findings and the methods we used for this study in Masbate leads us and future scientists to further analyze the geometry, kinematics, mechanics, and dynamics of fault movement in the area for improved seismic hazard mitigation. Specifically, the comprehensive slip data and observed unusually long rupture lengths along the fault measured through the optical correlation method agree with previous characterizations by Besana and Ando (2005), indicating significant movement despite the presence of creep. These sentences are also added in the conclusions.

Line 21 – If these preliminary assessments are not part of the work presented in this study, use past tense (showed or imaged). ✔

Line 24 – Unclear what "alongside the reduced coherence due to the interval" means in this context.

Response: This refers to the phase coherence of the interferograms produced by Tiongson and Ramirez (2022). The 12-day interval that they used will have less coherence compared to a shorter 6-day interval. We preferentially employed the shorter interval knowing the vegetative cover of the study area. The sentence is improved to remove the confusion

Lines 26-27 – How different are the expected insar and seismic models for slip compared to the expected slip calculated from moment based on the Wells and Coppersmith equations? Since the data that those equations are based on are fairly cloudy (i.e., they do not exactly follow a line – see figures 10 and 11 in Wells and Coppersmith 1994), there is a sizeable range allowed for the slip for any given magnitude. Suggest add more details or delete this sentence.

Response: The three previous studies that we cited resulted in the following: 3-30 cm LOS displacement (PHIVOLCS, 2020), >15 cm LOS displacement (Tiongson and Ramirez, 2022), and >1.0 m peak ground displacement (Simborio et al, 2022). If we convert these values to moment magnitudes, the values would be Mw 5.6 to 6.4 (PHIVOLCS, 2020), Mw 6.2 (Tiongson and Ramirez, 2022), and Mw 6.8 (Simborio et al., 2022). Looking at the range of peak displacements from Wells and Coppersmith (1994), a Mw 6.6 earthquake should produce displacements between ~0.45 to ~1 m, considering a 95% confidence interval. This was our basis for saying that the previous studies have under- and overestimated results.

Line 28 – Delete "the novel". Optical image correlation methods are well established. ✔

Line 32 – Delete "relatively". State information about the 2003 event. Not all readers will be familiar with it. What magnitude? On what fault? ✔

Lines 42-43 – Consider using consistent units for tectonic rates (i.e., both in cm or both in mm) ✔

Line 48 – Delete "extant" ✔

Figure 1 caption – Delete "Bathymetry data from GEBCO" in the description for (C). No bathymetry data are shown in panel C. Earthquake "epicenters" not earthquake "centers". ✔

Lines 70-77 – Consider if this detailed geologic information is necessary to include to understand the results or discussion.

Response: Thank you for the suggestion. We briefly explored some possible lithological controls on the rupture process in the discussions, which makes these lines relevant.

Line 105 – "Ground motion" refers to ground shaking. "Surface displacement" is more appropriate here. ✔

Line 109 – Delete "this study" ✔

Line 110-111 – Not sure what is meant by this sentence. Consider deleting. ✔

Line 145 – Replace "was' with "were" ✔

Line 158 – Which dimension is profile width and which is length? What is profile spacing? Is there space between the profiles or is every section of fault considered in a profile?

Response: The profile width is the wider dimension (~1.0 km) across the fault and vice versa. Every section of the fault is included in the profiling. A sample configuration is shown in Supplement S1.

Line 165-166 – The moving mean may have minimal smoothing, but the size of the profile width and profile spacing both contribute to smoothing the overall slip distribution (e.g., see comparison of hand-measured vs optical-measured offsets for the same earthquakes in Figure 9 in https://doi.org/10.1002/esp.5294)

Response: Thank you for the clarification.

Table 2 – Consider replacing "granules" with "scenes" ✔

Line 193 – Fieldwork occurred almost two years after the earthquake! There could be significant geomorphic or human modification of offset features in the interim, and it's likely that many offsets and evidence of surface rupture have already been erased given the small magnitude of surface slip. This should be discussed somewhere.

Response: Thank you for pointing it out. We included a clarifying paragraph in this section as follows "Due to

the two-year interval between the earthquake and fieldwork, geomorphic or human modifications of offset features may have occurred. Additionally, many offsets and evidence of surface rupture may have been erased given the small magnitude of surface slip. The significance of these changes can be assessed by correlating field data with other measurement techniques to determine the slip."

Line 200 – Please provide more details on the field measurements. How were offset measurements made in the field? How is uncertainty characterized?

Response: We added further details on this as can be seen in section 3.4.

Line 209 – Peaks in what dataset?

Response: These refer to the peaks of Figures 3a and 3b. We added "peaks of the frequency plots" for clarity.

Line 214 – Unclear what is meant by "days without earthquakes are closer to each other". Please rephrase. ✔

Table 3 – Consider if this table is necessary since the focal mechanisms are shown in Figure 4, and this information is easier to understand from the visual focal mechanisms than numbers in a table. ✔

Line 221 – Delete "(3)" ✔

Figure 4 – Great figure!

Response: Thank you. Much appreciated.

Table 4 – Consider if this table is necessary. Some of this information is stated in the text in section 4.1.1, and it could all be included there. ✔

Section 4.2.1 – Add references to Figure 5 within this paragraph. ✔

Figure 5 – Add north arrow and legend for line colors to panels A and B. For the color scales in panel A and B is white 0? If not, adjust color scale so that white is 0. Consider if there is a better color scale for panels C, D, and F because rainbow scales are difficult to perceive (e.g., https://www.scientificamerican.com/article/end-of-the-rainbow-new-map-scale-is-more-readable-by-people-who-are-color-blind/ and https://hess.copernicus.org/articles/25/4549/2021/hess-25-4549-2021.html). Label east and west blocks in panels D, E, F, G. In explanation for E: "negative Y axis"

Response: This is noted. All figure symbology are replaced with a single-hue sequential color scale. The north arrow and scale bar are moved to panel A to immediately assist the observer. East and west notations are added to panels D, E, F, and G.

Figure 6 – The peak ~0.60 cm value seems anomalously high. Are there profiles adjacent to this profile (or could more profiles be analyzed) that can corroborate the peak value? Is the "overall trendline" the weighted moving mean discussed in the text or calculated a different way? Please explain.

Response: The ~0.60 cm value wouldn't be an anomaly. Multiple image pairs agreed to this value as indicated by the overlapping circles. The value was excluded by the weighted moving mean due to smoothing. And yes, the weighted moving mean refers to the "overall trendline", we corrected the legend label for clarity.

Line 273 – "which may be due to noise" --> this signal could also be due to distributed deformation. It is well known that far-field signals capture distributed deformation over 10s-100s of meters from the primary fault trace. What is the evidence for or against noise versus distributed deformation?

Response: Thank you for the explanation. It is challenging to discern between the two due to the dataset and land cover. However, the location of box 32 has a rugged topography, which may indicate the trend may be from noise rather than distributed deformation.

Line 281 – From Figure 6, it looks like the peak profile measurement is 60.6 cm, not the maximum value of the moving mean, but this sentence says that's the peak of the moving mean. Check values, and please give average and maximum of both the measurements and the moving mean. Also, state if uncertainties are 1 or 2 sigma.

Response: The values discussed in this paragraph (lines 276-282) refers to the individual data points and not the weighted moving mean. We preferred to take the peak value was taken from the individual data points, while the average value is from the moving mean. If necessary, we can mention the maximum and average of both approaches.

Figure 7 – Explanation for B should include "LOS"; explanation for D should include "unwrapped". In panel D, is white on the colorbar set to 0? Because it seems like most of the deformation is occurring on the dashed fault and not on the primary fault, but that could be an artifact of the visualization and colorbar. For diverging colorbars, always make sure that 0 is white.

Response: This is duly noted. The captions are updated. The colors will be replaced with a sequential scale.

Figure 8 – Check that the flight and look directions are correct. They do not seem perpendicular to each other as they should be for ascending and descending scenes. Add north arrows and line legend to panels A and B. Consider replacing the rainbow colorbar (see comment for Figure 5)

Response: Thank you. We revisited the metadata of the involved Sentinel-1 frames and the rotations are valid per the look and flight directions of the specific dataset. As for the colors, they are replaced with a sequential color scale. North arrows are also added.

Figure 9 – What are the parameters for making the weighted moving mean? It seems like the moving mean should follow the two higher measurements at ~9 km more closely.

Response: The smoothing factor used here is 0.2.

Line 345 – delete "down" ✔

Figure 10 – Label panels D-H with the amount of displacement measured at each site. ✔

Figure 11 – The datapoints are difficult to see. Plot in a contrasting dark color such as blue or black. Please show uncertainty error bars for each measurement. The points at the right end of the rupture could be represented with a distribution and error bars rather than the slip distribution going steeply up in a very short distance, which is unrealistic. ✔

Line 371-372 – What figure is the "solid green line" in reference to?

Response: Thank you for pointing it out. This was meant to be "gray" instead of green, and it will be corrected.

Figure 12 – Field data points are difficult to see and lack uncertainty. Plot field data in darker color (blue or black) and with error bars. ✔

Lines 425-428 – "Our measured…field offset distribution" --> Surprising! The opposite might be expected. Short wavelength variability in field measurements is generally much larger than variability from optical image correlation (https://doi.org/10.1002/esp.5294 and references therein), and geomorphic and human modification of offset features would likely exacerbate this issue. Are there any other possibilities that could explain the excellent agreement between the field and optical displacement measurements?

Response: We appreciate the reviewer's comments. However, we stand by our findings and we cannot alter neither our field measurements nor the image correlation results. As for the variability of slip distribution, there are areas where variability is present and was discussed in the manuscript. We calculated a coefficient of variability (Cv) = 0.36 (Reitman et al, 2021), which shows that our data is well within the global range. Lastly, per the reviewer #3, our "*remote sensing results accurately capture ground displacements and provide greater along-strike coverage. The similarity between the image correlation results and field measurements is a good point rather than a bad point, and emphasizes the strength and usefulness of the method.*"

Line 427 – subject-verb agreement issue (ruptures…is) ✔

Line 432 – Delete "merely" and "results". ✔

Lines 471-473 – The 2-day lag between the earthquake and the first interferogram used to calculate "post-seismic" slip means that all estimates of post-seismic slip are minimums. This point could be made earlier and repeated when the magnitude of post-seismic slip is discussed.

Response: This is duly noted. We added "However, due to a two-day observation lag between the earthquake and the earliest Sentinel-1 pass, all our measurements herein are lower bound estimates." in the results section and mention it accordingly when appropriate.

Line 482 – This is a strike-slip fault, not normal or thrust. Please add appropriate references for strike-slip post-seismic deformation. ✔

Line 497 – Why is surface rupture length assumed to be symmetric around peak displacement? Are there studies to support this assumption? Though many strike-slip surface ruptures are roughly elliptical, they are often at least somewhat asymmetric (as is stated a few lines later)

Response: Due to the lack of submarine data, one of the approaches was to interpolate by symmetry. Nevertheless, we made sure to emphasize that the assumption should be taken with a grain of salt and provided other estimations of the rupture length.

Section 5.3.4 – It might be useful to also calculate Mo and Mw from rupture area, slip, and rigidity parameters (using a range for parameters that are unconstrained) to compare to the estimated Mw from the Wells and Coppersmith equations. They should be similar, but if they are very different that might indicate useful information about the rupture.

Response: Our earliest drafts included the other rupture parameters mentioned in this comment. However, since we can only access the government earthquake data to a limited extent, and that inversion is outside our scope, we opted to exclude the other rupture parameters in presenting the results. We can revisit this when we gain access to the raw earthquake data and upon the conduct of geodetic inversion in our next papers.

Line 522 – At least 18%! That's a minimum given the 2-day window lacking in the post-seismic insar time series stack. State that it is a minimum. ✔

Line 561 – Again, 18% and Mw 6.15 are minimums due to the 2-day delay. Must be stated that these are minimums. ✔

Lines 566-568 – Again, the opposite might be expected…that weathering and erosion create more variability in field measurements. What are other reasons this could be true?

Response: Thank you for the comment. However, these are the results we acquired and reviewer 3 noted the notion of our observations that is our results accurately capture the field movements.

Lines 582-584 – The final sentence is a nice sentiment, but how exactly this study improves resilience to hazards hasn't been addressed in the text, so it seems out of place in the conclusion. Consider either adding to the discussion how this work addresses hazard vulnerability or changing the final sentence.

Response: According to scientific writing guidelines (Bouchrika, 2021; Sacred Heart University, n.d.), a good paper writes about the larger purpose of the study and puts the objectives of the work into context, which future investigators may pursue. We have added the following in the conclusions: "The findings and the methods we used for this study in Masbate leads us and future scientists to further analyze the geometry, kinematics, mechanics, and dynamics of fault movement in the area for improved seismic hazard mitigation. Specifically, the comprehensive slip data and observed unusually long rupture lengths along the fault measured through the optical correlation method agree with previous characterizations by Besana and Ando (2005), indicating significant movement despite the presence of creep."

Reviewer 3

- The distinction between co-seismic and post-seismic deformation is a key aspect of this project, yet no truly purely coseismic imaging was possible. At best the remote sensing data include two days of afterslip (the period when that rate is presumably highest) and in general, the measurements are made on data that include months of afterslip. Care should be taken to very explicitly distinguish these results; multiple time windows might provide an opportunity to define the afterslip rate decay and thus estimate how much may have accumulated in the first two days, thus providing a better estimate of coseismic slip. Otherwise what you have is just total slip and a minimum estimate of postseismic slip, and it should be presented as such.

Response: Following the paper of Elliot et al (2020), we chose to address this comment by adding the words "may be inferred to represent the coseismic slip" whenever necessary in our manuscript. This addresses the lack of a "truly purely coseismic slip imaging". For the second point, we emphasized that the post-seismic measurements are lower bound estimates, in accordance with this comment and to a similar point raised by reviewer #2.

- In a couple places, the authors refer to a presumed slip barrier, but it is unclear what exactly this interpretation involves or implies or is based on, and given the legitimacy of scientific debate over rupture barriers/ boundaries, etc., these terms should only be introduced confidently with clear explanation. In this case, it is unclear to me how a gradually tapering slip distribution implicates a "slip barrier" as opposed to simply being a manifestation of a slip profile across a finite crack. The authors have not explored the possibility that it results from an increase in the depth at which slip is concentrated, with less reaching the surface the farther north it goes. These ideas should be discussed if you wish to invoke a "slip barrier" model/conclusion; otherwise, as it is only mentioned briefly twice and is not what the paper hinges on, it may be better simply omitted.

Response: Thank you and we agree that further research is needed to define this in the study area. In line with this, we will omit this point to avoid scientific inaccuracies.

For the specific comments, our responses are itemized below. Minor changes that do not need an exposition are marked with a check (✔) for simplicity.

3 - should the second instance of "displacement" in this sentence rather say "offset" as it is referring to the relative total displacement across the fault rather than the net displacement of either side, which is what the displacement fields show. Also can it be described with kinematics (i.e., left-lateral) as the postseismic offset is?

Response: Thank you for the comment and noticing. We replaced the particular word with "offset". We also included the movement sense of the fault as it provides an important context in the abstract.

3 - rather than "in Cataingan", the location of this asperity should be described relative to the fault rupture or epicenter; rather than "lone" one might instead say "single" as this result does not necessarily require that this represents the only asperity present on the fault. ✔

4 - "left lateral" is less jargon-y than "sinistral" and may make for a clearer alternative phrase; repeating comment regarding using "offset" instead of "displacement" when describing the relative displacement across the fault ✔

6 - Does tapering slip require a slip barrier? Any rupture will inherently have slip taper toward the crack tips; consider rephrasing this to remove "barrier" or insert text that explains what about the tapering slip suggests a barrier rather than just a rupture tip

Response: Thank you for the clarifications. We excluded the slip barrier concept.

7 - the rupture length has not yet been mentioned—have you derived it from the remote sensing data? If so, state it in an additional sentence in the abstract here to justify this statement. ✔

14 - what magnitude was the strong aftershock? The specific magnitude is relevant to understand whether it would be likely to produce further ground deformation or was below that threshold. ✔

17 - hypocenter, since the depth is listed ✔

21 - add a word to modify "butterfly" for clarity, such as "butterfly-shaped" ✔

22 - double-check the grammar of this sentence, perhaps a simple correction would be to move "descending track" to just before "line-of-sight" and add another "LOS" after ascending track or before "ground deformation" ✔

25 - spectral analysis “of seismic waveforms”* should be specified for clarity, as remote sensing and displacement fields can also involve spectral analysis ✔

27 - insert “compared to the slip predicted from global scaling relationships based on moment magnitude” ✔

45 - consider using “left-lateral” instead of “sinistral” ✔

47 - ASEAN should perhaps be spelled out on the first instance ✔

49 - why convex? difficult way to describe a 2D fault trace, as it must be relative to one side… would be concave on the other side…. Perhaps just “curved” would be better? or curving ✔

55 - segment “… of the PF…” ✔

63 - left lateral ✔

64 - “extensional” instead of “extension” ✔

66 - this sentence should be rewritten for clarity; firstly, the relationship of a fault and the stress regime is perhaps more two-way causal. The fault is present, so what is the stress regime altered *from*? It’s unclear exactly how you mean the stress state is influenced by the fault, perhaps “the SSF influences the local stress regime due to multiple bends and varying orientation”. “in between the junction of the SSF and PF” doesn’t make sense as a local for either of the possible subjects it refers to, which is also unclear—the SSF itself or the seismic profiles?

Response: Much appreciated. We modified it to “The SSF influences the local stress regime expressed by alternating extensive and compressive structures, as it interacts with the PF.”

84 - Survey, not Service; consider citation specifically of ANSS ComCat (https://earthquake.usgs.gov/data/comcat/) ✔

85 - epicenters, rather than centers ✔

90-91 - presumably these offsets were measured over a period that included the postseismic slip and so co- vs post-seismic slip magnitudes cannot be deconvolved; if accurate, this should be stated here to explain the subsequent conclusion that the excess slip/moment may have been the post-seismic component

Response: The mentioned measurements and statements on excess slip/moment were from the cited authors. Per their papers, the field investigation began on 18 Feb. 2003, three days after the 15 Feb. 2003 event. We agree that their measurements would have included some post-seismic component, but the papers themselves didn’t divulge on this aspect.

95 - hairline fractures… along the fault? Specify whether these were clearly tectonic or possibly had some other origin.

Response: The original authors suggested that these may indicate the presence of post-seismic slip. As for their location of occurrence, these fractures were located on an asphalt pavement along the southern terminus of the 2003 rupture. The possible origin of these were discussed in the succeeding sentences 96-97.

98 - rather than presumed to *cause* the subsequent earthquake, it would be more accurate in a current understanding to say “presumed to have reduced the time elapsed before the 2020 earthquake” ✔

106 - would be good convention to specify the spatial resolution of the PlanetLabs images. ✔

145-147 - this sentence could be rewritten for a bit more clarity. Something like, “the rasters chosen to extract profiles and calculate across-fault offsets were selected based on…” ✔

148 - specify when using “offset”: “pixel-offset” or “ground-offset”. Again I suggest using “displacement” to refer to absolute position change, and “offset” to refer to relative change across the fault (generally 2x displacement magnitudes). Thus the rasters you have are displacement rasters. They might also reasonably be called “pixel offset” rasters but note that offset of pixels from their pre-event positions is different from our domain-specific use of “offset” to refer to separation of features across the fault, which is generally double the value of displacement on either side and is a closer approximation of a different conceptual quantity: the fault slip.

Response: This is well noted. Thank you for the clarification. We updated the terminologies.

155 - "profiling" hasn't been methodologically defined yet here and may thus be an ambiguous term to use without that definition. You may wish to define it in a prior sentence, or explicitly say, "taking fault-perpendicular profiles (transects) of displacement data in order to measure the net fault-parallel offset". ✔

191 - presumably this means decorrelation in the coseismic interferogram due to higher magnitudes of deformation; you may wish to state that explicitly for clarity. ✔

193 - across the ground rupture instead of "of" the ground rupture ✔

224 - may be worth noting relative horizontal location uncertainties; I see USGS is 7km which may explain the discrepancy

Response: Thank you. We have gone over the reference files again and the uncertainty ranges are unavailable. However, the discrepancies aren't surprising knowing that PHIVOLCS would have a denser network of instruments for the study area.

257 - left lateral ✔

279 - describe the distribution along strike more accurately. "followed by an increase" is a temporal term... quantify slip values with numbers and specify relative location along strike using geographical terms like northwest or southeast. ✔

280 - not followed by, that's a time term; "south of the 60 cm high, displacement declines southward toward the shoreline" ✔

288 - left-lateral ✔

341 - are "subparallel" and "at a low angle" meant to contrast? They sound synonymous; perhaps omit "and at a low angle" or quantify the range of observed strikes in addition to the mean ✔

342 - "the northern terminus was observed" - does this mean the end of the crack tip? Because a measurement of 0.5 cm may be the last resolvable measurement of offset along the surface rupture but care should be taken to explain whether it simply provides a minimum northward constraint on where surface rupture is observed or whether the actual end of the crack is observed and no others can be discovered north of it ✔

345 - remove "down" ✔

350, 355 - "the rupture is continuous southward" is a more neutral description of its characteristics; "the rupture continued" would be how to describe its coseismic propagation, which presumably was northward from the hypocenter ✔

351 - can omit "for" ✔

379 - nothing you have measured is definitively coseismic, correct? In fact it is presumed all of these methods capture co- and post-seismic displacement without being able to fully discern them

Response: Similar to the approach of Elliot et al (2020) in the Tajikistan paper, we will add "may be inferred to represent the coseismic slip" whenever necessary in our manuscript.

383-384 - this sentence is meant to specifically apply to the postseismic interferograms, correct? Specify. ✔

384 - rather than qualitatively say "low", quantify the uncertainty ✔

391 - "higher"* uncertainties; again, quantify ✔

412 - inverse, rather than indirect ✔

431-432 - I do not believe that after all this your null hypothesis would be that the remote sensing results are "merely arbitrary". You would perhaps rather say that the agreement with field measurements "indicate that remote sensing results accurately capture ground displacements and provide greater along-strike coverage" ✔

451-454 - this is a slightly unclear sentence, as phrased. Rephrase to make clearer inferences and conclusions based specifically on well articulated observations. Consider changing to the active voice to help explain what observations lead to which inferences. "mostly vertical rupture propagation" doesn't make sense for a long strike-slip rupture; what does "a southwesterly component" mean or refer to? Is this sentence describing fault attitude? Subvertical with a slight southwest dip? The subject as written is "rupture propagation" which doesn't

make sense in this context.

Response: Thank you for this one. We will remove the rupture propagation idea and instead focus on the slip variability aspect to equate to fault complexity.

458 - how does the decreasing slip trend "mark" or indicate the presence of a specific barrier? Declining slip is expected for any secular crack; in fact an anomalous barrier may be expected to make slip decline more abruptly rather than smoothly? This conclusion bears further exploration and/or explanation in your text. Such a gradual, low magnitude and low gradient decline in slip actually suggests an anomalously low stress/strain concentration toward the northern end of the rupture…

Response: Thank you and we agree after further deliberation. This will no longer be included.

467 - The relationship described here seems inverted: those various processes are accommodated by postseismic deformation; postseismic deformation is caused by one or all of those processes.

Response: Thank you for pointing it out. We have restructured this section.

468 - I do not believe you have presented a coseismic slip distribution. At best you capture the earthquake and the first two days of postseismic (presumably the fastest!), or, more realistically since coseismic profiles were only derived from optical images, the first month (!) of afterslip. You say as much in the subsequent sentences. This sentence warrants a more in depth and logical explanation or walk-through. First you must define how you assess the component of the measured slip which is coseismic versus postseismic; then explain how they relate (proportionally and along strike), and then explain how that means or suggests that postseismic slip is stress driven (mustn't it be? Is this a novel conclusion?) Your subsequent paragraphs may begin to do this, but that means this sentence is preemptive.

Response: Similar to the responses above, we added that we infer our measurements to be coeval to the coseismic slip. In addition, its agreement with the field data effectively gives us a combined dataset for this particular period. The most we can add is the improved description of its relationship with the minimum post-seismic dataset, and we are grateful for the suggestions on describing such.

501 - "slip distributions" rather than "profiles" ✔

557 - "the peak coseismic offset is equivalent to a moment magnitude…" doesn't make sense or is stated incorrectly. Based on scaling relationships the peak slip is consistent with that magnitude. Also remember that you have no strictly coseismic displacement measurements

Response: We inserted "the preferred (or inferred) coseismic offset" to emphasize our inference.