

Reviewer's original comments are in black italic text

Manuscript Author's responses are below the relevant comment in blue text.

This is an important study that should be published. The authors review two recent publications that use InSAR (interferometric synthetic aperture radar) to assess coastal subsidence, and point out various discrepancies between the studies. This kind of rigorous comparison is long-overdue, as InSAR-related studies have proliferated, sometimes with conflicting results.

We thank the reviewer for the supportive comment.

I just have a few comments/suggestions the authors may wish to consider.

- 1. Figure 2 is a good summary of the paper. The best fit lines could be omitted, I don't think they add much, but I leave that to the authors' discretion. One statistic I would like to see here is the RMS difference between the 1 to 1 line and the combined InSAR data. Without any judgement as to which InSAR study is better, that single number gives a rough estimate of the utility of the InSAR approach in the various types of terrain. It's pretty clear the technique works well in developed areas (Figure 2e) given the presence of numerous strong scatterers, as the authors point out.*

We have added the RMS difference in Figure 2 as the reviewer suggested in the revised manuscript. The RMS differences across all land cover types are consistently close to $\sim 3 \text{ mm yr}^{-1}$.

- 2. Tropospheric error: the authors point out that errors related to tropospheric water vapor could bias the InSAR results. This is true, but there are ways around this problem – see for example Zebker et al. (IEEE Trans Geosci & RS, 2023). So it's worth considering in some detail how the two InSAR studies being assessed dealt with the problem. I agree that if not done carefully, this is certainly a potential source of error.*

While tropospheric delay is already discussed in the manuscript as a potential source of error, we agree that it is important to clarify how it is addressed in the two InSAR studies being compared. In the revised manuscript, we have expanded Table 1 and the corresponding text to explicitly describe the different strategies used to mitigate tropospheric effects.

- 3. Section 3.2: re: GNSS measurements restricted to VLM: Karegar et al (GRL, 2020) attempted to quantify this aspect of the problem. While their analysis was restricted to just a few sites, I believe their results support the point you are making here.*

This is a good suggestion. We have cited Karegar et al. (2020) and briefly discussed their findings in the context of GNSS-derived VLM limitations in the revised manuscript.

- 4. Section 3.2, exclusion of Texas sites: this is unfortunate, as the western part of the state is characterized by relatively dry landscape with limited vegetation, hence optimum for*

InSAR. It may be worth pointing out that petroleum-related activities can sometimes lead to uplift, in the case of fluid injection for fracking, waste water injection, or secondary oil recovery. It's possible to match injected volumes to observed uplift with physically plausible models, providing some verification. See for example Deng et al (JGR, 2020).

We agree that parts of Texas, particularly in the west, potentially provide favorable conditions for InSAR due to low vegetation and high coherence. We also acknowledge that petroleum-related activities can induce uplift signals that may be independently constrained (e.g., Deng et al., 2020). However, the objective of this study is to evaluate the consistency between two coastal InSAR products in regions where vertical land motion is dominated by relatively spatially coherent and well-characterized subsidence processes. Areas influenced by multiple driving mechanisms, such as fluid injection and extraction, introduce more complex and potentially localized deformation signals that would complicate this comparison. In addition, the W24 dataset does not cover western Texas, preventing a consistent evaluation between the two products in that region. For these reasons, we restrict our analysis to areas with predominantly subsidence-driven signals, while recognizing that Texas represents a valuable setting for future investigations of InSAR performance under more complex deformation regimes.

5. *Section 3.5 (GIA): Do the results of Zumberge et al (JGR, 2022) bear on this issue?*

Zumberge et al.'s results also suggested a larger role for GIA than previously assumed, so we have incorporated their findings in our manuscript.

6. I suggest adding a separate section describing how the two InSAR studies deal with the issue of reference sites (this is separate from reference frame). This is an obvious source of mismatch between the studies. Unlike GNSS, InSAR is a relative technique. To turn InSAR data into rates of VLM or SEC, it is necessary to make some assumptions concerning a stable area (VLM/SEC assumed to be zero) to which the rest of the results are referenced, or one or more GPS/GNSS sites whose rates of motion are well understood, and a least squares or similar adjustment is applied to minimize misfit between the reference sites and the larger data set. If the latter, then the difference between VLM and SEC, as well as the anchor depth of the GNSS monument, is certainly relevant.

We thank the reviewer for this important comment. We agree that differences in referencing strategies may contribute to the mismatch between the two InSAR products. In the revised manuscript, we have added a section noting that W24 applies a spatially uniform GNSS-based referencing, whereas O24 incorporates GNSS observations through a stochastic inversion framework, resulting in spatially varying constraints. We have clarified that these methodological differences can introduce both offsets and long-wavelength spatial discrepancies and should be considered when interpreting the results.