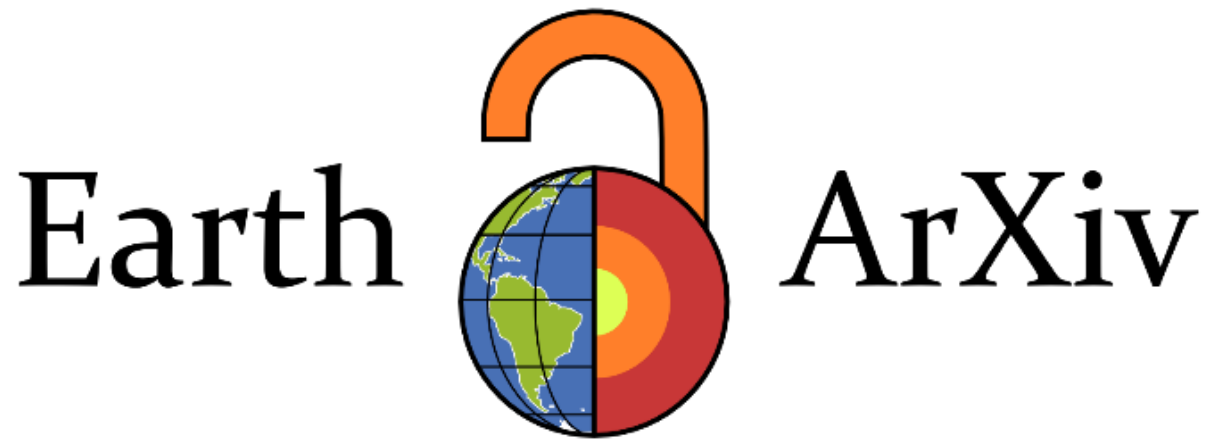




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Validating and Comparing Open InSAR Vertical Land Motion Products: Lessons from Three Cases

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Abstract

Open, satellite-derived vertical land motion (VLM) products are increasingly used to estimate relative sea-level rise and coastal hazard, often well beyond the studies that produced them. Researchers often infer the reliability of these VLM products from agreement with other products rather than validating them against independent observations. Researchers then sometimes attribute differences between products to processing without testing. Here, we examine three cases:

- 1) We independently processed Sentinel-1 data for 17 of the 48 coastal cities in a widely cited open dataset provided by Tay et al. (2022) over nearly the same observation window, and compared both products with Global Navigation Satellite System (GNSS) vertical velocities. After transforming both to IGS14, our rates agree with GNSS at $R^2 = 0.87$, whereas the transformed Tay et al. rates agree at $R^2 = 0.3$. Tay et al. velocity maps also show patterns consistent with known signatures of coregistration, phase-unwrapping, and pair-selection errors.
- 2) We show that a recent comparison of InSAR VLM products for coastal California by Govorcin et al. (2025) attributes their differences to Blackwell et al. (2020) to spatial filtering without controlling for observation epoch, using rate values that do not appear in the source, quantifying resolution, or accounting for localized horizontal motion. That untested attribution has since been cited as evidence of inconsistency among InSAR products.
- 3) Third, a comparison for New York City by Buzzanga et al. (2023) contrasts their products from different epochs with Ohenhen et al. (2023) to conclude that an Ohenhen et al. dataset lacks hot spots, although its time series show those hot spots as transient, and reprocessing with the Ohenhen et al. method over the same epoch as Buzzanga et al. recovers them.

In cases 2 and 3, disagreement was attributed to processing. Therefore, we propose a six-step protocol for validating and comparing VLM products, along with a community-maintained open archive to make these checks routine.

1. Introduction

Vertical land motion (VLM) is the component of relative sea-level rise that can vary over short distances, and in many coastal cities it exceeds the rate of climate-driven sea-level rise (Nicholls et al., 2021; Oelmann et al., 2026). Interferometric synthetic aperture radar (InSAR) is a widely used technique to map VLM at the meter-to-kilometer scales relevant to coastal planning (Bürgmann et al., 2000; Ohenhen et al., 2026; Ohenhen et al., 2024; Shirzaei et al., 2026a). InSAR measurements are, however, relative in space and time and thus must be tied to a global reference frame before they can be combined with sea-level observations or compared between locations (Shirzaei et al., 2021; Wöppelmann and Marcos, 2016).

InSAR-based VLM products are now widely distributed as open data. Examples include the European Ground Motion Service (EGMS) (Costantini et al., 2021), NASA's Advanced Rapid Imaging and Analysis (ARIA) and Observational Products for End-Users from Remote Sensing Analysis (OPERA) programs, as well as datasets released within individual studies (Blackwell et al., 2020; Govorcin et al., 2025; Tay et al., 2022). Some of these products, such as EGMS, have been independently evaluated against GNSS observations, yielding good agreement for coastal VLM in Europe (Minderhoud et al., 2025; Thiéblemont et al., 2024). However, many of these products, which feed into sea-level projections, exposure assessments, and planning documents, lack such validation or control for the reference frame.

Making datasets openly available comes with an important responsibility. When data are easy to access and reuse, any errors they contain can spread into subsequent analyses and products. The same concern applies to how we interpret and discuss these datasets. A possible explanation for a discrepancy, even when presented cautiously, may later be cited as an established finding. Avoiding these problems requires independent validation using observations collected over comparable time periods and methods that account for uncertainties in both datasets. Importantly, agreement between two products is useful for benchmarking, but it should not be mistaken for independent validation (Shirzaei et al., 2026a).

Here we examine three cases in which these distinctions were not maintained. Case 1 (Section 3) compares a widely cited 48-city dataset (Tay et al., 2022) with an independent reprocessing of 17 of its cities and with GNSS. Case 2 (Section 4) examines a comparison between two InSAR VLM products for coastal California, including Govorcin et al. (2025) and Blackwell et al. (2020). Case 3 (Section 5) examines a similar comparison for New York City, comprising Buzzanga et al. (2023) and Ohenhen et al. (2023). We next describe how errors can propagate (Section 6), propose a protocol for validating and comparing VLM products (Section 7), and outline a community-maintained open VLM archive (Section 8).

We note that the purpose of this study is not to rank products or research groups. Two of the products examined are our own (Blackwell et al., 2020; Ohenhen et al., 2023), and we hold them to the same standard. All analyses use public data, and any group can repeat every test we propose.

2. Data and Methods

2.1 InSAR processing

We randomly selected 17 of the 48 cities in Tay et al. (2022) and processed Sentinel-1 data for 2014–2020, consistent with Tay et al.'s study period. We used the Wavelet-based InSAR (WabInSAR) algorithm (Lee and Shirzaei, 2023; Shirzaei, 2013; Shirzaei and Bürgmann, 2012; Shirzaei and Bürgmann, 2013). We coregistered images using standard matching, then enhanced spectral diversity, keeping azimuth phase error below 1% (Shirzaei et al., 2017). We selected

interferometric pairs using dyadic downsampling and Delaunay triangulation, yielding triplets with short, medium, and long temporal baselines (Lee and Shirzaei, 2023). Interferograms were generated with GAMMA (Werner et al., 2000). We discarded distributed scatterers with coherence below 0.65 and persistent scatterers with amplitude dispersion above 0.3.

We applied phase unwrapping using a minimum-cost-flow algorithm designed for a sparse network of pixels (Costantini, 1998; Costantini and Rosen, 1999). We corrected orbital error (Shirzaei and Walter, 2011), topography-correlated atmospheric delay, and DEM error (Lee and Shirzaei, 2023; Shirzaei, 2013; Shirzaei and Bürgmann, 2012), and estimated line-of-sight (LOS) time series and rates by reweighted least squares. We projected the LOS rates to the vertical using local incidence angles, assuming deformation is predominantly vertical and that horizontal motions can be estimated and removed using a planar approximation. This assumption is reasonable for the subsidence settings examined but can bias vertical rates where horizontal motion is significant (Section 4.4). We tied vertical rates to IGS14 using the global GNSS-based VLM model (Hammond et al., 2021) and an affine transformation (Ohenhen et al., 2024).

2.2 Treatment of the Tay et al. (2022) product

Tay et al. (2022) report relative local land subsidence and thus their velocities referenced to a local point in each city rather than to a global frame. Referencing to a local point removes a constant offset from the velocity field (and, depending on processing, possibly a long-wavelength ramp), but it should not affect spatial variation within the city. Thus, an affine transformation, consisting of an offset and two planar gradients per city, can reasonably restore a global reference without altering the product's internal pattern (Ohenhen et al., 2024).

For both products, the VLM produced in this study and those reported by Tay et al., we estimated these parameters per city by fitting the InSAR rates to the GNSS-based VLM model of Hammond et al. (2021) sampled at the InSAR pixels (Ohenhen et al., 2024). Through visual inspection, we ensured that spatial features in the velocity maps (Section 3.2) are not altered by this step.

Note that since the Hammond et al. (2021) model is derived from GNSS, the validation in Section 3.1 is not fully independent of the transformation. However, because both products are transformed identically, the transformation cannot explain their differences, but it may inflate each product's absolute agreement.

2.3 GNSS and agreement statistics

We used 15 available GNSS vertical velocities in IGS14 from the Nevada Geodetic Laboratory, estimated with the MIDAS estimator (Blewitt et al., 2016) across 17 cities. InSAR rates were averaged within a 100 m radius of each station. For validation, we report the Pearson correlation coefficient (r), coefficient of determination (R^2), mean bias, and dispersion (standard deviation of differences).

3. Case 1: A Widely Cited Multi-City Open Dataset

Tay et al. (2022) derived relative local land subsidence velocities for the 48 largest coastal cities from Sentinel-1 data, processed with NASA-JPL's ARIA-SG/ISCE workflow and MintPy, and released the results openly. The dataset was the first self-consistent cross-city comparison of coastal subsidence and had been cited 162 times when we prepared this manuscript. Many citing studies use it as VLM in relative sea-level assessments, so its accuracy matters beyond the original study.

3.1 Comparison with GNSS

Figure 1A shows a bivariate plot comparing vertical rates measured at 15 GNSS stations with corresponding InSAR observations. In their original local reference, the Tay et al. rates show no agreement with GNSS, as expected for relative velocities. After transforming both products to IGS14, GNSS vertical velocities agree with our rates at $R^2 = 0.87$ and with the transformed Tay et al. rates at $R^2 = 0.3$ (Fig. 1A; Table 1). Because both products cover nearly the same period, differences in observation epoch cannot explain the gap, and because both are tied to IGS14 in the same way, neither can the reference frame.

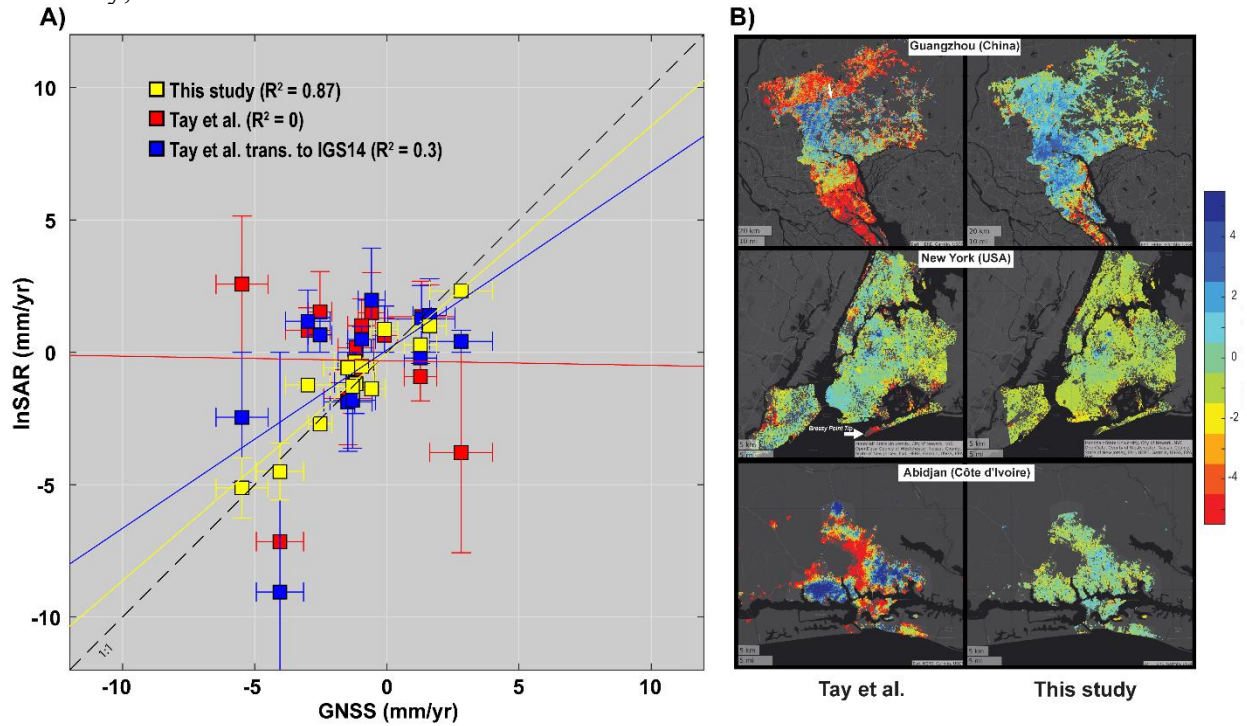


Figure 1. Validation of InSAR VLM rates with GNSS. **A)** Results from this study (in yellow) are compared with those of Tay et al. (in red) in their original reference frame (in red) and those transformed to IGS14 frame (in blue), and both are validated against GNSS vertical rates reported by Nevada Geodetic Lab. A coefficient of determination (R^2) is reported that quantifies the level of agreement between each dataset and GNSS observations. The bars indicate 1-sigma errors. **B)** Shown are examples of VLM rate maps reported by Tay et al. for the cities of Guangzhou, New York, and Abidjan, compared with those we obtained here. For more comparisons, see Supplementary Materials Figures S1-S14.

Table 1. Agreement between InSAR vertical rates and GNSS vertical velocities (IGS14) at 15 stations.

PRODUCT	REFERENCE FRAME	r	R^2	MEAN BIAS (MM/YR)	DISPERSION, 1σ (MM/YR)
This study	IGS14	0.93	0.87	-0.12	0.79
Tay et al. (2022), transformed	IGS14	0.53	0.28	-0.44	2.44
Tay et al. (2022), as published (context only)	Local	-0.07	0.01	-0.73	3.48

3.2 Features in the velocity maps

As noted, the velocity maps of the two products differ in ways that a reference-frame transformation cannot explain (Fig. 1B; Figs. S1–S14). We discuss possible causes of these differences below using 3 example cities. Firstly, the city of Guangzhou. The Tay et al. field contains parallel discontinuities perpendicular to the satellite flight direction. This pattern is consistent with residual azimuth misregistration between Sentinel-1 TOPS bursts (Shirzaei et al., 2017). Our product shows no such discontinuities. Secondly, New York City. The Tay et al. product shows subsidence faster than -5 mm/yr at Breezy Point Tip, which is absent from our product. The isolated, sharply bounded signal is consistent with a phase-unwrapping error. A later study of the New York metropolitan area (Buzzanga et al., 2023) also does not report this rapid subsidence. Thirdly, the city of Abidjan. The Tay et al. product shows widespread subsidence and uplift exceeding 5 mm/yr in absolute value, whereas our product shows rates mostly within ± 1 mm/yr with localized pockets of faster subsidence. Spatially coherent, large-amplitude signals of both signs are consistent with the bias that can arise when interferometric networks are dominated by short temporal baselines (Ansari et al., 2020; Lee and Shirzaei, 2023).

The remaining 14 cities (Figs. S1–S14) show features of the same types to varying degrees. Because Tay et al. applied the same workflow to all 48 cities, we caution users about potential error propagation from these VLM products.

3.3 Limitations

This comparison should be considered as a first check rather than a comprehensive validation. This is because GNSS coverage is limited to 15 stations, making the statistics sensitive to individual stations, and the GNSS velocities are MIDAS full-record rates rather than rates estimated over the InSAR window. Also, the attributions in Section 3.2 are solely based on the spatial characteristics of the signals, suggesting resemblance to known artifact types but have not been confirmed by reprocessing the Tay et al. interferograms.

4. Case 2: Comparing Products Across Different Epochs in Coastal California

Govorcin et al. (2025) present a high-resolution, GNSS-constrained Sentinel-1 VLM product for the California coast (2015–2023). They show that locally variable, nonlinear VLM can more than double regional relative sea-level rise by 2050 in parts of San Francisco and Los Angeles, and can widen 2050 projection uncertainty by up to 0.4 m. These findings are well supported, and we agree with their proposal to update the VLM component of projections as new observations arrive.

In a short comparative passage, they report that Blackwell et al. (2020) give subsidence rates "of up to -3 mm/year" at San Francisco International Airport (SFO), Foster City, and Bay Farm Island, where they observe rates exceeding -5 mm/yr. They also state that localized motion at Palos Verdes, Long Beach, the Santa Monica fault zone, Torrey Pines State Park, and Chula Vista is "not documented" by Blackwell et al., and propose post-processing filtering and interpolation as "one possible explanation." We argue below that the evidence presented does not support this attribution. We do not claim either product is wrong or that they should agree; we claim only that the cause of their differences remains unestablished.

4.1 Observation epoch is not controlled

Blackwell et al. (2020) jointly inverted ALOS (2007–2011) and Sentinel-1 (2014–2018) LOS rates with GNSS velocities spanning 1996–2018. Govorcin et al. (2025) use Sentinel-1 data from 2015–2023. The InSAR windows overlap only during ~2015–2018, and the ALOS contribution predates the Govorcin et al. record entirely. Logically, the effect of a signal present or accelerating in only one epoch is attenuated in a joint multi-epoch solution. This is the design goal of Blackwell et al.'s study: by increasing observation length, it aims to obtain a long-term rate least affected by transients.

Govorcin et al. show in their results that several comparison sites are nonstationary. They report trend shifts at Long Beach injection and extraction wells (February 2021), slowing subsidence at Chula Vista (October 2019), and elevated temporal variability (3–5 mm/yr) over the Santa Clara, Santa Ana, and Chula Vista aquifers. Additionally, landslide velocities at Palos Verdes respond to precipitation (Handwerger et al., 2022) and the Torrey Pines signal is associated with a cliff collapse in January 2023. At these sites, different rates are the expected outcome of Govorcin et al.'s own framework rather than evidence of a processing deficiency in Blackwell et al.'s study. Their argument is even weaker at the San Francisco Bay fill sites, where compaction is expected to be quasi-steady. At that site, the central issue is how rates were attributed (Section 4.2).

In our recent work, we reported a similar epoch effect that may have skewed the discussion of products agreement. On the U.S. Gulf Coast, GNSS vertical velocities at 88 stations estimated over 2007–2020 and 2017–2020 differed by -0.3 mm/yr on average but by 3.7 mm/yr at the 95th percentile, and validating InSAR products from one epoch against GNSS rate over the other epoch increased residual dispersion by 19–22% (Shirzaei et al., 2026b), comparable to the values at issue in Govorcin et al.

4.2 The reported comparison value cannot be traced to the cited source

Blackwell et al. (2020) contains no statement of "up to -3 mm/year" at the Bay sites. In fact, it reports that the Bay perimeter is subsiding at rates reaching -5.9 mm/yr, that subsidence exceeds -5 mm/yr in the Bay Area, and that SFO is subsiding faster than -2.0 mm/yr, a lower bound. No rates are given for Foster City or Bay Farm Island. At the Bay perimeter scale, the two studies' reported maxima are therefore similar. If the -3 mm/yr figure was extracted from the dataset, the location, averaging footprint, and reference frame should have been stated. Interestingly, footprint dependence is evident within Govorcin et al. itself, which describes the same sites as exceeding -5 mm/yr in the comparison and as subsiding by more than 10 mm/yr in the projection analysis.

4.3 The filtering explanation is not quantified

The two products have comparable pixel spacing (~ 70 – 100 m for Blackwell et al. and 90 m for Govorcin et al.), and both involve interpolation and filtering, although in different ways. Blackwell et al. used a temporal wavelet filter to remove variations with periods shorter than one year. This does not introduce spatial smoothing. They also kriged Sentinel-1 LOS and GNSS horizontal velocities onto ALOS pixels. Govorcin et al., in contrast, interpolated GNSS observations onto a 25 km grid using least-squares collocation, applied a 25 km low-pass filter to the calibration surface, and interpolated gaps in the data. To determine whether filtering explains the observed differences, the effective spatial resolution of each product would need to be evaluated directly, for example through spectral or variogram analysis. No such analysis was

performed for either product. Moreover, describing certain features as "not documented" does not establish that they are absent from the data. Their presence or absence in the Blackwell et al. product can be checked directly.

Another possible explanation that was not examined is the difference in long-wavelength calibration. Govorcin et al. used a GNSS-based calibration model that excludes stations affected by localized motion or vertical rates exceeding 3 mm/yr. This reduces GNSS constraints precisely in the coastal areas experiencing the greatest subsidence. Blackwell et al., by contrast, used individual station velocities and an affine transformation. Importantly, Li et al. (2026) describe the differences over the San Francisco Peninsula as largely regional in character. Such broad spatial differences are unlikely to result from pixel-scale filtering alone and may instead reflect differences in calibration or reference-frame treatment. These possibilities should be investigated before attributing the discrepancies to spatial filtering.

4.4 Localized horizontal motion affects the benchmark features

Both products use a smoothly varying GNSS-derived field to account for north-south motion, which near-polar InSAR observations aren't very good at detecting. Because the ascending and descending LOS vectors have north-south components in the same direction, errors in estimating that north-south motion can affect the calculated vertical velocity. When we merge the two different viewing angles, the east-west movements tend to cancel each other out, but if there's an error in the north-south velocity (let's call it δv_N), it creates a vertical bias of about $\delta v_U \approx (\bar{u}_N/\bar{u}_U) \cdot \delta v_N$, where $\bar{u}$ are LOS unit vectors (Hanssen, 2001). Based on the values from Govorcin et al. (with $\bar{u}_N$ around 0.11 and $\bar{u}_U$ around 0.78), this results in δv_U being about 0.14 times δv_N . In areas prone to landslides or coastal erosion, where horizontal movement can be several centimeters each year, this vertical velocity bias can add up to several millimeters per year. Three of the five sites Govorcin et al. point out, i.e., Palos Verdes, Torrey Pines, and the Santa Monica fault zone, are in these kinds of areas. This suggests that how north-south motion is handled could explain some of the reported discrepancies, making these locations less ideal for isolating the effects of spatial resolution.

4.5 Benchmarking is not validation

Govorcin et al. support their higher rates as consistent with Shirzaei and Bürgmann (2018), Xu et al. (2021), and Lee and Shirzaei (2023). None of these studies align with Blackwell et al.'s findings. Looking at the GNSS statistics, we see some differences: Blackwell et al. reported a standard deviation of 0.98 mm/yr across 159 stations, while Govorcin et al. reported a root mean square error of 1.8 mm/yr from 170 stations. Although these metrics aren't directly comparable, they don't support the idea that Blackwell et al.'s work is less reliable. A key point from Govorcin et al.'s research is the notion that VLM is not steady over time. This means that using stationary historical rates for predictions can lead us astray. If we apply this logic consistently, we should treat any differences in rates between the 2007–2018 product and the 2015–2023 product as potential signs of real geophysical changes. Only after ruling out this possibility should we attribute those differences to how the data was processed.

4.6 A test that would resolve the question

All of the following can be done with public data: (i) estimate GNSS vertical velocities at coastal California stations over 2007–2018 and 2015–2023 and report the distribution of differences; (ii) validate both products against the same GNSS stations over each product's own window and over the other's, reporting bias and dispersion separately; (iii) extract both products at the eight sites

with identical footprints after reference-frame harmonization, using the Blackwell et al. Sentinel-1-period velocities against Govorcin et al. for 2015–2018 where a common window is needed; and (iv) compare spatial power spectra of both products over a common area. We invite the authors of both products to carry out these tests with us.

5. Case 3: Comparing Products Across Epochs in New York City

Buzzanga et al. (2025) mapped VLM across the New York City metropolitan area from Sentinel-1 data spanning May 2016 to March 2023, at 30 m resolution. Two GNSS stations tie the map to ITRF2014, and where GNSS and InSAR overlap, they agree within 0.7 mm/yr. Their median rate is -1.6 mm/yr, consistent with glacial isostatic adjustment. More interesting are the local anomalies: runway 13/31 at LaGuardia Airport subsides at -3.7 mm/yr, while Newtown Creek and Woodside are rising. For coastal planning in New York, this level of detail matters.

Comparing their results with earlier work is an example of Case 2. They use the open VLM dataset from Ohenhen et al. (2023) as their reference point, which performed a joint inversion of ALOS (2007–2011), Sentinel-1 (2015–2020), and GNSS data. Buzzanga et al. concede that the two products are “not directly comparable,” given their different periods and methods, and note that both yield the same median of -1.6 mm/yr. That said, they describe the earlier dataset as nearly uniform in space (99.7% of estimates under 0.5 mm/yr) and take this as evidence that it captures no subsidence or uplift hot spots.

5.1 Observation epoch

The two InSAR records overlap only from 2016 to 2020, and the ALOS part of Ohenhen et al. ends years before Buzzanga et al.’s record begins. Their own time series also show that the uplift hot spots are short-lived. Woodside rose at 6.9 mm/yr between May 2016 and December 2019 and has not moved since. At Newtown Creek, uplift slowed after 2019, from 2.6 to 1.8 mm/yr. A joint solution spanning 2007 to 2020 would be expected to show such signals only weakly, if at all (Section 4.1). So the passage first concedes that the products cannot be compared directly, then treats the missing hot spots as a real difference between them.

5.2 An epoch-matched check

A matched-epoch comparison is now possible. Daud et al. (2026) processed 215 ascending Sentinel-1 scenes over New York City (May 2016 – May 2024, nearly the Buzzanga et al. window) with the same multitemporal InSAR approach used for Ohenhen et al. (2023) (Lee and Shirzaei, 2023; Shirzaei, 2013). Pixel spacing is ~ 15 m; the reference is IGS14 and RMSE against six GNSS stations is 0.2 mm/yr. This product shows LaGuardia runway 13/31 subsiding at more than -5 mm/yr; JFK subsides too, and several sites in Queens and Brooklyn show short-lived uplift. At one site, the ground rose ~ 30 mm during 2018 and then began sinking. Shirzaei (2023), using the same approach on an earlier Sentinel-1 stack, obtained much the same pattern.

So the hot spots are there when our method is run over the right years. Nothing in the processing removes them. What differs in Ohenhen et al. (2023) is the 2007–2020 multi-epoch design, and that is the more likely reason the features are muted. We should be clear that both products are ours: this isolates the method; it does not validate it independently, and an independent product covering 2016–2024 would be a better test.

5.3 Resolution and validation

Resolutions also differ, ~30 m against ~90 m. Some of the difference in resolved detail may come from that, though it has not been tested (Section 4.3). No common-window GNSS validation was done for either product as part of the comparison, and the study area has only nine GNSS stations. At the scale of a single hot spot, that is thin support in any case. Both products convert LOS rates to vertical. Buzzanga et al. work from one ascending track and acknowledge that local horizontal motion, where it exists, could bias their rates.

In the same passage, Buzzanga et al. describe the Tay et al. (2022) product for New York as noisier than theirs, with rates ranging from -18.9 to 7.3 mm/yr and gaps over many of the hot spots. They reached this conclusion independently, and it agrees with Case 1.

5.4 A test that would resolve the question

Daud et al. (2026) address the central question, but the remaining tests in Section 4.6 still apply: (i) estimate GNSS vertical velocities at New York stations over 2007–2020 and 2016–2023 and compare them; (ii) extract the Buzzanga et al. and Daud et al. velocities at the hot spots with identical footprints in a common frame; and (iii) compare spatial power spectra of the products over a common area.

6. Synthesis: How Errors Propagate

The three cases share one problem: agreement or disagreement between products is treated as evidence about accuracy, without validation against independent observations over a matched window. Table 2 lists the specific issues in each case and the protocol step (Section 7) that addresses each.

Table 2. Issues identified in the three cases and the protocol step that addresses each.

Issue	Case 1 (Tay et al.)	Case 2 (Govorcin vs. Blackwell)	Case 3 (Buzzanga vs. Ohenhen)	Protocol step
Epoch not controlled	Controlled (near-identical windows)	Windows overlap ~2015–2018 only; sites nonstationary	Windows overlap 2016–2020 only; hot spots transient	1, 2
Reference frame not harmonized	Relative product reused as VLM	Differing frames and calibration	No common-frame extraction reported	3
Values or sources not as cited	—	"Up to -3 mm/yr" not stated in source	Variability metric undefined	3
Processing or resolution attributed without test	Artifacts not reported in source	Filtering explanation untested	Absence of hot spots presented as a product difference; recovered by same-epoch reprocessing	4
Horizontal motion ignored	Vertical-only projection (both products)	Landslide, cliff, and near-fault benchmarks	Single-geometry vertical projection	5
Benchmarking treated as validation	No multi-city GNSS validation reported	"Consistent with other studies"; later cited as evidence of InSAR unreliability	No common-window GNSS validation of either product	2, 6

Errors travel in two ways. One is through the data themselves: an open dataset gets reused in downstream sea-level and exposure assessments, and each one inherits its artifacts (Case 1). The other is through statements, where one paper's tentative explanation is cited as a finding in the

next. Case 2 illustrates this, and Case 3 shows this kind of comparison is not a one-off. Govorcina et al. (2025) offered filtering as “one possible explanation.” Li et al. (2026) then cited it in support of a broad recommendation to treat InSAR rates below 5 mm/yr with utmost caution, and that recommendation turns out to rest largely on how the comparison was built (Shirzaei et al., 2026b).

Open distribution amplifies both routes. One practice would stop both. When a dataset is reused quantitatively and independent observations exist, it should be validated over its own time window; when no such observations exist, the limitation should be said plainly. Do not blame any discrepancy between two datasets on either dataset until a test rules out differences in epoch, reference frame, footprint, and viewing geometry.

7. A Protocol for Validating and Comparing VLM Products

We propose six steps, extending Shirzaei et al. (2026b). Steps 1–2 apply to any product before it is reused; steps 3–6 apply before a difference between products is attributed to processing. All six can be carried out with public data.

1. **Epoch test.** Estimate GNSS vertical velocities over each product’s observation window, and report the distribution of differences between the windows, both overall and at the sites discussed. If these differences match the discrepancy between products, control the epoch before making any attribution.
2. **Window-matched validation.** Validate each product against GNSS over its own window. Use only stations that played no role in reference-frame tying or calibration, including indirect roles through a GNSS-based model. Where that is not possible, use leave-one-out tests. Report bias and dispersion separately, together with Bland–Altman limits of agreement and Lin’s concordance correlation coefficient. When two products are compared, also validate each over the other’s window.
3. **Harmonized, documented extraction.** Express both products in a common reference frame. Quote values as stated in the source and identify each as a peak, mean, lower bound, or upper bound. For values extracted from the data, state the location, footprint, and frame, and use identical footprints for both products.
4. **Resolution test.** Before attributing a missing feature to filtering, compare spatial power spectra or variograms and valid-pixel densities over common areas. Show that the feature would be detectable in the product where it is missing. Check the dataset itself, not only the paper’s description of it.
5. **Horizontal-motion check.** Do not use features on landslides, eroding cliffs, or near active faults as resolution benchmarks unless their horizontal motion is independently constrained. Report the vertical bias that plausible unmodeled north–south motion would introduce.
6. **Calibration sensitivity.** Repeat the long-wavelength calibration with alternative GNSS inputs, including fast-moving coastal stations, and report how rates at the sites of interest change.

Only differences that persist after these steps can reasonably be attributed to processing, and attributions that have not been tested should be presented as such. We also recommend that dataset releases state the observation window, reference frame, calibration strategy, and per-pixel uncertainty, and include a GNSS validation table, so that any user can carry out steps 1–3.

8. Toward a Community Open VLM Archive

The protocol in Section 7 is laborious because each group must assemble datasets, harmonize frames, and manually gather GNSS. A community-maintained archive would make it routine. We propose an open-science framework for VLM data archives built on fair funding, high data quality, and inclusive participation (Fig. 2). Data should be shared in standardized formats with rigorous metadata, including transparent reporting of methods and uncertainty and clear guidance on interpretation, to limit misuse. Quality control should combine open peer review, cross-disciplinary validation, and regular audits, which also reduce conflicts of interest. Licensing should balance open access with ethical use and attribution, particularly for sensitive data. Research agendas should prioritize underserved regions, fundamental science, and community-driven projects, and training in VLM data literacy is needed to broaden global participation. Recognize contributions of all kinds, and make both data use and funder influence transparent.

Funding concentration is a particular threat to these principles. When a few institutions control most research funding, priorities follow their interests. Less mainstream questions go unexplored, short-term, high-yield projects are favored over fundamental or socially important work, and rigid criteria discourage interdisciplinary approaches. Funders with commercial interests may also push for proprietary outcomes and restricted data access. Community-led efforts, often best positioned to address local or emerging problems, rarely receive support from these streams.

Two technical developments would advance this framework. The first is robust, efficient algorithms that automate InSAR time-series generation at a quality comparable to manually processed products. The second is a community VLM model, to which groups contribute their products in a common format for shared validation and benchmarking, including uncertainties. Beyond establishing consensus on contemporary VLM rates, such a model would allow systematic integration of time-variable VLM into projections of relative sea-level rise, at scales from individual cities to the globe.

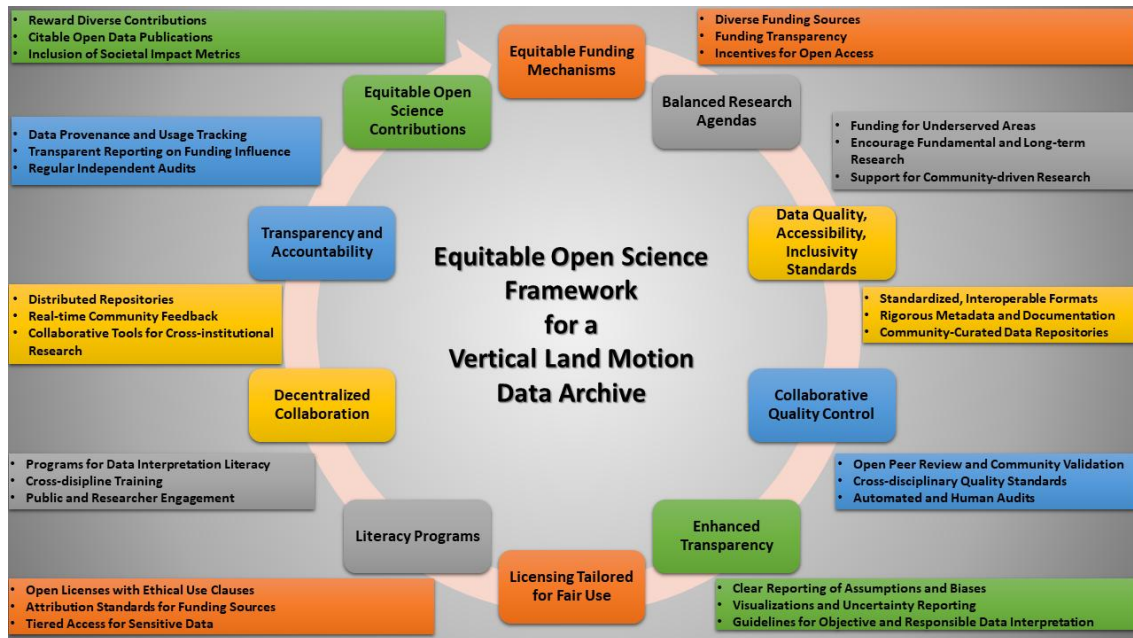


Figure 2. An equitable open science framework that ensures universal access to high-quality vertical land motion (VLM) data while minimizing misinterpretation and error propagation.

9. Conclusions

Open InSAR VLM products are among the most consequential datasets in coastal science, and their openness gives them reach. That reach becomes a liability when users reuse or compare products without validation. In Case 1, an openly released multi-city dataset agrees with GNSS at $R^2 = 0.3$ when compared over nearly the same epoch and in a common reference frame, against 0.87 for an independent reprocessing, and its velocity maps contain features consistent with processing artifacts. In Case 2, a discrepancy between two California products was attributed to filtering without controlling for epoch, source values, resolution, or horizontal motion, and was then cited as evidence against InSAR more broadly. In Case 3, a New York City product was contrasted with an earlier dataset from a different epoch, although the hot spots at issue are transient and appear when the earlier method is applied to the same epoch.

None of these problems needs new technology to fix. Each is addressed by validating products against independent observations (e.g., GNSS) over their own windows, harmonizing reference frames and footprints before comparison, and testing explanations before publishing. A community archive that runs these checks automatically would make good practice the default, and would let users of open VLM data rely on it with the confidence its importance warrants.

Data Availability

The InSAR VLM rates and uncertainties generated in this study are available at <https://doi.org/10.7294/25864435>. The Tay et al. (2022) data are available at <https://doi.org/10.21979/N9/GPVX0F>. The Govorcin et al. (2025) product is available at <https://doi.org/10.5281/zenodo.11154177>. The Buzzanga et al. (2023) product is available at <https://doi.org/10.5061/dryad.cz8w9gj85>, and the Ohenhen et al. (2023) product is available at <https://doi.org/10.7294/19350959.v3>. GNSS data are from the Nevada Geodetic Laboratory (<http://geodesy.unr.edu/>). Sentinel-1 data were obtained from the Alaska Satellite Facility.

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Competing Interests

The author declares no competing interests.

Author Contributions

MS conceived the study, developed the codes, processed the SAR data, and wrote the manuscript.

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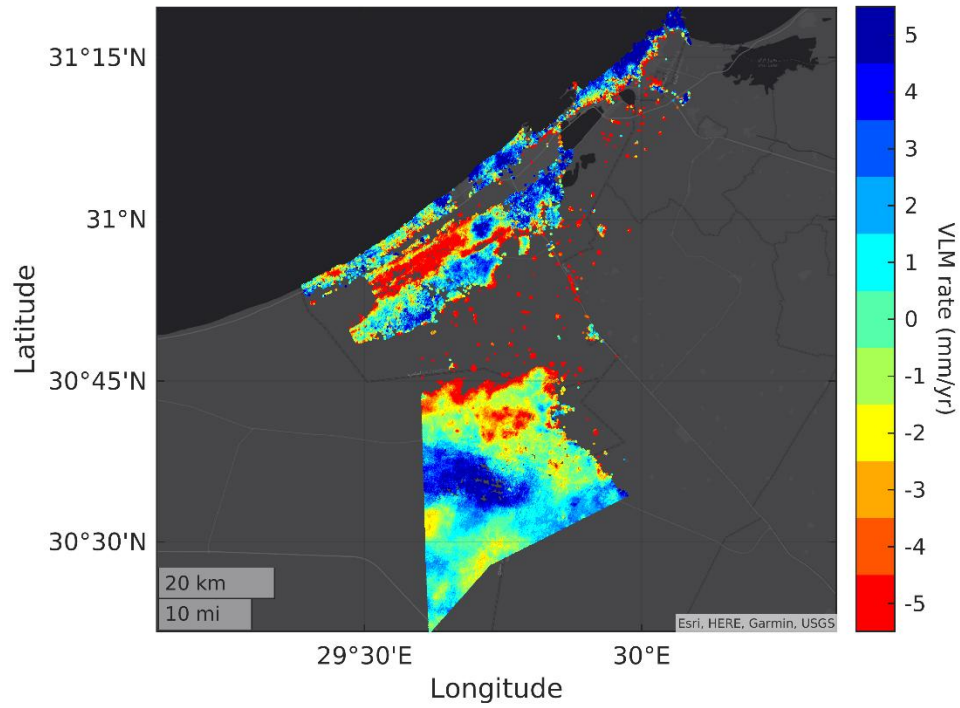
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Supplementary Online Materials

Supplementary Online Figures S1-S14

A. Tay et al.



B. This study

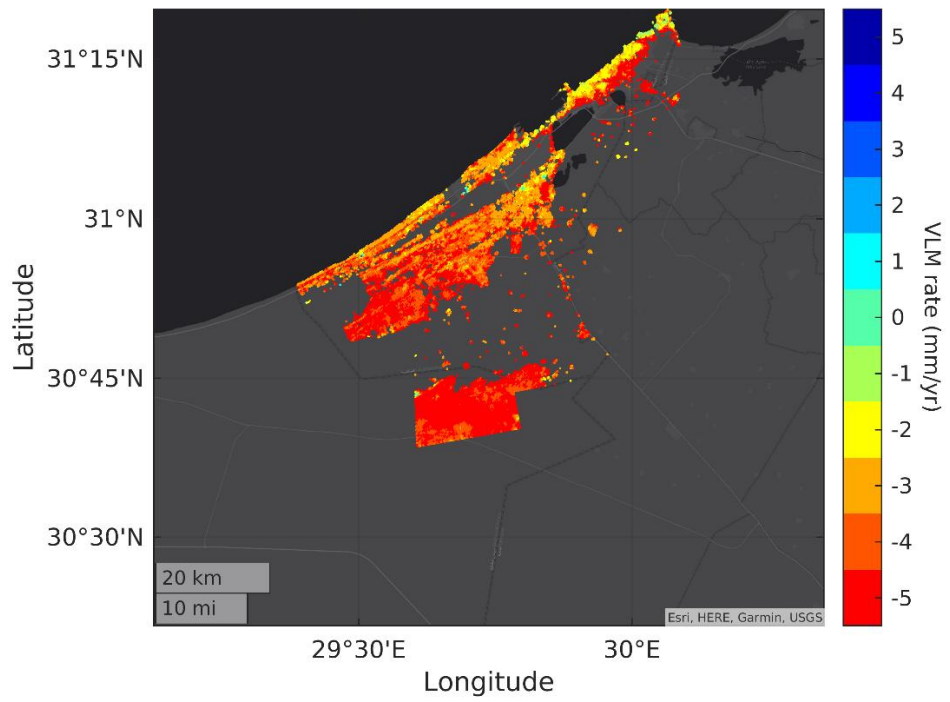
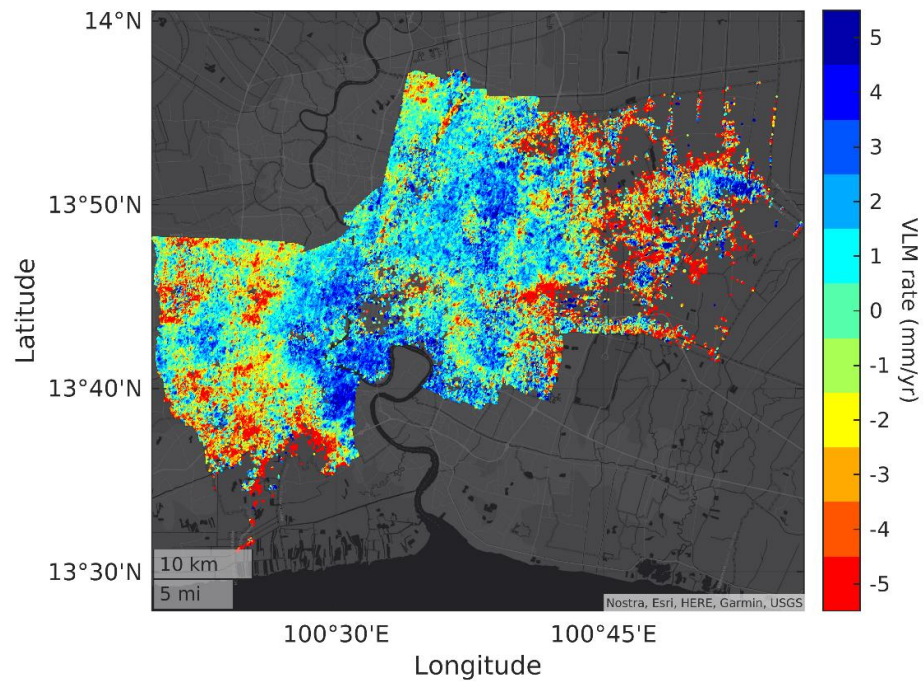


Figure S1. Alexandria, Egypt. VLM rate distribution comparing results from Tay et al. with those from this study in the IGS14 reference frame.

A. Tay et al.



B. This study

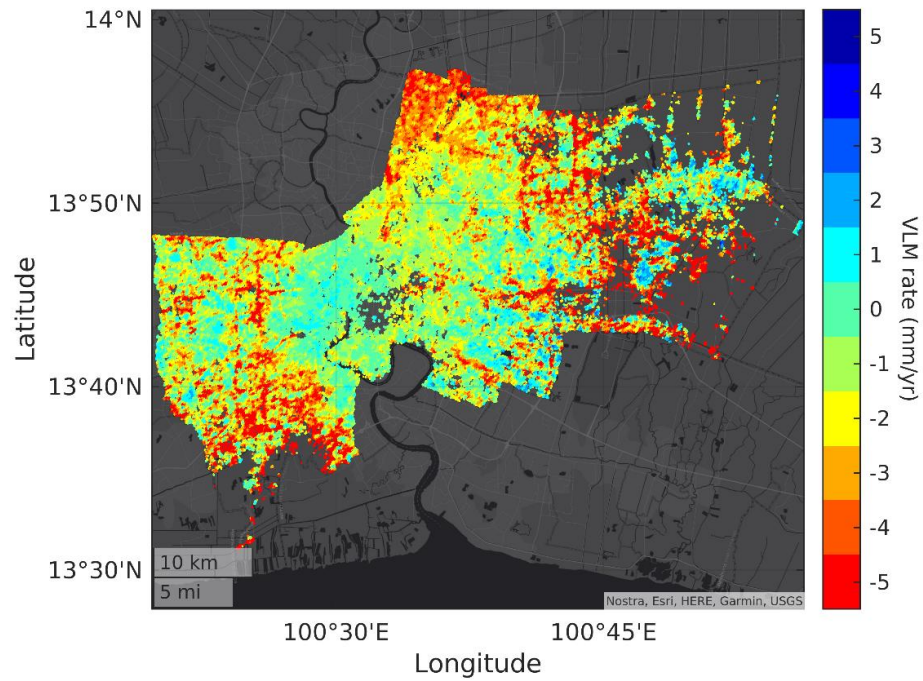
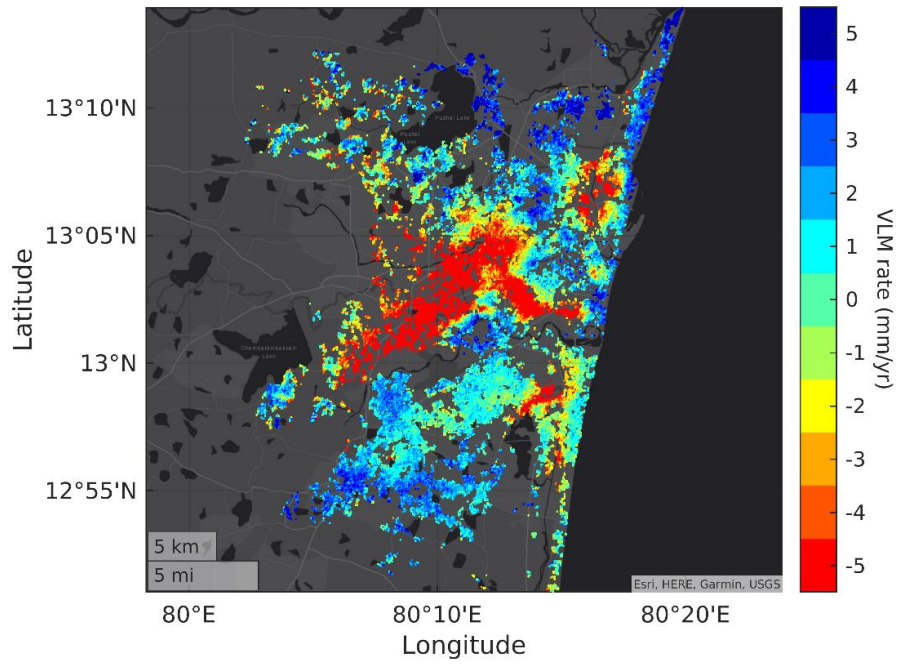


Figure S2. Bangkok, Thailand. VLM rate distribution comparing results from Tay et al. with those from this study in the IGS14 reference frame.

A. Tay et al.



B. This study

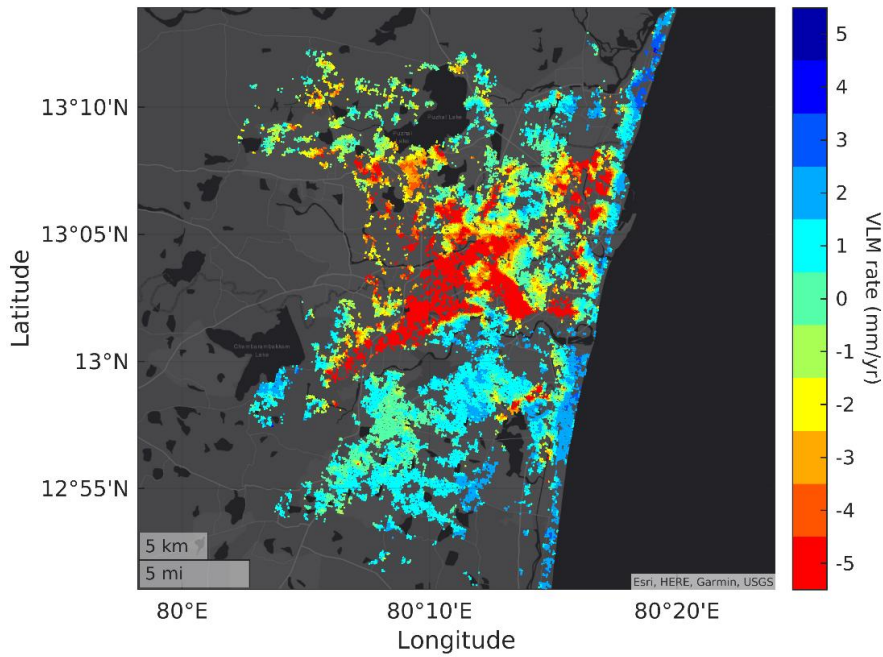
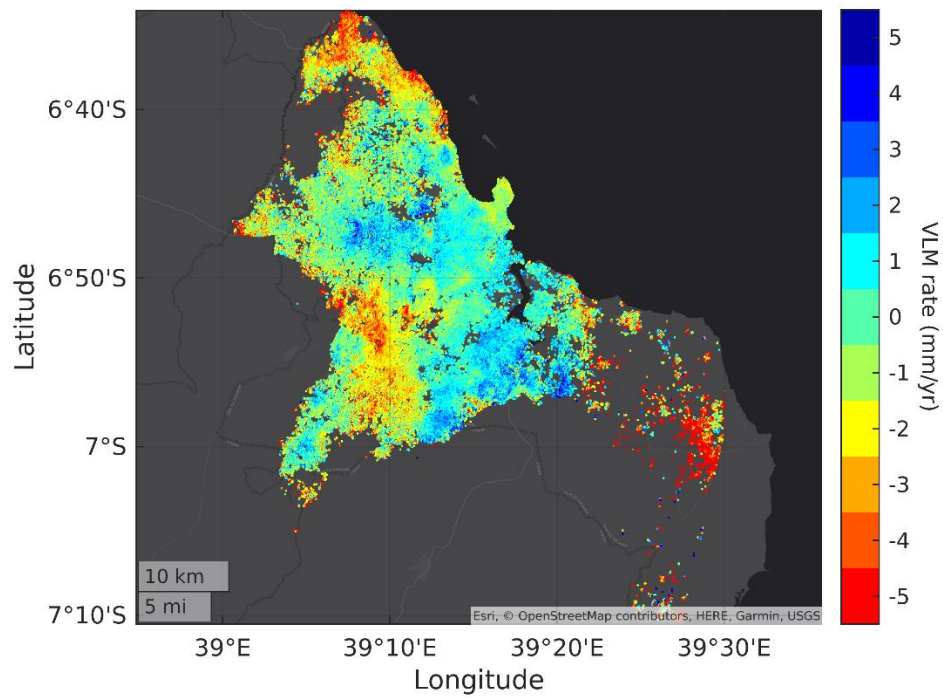


Figure S3. Chennai, India. VLM rate distribution comparing results from Tay et al. with those from this study in the IGS14 reference frame.

A. Tay et al.



B. This study

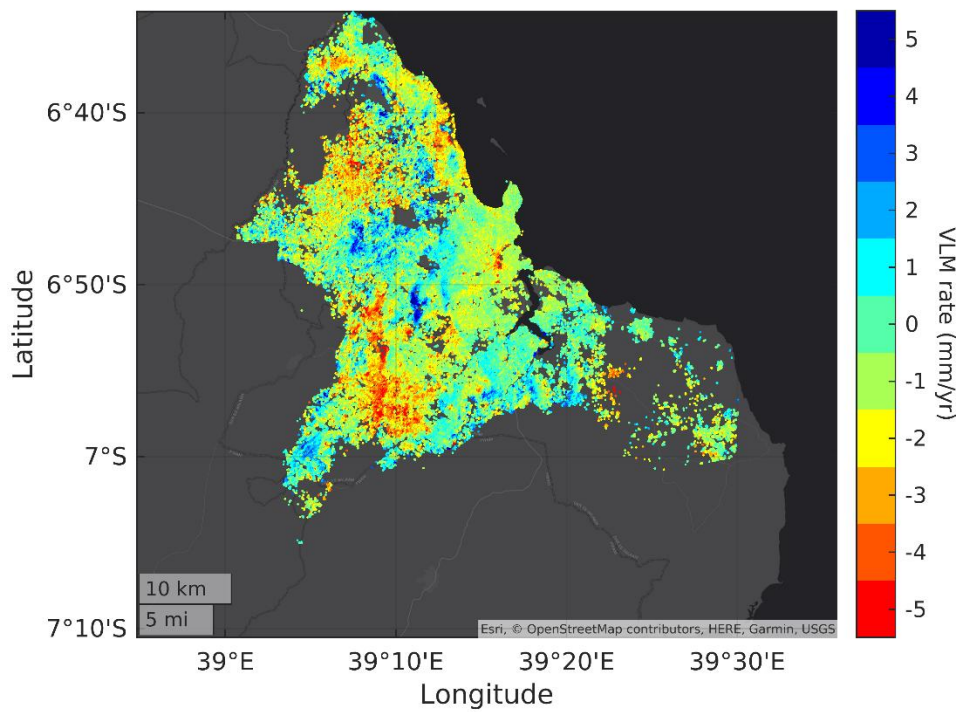
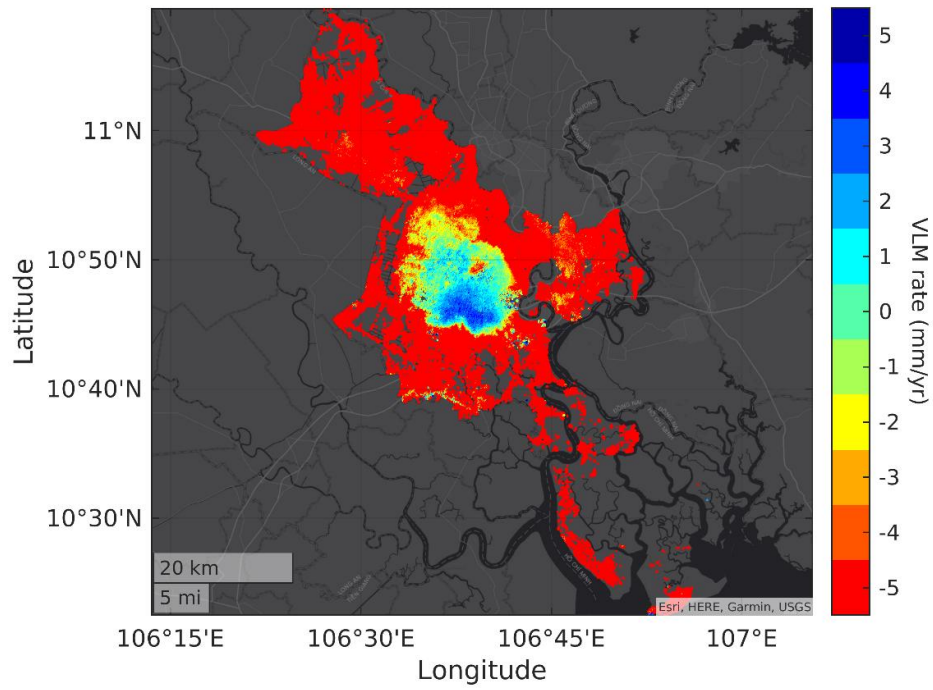


Figure S4. Dar es Salaam, Tanzania. VLM rate distribution comparing results from Tay et al. with those from this study in the IGS14 reference frame.

A. Tay et al.



B. This study

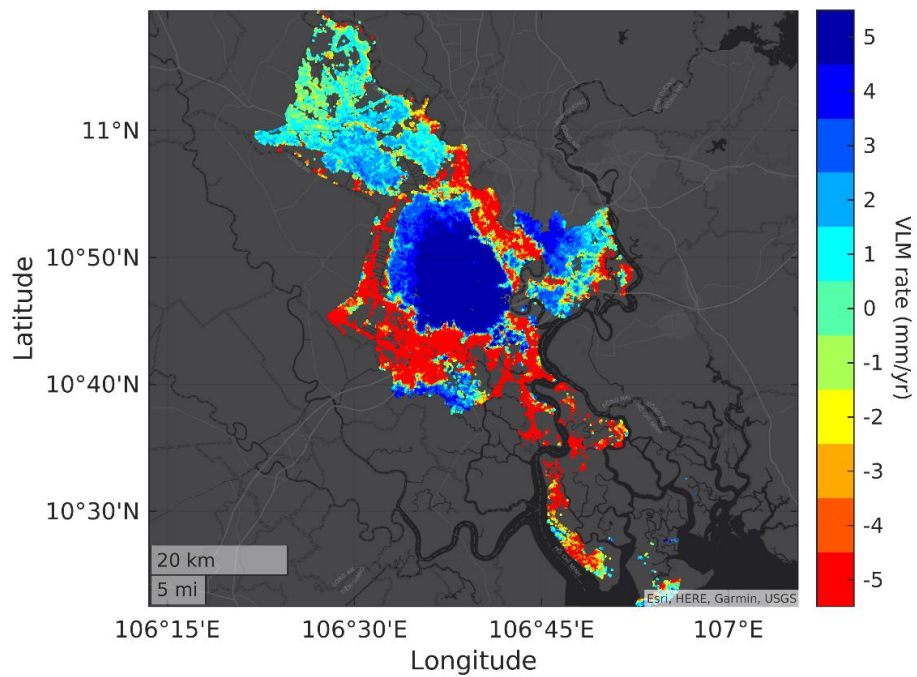
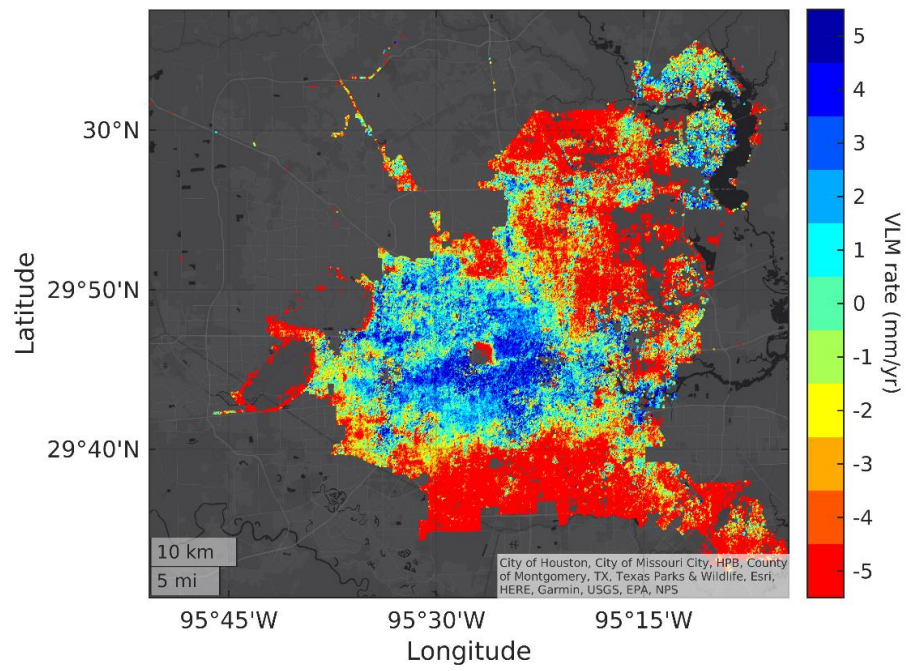


Figure S5. Ho Chi Minh, Vietnam. VLM rate distribution comparing results from Tay et al. with those from this study in the IGS14 reference frame.

A. Tay et al.



B. This study

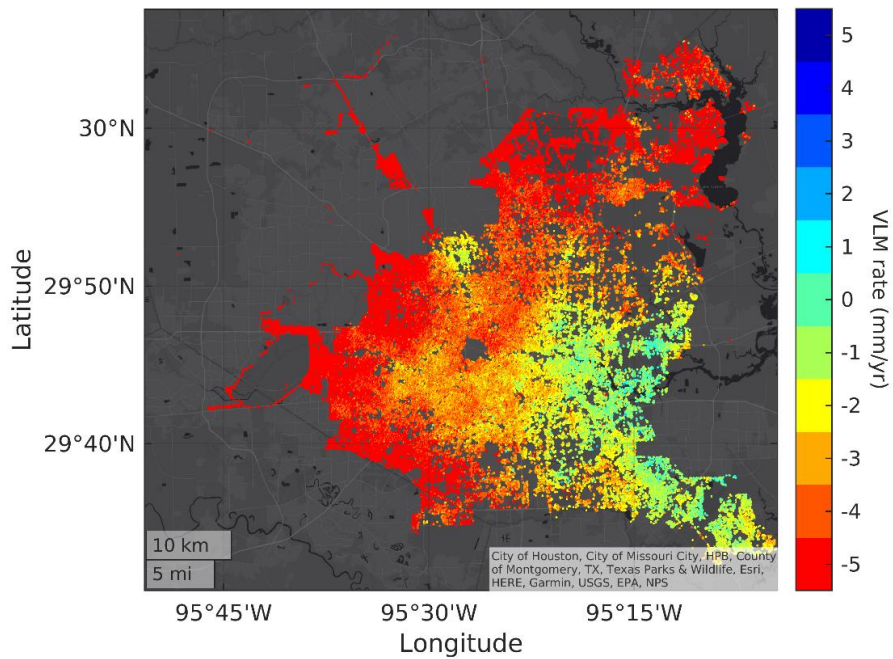
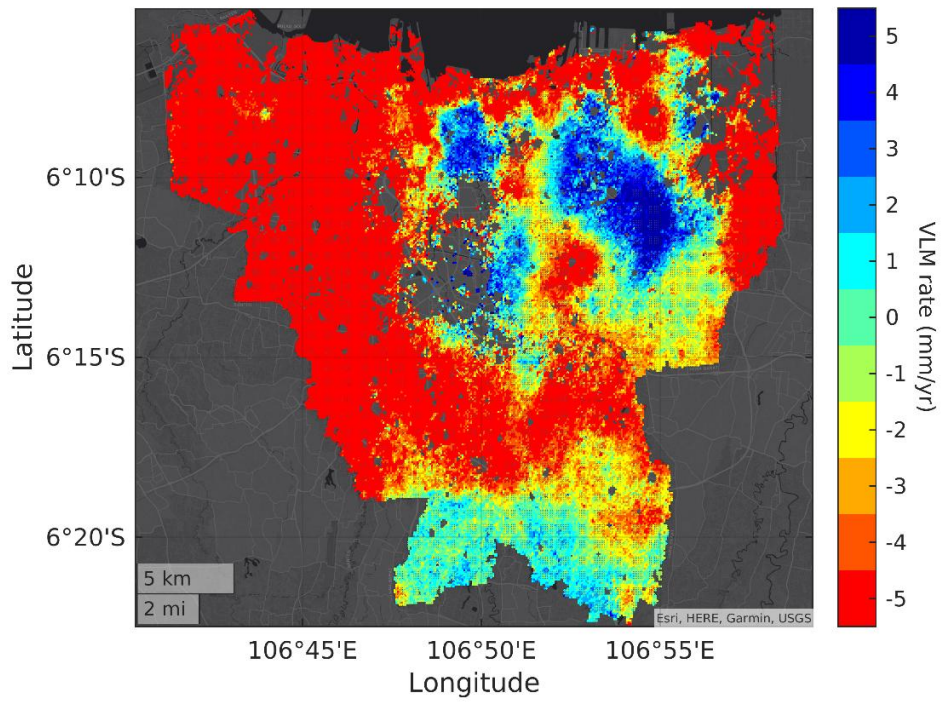


Figure S6. Houston, USA. VLM rate distribution comparing results from Tay et al. with those from this study in the IGS14 reference frame.

A. Tay et al.



B. This study

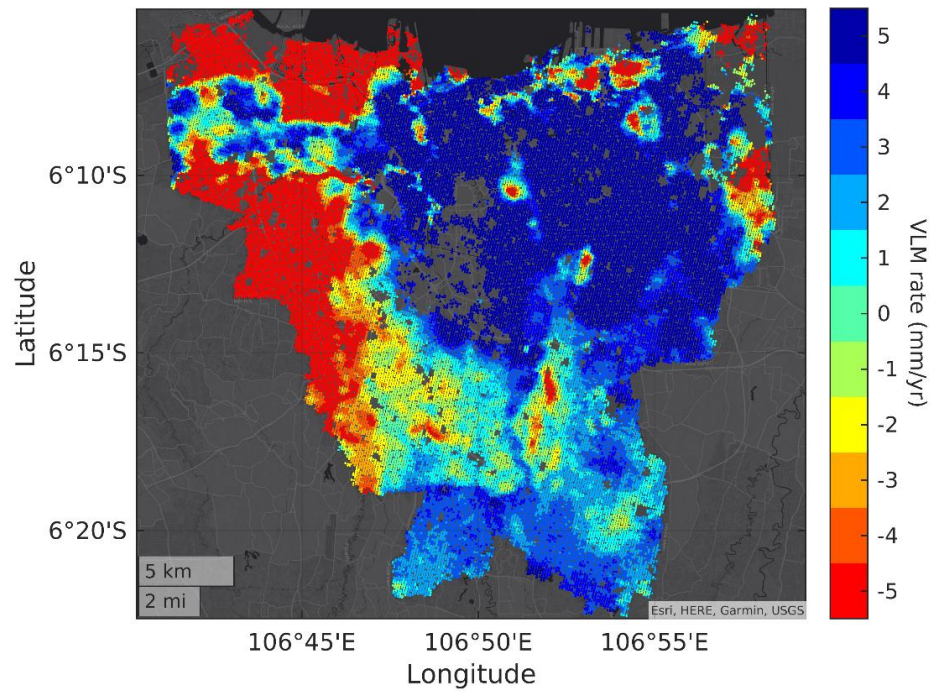
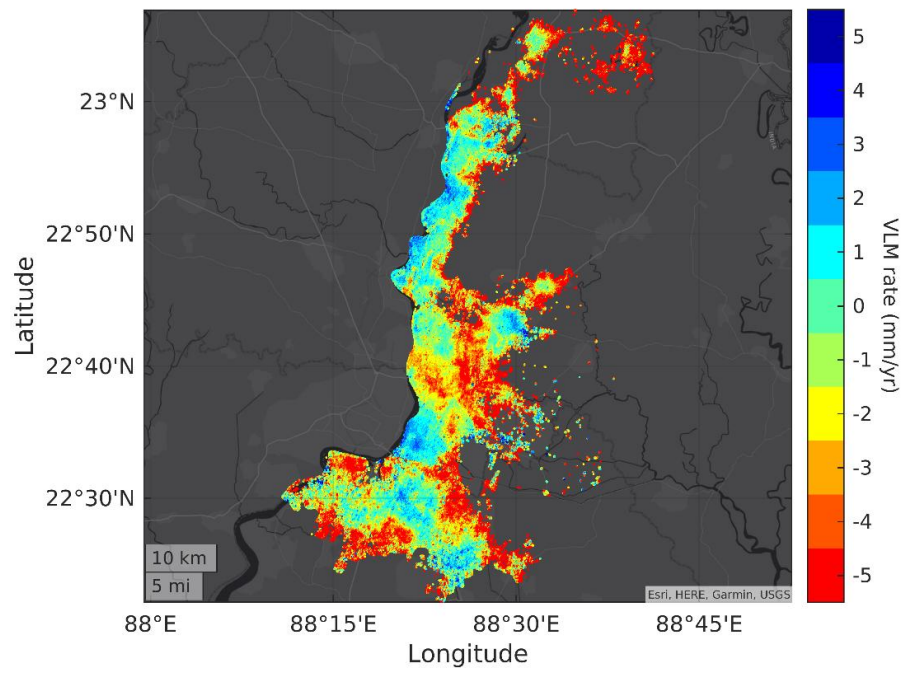


Figure S7. Jakarta, Indonesia. VLM rate distribution comparing results from Tay et al. with those from this study in the IGS14 reference frame.

A. Tay et al.



B. This study

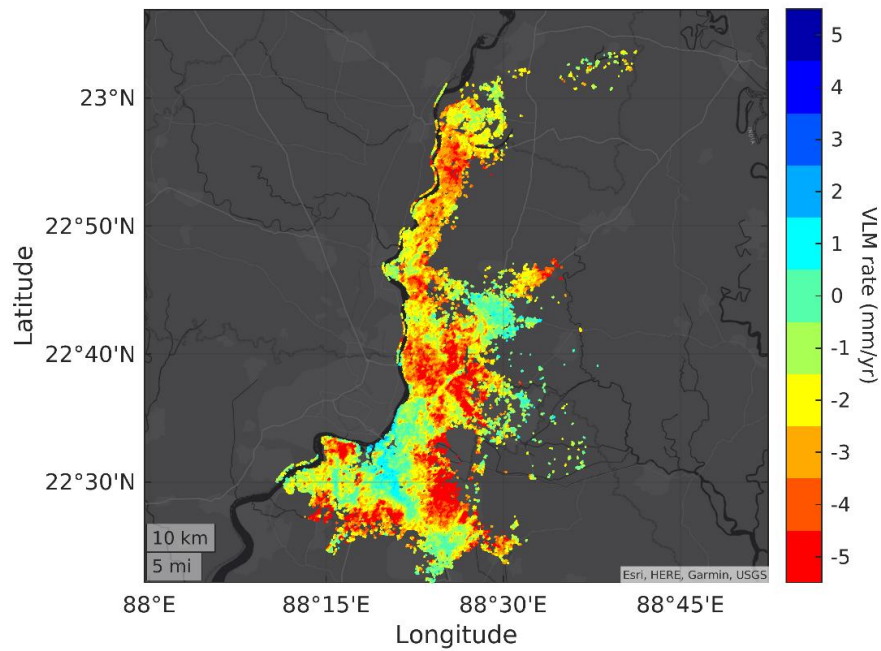
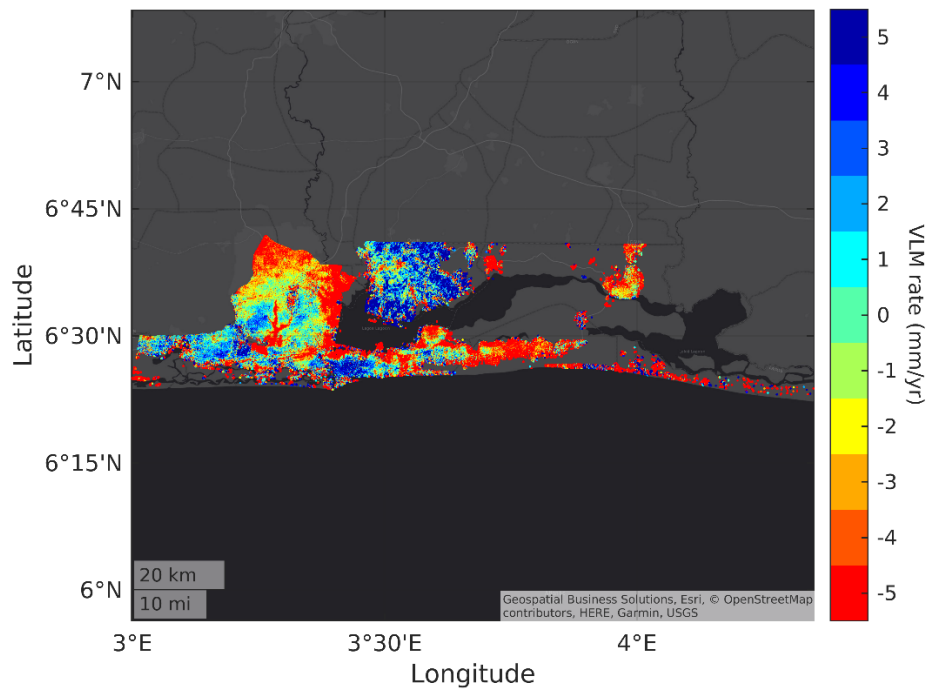


Figure S8. Kolkata, India. VLM rate distribution comparing results from Tay et al. with those from this study in the IGS14 reference frame.

A. Tay et al.



B. This study

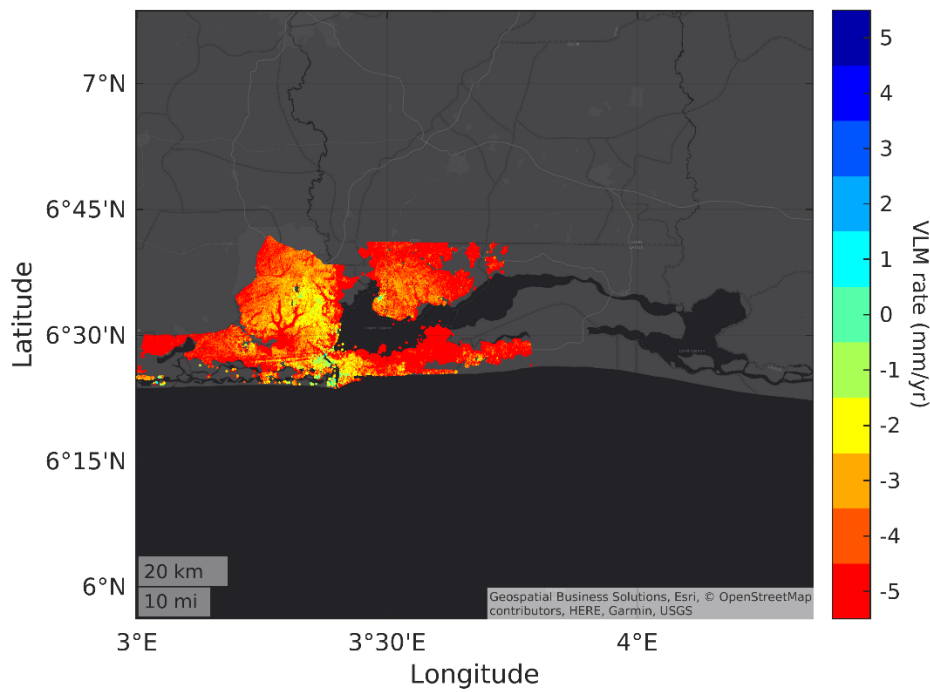
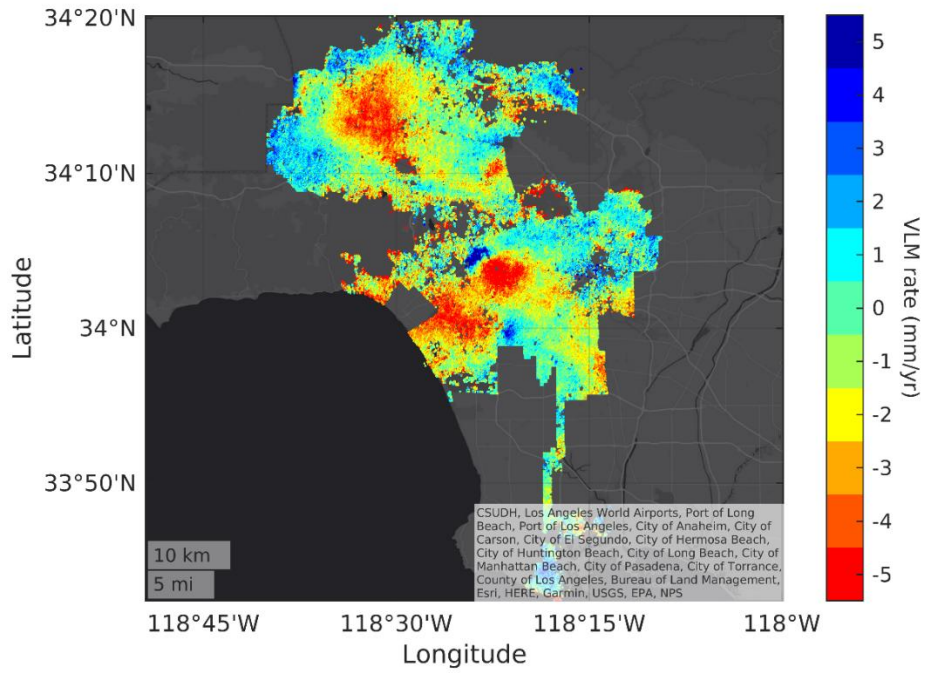


Figure S9. Lagos, Nigeria. VLM rate distribution comparing results from Tay et al. with those from this study in the IGS14 reference frame.

A. Tay et al.



B. This study

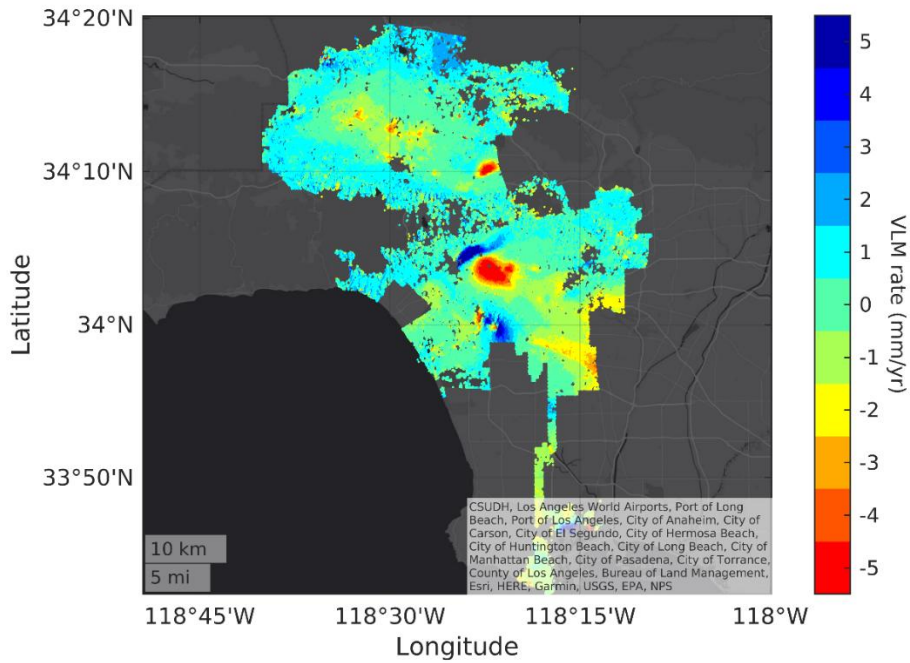
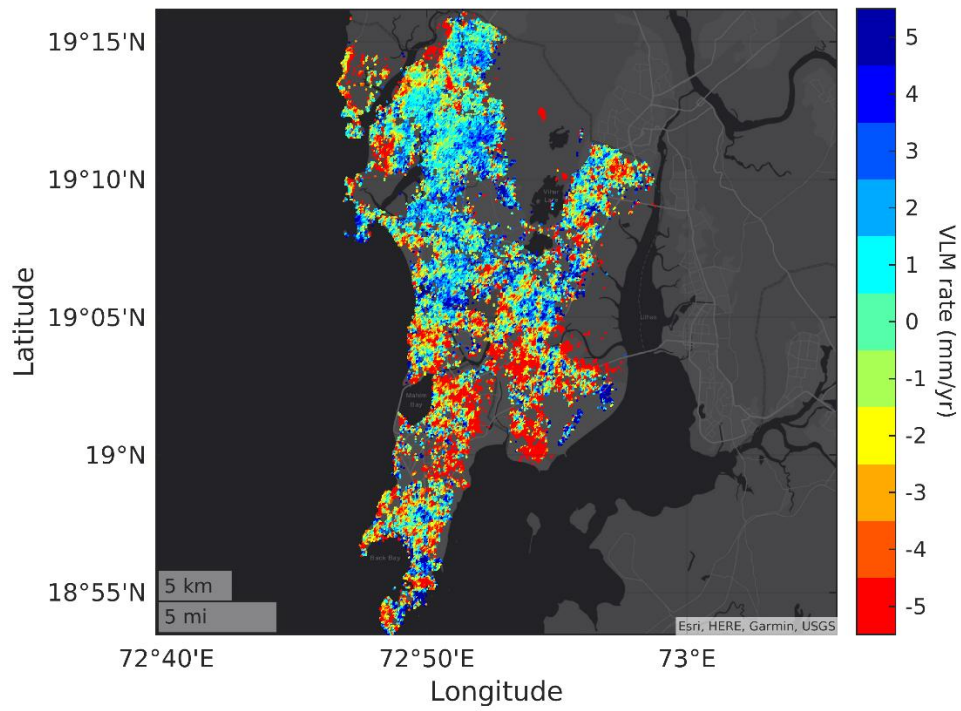


Figure S10. Los Angeles, USA. VLM rate distribution comparing results from Tay et al. with those from this study in the IGS14 reference frame.

A. Tay et al.



B. This study

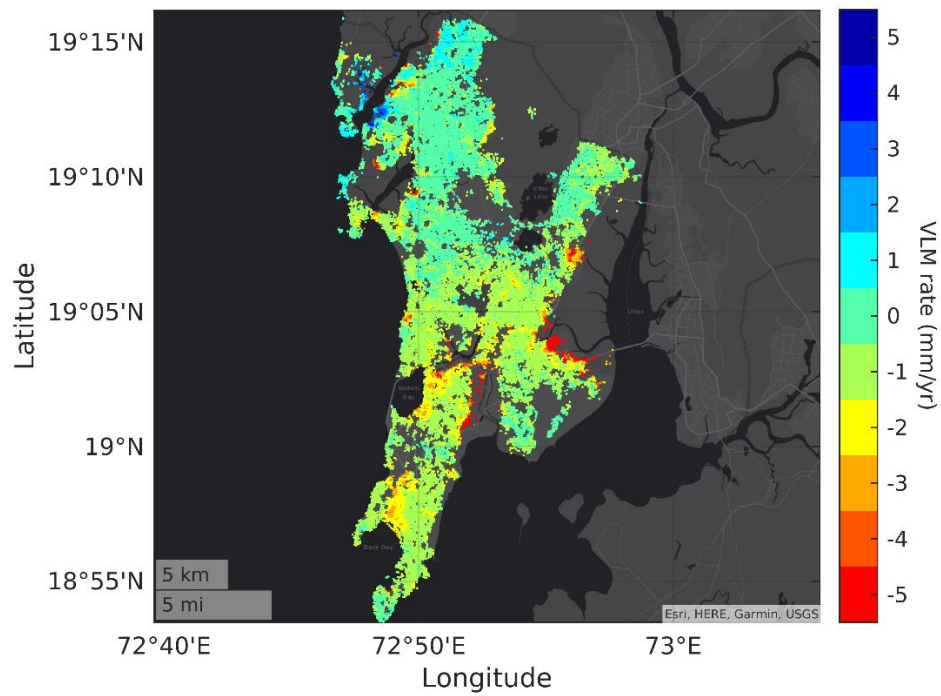
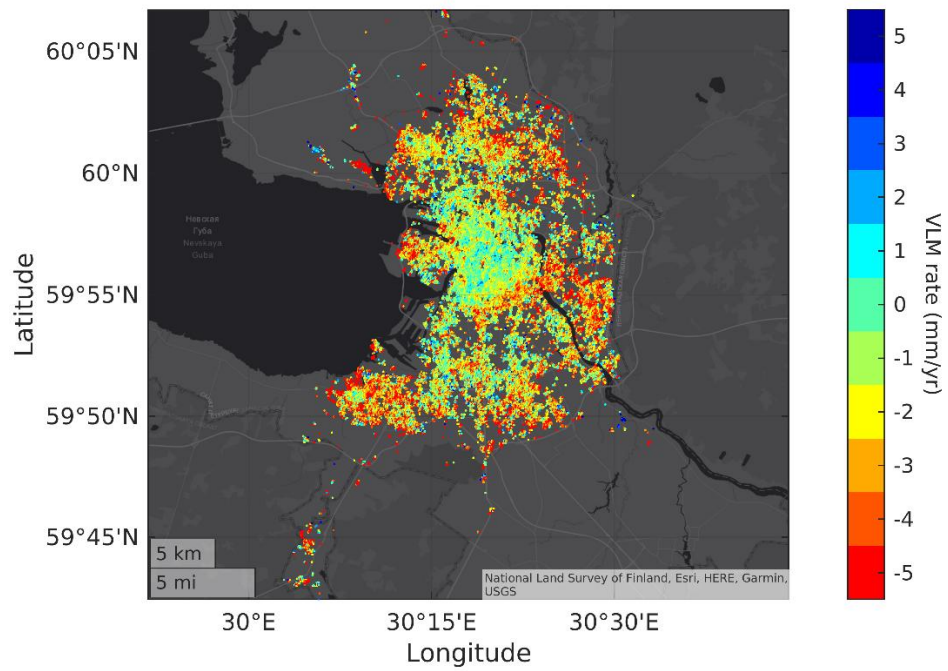


Figure S11. Mumbai, India. VLM rate distribution comparing results from Tay et al. with those from this study in the IGS14 reference frame.

A. Tay et al.



B. This study

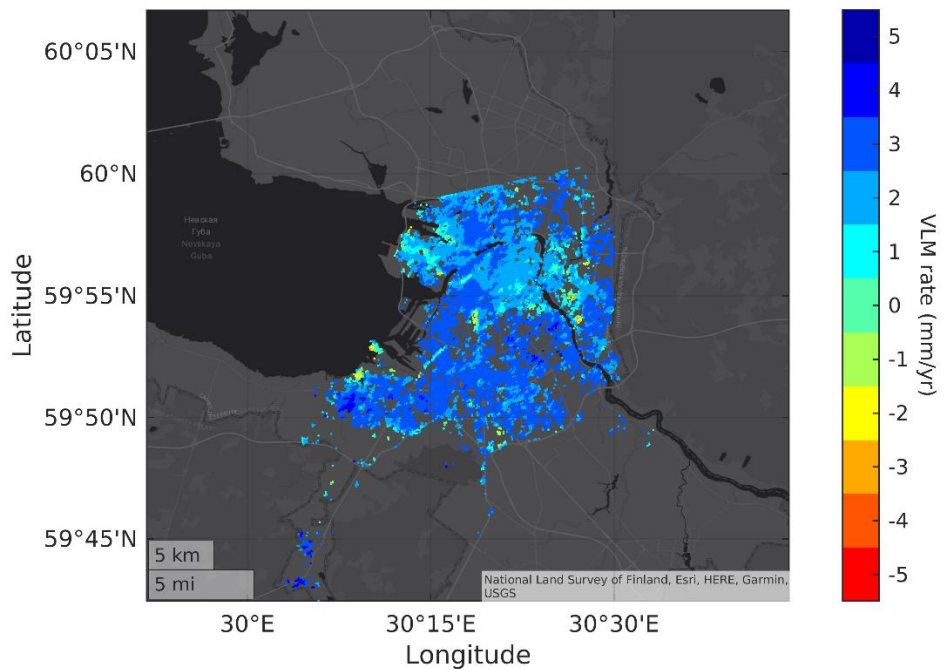
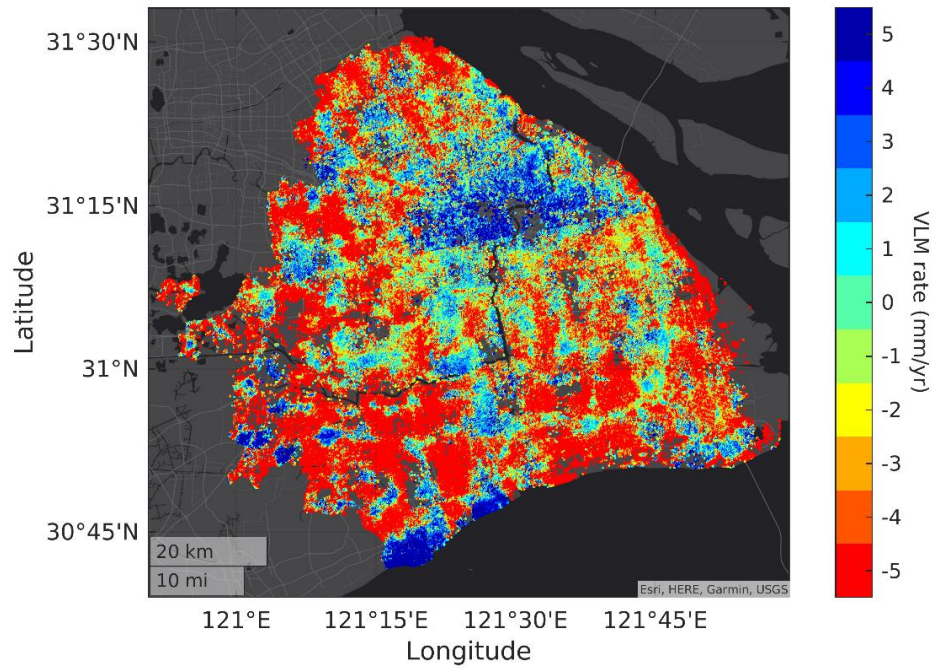


Figure S12. Saint Petersburg, Russia. VLM rate distribution comparing results from Tay et al. with those from this study in the IGS14 reference frame.

A. Tay et al.



B. This study

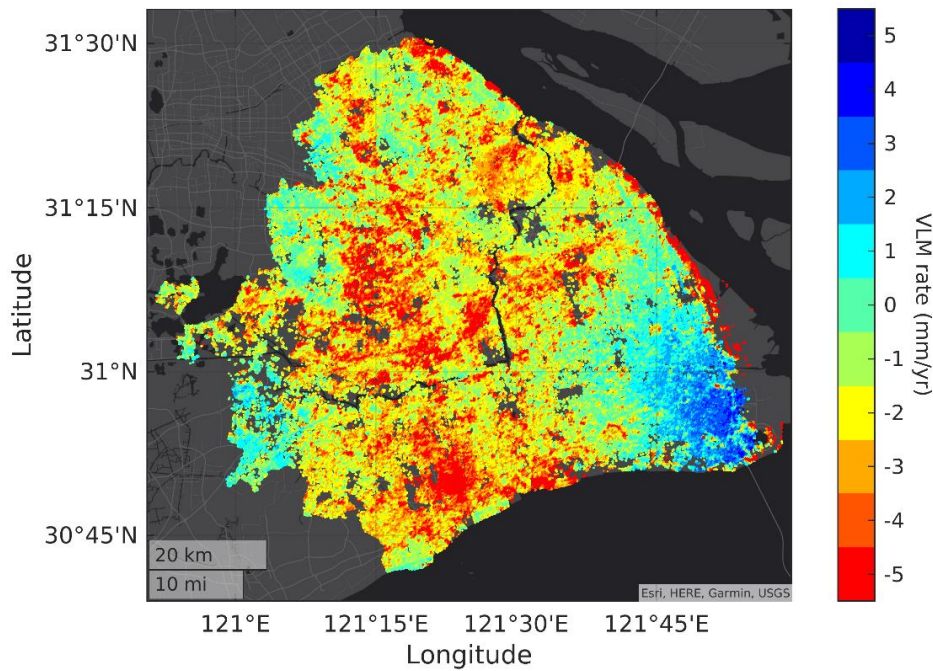
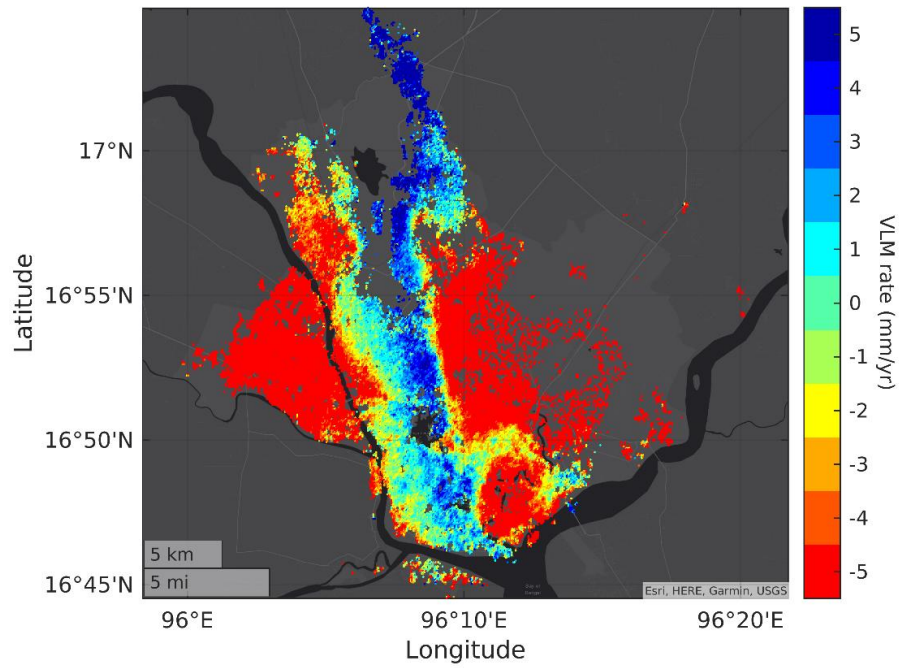


Figure S13. Shanghai, China. VLM rate distribution comparing results from Tay et al. with those from this study in the IGS14 reference frame.

A. Tay et al.



B. This study

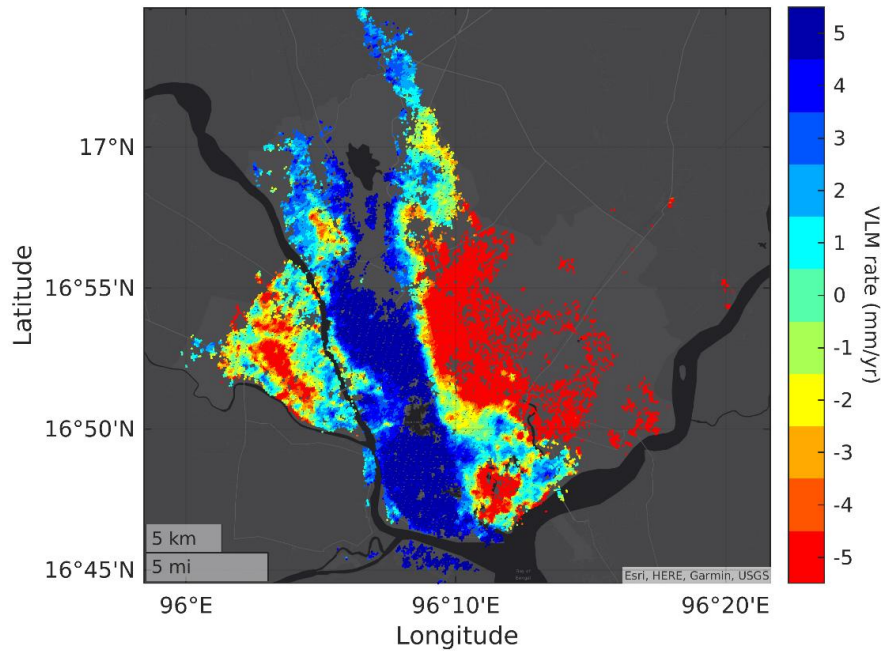


Figure S14. Yangon, Myanmar. VLM rate distribution comparing results from Tay et al. with those from this study in the IGS14 reference frame.