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Peer Review File

Coastal land subsidence accelerates timelines for future flood exposure in Hawai‘i

Corresponding Author: Dr Kyle Murray

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

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Dear Dr Murray,

Your manuscript titled "Coastal land subsidence accelerates timelines for future flood exposure in Hawai‘i" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please consider the following editorial thresholds when revising your manuscript:
1. Adequately describe the methodology employed in the paper (e.g., description of the digital elevation model) such that the analysis is reproducible.
2. Justify or revise some aspects of the methodology (e.g., analysis and discussion of uncertainties in anchoring InSAR measurements to GNSS stations, the use of Sentinel-1 data in vegetated areas).
3. Further discuss processes driving subsidence patterns (e.g., why predicted flooded area declines over time when accounting for subsidence, whether subsidence should be expected to continue at a similar rate in the future).

Please highlight all changes in the manuscript text file and submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Nicole Khan, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0002-9845-1103

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment

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• Life sciences
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https://www.nature.com/documents/nr-reporting-summary.zip

Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist: Communications Earth & Environment formatting checklist

and also in our style and formatting guide Communications Earth & Environment formatting guide .

*** DATA: Communications Earth & Environment endorses the principles of the Enabling FAIR data project (http://www.copdess.org/enabling-fair-data-project/). We ask authors to make the data that support their conclusions available in permanent, publically accessible data repositories. (Please contact the editor if you are unable to make your data available).

All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.

In particular, the Data availability statement should include:
- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)
- Accession codes where appropriate
- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to discipline-specific, community-recognized repositories, where possible and a list of recommended repositories is provided at http://www.nature.com/sdata/policies/repositories.

If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

I reviewed the contribution "Coastal land subsidence accelerates timelines for future flood exposure in Hawai'i" by Murray et al., submitted to Communications Earth & Environment. This article combines InSAR data from the Sentinel-1 and ALOS-1 missions with a new, high-accuracy digital elevation model to evaluate flood hazards along the south shore of O'ahu and the contribution from vertical land motion. The paper is thoughtful, well-written and the results appear generally robust and of high importance so I feel it should be published with minor revisions. I have a few general concerns and possible areas for improvement:

My first concern relates to the quality of the Sentinel-1 data in vegetated areas. There appears to be a strong negative velocity bias in most or all vegetated areas across all the islands, which is not present (or present to a lesser degree) in the ALOS-1 data. The authors are generally careful not to interpret these velocities, which is good, but I feel that some readers may miss these caveats when viewing the VLM rates and could wrongly interpret these biased velocities. I suggest revising the coherence or phase closure thresholds to better mask these low-coherence and biased regions in the Sentinel-1 data to avoid any potential confusion by readers.

My second suggestion is that the DEM clearly represents a significant investment of effort and time, and appears to be of very high quality, yet it is not described in any detail except in the methods section, and the reader gets the impression its use in the main text is almost an afterthought. This DEM covers all of coastal O'ahu and would undoubtedly be of high interest to other researchers as it addresses several problems related to inundation modeling (culverts, bridges, etc.). Has it been previously published elsewhere, or is it newly created for this study? If it is a new product, I suggest highlighting its qualities and advantages in more detail in the main text and making sure it is available in the supplement.

Finally, how carefully have the authors considered the potential that future subsidence may not continue at the same rate? They note that the rates are similar between the ALOS and Sentinel observation periods but then use this 20-year consistency to argue for the assumption that rates will continue for another century. Although it may contribute only moderately to the general conclusions, I feel this assumption is questionable and merits some further discussion.

Minor comments:

L45: missing period at end.

L78: "will be" is a rare future tense in the mostly present-tense article. Suggest changing to present tense.

Figure 2: needs credit / citation for basemap.

L119: Mapunana -> Mapunapuna

L126: understanding -> understand

L134-136: I believe the numbers or units are incorrect throughout this paragraph. For example, 320 square meters is an area approximately 17 meters (55 feet) on a side, which is smaller than a basketball court. Figure 4 indicates it should be closer to 0.3 square kilometers. Remember that 1 km^2 is 1e6 m^2, not 1e3.

Figure 5: the color data on the map are discrete, in 10-year increments, so the colorbar should also be made discrete as this would make the figure easier to read.

L156-159: Capitalize C and L-band

L196: missing figure number

L197: dominate -> dominant

L302: "will be submerged at average high tide" - I think this statement is not correct and greatly overstates the flood hazard, given that the elevation model is referenced to the 50% annual exceedance probability (L286). Thus, I believe this should state "will be submerged on average once every other year" or similar.

I enjoyed reading this manuscript, and encourage the authors to contact me with any questions.
Eric Lindsey, eol@unm.edu

Reviewer #2 (Remarks to the Author):

Review Summary: The influence of VLM on coastal flood risk is an emerging topic of interest, and InSAR is now allowing scientists to quantify this risk. This paper on this issue in Hawaii, is a nice contribution on this topic, is well-written, easy to follow, and elevates the importance of better assessing this coastal hazard to inform climate adaptation planning. Another important contribution is highlighting the issues in resolving VLM in vegetated and steep terrains. The discussion concludes with a thoughtful summary of the need to better understand the variety of contributions to compound flooding in coastal settings. Aside from addressing a few minor comments below, the paper is suitable for publication.

Line comments:
Line 8- average rate of SLR over what time period and for what region? Current rate is ~4.5 mm/yr per Hamlington et al.: Hamlington, B.D., Bellas-Manley, A., Willis, J.K. et al. The rate of global sea level rise doubled during the past three decades. Commun Earth Environ 5, 601 (2024). https://doi.org/10.1038/s43247-024-01761-5

Line 35- is it possible to resolve these rates to two decimal places, i.e., hundredths of a millimeter? In Table 1 there is only one decimal place. Recommend being consistent.

Line 46- yes, and VLM at a tide gauge or GNSS station may not reflect the local/regional signal as VLM rates can vary widely even over 100's to 1000's of meters.

Line 76- here and throughout, how do you define significant, relevant, poor, etc., VLM signals?

Figure 2- change in rate very interesting…but very important issue related to VLM and SLR, including that of extrapolating a fixed rate when in fact, the rate may be nonlinear and therefore there's a lot of uncertainty in the resulting flood exposure projections. For example, line 116 assumes a linear rate, and is noted in line 292. This could be more thoroughly explained in the discussion.

Line 110- so areas most likely to flood every year? I had to read this a few times to understand- consider clarifying.

Line 133- relative to what year?

Line 134- are these units correct? This is a very small area.

Figure 5- cool presentation of this analysis!

Line 176- yes, so how does this nonlinearity factor into your flood exposure uncertainty?

Line 196: Figure reference??

Line 263- references to data?

Supporting Information- These figures are very helpful, but the captions would benefit from a more detail, such as listing the age range of the Sentinel and ALOS data sets being analyzed, explaining the flooding uncertainty estimates (Fig. S27), etc..

Reviewer #3 (Remarks to the Author):

Summary:
The authors present updated measurements of vertical land motion (VLM) in the Hawai'ian Islands, with a focus on coastal regions, using InSAR data from two wavelengths and referenced to local GPS/GNSS data. They calculate updated timelines of flooding forecasts around the main urban center in O'ahu by statistically simulating flood scenarios given sea level rise rates, the new measured VLM rates, and local elevation with mosaicked DEMs. Ultimately they find that accounting for subsidence potentially exposes up to 53% more land in the Mapunapuna region with flooding timelines accelerated up to 50 years.

General comments:
The main part of the paper, which deals with subsidence and flood forecasting in urban O'ahu, has very interesting results that have been synthesized well with sea level rise/flooding forecast data and local DEMs. Because VLM can be a very localized phenomenon, these are very important results for resilience planning in O'ahu. At the same time, beyond O'ahu, the method of incorporating spatially dependent VLM measurements into flood forecasts using the statistical Monte Carlo sampling approach is very interesting and could be important for updating flood forecasts in coastal areas worldwide -- this is a major contribution of the paper, as most forecasts I have seen consider a spatially uniform VLM rate when incorporating into flood forecasts. This method would be very valuable for updating flood forecasts in other areas, especially with the proliferation of InSAR data. The data analysis is transparent and using widely implemented free and open InSAR processing packages.

I would have appreciated a little more discussion and uncertainty analysis regarding the methods of referencing to GNSS stations, since this is the primary method of anchoring the InSAR measurements. Firstly, I am curious as to the method of why the average subsidence rates from two different stations were averaged. I think further justification and clarification is needed as to why this is a valid assumption given the geology and instrumentation of both stations (are they both anchored to similar surfaces? Are they comparable sensors with similar uncertainties?) as well as the spatial and temporal subsidence patterns. Next, it was stated that one criteria for selecting these GNSS stations was their proximity to the coast. Given this, I think it is important to further discuss to what extent it can be expected that the GNSS stations are measuring the same kind of signal as InSAR, given that sediment compaction is the expected subsidence mechanism? As in -- are these GNSS stations anchored closer to the surface rather than bedrock? Are there seasonal expansions and compressions expected? It would be important to understanding whether there are unaccounted sources of error propagating from this source. Generally, given that the GNSS uncertainty should also propagate to InSAR, it would be useful to discuss how this is taken into account when two stations are averaged.

I am also curious as to why the impact on the predicted flooded area declines over time when accounting for subsidence (Figure 4/Line 136). Is this because sediment compaction is being assumed as the driving mechanism, or because of the relative distribution of elevation around O'ahu, both or something else? Also, I wonder why the lower bound for the "with subsidence" predictions could potentially be rather lower than the "without subsidence" case? I am not sure if I am interpreting this correctly but does this suggest that if we account for subsidence, there might be less area exposed to floods than if we do not account for subsidence? I am confused as to why this is the case. If this is the case, how would this change flooding timelines in the extreme cases? It would be useful if the authors could further discuss this point and its implications for flood/resilience planning in Hawaii since this is key to the conclusions.

Finally, I would suggest that the scope of the paper be limited to the urban O'ahu VLM results and flood modeling which would also bring the paper more in line with the abstract and presented results. As acknowledged by the authors, the InSAR analysis especially of many of the more vegetated islands are noisy and possibly do not represent results of strong significance (e.g., Figure S7 shows signals that still look topography related, and Figure S6 shows processing errors in the strong discontinuities across swaths and is not discussed); to me, these results end up raising more questions rather than providing a comprehensive view of VLM. For example I would want to know more about the nature of the signal in irrigated/cultivated areas such as agriculture and golf courses, especially evolution over time and whether they are seasonal, to better understand whether they are artifacts or represent actual subsidence. Further, as also acknowledged by the authors, the data processing results for Hawai'i Island feels out-of-scope for the main thrust of the paper, given that the primary subsidence and uplift signals on Hawai'i are volcanic in nature and that there is already an extensive history of using InSAR to study volcanic dynamics at Manua Loa and Kilauea (as recognized and appropriately cited by the authors). But even bringing up this area of research feels adjacent to this paper's main conclusions about the role of VLM in sea level rise and updating flooding timelines. Generally, I don't feel that there was sufficient analysis and discussion to support any further understanding of what mechanisms are potentially driving these signals on the other islands, so these results are somewhat distracting relative to what I consider the main contributions of the paper (subsidence around O'ahu and derivation of updated flood exposure timelines). In this sense also the title (and the contention in lines 51-52) could be a bit confusing as it suggests that the flooding timelines of the Hawai'ian Islands as a whole are analyzed, but the abstract and results support and show only analysis of urban O'ahu. Making this all consistent would be ideal.

Overall, the authors have presented very important results showing VLM results for urban O'ahu and demonstrating a method of integrating spatially heterogeneous VLM measurements into local flood forecasts. I am sure that by addressing the above points, as well as more minor points listed below, would strengthen and streamline the paper to better bring these important contributions to light.

Other technical comments:
- Lines 188-190: I am not familiar with the orientation of the buildings here, is this actually true for the angles at which the orbits of both ALOS and ascending/descending Sentinel passes intersect with the orientation of the buildings? Furthermore, do we have reason to believe the all buildings would entirely mask any subsidence? There have been many other examples of compaction in many other large cities which should have much larger and denser buildings e.g., NYC, where buildings are required to be anchored to bedrock, such as in this recent study: https://www.science.org/doi/full/10.1126/sciadv.adi8259; some justification for why this specifically applies here would be useful.
- Lines 169-192: I might have missed it but I wasn't sure why the authors focused on sediment compaction as the sole source of subsidence? I think it would be good to clarify with a little more detail why other non-volcanic deformation sources (e.g., erosion, groundwater pumping) were not considered as driving mechanisms.
- Lines 285-288: I am curious if the temporal rates measured by InSAR around O'ahu generally support a linear extrapolation of VLM for generating future DEMs, or are there locations where the VLM rate looks more nonlinear?

Minor issues:
- Line 45: There should be a period at the end of the sentence here
- Middle column in Table 2 for Sentinel-1 Descending is not center justified.
- Lines 62-62, 156, 158: L-band and C-band should be capitalized
- Line 122: "wide spread" should be one word ("widespread")
- Line 138: needs a period at the end of the sentence
- Figure 4: Need closing parenthesis at the end of the caption, behind "Section 4.3."
- Line 196: The reference to the figure is broken (shows "??")
- Line 245: I would suggest not starting a sentence with "and" in a formal article, perhaps "In addition" or "Furthermore"
- Line 274: "data was" should be "data were"

benefits**

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Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr Murray,

Your revised manuscript titled "Coastal land subsidence accelerates timelines for future flood exposure in Hawaiʻi" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment

Nicole Khan, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0002-9845-1103

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I reviewed the revised contribution "Coastal land subsidence accelerates timelines for future flood exposure in Hawai'i" by Murray et al., submitted to Communications Earth & Environment. I appreciate the changes the authors have made in response to my comments and those of the other reviewers. In particular, I appreciate the added mention of the DEM in the abstract, and the reduced focus on the other islands / vegetated areas with noisy data in the main text. The added discussion about the assumption of continued linear motion is also helpful, though I wish it were slightly more nuanced. Overall I feel the manuscript is now in a good state for publication and have only two very minor comments.

- I like the added discussion that the lack of observed subsidence in Waikiki/downtown does not necessarily reflect a true absence of subsidence, given that the majority of pixels represent buildings rather than ground surface (end of section 3.1). I'd just suggest rephrasing the last sentence to be more specific, for example: "However, ground-based surveys or much higher resolution InSAR imagery would be required to assess the presence or absence of land subsidence here."

- Regarding the assumption of a constant linear rate of subsidence in the future (section 3.3), it may help to consider what are the most likely possible future alternative dynamics? If compaction of unconsolidated fill is the major culprit, we might expect a gradual decrease in the subsidence rate, for example - but if as you state the observed rate is fairly constant over 20 years, then you could argue that the decay constant for this process is very long and therefore well approximated by a linear rate for now. I'd say adding a few words to this effect would increase the credibility of this added statement.

I enjoyed reading this manuscript, and encourage the authors to contact me with any questions.
Eric Lindsey, eol@unm.edu

Reviewer #2 (Remarks to the Author):

The authors have sufficiently addressed all my comments.

Reviewer #3 (Remarks to the Author):

The revised manuscript looks excellent. The authors have satisfactorily addressed my comments as part of the revision.

My only remaining comment, very minor, relates to the supplemental information. I still feel that, in the caption of Fig. S6, it would be useful to acknowledge the swath discontinuities as a processing error in the ALOS-1 data of the Hawai'i big island, given the abrupt sign reversals across the swaths in some areas. The caption already acknowledges that there are remaining sources of error in the processing result, but I think explicitly pointing out the swath discontinuities will help interested readers who are less familiar with InSAR processing details to put these results in context, especially as a comparison to the Sentinel-1 result in Fig. S5.

Otherwise, I believe the paper is ready for publication.

Reviewer comments in black
Author responses in blue

We are impressed with the quality of all three of the reviews for this manuscript and would like to thank the reviewers for their critical, yet professional and insightful comments. We believe the manuscript has been greatly improved with the implementation of their comments and suggestions.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

I reviewed the contribution “Coastal land subsidence accelerates timelines for future flood exposure in Hawai‘i” by Murray et al., submitted to Communications Earth & Environment. This article combines InSAR data from the Sentinel-1 and ALOS-1 missions with a new, high-accuracy digital elevation model to evaluate flood hazards along the south shore of O’ahu and the contribution from vertical land motion. The paper is thoughtful, well-written and the results appear generally robust and of high importance so I feel it should be published with minor revisions. I have a few general concerns and possible areas for improvement:

My first concern relates to the quality of the Sentinel-1 data in vegetated areas. There appears to be a strong negative velocity bias in most or all vegetated areas across all the islands, which is not present (or present to a lesser degree) in the ALOS-1 data. The authors are generally careful not to interpret these velocities, which is good, but I feel that some readers may miss these caveats when viewing the VLM rates and could wrongly interpret these biased velocities. I suggest revising the coherence or phase closure thresholds to better mask these low-coherence and biased regions in the Sentinel-1 data to avoid any potential confusion by readers.

This has been a topic of much deliberation throughout this project. It is very difficult to strike the correct balance between removing the untrustworthy data while preserving the reliable data. Because our masking thresholds are already strict (spatial coherence > 0.8, temporal coherence > 0.8, non-zero phase closure < 5%, water , shadows, velocity misfit < 1 mm/yr) we have found that raising the minimum coherence thresholds any more began to remove good data as well. Another idea was to use land cover data to mask out vegetated areas. However there are errors and inconsistencies in the land cover data, and the most recent high resolution data is from 2011 for Hawaii.

As you know, these issues are still a major topic of research in the field of InSAR, and ultimately, Sentinel-1 C-band data is limited. We use a wider range and stricter set of criteria for masking than other similar analyses in the published literature. For example, Buzzanga et al., 2023 uses a very similar processing scheme but is only using temporal coherence for masking criteria (with threshold of 0.8). And although their study area of New York is generally less vegetated than Hawaii, it includes expansive heavily vegetated regions like the Hackensack wetlands and many large parks and golf courses, etc.

In order to better ensure the less attentive reader is not mislead by some of the more questionable vegetated regions we have done the following:

- All descriptions of the velocity maps (besides for the Mapunapuna Fig. 2 which has no vegetation) are now in the supplemental materials document. This will leave no questionable data for the reader to interpret in the main text.
- In the supplemental document we have added text that emphasizes the limitations of InSAR, particularly in vegetated regions. Each caption has a brief statement about potential false signals from ground surface properties.

My second suggestion is that the DEM clearly represents a significant investment of effort and time, and appears to be of very high quality, yet it is not described in any detail except in the methods section, and the reader gets the impression its use in the main text is almost an afterthought. This DEM covers all of coastal O'ahu and would undoubtedly be of high interest to other researchers as it addresses several problems related to inundation modeling (culverts, bridges, etc.). Has it been previously published elsewhere, or is it newly created for this study? If it is a new product, I suggest highlighting its qualities and advantages in more detail in the main text and making sure it is available in the supplement.
We have indeed decided this will be the publication to introduce the new DEM mosaic. We have added text in the abstract (and a tiny bit in the introduction) to emphasize the DEM and highlight that it is a feature of this publication. Further, we have added text in the methods section (4.2) to highlight the qualities and advantages in more detail. We will ensure that the DEM is available as supplemental.

Finally, how carefully have the authors considered the potential that future subsidence may not continue at the same rate? They note that the rates are similar between the ALOS and Sentinel observation periods but then use this 20-year consistency to argue for the assumption that rates will continue for another century. Although it may contribute only moderately to the general conclusions, I feel this assumption is questionable and merits some further discussion.

We agree that this is a key assumption and limitation of the paper/analysis. Given the information we have (past 20 years of subsidence trends), a continuation of the linear trend is the best model we have to estimate future behavior. We also agree that the paper lacked discussion and clarity about this assumption. To this end, we added a new section in the discussion called 'Key assumptions and limitations' with some explicit discussion about the linearity assumption

"In this study, we assume a constant, linear future rate of subsidence based on the observed consistency and linearity in ALOS-1 and Sentinel-1 datasets over the past 20-year period. This is an informed assumption, reflecting the general trends observed in recent history. However, it is important to acknowledge that this assumption may not fully capture the complexities of future subsidence dynamics, which are unknown. Subsidence rates can be influenced by a wide range of factors. This highlights the need for continued monitoring of subsidence in urban areas to continuously refine our understanding of future subsidence rates and future flood exposure."

Minor comments:

L45: missing period at end.
Added a period

L78: "will be" is a rare future tense in the mostly present-tense article. Suggest changing to present tense.
Changed to "is"

Figure 2: needs credit / citation for basemap.
Added citation to Fig. 2 and 5.
Esri, HERE, Garmin, and the GIS User Community. World light gray base [basemap]. Available from ArcGIS Online (2024). Accessed: 2024-07-15.

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Corrected this typo

L126: understanding -> understand
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Corrected this (thank you for catching my elementary school math mistake!)

Figure 5: the color data on the map are discrete, in 10-year increments, so the colorbar should also be made discrete as this would make the figure easier to read.
Changed the colorbars in Fig. 5 to be discrete

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Corrected missing figure number

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exceedance probability (L286). Thus, I believe this should state “will be submerged on average once every other year” or similar.
Changed to “on average once every other year”. (We originally referenced to mean higher high water, and this was left over from that)

I enjoyed reading this manuscript, and encourage the authors to contact me with any questions.
Eric Lindsey, eol@unm.edu
Thank you Eric!

Reviewer #2 (Remarks to the Author):

Review Summary: The influence of VLM on coastal flood risk is an emerging topic of interest, and InSAR is now allowing scientists to quantify this risk. This paper on this issue in Hawaii, is a nice contribution on this topic, is well-written, easy to follow, and elevates the importance of better assessing this coastal hazard to inform climate adaptation planning. Another important contribution is highlighting the issues in resolving VLM in vegetated and steep terrains. The discussion concludes with a thoughtful summary of the need to better understand the variety of contributions to compound flooding in coastal settings. Aside from addressing a few minor comments below, the paper is suitable for publication.

Line comments:
Line 8- average rate of SLR over what time period and for what region? Current rate is ~4.5 mm/yr per Hamlington et al.:
Hamlington, B.D., Bellas-Manley, A., Willis, J.K. et al. The rate of global sea level rise doubled during the past three decades. Commun Earth Environ 5, 601 (2024).
https://doi.org/10.1038/s43247-024-01761-5
Changed to: “This subsidence rate, much higher than the long-term average SLR rate in Hawaii (1.54 mm/yr since 1905),...”

Line 35- is it possible to resolve these rates to two decimal places, i.e., hundredths of a millimeter? In Table 1 there is only one decimal place. Recommend being consistent.
We have changed reported values to one decimal place here and throughout the rest of the text.

Line 46- yes, and VLM at a tide gauge or GNSS station may not reflect the local/regional signal as VLM rates can vary widely even over 100’s to 1000’s of meters.
Added this sentence to that paragraph: “VLM at a tide gauge or GNSS station may not capture the true spatial variation in VLM, which can vary widely between sites.”

Line 76- here and throughout, how do you define significant, relevant, poor, etc., VLM signals?
Changed to: “Across all islands, the most significant and relevant VLM signals in terms of potential societal impact are located on the south shore of Oʻahu due to the extensive coastal engineering activities and development, as well as extensive infrastructure and population that may be vulnerable to flooding.”

Figure 2- change in rate very interesting…but very important issue related to VLM and SLR, including that of extrapolating a fixed rate when in fact, the rate may be nonlinear and therefore there's a lot of uncertainty in the resulting flood exposure projections. For example, line 116 assumes a linear rate, and is noted in line 292. This could be more thoroughly explained in the discussion.
We agree that this is a major assumption and limitation of the paper/analysis. Given the information we have (past 20 years of subsidence trends), a continuation of the linear trend is the best model we have to estimate future behavior. We also agree that the paper lacked discussion and clarity about this assumption. To this end, we added a new section in the discussion called 'Key assumptions and limitations' with some explicit discussion about the linear assumption

"In this study, we assume a constant, linear future rate of subsidence based on the observed consistency and linearity in ALOS-1 and Sentinel-1 datasets over the past 20-year period. This is an informed assumption, reflecting the general trends observed in recent history. However, it is important to acknowledge that this assumption may not fully capture the complexities of future subsidence dynamics. Subsidence rates can be influenced by a wide range of factors. This highlights the need for continued monitoring of subsidence in urban areas to continuously refine our understanding of future subsidence rates and future flood exposure."

Line 110- so areas most likely to flood every year? I had to read this a few times to understand- consider clarifying.
Yes, we added some further clarification to the end of that sentence: "(about once every other year)"

Line 133- relative to what year?
Added "(relative to 2020)" in that sentence.

Line 134- are these units correct? This is a very small area.
They were indeed incorrect. Changed from m to km.

Figure 5- cool presentation of this analysis!
Thank you :)

Line 176- yes, so how does this nonlinearity factor into your flood exposure uncertainty?
We have added more discussion to justify, and explicit text about the limitations of the assumption of linearity. (section 3.3)

Line 196: Figure reference??
Fixed this error

Line 263- references to data?
Added references for these data

Supporting Information- These figures are very helpful, but the captions would benefit from a more detail, such as listing the age range of the Sentinel and ALOS data sets being analyzed, explaining the flooding uncertainty estimates (Fig. S27), etc..
Added more descriptions in the figure captions

Reviewer #3 (Remarks to the Author):

Summary:
The authors present updated measurements of vertical land motion (VLM) in the Hawai'ian Islands, with a focus on coastal regions, using InSAR data from two wavelengths and referenced to local GPS/GNSS data. They calculate updated timelines of flooding forecasts around the main urban center in O'ahu by statistically simulating flood scenarios given sea level rise rates, the new measured VLM rates, and local elevation with mosaicked DEMs. Ultimately they find that accounting for subsidence potentially exposes up to 53% more land in the Mapunapuna region with flooding timelines accelerated up to 50 years.

General comments:
The main part of the paper, which deals with subsidence and flood forecasting in urban O'ahu, has very interesting results that have been synthesized well with sea level rise/flooding forecast data and local DEMs. Because VLM can be a very localized phenomenon, these are very important results for resilience planning in O'ahu. At the same time, beyond O'ahu, the method of incorporating spatially dependent VLM measurements into flood forecasts using the statistical Monte Carlo sampling approach is very interesting and could be important for updating flood forecasts in coastal areas worldwide -- this is a major contribution of the paper, as most forecasts I have seen consider a spatially uniform VLM rate when incorporating into flood forecasts. This method would be very valuable for updating flood forecasts in other areas, especially with the proliferation of InSAR data. The data analysis is transparent and using widely implemented free and open InSAR processing packages.

I would have appreciated a little more discussion and uncertainty analysis regarding the methods of referencing to GNSS stations, since this is the primary method of anchoring the InSAR measurements. Firstly, I am curious as to the method of why the average subsidence rates from two different stations were averaged. I think further justification and clarification is needed as to why this is a valid assumption given the geology and instrumentation of both stations (are they both anchored to similar surfaces? Are they comparable sensors with similar uncertainties?) as well as the spatial and temporal subsidence patterns. Next, it was stated that one criteria for selecting these GNSS stations was their proximity to the coast. Given this, I think it is important to further discuss to what extent it can be expected that the GNSS stations are measuring the same kind of signal as InSAR, given that sediment compaction is the expected subsidence mechanism? As in -- are these GNSS stations anchored closer to the surface rather than bedrock? Are there seasonal expansions and compressions expected? It would be important to understanding whether there are unaccounted sources of error propagating from

this source. Generally, given that the GNSS uncertainty should also propagate to InSAR, it would be useful to discuss how this is taken into account when two stations are averaged.
Thank you for this suggestion. We agree this section was lacking in justification and discussion. We have added propagation of the GNSS uncertainties to the final VLM rates, which was originally left out. We have updated the reported uncertainty values throughout the paper. The effect on uncertainties was insignificant and has no bearing on the flood model or interpretations. However, it adds additional precision and robustness to the manuscript. We have further added significantly more detail and discussion to that paragraph end of section 4.1 to better address the points you have made above.

I am also curious as to why the impact on the predicted flooded area declines over time when accounting for subsidence (Figure 4/Line 136). Is this because sediment compaction is being assumed as the driving mechanism, or because of the relative distribution of elevation around O'ahu, both or something else? Also, I wonder why the lower bound for the "with subsidence" predictions could potentially be rather lower than the "without subsidence" case? I am not sure if I am interpreting this correctly but does this suggest that if we account for subsidence, there might be less area exposed to floods than if we do not account for subsidence? I am confused as to why this is the case. If this is the case, how would this change flooding timelines in the extreme cases? It would be useful if the authors could further discuss this point and its implications for flood/resilience planning in Hawaii since this is key to the conclusions.
We have added some additional explanation to the end of section 2.2.1. To summarize, the predicted flooded area is not shown to decline over time, but rather after a certain time, the relative effect of subsidence starts to decay. It is a bit counterintuitive at first, but you can imagine after a certain time, many of the subsiding areas are already flagged as submerged pixels, so continued subsidence of those pixels has no effect on expanding the flooded area. The error bound shown in fig. 4 is based on the Monte Carlo error simulations taking into account all sources of uncertainty. So it is not just the addition of subsidence that contributes to the error bound. The error bound for the case 'without subsidence' was left out of the figure because it makes the figure more cluttered and more complicated to interpret. The bound is also not relevant for the interpretations we make, which are all related to the case 'with subsidence'.

Finally, I would suggest that the scope of the paper be limited to the urban O'ahu VLM results and flood modeling which would also bring the paper more in line with the abstract and presented results. As acknowledged by the authors, the InSAR analysis especially of many of the more vegetated islands are noisy and possibly do not represent results of strong significance (e.g., Figure S7 shows signals that still look topography related, and Figure S6 shows processing errors in the strong discontinuities across swaths and is not discussed); to me, these results end up raising more questions rather than providing a comprehensive view of VLM. For example I would want to know more about the nature of the signal in irrigated/cultivated areas such as agriculture and golf courses, especially evolution over time and whether they are seasonal, to better understand whether they are artifacts or represent actual subsidence. Further, as also acknowledged by the authors, the data processing results

for Hawai'i Island feels out-of-scope for the main thrust of the paper, given that the primary subsidence and uplift signals on Hawai'i are volcanic in nature and that there is already an extensive history of using InSAR to study volcanic dynamics at Manua Loa and Kilauea (as recognized and appropriately cited by the authors). But even bringing up this area of research feels adjacent to this paper's main conclusions about the role of VLM in sea level rise and updating flooding timelines. Generally, I don't feel that there was sufficient analysis and discussion to support any further understanding of what mechanisms are potentially driving these signals on the other islands, so these results are somewhat distracting relative to what I consider the main contributions of the paper (subsidence around O'ahu and derivation of updated flood exposure timelines). In this sense also the title (and the contention in lines 51-52) could be a bit confusing as it suggests that the flooding timelines of the Hawai'ian Islands as a whole are analyzed, but the abstract and results support and show only analysis of urban O'ahu. Making this all consistent would be ideal.
We have also gone back and forth about whether or not to limit the paper to Oahu. The final decision was to greatly trim down mention of the other islands, and move all of the figures to the supplemental. That left only two paragraphs (2.1.2 and 2.1.3) which give a brief overview of deformation on the other islands. The justification for leaving these was that there is simply a lot of interest in knowing if similar patterns of deformation are present in the rest of Hawaii. This has been perceived through interactions with other researchers as well as local stake-holders. It requires a lot of time and effort to process the data to get results across each of the islands, and it is quite valuable for many people to know even a null result. Because the results can't easily stand as a separate publication, we have determined that the best way to make these results available to those who are interested, is to have them as supplemental material.

To compromise on this point, and help make the paper more concise and organized, we have moved the VLM descriptions (2.1.2 and 2.1.3) to the supplemental, as well as the paragraph in the introduction about volcanic deformation. We have also added some text throughout the paper to make these decisions more clear for the reader.

Overall, the authors have presented very important results showing VLM results for urban O'ahu and demonstrating a method of integrating spatially heterogeneous VLM measurements into local flood forecasts. I am sure that by addressing the above points, as well as more minor points listed below, would strengthen and streamline the paper to better bring these important contributions to light.

Other technical comments:
- Lines 188-190: I am not familiar with the orientation of the buildings here, is this actually true for the angles at which the orbits of both ALOS and ascending/descending Sentinel passes intersect with the orientation of the buildings? Furthermore, do we have reason to believe the all buildings would entirely mask any subsidence? There have been many other examples of compaction in many other large cities which should have much larger and denser buildings e.g., NYC, where buildings are required to be anchored to bedrock, such as in this recent study: https://www.science.org/doi/full/10.1126/sciadv.adi8259; some justification for why this specifically applies here would be useful.

It is not as much the angle of the orbits and orientation of the buildings that could cause an issue. It is the fact that given the look angle of the satellite, much of the energy of the radar does not make it to the ground. It is instead scattered off of the buildings (some returned, some not). This happens if the buildings are tall enough and the spaces between them are relatively narrow. If you look carefully at Buzzanga et al. results in NYC that you mentioned, you'll find the deforming lands are not in the areas where there are large skyscrapers like Manhattan. Instead, they are in places like near Newton Creek, which is a more industrial area that is in some ways similar to the Mapunapuna area on O'ahu in terms of the style and height of buildings relative to the spaces between them.
Nonetheless, we agree our reasoning here is rather speculative and more research needs to be done to truly understand this problem. We have added text to that section in an attempt to better characterize this.

- Lines 169-192: I might have missed it but I wasn't sure why the authors focused on sediment compaction as the sole source of subsidence? I think it would be good to clarify with a little more detail why other non-volcanic deformation sources (e.g., erosion, groundwater pumping) were not considered as driving mechanisms.
We agree there was insufficient discussion of other drivers of subsidence. Thus, we have added a new paragraph called 'Driving mechanisms of subsidence' in the discussion (in place of 'compaction of sediments'). It includes the following text:
"There are a range of possible drivers of subsidence in coastal and urban areas {shirzaei_measuring_2021}. Given the spatial scale and geologic setting, we can fairly confidently rule out glacial isostatic adjustment, tectonic, and volcanic activity as the primary drivers of subsidence in Oʻahu. Groundwater extraction is commonly cited as the primary driver of subsidence of this scale in many urban areas. However, the aquifer systems in Hawai'i are predominantly composed of coarse-grained, highly permeable volcanic materials. This is in contrast to the fine-grained aquitard layers that are generally thought to be the source of long-term sustained groundwater-related compaction {murray_short-lived_2018}. This leaves compaction of sediments as the most likely driver of subsidence. This is supported by the fact that many of the areas of observed subsidence coincide with locations of unconsolidated artificial fill."

- Lines 285-288: I am curious if the temporal rates measured by InSAR around O'ahu generally support a linear extrapolation of VLM for generating future DEMs, or are there locations where the VLM rate looks more nonlinear?
We added a new section in the discussion called 'Key assumptions and limitations'
In particular:
"In this study, we assume a constant, linear future rate of subsidence based on the observed consistency and linearity in ALOS-1 and Sentinel-1 datasets over the past 20-year period. This is an informed assumption, reflecting the general trends observed in recent history. However, it is important to acknowledge that this assumption may not fully capture the complexities of future subsidence dynamics. Subsidence rates can be influenced by a wide range of factors. This highlights the need for continued monitoring of subsidence in urban areas to continuously refine our understanding of future subsidence rates and future flood exposure."

Minor issues:
- Line 45: There should be a period at the end of the sentence here
Corrected this typo

- Middle column in Table 2 for Sentinel-1 Descending is not center justified.
Fixed this

- Lines 62-62, 156, 158: L-band and C-band should be capitalized
Changed capitalization here and throughout

- Line 122: "wide spread" should be one word ("widespread")
Changed to widespread

- Line 138: needs a period at the end of the sentence
Added a period at the end

- Figure 4: Need closing parenthesis at the end of the caption, behind "Section 4.3."
Added parenthesis.

- Line 196: The reference to the figure is broken (shows "??")
Corrected this error

- Line 245: I would suggest not starting a sentence with "and" in a formal article, perhaps "In addition" or "Furthermore"
Changed to “Furthermore”

- Line 274: "data was" should be "data were"
Changed to “data were”

Reviewer #1 (Remarks to the Author):

I reviewed the revised contribution “Coastal land subsidence accelerates timelines for future flood exposure in Hawai‘i” by Murray et al., submitted to Communications Earth & Environment. I appreciate the changes the authors have made in response to my comments and those of the other reviewers. In particular, I appreciate the added mention of the DEM in the abstract, and the reduced focus on the other islands / vegetated areas with noisy data in the main text. The added discussion about the assumption of continued linear motion is also helpful, though I wish it were slightly more nuanced. Overall I feel the manuscript is now in a good state for publication and have only two very minor comments.

- I like the added discussion that the lack of observed subsidence in Waikiki/downtown does not necessarily reflect a true absence of subsidence, given that the majority of pixels represent buildings rather than ground surface (end of section 3.1). I’d just suggest rephrasing the last sentence to be more specific, for example: “However, ground-based surveys or much higher resolution InSAR imagery would be required to assess the presence or absence of land subsidence here.”

We added:
Roads and other areas between buildings may still be subsiding undetected. However, ground-based surveys or much higher resolution InSAR imagery would be required to assess the presence or absence of land subsidence here.

- Regarding the assumption of a constant linear rate of subsidence in the future (section 3.3), it may help to consider what are the most likely possible future alternative dynamics? If compaction of unconsolidated fill is the major culprit, we might expect a gradual decrease in the subsidence rate, for example - but if as you state the observed rate is fairly constant over 20 years, then you could argue that the decay constant for this process is very long and therefore well approximated by a linear rate for now. I’d say adding a few words to this effect would increase the credibility of this added statement.

We have modified/added the following to section 3.3:
However, it is important to acknowledge that this assumption may not fully capture the complexities of future subsidence dynamics. Subsidence rates can be influenced by a wide range of factors. For example, assuming compaction of unconsolidated fill is the main driver of subsidence, we might expect a gradual decrease in the subsidence rate over time. This decay is not found in our observations, which may imply the decay constant for this process is long enough to be approximated with a linear trend at least in the coming decades. This uncertainty highlights the need for continued monitoring of subsidence in urban areas to continuously refine our understanding of future subsidence rates and future flood exposure.

I enjoyed reading this manuscript, and encourage the authors to contact me with any questions.
Eric Lindsey, eol@unm.edu

Reviewer #2 (Remarks to the Author):

The authors have sufficiently addressed all my comments.

Reviewer #3 (Remarks to the Author):

The revised manuscript looks excellent. The authors have satisfactorily addressed my comments as part of the revision.

My only remaining comment, very minor, relates to the supplemental information. I still feel that, in the caption of Fig. S6, it would be useful to acknowledge the swath discontinuities as a processing error in the ALOS-1 data of the Hawai'i big island, given the abrupt sign reversals across the swaths in some areas. The caption already acknowledges that there are remaining sources of error in the processing result, but I think explicitly pointing out the swath discontinuities will help interested readers who are less familiar with InSAR processing details to put these results in context, especially as a comparison to the Sentinel-1 result in Fig. S5.

We added:
Furthermore, given the relatively sparse time series for ALOS-1, tropospheric delay effects also remain, particularly in areas of high topographic relief. And because this image is a mosaic of multiple swaths/frames from different dates, discontinuities appear at their edges which accentuate the errors noted above.

Otherwise, I believe the paper is ready for publication.