Decision Letter Response: "2023 Kīlauea Horizontal-Component Signal/Noise Assessment of a Large Three-Component Nodal Array"

June 18, 2026

We would like to express our appreciation to the Editor and the Reviewers for their insightful comments and suggestions, which have aided us in improving the quality of the manuscript. We have modified the paper carefully according to the provided comments and suggestions. Revisions in the marked version are indicated by a red font; the previous wording is indicated by a blue font with a strike through the prose. Our responses to individual comments in this response document may be found below in blue font. **Note: the line numbers mentioned in the response to individual comments correspond to the marked version.**

Regards,
The Authors

Response to the Editor (Oliver Lamb)

We thank you for handling our submission and for the constructive decision. We agree with the shared concern of both reviewers, that the original manuscript read more like a technical report than a study of broad interest and that its reference list was too thin to place the work in its proper context. In the revised version we have substantially expanded and modernized the reference list, replacing the LinkedIn posts and manufacturer pages with peer-reviewed studies, and we have broadened the framing from a deployment-specific report toward a result of general interest. The Introduction now situates the work within the horizontal-component node literature and states the multi-hardware design rationale explicitly: the 2023 K=ilauea deployment co-locates several surface-deployed node designs (spiked, sand-coupled, and leveling-leg) with permanent HVO broadband sensors under a common, persistently windy setting, so that the comparison across instrument packages, rather than the dataset alone, is the contribution. The Conclusions now add an interpretive paragraph that positions the results relative to prior single-instrument characterizations, connects each station subset to the analyses it is intended to support (interferometry, S-wave arrival-time picking, moment-tensor inversion, and S-wave attenuation tomography), and identifies how the geometry-based wind-sensitivity framework generalizes to other wind-exposed, burial-restricted deployments. We have also added a generalizable finding to the abstract, that taller, narrower spike-mounted nodes are more susceptible to wind-induced rocking than shorter, wider nodes on leveling legs. As you suggested, we have also added a sentence to the Acknowledgements recognizing the reviewers' contributions. We hope the revised manuscript now reads as a study of broader interest, and our point-by-point responses to both reviewers follow below.

Reviewer 1:

Major Comments

R1-1: The manuscript contains clear descriptions of temporary 3-C seismometer deployment strategies and multi-component noise levels in an interesting volcanic study area with subset areas that have different impact and logistical requirements. It is related to practical decisions for many seismic researchers. However, the manuscript reads like a direct report of measurements completed rather than a research study seeking to build on prior work and advance the collective state of understanding. It could be improved by further placing this study in the context of the literature and conducting potential seismic method tests with the different instrumentation packages. To provide more substantial research results and discussion of implications I encourage the authors to test using their different data types. For example, try picking S waves with an automated algorithm that provides probabilities/uncertainties if S wave detection is a key seismic process of interest (or some other seismic method that uses horizontal components). Then the results would have clearer implications for methods used to learn about seismic sources or structures.

R1-1R: We thank the reviewer for this suggestion and agree that method-focused tests would be valuable. We have, however, kept the scope centered on dataset documentation and horizontal-component quality control, for two reasons.

First, the manuscript is already at the upper bound of Seismica's length and figure allowance for a research article (about 10,000 words and ten figures), with substantial additional material carried in the Supplement. A demonstration such as automated S-wave picking with formal uncertainties, done properly across the different instrument packages, would require its own data, figures, and validation, and would constitute a separate study rather than an addition to this one.

Second, we respectfully push back on the characterization of the manuscript as a direct report of measurements. To our knowledge this is the first study to characterize horizontal-component noise and H/V as a function of deployment configuration, including burial versus surface coupling, for the SmartSolo IGU-16HR-3C, and to do so jointly with co-deployed GTI U3 nodes and buried HVO broadband sensors across a common, wind-dominated field setting. Earlier observations for these nodes are limited to small or single-configuration deployments (Dean et al., 2018; Dean and Grant, 2024), so the controlled, large-scale comparison presented here advances, rather than merely reports, the understanding of how node geometry and deployment configuration control horizontal-component performance.

We are aware that Seismica's article types include report formats intended for instrument deployments and field campaigns, and we considered framing the manuscript accordingly. We have chosen to retain it as a research article, because its contribution is not the dataset alone but the comparative analysis of how deployment configuration and node geometry govern horizontal-component S/N across co-deployed instrument types, which we regard as a research result of general relevance.

To address the spirit of the comment within the available length, we have broadened the introduction's connection to the horizontal-component node literature (see our response to the following comment) and added a paragraph to the Conclusions that positions the study relative to prior work and identifies the method tests the dataset is intended to support, including automated S-wave picking with uncertainties, moment-tensor inversion, S-wave attenuation tomography, and interferometry, applied to the instrument packages and frequency bands where horizontal-component S/N is adequate (lines [332–343]).

R1-2: Horizontal components of 3-C nodes (buried and surface deployed) have been used quite a bit over the past several years for local earthquake characterization (Trow et al., 2018 SRL; Wang et al., 2020

GRL; Obermann et al., 2022 SRL), teleseismic receiver functions which require joint use of horizontal and vertical channels (Liu et al., 2018 SRL; Ward et al., 2018 GRL), surface wave tomography with Rayleigh and Love waves (Obermann et al., 2022 SRL; Wu et al., 2023 EPSL), and active source imaging with P and converted S waves (Parker et al., 2018 SRL; Duan et al., 2025 Nature), among other uses. There are also studies including results related to noise levels from wind or other sources: Johnson et al., 2019 SRL; Farrell et al., 2018; Wang et al., 2020 GRL. These studies and others provide related perspectives on noise, wind contributions, the importance of burial when possible, and potential effects on small earthquake detection. Some references are provided here in an effort to help brainstorm connections to the research literature, but there is a wide variety available.

R1-2R: We thank the reviewer for these references, which we found helpful. Several have been incorporated to better connect the study to the horizontal-component node literature: the Introduction now situates the work among analyzes that depend on horizontal components, including teleseismic receiver functions (Ward and Lin, 2017), dense-array imaging (Schmandt and Clayton, 2013), ambient-noise and surface-wave imaging (Obermann et al., 2022), and the broader range of large-N geophone-array applications (Karplus and Schmandt, 2018), and the Conclusions identify the methods the dataset is intended to support (lines [56–68] and [335–346]).

We would, however, respectfully clarify the purpose of the manuscript. Its aim is not to re-demonstrate these established uses, but to provide the horizontal-component signal/noise foundation those uses require, for a dataset whose configuration is, to our knowledge, unusual. The 2023 Kīlauea deployment co-locates several surface-deployed node designs (spiked and sand-coupled SmartSolo and leveling-leg GTI U3 nodes) with buried HVO broadband sensors under a common, persistently windy setting where permits and geology precluded burial. Prior performance characterizations have typically examined a single instrument type (Ringler et al., 2018; Johnson et al., 2019), whereas the contribution here is the side-by-side comparison across instrument packages and deployment configurations. The station subsets and frequency bands we provide are therefore built specifically for this dataset, so that researchers can select appropriate stations for receiver functions, S-wave picking, moment tensors, attenuation tomography, or interferometry rather than infer suitability from a single sensor type or from deployments under different conditions. We have added text to the Introduction making this design rationale explicit (lines [63–68]).

R1-3: I think there is a technically sound foundation shown in the manuscript, but there is considerable work needed to transform it from a technical report to research paper. If readers understood how different strategies would map into performance for particular method(s) then I think the manuscript could be more impactful.

Recommendation: Resubmit for Review.

R1-3R: We appreciate the reviewer's assessment of the technical foundation, and we have responded to the broader framing in our replies to the two comments above, where we make the case for the study's novelty and add literature context. Here we address the specific point that the manuscript would be more impactful if readers understood how deployment strategies map onto method performance.

We would respectfully suggest that this mapping is the central content of the manuscript, expressed through the variable that governs method performance rather than through end-to-end demonstrations of each method. For every node type and deployment configuration (spiked and sand-coupled SmartSolo, leveling-leg GTI U3, and buried HVO broadband), we quantify horizontal-component S/N as a function of frequency and wind speed and translate it into the station subsets and frequency bands over which each configuration is adequate for specific analyses: interferometry below 1.2 Hz, and S-wave arrival-time picking, moment-tensor, and attenuation work to at least 10 Hz. Because horizontal-component S/N in the relevant band is what ultimately limits these methods, this S/N-based characterization is the strategy-to-performance mapping. Running each method on each subset would illustrate it but would not change which strategies are usable for which analyses, and, as noted above, would exceed the format and constitute separate studies.

To make this linkage explicit for readers, the Conclusions paragraph added in response to the comments above now connects each station subset to the methods it is intended to support (lines [335–346]). We hope this clarifies that the guidance the reviewer is seeking is provided directly, in a form that is portable across the different instrument packages.

That said, we agree the method-specific tests the reviewer describes would be valuable, and we have released the station subsets and per-station H/V S/N estimates precisely so that the community, and we ourselves in follow-up work, can pursue them on a sound footing. We would welcome such studies building on this dataset.

Reviewer 2:

Major Comments

R2-1: This manuscript aims to make a rich and unique dataset more accessible to the community through a detailed analysis of horizontal-component data quality from a dense nodal deployment at Kīlauea caldera, and I commend the authors for taking this on. Careful documentation of horizontal noise on nodes across deployment conditions is genuinely useful, particularly as these types of deployments continue to increase. My main reservation is one of scope and framing. As written, the central result is a categorization of stations by a data-quality metric, with limited discussion of how the findings might generalize to deployment planning in similar settings (e.g., other volcanic deployments) or how they compare to observations from other deployments. The manuscript would be considerably strengthened by developing this broader context.

R2-1R: We thank the reviewer for the generous framing and for engaging so constructively. Several points raised here are addressed by changes made in response to both reviewers, which we draw together and extend below.

We would push back gently on the description of the central result as a per-station categorization by a data-quality metric. The station subsets are the practical output, but they rest on a comparative, physically grounded characterization of horizontal-component performance across several co-located hardware types (spiked and sand-coupled SmartSolo, leveling-leg GTI U3, and buried HVO broadband sensors) under a common, wind-dominated setting. As we also note to Reviewer 1, we considered framing the manuscript as one of Seismica's deployment-oriented report types, but prefer to retain it as a research article: the multi-hardware comparison, and the wind-sensitivity framework that explains it, are a research result rather than a dataset summary.

On generalization to deployment planning, the relative wind-sensitivity estimates (Table 2) are derived from node geometry alone, through a height to half-base-width torque proxy, and are shown to predict the observed H/V amplification relative to the buried HVO sensors (Fig. 4). Because the framework depends on geometry rather than on the particular instruments used here, it transfers to other node designs and to other wind-exposed, burial-restricted settings, including volcanic deployments; Section 7 gives a concrete, transferable surface-deployment strategy for loose materials such as tephra, which the reviewer separately notes is relevant to other volcanic settings. To make this multi-hardware, deployment-planning framing explicit, we have added text to the Introduction and Conclusions (lines [56–68] and [335–346]).

On comparison with other deployments, the Introduction now situates our results among prior performance characterizations, which have largely examined a single instrument type or a limited deployment (Ringler et al., 2018; Johnson et al., 2019; Farrell et al., 2018; Dean and Grant, 2024); the contribution here is the side-by-side comparison across instrument packages under shared conditions (see also our response to Reviewer 1's second comment). We hope these additions provide the broader context the reviewer seeks.

R2-2: This gap is reflected in the reference list, which is currently quite thin (14 references). Two are FDSN DOIs for the temporary and permanent HV networks, one is an unpublished manuscript describing the dataset, two are LinkedIn posts (an original and a comment), three are instrument-manufacturer websites, and one is a Zenodo archive for the author's analysis code. Of the remaining six, most are methodological, and none concern other deployments using the same or similar instruments in different settings. Engaging with that literature would help the authors position their results within a wider context and turn what is currently a missed opportunity into a generalized discussion. As it stands, the manuscript reads more like a technical report than a study of broad interest, and I'm not able to recommend it for publication in its current form. I do think there are several viable paths forward, outlined below, and I'd be glad to see a revised version along any of them.

While this might be a bit outside my role as a reviewer, in the interest of being constructive, here are a few possible pathways that would help in my opinion:

1. Since the overview manuscript for the deployment itself (Kramer et al., 2026, unpublished) does not yet appear to be finalized, could these results be incorporated there instead? This would require streamlining, but if the primary outputs are lists of usable horizontals for different analyses, much of that could likely live in a supplement. This seems like exactly the kind of material that belongs in a manuscript describing the deployment and how to approach analyses with the data.

2. The manuscript has associated code. Could it be reframed as a methods paper presenting that QC-assessment code, using this dataset as a worked example? That framing would support generalization, enabling the community to apply the code more broadly, while still presenting the Kīlauea findings.

3. If the authors prefer to keep the focus on the HV S/N results at Kīlauea, then I'd encourage broadening the manuscript with a discussion section that compares to other deployments and contextualizes the findings. As written, the manuscript largely presents results without a discussion, and in my view a standalone paper on this subject needs that interpretive component.

R2-2R: We thank the reviewer for laying out these options so clearly. We have followed the third path: retaining the focus on the horizontal-component S/N results at Kīlauea while broadening the manuscript with added context and discussion.

On the reference list, we have substantially expanded and modernized it, replacing the LinkedIn posts with peer-reviewed sources and adding studies that characterize nodal and three-component performance in other deployments (Ringler et al., 2018; Johnson et al., 2019; Farrell et al., 2018; Ward and Lin, 2017; Karplus and Schmandt, 2018; Schmandt and Clayton, 2013; Parker et al., 2018; Trow et al., 2018; Dean et al., 2018; Dean and Grant, 2024; Zeckra et al., 2022). As described in our responses to Reviewer 1, the Introduction now situates the work within this literature, and the Conclusions add an interpretive paragraph that connects the station subsets to the analyses they support and identifies how the findings generalize to other wind-exposed, burial-restricted deployments (lines [56–68] and [335–346]). Together these turn what was a results-only presentation into one with the broader context the reviewer requested.

As we also note to Reviewer 1, we considered framing the manuscript as one of Seismica's deployment-oriented report types, but prefer to retain it as a research article, because the multi-hardware comparison and the geometry-based wind-sensitivity framework are a research result rather than a dataset summary.

For completeness, we comment briefly on the other two paths. The submitted deployment-overview manuscript (Kramer et al., 2026) has a different scope, and the detailed horizontal-component assessment provided here would lose its comparative character if reduced to a supplement there, so we keep it as a standalone study. The associated quality-control code is archived and citable (O'Connell, 2026) and is referenced in the manuscript; however, the contribution here is the comparison across co-located instrument packages at Kīlauea rather than a general software or methods paper, which we would regard as a separate effort. We hope the chosen path addresses the reviewer's concerns.

R2-3: One additional note. I have provided line by line comments; however, I refrained from detailed feedback in sections 5-7, as these read more like a technical report on this specific deployment. I believe details here could be streamlined to allow for more space for interpretations in a revised manuscript.

R2-3R: We thank the reviewer for this note. Consistent with the third path described above, we have added the interpretive material the reviewer is seeking, principally in the Introduction and Conclusions, which now place the horizontal-component results in the context of prior node deployments and connect the station subsets to the analyses and settings they inform (see our responses above). We have retained the deployment detail in Sections 5–7, however, because documenting how each instrument and emplacement actually performed is central to the contribution of a deployment-characterization paper, and because, as the reviewer notes in a later comment, the Section 7 surface-deployment technique is itself relevant to other volcanic settings rather than specific to this deployment. We have therefore prioritised adding interpretation over removing documentation, so that the broader balance the reviewer requests is achieved by expanding the interpretive sections rather than by thinning the deployment record that gives the dataset its value.

Minor Comments

R2-4: Lines 12-14: This reads more like a technical report to project collaborators on a dataset, rather than a subject appropriate for a standalone manuscript. Abstract (and manuscript as a whole) would be strengthened by including relevance beyond this specific deployment.

R2-4R: We agree the manuscript should convey relevance beyond this single deployment, and several changes already described address this directly. The Introduction now situates the work within the horizontal-component node literature and sets out the multi-hardware design rationale; the Conclusions add an interpretive paragraph linking the station subsets to the analyses they support and to other wind-exposed, burial-restricted settings; and Section 7 gives a transferable surface-deployment strategy for loose materials such as tephra that is relevant to other volcanic deployments (see our responses to Reviewer 1 and to the scope and reference-list comments above). For the abstract specifically, we have added the generalizable finding that taller, narrower spike-mounted nodes are more susceptible to wind-induced rocking than shorter, wider nodes on leveling legs, a geometry-based relationship that transfers to other node designs and settings rather than being specific to Kīlauea. Together these shift the framing from a deployment-specific summary toward results of broader applicability.

R2-5: Lines 19-21: Consider moving this sentence to the abstract rather than the non-technical summary.

It contains technical information about node-design that a non-expert is unlikely to understand or contextualize. However, the takeaway of how node installation affects instrument performance is useful information to include in the abstract.

R2-5R: We agree. We have removed the node-design sentence from the non-technical summary and added the takeaway to the abstract, which now notes that taller, narrower spike-mounted nodes are more susceptible to wind-induced rocking than shorter, wider nodes on leveling legs. The revised abstract remains within Seismica's 200-word limit.

R2-6: More broadly, I found the abstract and non-technical summary to be repetitive, with too much technical information in the non-technical summary. Another example includes information about where to find the data currently. Another is the similarity between this sentence in the abstract, "We use horizontal-to-vertical spectral ratios (H/V) to assess horizontal-component signal-to-noise ratio (S/N) as functions of frequency and wind speed, and to quantify how node geometry and deployment configuration affect wind sensitivity." and this in the non-technical summary, "Using horizontal-to-vertical spectral ratios (H/V), we evaluate wind-related noise across the network as functions of frequency and wind speed." These two sentences largely say the same thing, using much of the same technical jargon and wording.

R2-6R: We thank the reviewer and take the point. The non-technical summary had carried over technical wording from the abstract (for example, the horizontal-to-vertical spectral ratio sentence) and repeated information, such as where the data reside, that is provided elsewhere. Following Seismica's guidance for non-technical summaries, we have rewritten it for a non-specialist audience: we remove the jargon and the duplicated data-location text and instead emphasize why the dataset matters, what was done in plain language, and the practical impact. The abstract and non-technical summary now serve their distinct audiences with minimal overlap in wording.

R2-7: Line 23: It took me a couple of reads to realize this is consistent with the abstract and indeed four station subsets are discussed in line 12 (three related to S-wave and earthquakes, and one additional for interferometry). Consider adjusting the abstract text to make it clear that these are four distinct subsets. These two sentences are another example of the concern related to similarities between the abstract and non-technical summary.

R2-7R: We have addressed both aspects of this comment. As described in our response to the previous comment, the non-technical summary has been rewritten and no longer enumerates the station subsets, which removes the overlap with the abstract that prompted the cross-reading. In the abstract, we have made the count explicit: it now states that we identify four station subsets, three suitable for earthquake and active-source S-wave detection and picking and one of 1,099 stations suitable for lower-frequency interferometry, so that the four distinct subsets are clear on a first read.

R2-8: Line 31: The manuscript says the data are publicly available, but the citation for Kramer et al., 2026 simply says "unpublished". The data do appear to be available, and the FDSN network code is included in the manuscript, but the citation of this unpublished manuscript is puzzling.

R2-8R: We thank the reviewer for catching this. Data availability is independently established by the FDSN network DOI (network 6F; Denlinger et al., 2023), which is the source we now cite for availability and which the reviewer notes is present. Because the deployment-overview manuscript (Kramer et al., 2026) is not yet published nor preprinted, and Seismica does not permit citing unpublished work without a preprint DOI, we have removed that citation. The deployment details needed here are given directly in this manuscript, and the earthquake count is now attributed to the released earthquake catalogue (Roecker et al., 2026). Should the overview become available before publication, we will be glad to reinstate the reference.

R2-9: Lines 42-44: Consider rewriting this sentence for clarity. As written it states, "These node subsets support [the analyses] by specifying frequency bandwidths where average horizontal-component S/N is sufficient". It's unclear to me how the node subsets themselves "specify frequency bandwidths". Or perhaps there is something implicit that the node subsets are categorized by frequency, but it is unclear within the manuscript at this stage.

R2-9R: We agree the original phrasing was unclear. The node subsets do not "specify" frequency bandwidths; rather, each subset is the group of stations whose average horizontal-component S/N is sufficient over a target frequency band, and that band is what matches it to particular analyses. We have rewritten the passage (lines [69–73]) to state this directly: the subset usable to 1.2 Hz supports interferometry, and the three subsets usable to at least 10 Hz support S-wave arrival-time picking, moment-tensor analysis, and S-wave attenuation tomography.

R2-10: Lines 58-60: This is the third time in two pages where a similar sentence mentioning the variability of ground conditions appears. See lines 4-6, and lines 35-36, the latter of which is in the same introduction section. This is not in isolation; I found the abstract, non-tech summary, and introduction to be repetitive, and this is one key example. But I think the manuscript can likely be shortened in these sections to more efficiently convey the same information.

R2-10R: We agree, and we have removed the redundancy. The statement of varied ground conditions previously appeared in the abstract, in the opening Introduction paragraph, and again where the node types are introduced; it now appears once in the abstract and once, in operational form, in the paragraph introducing the node types, where it motivates the different node designs and deployment methods. The duplicated sentence in the first Introduction paragraph has been deleted, and that paragraph tightened. More broadly, together with the rewritten non-technical summary, we have reduced the overlap among the abstract, non-technical summary, and Introduction so that the deployment context is conveyed once in each section rather than repeated (lines [89–91]).

R2-11: Line 88: It would be helpful to have the geographic reference points of Volcano and "Hali Pali Road" displayed on a map for those unfamiliar with the area. To emphasize this point, I am not sure what Hali Pali Rd is. Perhaps Hilina Pali Rd? Is there a specific location along the road where the wind measurements are taken from, as the distance between the start and end of the road is approximately equal to the distance between Kīlauea crater and the start of the road.

R2-11R: Thank you for catching this. "Hali Pali Rd" was indeed a typo for Hilina Pali Rd, which we have corrected throughout. We have added Volcano and Hilina Pali Rd to the location map (Figure 1) for readers unfamiliar with the area.

R2-12: Lines 94-97: I'm not sure it is appropriate, or at least best-practice, to cite a LinkedIn post and comment in a peer reviewed publication. There are many peer-reviewed publications that have used SmartSolo nodes. I would encourage the authors to draw from this literature for comparisons to instrument performance in prior deployments with different locations and site conditions. If indeed this is the first publication to include substantial documentation of horizontal noise as a function of burial for these instruments, then I think this can stand on its own without a reference to a prior LinkedIn post that is not peer reviewed and is based on very limited data.

R2-12R: We agree, and we have removed the citations to the LinkedIn post and its reply (lines 94–97 and line 144 of the marked manuscript). The underlying mechanism is well established in the peer-reviewed literature, and we now ground the discussion there. Compact 3C nodes carry their horizontal geophones high in the casing, so a node resting on the surface, even on its spikes, is poorly coupled and tends to rock under wind and ambient loading, which degrades the horizontal components and is a primary motivation for burial (Dean et al., 2018). This rocking produces a coupling resonance within the seismic band that falls preferentially on the horizontal components and appears as an anomalous peak in spectral ratios such as H/V (Dean and Grant, 2024); burial suppresses it by removing the room for the casing to rock. Because permits precluded burial at Kīlauea, our ∼1,576-node EPIC SS dataset provides what is, to our knowledge, the first substantial documentation of this horizontal-component artifact as a function of deployment configuration for these instruments, so the discussion now stands on that basis rather than on the prior LinkedIn exchange. The manufacturer specification that the IGU-16HR-3C is intended for buried deployment is retained with a citation to the product datasheet (SmartSolo Inc., 2026).

R2-13: Figure 2: Can these images be used and are references / credit needed? At least Panel (a) looks to be a lightly modified version of official SmartSolo product images (a quick image reverse google search returns this at the bottom of the page `https://blog.smartsolo.com/case-studies/smartsolo-nodes-paving-the-way-for-the-einstein-telescope/`). I didn't check the others, but they appear to be similar visually.

R2-13R: Thank you for catching this. The node images in Figure 2 are manufacturer product images. We have obtained written permission from SmartSolo Inc. (panels a and b) and Geophysical Technology, Inc. (panel c) to reproduce them, and have added a credit line to the caption. These images are reproduced with permission and are noted as third-party material excepted from the article's CC-BY license.

R2-14: Line 129: Please clarify how these are defined relative to the buried HVO stations. Is a buried station implicitly a value of 1 in this relative scale? Please state this explicitly in the manuscript. Also, are all the HVO permanent stations buried? I thought some locations at Kīlauea summit had vault or other variations in installation due to the challenging terrain. Is that considered here? Or are the HVO stations only the buried subset?

R2-14R: We appreciate this and have clarified both points. On the relative scale, the Table 2 wind-sensitivity ratios are geometric proxies (node height divided by half the base width) comparing the surface-deployed node types; the permanent HVO network stations are not on this geometric scale. The unity reference applies instead to Fig. 4b, where node H/V is expressed relative to the HVO network stations, so the network baseline is one by construction. We now state both points in the Table 2 caption, and we have distinguished the surface "HVO SS 0.2 Hz nodes" from the permanent "HVO network stations" to avoid ambiguity. On installation, the reviewer is correct that the permanent HVO network stations are not a uniformly buried set; they are well-coupled permanent installations, predominantly in vaults or shallow boreholes, with UWE on a concrete pedestal pier. Because the property relevant to the baseline is good coupling and the absence of wind-induced rocking rather than burial per se, we have replaced "buried" with "well-coupled permanent" where we describe the network reference (Fig. 3 caption and Section 3).

R2-15: Lines 133-134: "The ln mean H/V of the ln mean H/V curves" is confusing. Please consider rephrasing for clarity, perhaps explicitly mention a mean across frequencies if that is the correct interpretation?

R2-15R: We have rephrased this for clarity. The quantity is the log-domain mean, taken across the 12 HVO network stations, of their individual ln mean H/V curves, and it remains a function of frequency rather than a mean across frequencies. The revised sentence reads: "Averaged in the log domain across the 12 stations, the per-station ln mean H/V curves define a network baseline H/V curve (a function of frequency) for comparison to H/V calculated from nodes located on ground surfaces."

R2-16: Line 144: Break up run-on sentence. Same concerns about a LinkedIn post citation detailed in the Line 97 comment.

R2-16R: We have split the run-on sentence at line 144 into two sentences for readability. As above, we have removed the LinkedIn citation here and replaced it with the peer-reviewed references noted in our response to the Line 94-97 comment.

R2-17: Lines 150-152: How is the decoupling identified as opposed to other noise sources? A brief description of what metric in the data is used to discriminate between noise sources would be helpful.

R2-17R: We have added a sentence describing how these categories are discriminated in the H/V. Spike resonances appear as narrow peaks at characteristic frequencies, strong wind sensitivity elevates the higher-frequency H/V, and low-frequency noise is band-limited, whereas near-complete decoupling produces anomalously low, broadband H/V that departs from the rest of the spiked-node population.

R2-18: Section 7: I think this section is useful, as it has relevance to deployment techniques for environments that may be encountered in other volcanic settings. However, I found myself wishing that there were photos or perhaps diagrams (if photos don't exist) of the deployment technique. Some visual figure to accompany the description of the deployment technique would be helpful.

R2-18R: We appreciate this suggestion and agree the deployment strategy is the part of this work most transferable to other volcanic and burial-restricted settings. No photographs or diagrams of the tephra-deployment technique were taken in the field, and none exist to reproduce; the procedure was a simple, improvised field practice rather than an engineered installation. In place of a figure, we have expanded the prose in Section 7 so the technique is conveyed more concretely: the guiding principle is to seat each node in soil or tephra rather than directly on solid basalt wherever the surface allows, banking whatever loose material is at hand against the base to aid leveling and damp rocking, with the practical caveat that available tephra was often thin or patchy. Figure 8 shows the GTI Area 800 sites and the resulting broadband H/V.

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Decision Letter Response: “2023 Kīlauea Horizontal-Component Signal/Noise Assessment of a Large Three-Component Nodal Array”

August 14, 2026

We would like to express our appreciation to the Editor and the Reviewers for their insightful comments and suggestions, which have aided us in improving the quality of the manuscript. We have modified the paper carefully according to the provided comments and suggestions. Revisions in the marked version are indicated by a red font; the previous wording is indicated by a blue font with a strike through the prose. Our responses to individual comments in this response document may be found below in blue font. **Note: the line numbers mentioned in the response to individual comments correspond to the marked version.**

Regards,
The Authors

Response to the Editor (Oliver Lamb)

Thank you for your and the reviewers' assistance. With these changes we have addressed the reviewers' comments.

The review comments helped us expand the scope of this manuscript to include comparisons with other analyses of field data, which better helps readers, and particularly potential users of the public Kīlauea data set, efficiently avoid data pitfalls not documented in the data release documentation or elsewhere. Our concern is that, lacking this information, investigators may waste significant time processing tens of terabytes of 3-C data to constrain shallow structure, unaware of instrument artifacts that mask actual site-specific H/V signals at frequencies where they expect all nodes to perform well. This review process has improved a critically important document, and one that is late relative to the public release of these data in November 2025.

On the question of article type, we think the revised manuscript now stands as a Research Article rather than solely a data report. The station subsets are a quality assessment and would not, on their own, constitute an advance in understanding. They are not the finding. The finding is that horizontal-component wind noise on surface-deployed nodes is predicted, to first order, by node geometry through a height to half-base-width torque proxy (Table 2, L147–169), and that this prediction can be tested against observation because the 2023 Kīlauea deployment placed four surface configurations and 1,799 instruments alongside 12 permanent below-grade sensors under a common wind regime (Fig. 4b, L181–183). That result is not specific to this data set: it applies to any future deployment that must place multiple node designs on the surface under logistical constraints, and it is now stated with explicit limits on what the geometry alone does and does not account for (L162–169). The data set remains the occasion for the work, but it is no longer the whole of the contribution.

Reviewer 1:

Major Comments

R1-1: I appreciate the generally constructive response. I think the main points are clearer and better placed in context. I still think they could be made with an article (text and figures) about half the size and that would leave room for some quantitative testing of how deployment performance translate to attributes like S-wave picks, moment tensors, attenuation, etc. Yet, I also think the authors should have flexibility to maintain more a technical report format if it is at least well mapped to the related literature on applications. So I would not want to hold up publication on that element of study design. I'll note that enhancing the basic research context is not only a matter of giving credit to related research; thoroughly linking to the literature helps other authors find this work and related analysis approaches that can benefit their future research.

R1-1 R: Thank you for the insightful comments.

R1-2: Janiszewski et al 2024 SRL compared the BD3C-5 with Hawaii permanent network short period and broadband sensors in an overlapping field area, including tests using sandbags to aid surface deployment. Their noise analysis certainly seems worth integrating into this text. What aspects are consistent with this closely related prior study? Does this study reveal any different insights about those instrument and field procedures?

R1-2 R: Thank you for pointing us to Janiszewski et al. (2024), which is clearly the closest prior work to ours. Their deployment used the same IGU-BD3C-5 that we call the HVO SS 0.2 Hz node, in an overlapping area, against many of the same HVO permanent stations, which makes it a useful check on several of our claims.

We have added a paragraph at the end of Section 4 (L208–225) stating where the two studies agree and where this work diverges. Three results line up well. Janiszewski et al. (2024) attribute elevated summit-station noise to near-constant volcanic tremor at 1 to 5 Hz, the same band we identify. Their surface nodes are noisier than the below-grade permanent sensors preferentially on the horizontal components, and their surface-installed permanent broadband sensors are noisier at long periods than the vault, buried, and borehole installations, both of which are what we would expect if poorly coupled surface sensors are rocking. Their IGU-BD3C-5 horizontal noise also climbs steeply near and beyond the 5 s corner, matching the rise in HVO SS 0.2 Hz node H/V from 0.3 Hz to 0.11 Hz that we observe and that none of the other node types show.

Where we differ is that wind is a significant noise source in our data, and we think that difference is configuration-specific rather than a contradiction. Their nodes were low-profile, sandbagged, unspiked, and placed in accumulated soil chosen for shelter, whereas our grid geometry required placing nodes regardless of exposure and most of ours were taller spiked units; we find that wind sensitivity scales with node geometry. We also note that total-power comparisons mix site, source, and instrument effects across both components, whereas H/V conditioned on wind speed isolates the horizontal degradation that rocking produces. Janiszewski et al. (2024) explicitly asked for a direct comparison of instrument types and installation methods under common conditions, and our four surface configurations plus the 12 below-grade stations amount to roughly that, so we have framed the new paragraph accordingly.

We have also added the citation to the prior-work list in the Conclusions (L365) and to the Introduction (L48–50).

R1-3: Moment tensor inversion is mentioned as an application requiring higher data quality but little detail is given and there are no references to examples. Most moment tensor inversion with surface deployed seismometers (rather than borehole) is conducted with frequencies well below the corner of multiple instruments used here so more context seems warranted. Are the authors anticipating use of body waveforms, surface waveforms, first motion polarities or some mixture? I agree that it's an important application that requires good quality 3-C data. I'll note that recently Yang and Wang 2025 SRL specifically introduced a method for moment tensor inversion intended to help with dense nodal arrays.

R1-3 R: The reviewer is right that we named moment-tensor inversion without saying what we meant, and right that the band used in regional moment-tensor practice sits below the response corners of every node type deployed here. We are grateful for the pointer to Yang and Wang (2025), which resolves the ambiguity: what we anticipate is far-field body-wave inversion using P-wave polarities, P-wave amplitudes, and S/P amplitude ratios, not long-period waveform fitting. We have added a paragraph in Section 6 (L324–339) that says so and ties the requirement to specific station subsets. NaNDC and its application by Xin et al. (2026) are directly instructive here, since the latter used a 4 to 30 Hz passband on 29 nodes of the same SmartSolo IGU-16HR 3C model as our EPIC SS 5 Hz nodes. That band coincides with the band in which S-wave arrivals are detectable in our data, and the S/P ratios are three-component measurements that depend on faithful horizontal amplitudes, which is exactly what our subsets are constructed to identify. Two points from our results now appear explicitly. Xin et al. (2026) buried their nodes at about 20 cm, whereas permits precluded burial for us, so station selection by H/V S/N takes the place of burial and the 15 horizontally decoupled nodes in the Supplement should be excluded. Also, a 4 to 30 Hz passband would run through the 12 to 35 Hz spike resonances we document, which would bias the P and S amplitudes such methods rely on, so we note that the 610- and 486-station subsets exclude nodes whose spike resonances extend below 10 Hz and that narrowing the passband is advisable. We retain a brief note that lower-frequency point-source waveform inversion remains possible on the 1,099-station subset from roughly 0.25 to 1.2 Hz, with the 1 to 5 Hz tremor and Green's function accuracy as the limiting factors.

R1-4: Seismic attenuation studies with temporary geophone arrays are even rarer and hopefully an area of greater future application. One notable example with passive estimates of attenuation from a 3-C nodal array is Liu et al 2022 GRL who studied fault zone attenuation.

R1-4 R: We agree, and we thank the reviewer for pointing us to Liu et al. (2022), which we now cite in the Conclusions (L373–381). It is an apt example, since their Love-wave attenuation tomography rests entirely on horizontal-component amplitudes from a three-component nodal array, and it is the Love-wave result that resolves the asymmetric damage zone. We have added a short passage noting that passive attenuation estimation from temporary three-component nodal arrays is still uncommon, and distinguishing the two routes available with our data. Interferometric surface-wave attenuation of the kind Liu et al. (2022) apply needs stable horizontal amplitudes within the interferometry band, which the 1,099-station subset supports from roughly 0.25 to 1.2 Hz. Body-wave S-wave spectral attenuation instead needs reliable amplitudes across the fitted band, where wind-driven noise and spike resonances both bias the result, so the 486-station subset is the appropriate starting point. This is a case where the frequency-dependent H/V S/N estimates are directly useful, since they indicate station by station which band can be trusted for horizontal amplitudes.

Minor Comments

R1-1: Please ensure consistency in the appropriate change from PASSCAL to EPIC, including figure legends, captions, and text (see Figure 1 legend for at least one exception).

R1-1 R: Thank you for finding this oversight. It has been corrected throughout. In the manuscript this affected the Figure 1 legend and the legends of Figures 2, 4, 6, and 7, together with the Figure 5 caption. In the supplement it affected the data-file names, the Table S1 node-type labels, and Figures S1–S5, S8–S12, and S15–S23

R1-2: There many areas where supplement figure calls have "??"

R1-2 R: Thank you for finding this. The supplement was renumbered to a single S1–S23 sequence and the manuscript cross-references now resolve (L186, L188–190, L245–257, L262–266, L269–271, L337).

Reviewer 2:

Major Comments

R2-1: I appreciate the effort of the authors in this revision, and their perspective on the contribution of this manuscript to the broader seismological community. However, I still have concerns regarding the scope and framing of this manuscript for consideration as a Research Article to Seismica. I do not think that the revisions sufficiently contextualized the results within the broader literature. That is not to say that there isn't value in quality assessments of stations for users of this data; however, without broader contextualization, the impact is limited and, in my opinion, not appropriate on its own for a research article that "present advances in scientific knowledge or understanding".

R2-1 R: We appreciate the reviewer's willingness to engage with the framing question directly, and we have added material in this revision that we hope addresses the concerns. We would, however, distinguish two things that our previous framing conflated. The station subsets are a quality assessment, and we agree that on their own they would not constitute an advance in understanding. They are not the finding. The finding is that horizontal-component wind noise on surface-deployed nodes is predicted, to first order, by node geometry through a height to half-base-width torque proxy (Table 2, L147–169), and that this prediction can be tested against observation because the 2023 Kīlauea deployment placed four surface configurations and 1,799 instruments alongside 12 permanent below-grade sensors under a common wind regime (Fig. 4b, L181–183). We are not aware of a prior study with the sample size or the configuration diversity to perform that test, and Janiszewski et al. (2024) explicitly called for a direct comparison of instrument types and installation methods under common conditions. The station subsets are the applied product of that result rather than its substance. In this revision we have added a direct comparison of results with Janiszewski et al. (2024) in Section 4 (L208–225; see our response to Reviewer 1), a statement in Section 2 of why H/V was chosen over the alternatives and what it offers (L86–96), and an explicit statement in Section 3 of what the geometric proxy does and does not account for (L160–169). We have not restructured the manuscript further, since we believe the result was present but understated rather than absent, and we would rather sharpen its statement than expand the paper.

R2-2: For example, the revisions state the following:

"The Introduction now situates the work within the horizontal- component node literature and states the multi-hardware design rationale explicitly: the 2023 K=ilauea deployment co-locates several surface-deployed node designs (spiked, sand-coupled, and leveling-leg) with permanent HVO broadband sensors under a common, persistently windy setting, so that the comparison across instrument packages, rather than the dataset alone, is the contribution. The Conclusions now add an interpretive paragraph that positions the results relative to prior single-instrument characterizations, connects each station subset to the analyses it is intended to support (interferometry, S-wave arrival-time picking, moment-tensor inversion, and S-wave attenuation tomography), and identifies how the geometry- based wind-sensitivity framework generalizes to other wind-exposed, burial-restricted deployments."

However, the added text to the introduction and conclusions simply generally cites the increasing use of nodal deployments, and that previous studies investigating nodal data quality are often limited to single node types or are in controlled settings. While that does help to explain where this investigation sits within the broader nodal literature, I did not see any discussion or comparisons of actual results from other studies (apologies if I missed it). I was hoping to see a much more expanded comparison and discussion related to the generalizability of this study's findings beyond this deployment to address my original review concerns. For example, do other settings have similar relationships with wind noise, or decoupling problems – or is this something specific to the Hawaiian setting? What other methods are used to identify such issues? Are there advantages that this H/V approach offers?

R2-2 R: This is a fair criticism of the previous revision, which described where the work is placed without comparing results to anything. Three additions address it, all in existing sections.

On comparison of actual results, Section 4 (L208–225) now compares our findings directly with Janiszewski et al. (2024), whose deployment used the same SmartSolo IGU-BD3C-5 that we call the HVO SS 0.2 Hz node, in an overlapping area and against many of the same HVO network stations. We compare their attribution of elevated summit noise to 1–5 Hz volcanic tremor with ours, their observation that surface nodes are noisier than below-grade sensors preferentially on the horizontal components and that surface-installed permanent broadband stations are noisier at long periods than vault, buried, or borehole installations with our geometric wind-sensitivity result, and their steep rise in IGU-BD3C-5 horizontal noise near the 5 s corner with the increase in HVO SS 0.2 Hz H/V from 0.3 to 0.11 Hz that we observe and that no other node type shows.

We also identify where the two studies diverge and why (see our response to Reviewer 1 for the full text). On whether these behaviors are particular to Hawai'i, we have added a sentence to Section 3 (L126–129) noting that the underlying mechanism is not: rocking of poorly coupled surface sensors appearing preferentially on the horizontal components is reported for compact three-component nodes by Dean and Grant (2024) and for near-surface broadband sensors across a range of non-volcanic settings by Dybing et al. (2019). What is specific to this deployment is the magnitude and the onset frequency, which depend on the persistent trade winds, the platy lava and low-cohesion tephra substrates, and the 1–5 Hz volcanic tremor, and the manuscript is now explicit on that point.

On alternative methods and the advantages of H/V, we have added a short passage at the start of Section 2 (L86–96). Power spectral density and probabilistic PSD comparisons against Peterson (1993), co-located reference-sensor tests, three-channel coherence for self-noise, and correlation with co-located anemometry are the usual approaches. H/V was chosen here because it requires neither a co-located reference sensor nor absolute calibration, which is a practical advantage for an array of 1,799 instruments and matters given the gain discrepancies Janiszewski et al. (2024) documented for this instrument family; because it normalizes source and path effects common to all three components, leaving the horizontal-versus-vertical difference in which rocking appears; and because it yields site-response information as a byproduct. We also state its principal limitation, that H/V alone cannot distinguish a genuine site resonance from a coupling resonance, and that the wind-speed conditioning introduced here is what resolves such ambiguity.

R2-3: Another example:

In the response to review the authors state, "On generalization to deployment planning, the relative wind-sensitivity estimates (Table 2) are derived from node geometry alone, through a height to half-base-width torque proxy, and are shown to predict the observed H/V amplification relative to the buried HVO sensors (Fig. 4). Because the framework depends on geometry rather than on the particular instruments used here, it transfers to other node designs and to other wind-exposed, burial-restricted settings, including volcanic deployments." Is the transferability demonstrated anywhere in the manuscript, either quantitatively or based on similarity of results from prior literature? I understand that this is a physics-based calculation, but I do not think it takes things like ground characteristics, surrounding environment, etc into account. The deployment area, as stated elsewhere in the manuscript, has very unique ground characteristics, and is substantially exposed. I think the transferability of the results needs to at least be discussed or demonstrated in some capacity in the manuscript.

R2-3 R: The reviewer is right, and we should clarify that the sentence quoted appeared in our `response letter` rather than in the manuscript. The manuscript describes the wind-sensitivity ratios only as first-order predictions of H/V amplification relative to the permanent HVO stations (L160–161), and makes no claim of demonstrated transfer to other settings. The letter overstated this, and we have not repeated the claim here.

We have instead added an explicit statement of what the proxy does and does not account for, in Section 3 where the ratios are introduced (L162–169). The proxy is geometric and omits substrate stiffness, spike-soil interaction, and local wind exposure, all of which the reviewer correctly notes are unusual in this deployment area. Our own results bound how much those factors matter without our needing new analysis, because the manuscript already contains the relevant contrasts within single node types: GTI U3 nodes in area 1500, sited in a narrow channel that locally amplified wind speed, were the most degraded GTI area (L252–254; Supplemental Fig. S9), while GTI nodes seated in tephra in area 800 recorded the longest-offset vibroseis S-wave arrivals in the experiment (L354–357; Fig. 8); bucket nodes on the hill adjacent to area 1500 amplified from frequencies as low as 0.6 Hz (L254–256), whereas spiked nodes on graded road shoulders were among the least affected in the array (L315–318; Fig. 7a,c). These contrasts span a wide range without reversing the ordering of node types predicted by geometry, and that is now the sense in which we claim the framework is useful: it ranks configurations, it does not predict absolute noise levels or onset frequencies. We state plainly that the proxy has not been tested for substrates outside the range sampled here or for different wind regimes, and that the onset frequencies we report should not be expected to transfer even where the ranking does.

Minor Comments

R2-1: There are also minor errors throughout the manuscript that need to be addressed prior to resubmission. - The supplemental figure references did not come through correctly in the submitted manuscript. - Acronym introductions are repeated. - Figure 7 have a reference in panel d to a non-existent Figure 9b.

R2-1 R: The supplemental figure references are fixed (see above). One repeated acronym has been corrected; definitions in the abstract and introduction are retained as standard. The Figure 7d panel legend now reads "From Figure 7b" rather than 9b.

Thank you for finding these mistakes. They have been fixed.

R2-2: Overall I strongly encourage the authors to consider more substantial revisions and additions.

R2-2 R: We have taken this encouragement seriously in the present revision, which adds a direct comparison of results with the closest prior study (L208–225), a statement of the alternatives to H/V and its relative advantages and limitations (L86–96), evidence that the coupling mechanism is not specific to this setting (L126–129), an explicit statement of the geometric proxy's scope and untested cases (L162–169), and contextualization of the moment-tensor and attenuation applications against recent method papers (L324–339, L373–381). We have chosen to sharpen the statement of the result rather than expand the manuscript further, for the reasons given in our response to the reviewer's first major comment.

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