Response to Reviewers

Manuscript ID: adk3942

Title: Source mechanism of impulsive seafloor events that track submarine lava flows

First Author: Peifeng Wang

Dear Editor and Reviewers,

We thank you for your careful reading of our manuscript and for the thoughtful and constructive comments and suggestions. We have carefully revised our manuscript in response to all points raised. Below, we provide a point-by-point response to each comment.

Associate Editor Comments

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Comment 1: I found the clustering of the events quite interesting, and I think it would be very valuable to more completely document the clustering analysis. First, I don't think that you make very clear why you even undertake the clustering. I'm assuming that it is to produce stacked waveforms that allow for clear evaluation of (composite) first motions, as the individual events are too low SNR -- true? Regardless, include some statement on why you limit the analysis to these 2300 events clustered events, out of over 22,000 cataloged explosive events. Second, the spatio-temporal relationship of the events within each cluster is not clear. Adding a zoomed-in panel to Figure S2, focused on the primary northern events (i.e. eliminate the 3 outlying events) would allow for a better visual assessment of whether there is any spatial organization of the clusters. I'd also add an addition panel to show the events in time -- maybe mean displacement (or magnitude) versus time using the same colors/symbols, or something similar. Finally, just how different are the waveforms for each cluster? Figures S3 and S9 give one-cluster views. How about single-panel figure that shows the stacked waveform for all 30 clusters for one representative station? These details are not critical to your focal-mechanism analysis and subsequent modeling, but you do rely on the simplicity and repeatability of the clusters to dismiss more geological sources (line 162-163), so providing a bit more information on the character and nature of the clustering seems useful -- at minimum in the Methods and/or Supplement. Finally, I suggest providing your catalog of clustered events as a data product.

Response: Thank you very much for your thoughtful and detailed comments on our manuscript. We appreciate your suggestions and have made several revisions to clarify and expand our description of the clustering analysis.

First, regarding the purpose of the clustering: You are correct in your assumption. As suggested, we have now explicitly stated the motivation in the revised manuscript at lines 375-379:

"*Clustering is performed primarily to facilitate stacking to improve the signal-to-noise ratio of the waveforms for robust first-motion determination at all stations. Only clusters with more than 40 events (i.e., the top 30 clusters) were analyzed to ensure the stacked waveforms had* *sufficiently improved signal-to-noise ratio for robust analysis.*"

Second, in response to your suggestion to better visualize the spatiotemporal relationships of clustered events, we have revised and expanded the figures as follows:

(1) Figure S2 has been updated to include: ① A zoomed-in panel focusing on the Northern Rift Zone (panel B), excluding the three spatial outliers; ② A new time series panel (panel C) showing maximum displacement amplitude versus time, using consistent color/symbol coding for each cluster.

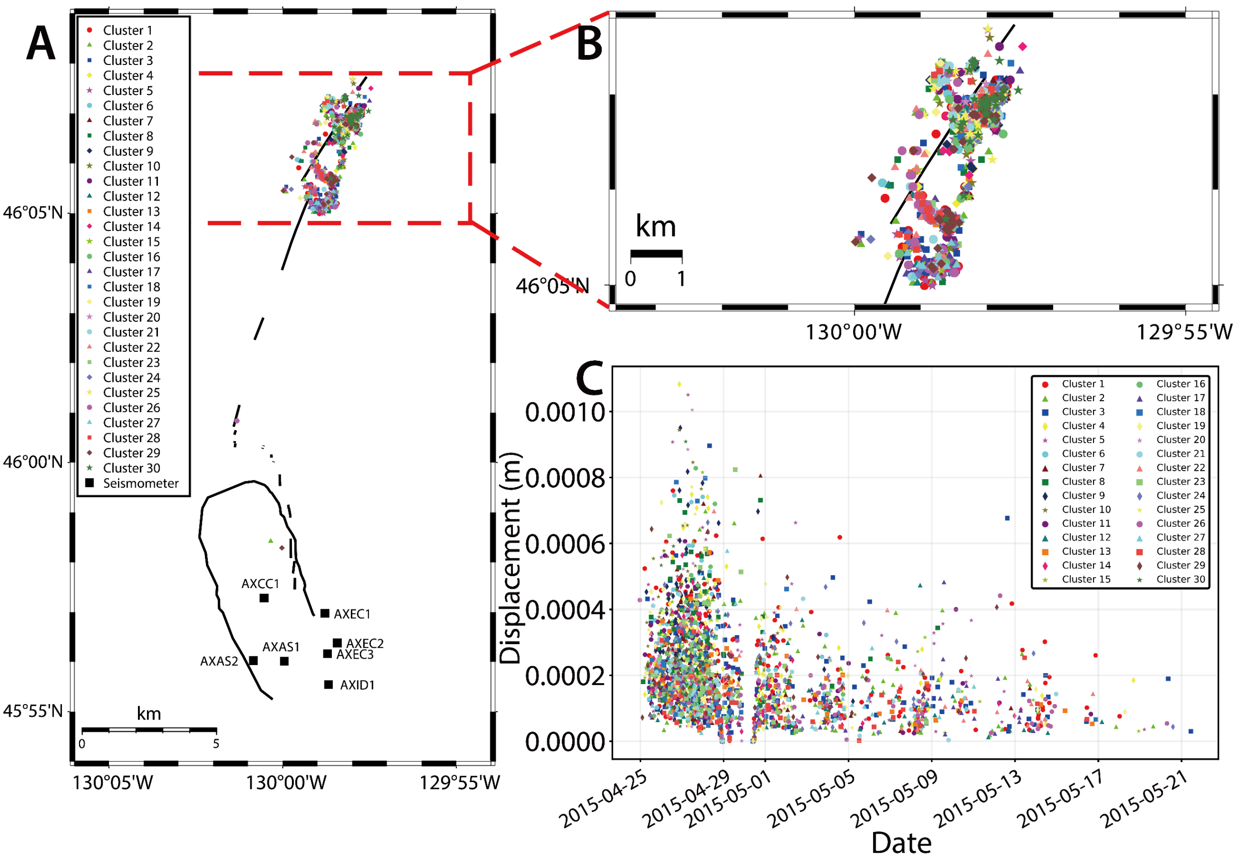

Fig. S2. Spatial and temporal distribution of impulsive events within the 30 identified clusters. (A) Map showing the locations of impulsive events associated with each of the 30 clusters, color-coded and symbolized as indicated in the legend. Ocean bottom seismometers are shown as black squares. (B) Zoom-in of the northern rift zone (marked by the red dashed box in panel A). (C) Maximum absolute seismic displacement amplitude for impulsive events within the 30 identified clusters over time, with each event color-coded and symbolized by cluster as in panels A and B.

(2) Figure S3 has been added to improve spatial clarity within each cluster. It presents spatial distribution of events for each of the 30 clusters in individual subpanels, all using the same latitude-longitude bounds as Fig. S2B. This facilitates clearer visualization of intra-cluster spatial structure that is difficult to resolve in the combined map view.

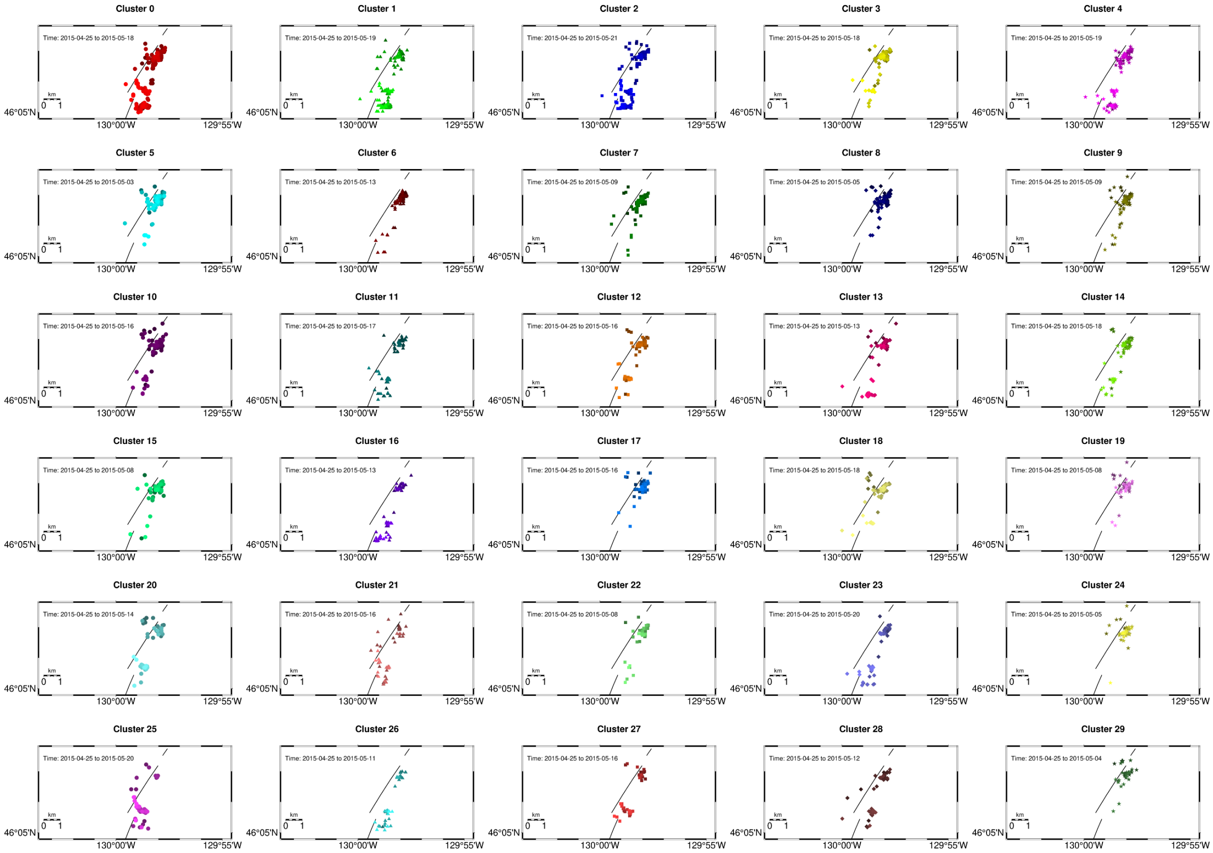

Fig. S3. Spatiotemporal distribution of impulsive events within each of the 30 identified clusters. Each subpanel displays events from a single cluster within the same latitude-longitude bounds as panel B in Fig. S2. Event symbols and colors correspond to the cluster coding used in Fig. S2, with the colors adjusted from bright to dark to indicate the earliest to latest occurrence times, respectively.

(3) Figure S4 presents the temporal evolution of maximum absolute displacement amplitude of impulsive events within each cluster in separate subpanels, complementing the global view in Fig. S2C and allowing more detailed assessment of time-dependent behavior within each cluster.

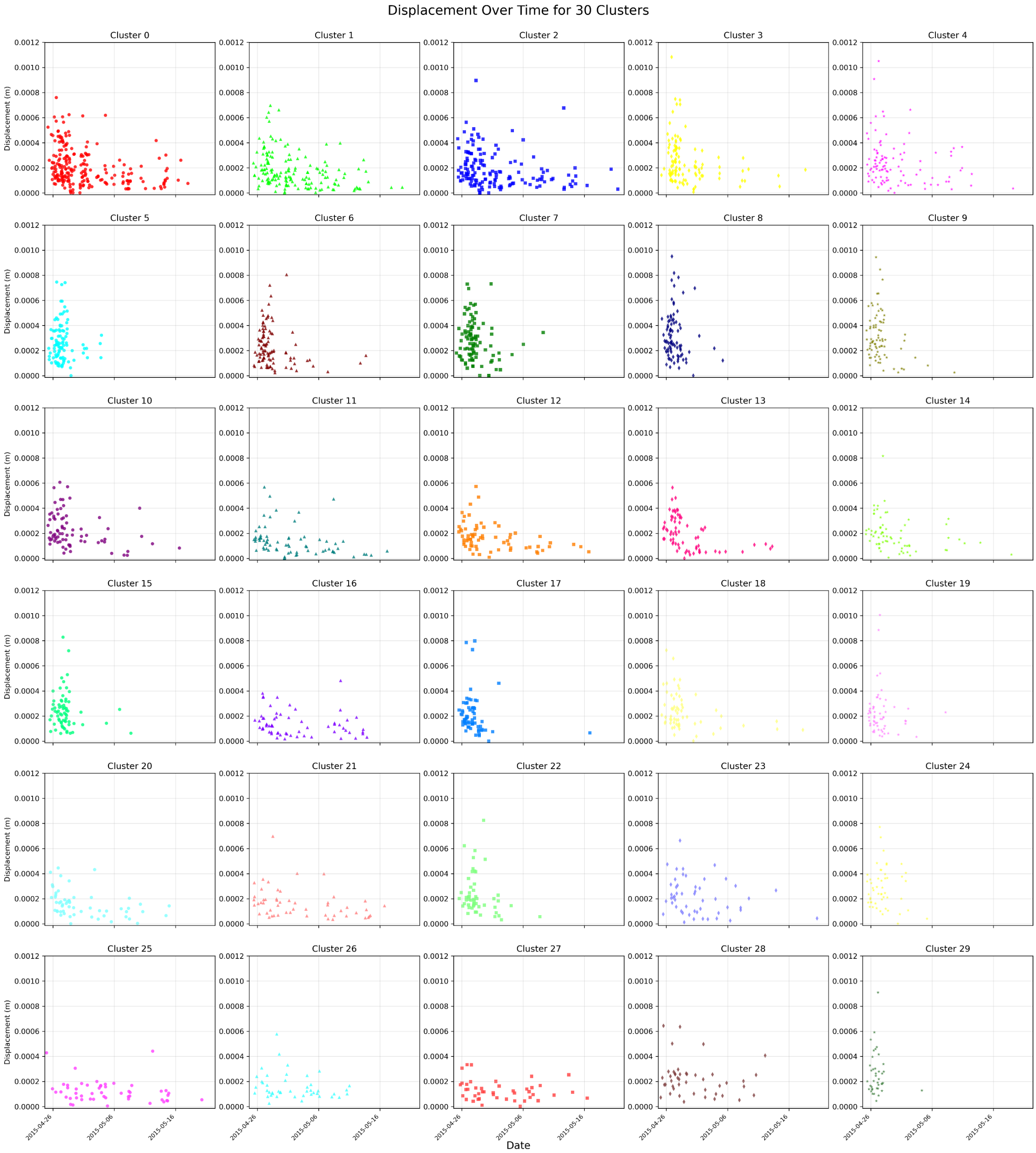

Fig. S4. Temporal distribution of maximum absolute seismic displacement amplitude of impulsive events within each of the 30 identified clusters using the same color and symbol scheme as in Figs. S2.

Third, to address your question about waveform similarity across clusters, we have added a new supplementary figure (Fig. S5), which presents stacked waveforms for all 30 clusters at different stations in a single panel. This allows for direct visual comparison of the waveform characteristics between clusters. We have also updated the main text at line 166 to reference the new Fig. S5 in our discussion of source repeatability:

"*Furthermore, repeating impulsive events with highly similar waveforms (Fig. S5) suggest a non-destructive, recurring source...*"

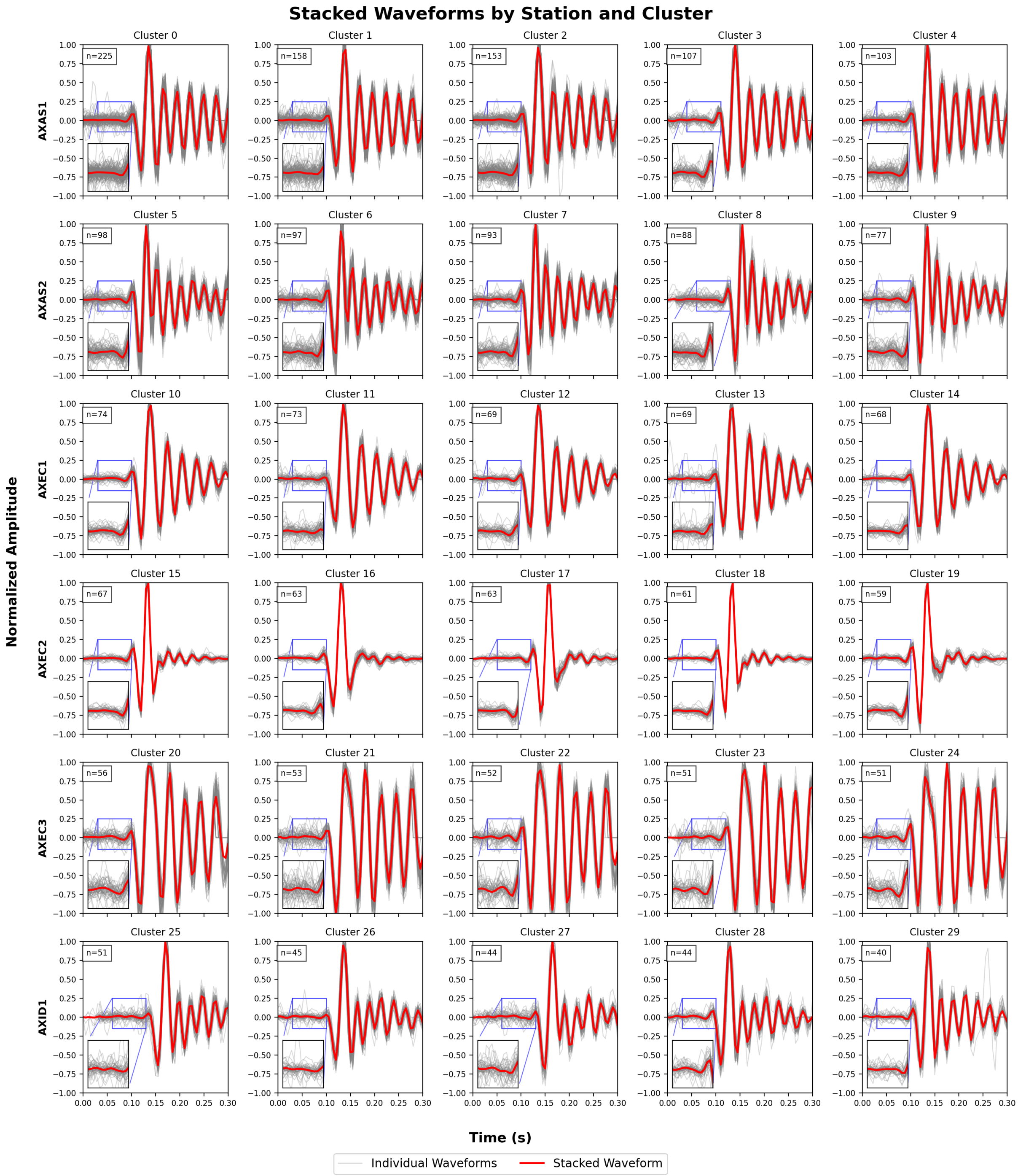

Fig. S5. Stacked vertical-channel seismic waveforms (4-40 Hz band-pass filter) for each of the 30 clusters recorded by different stations. The value of n in each cluster indicates the number of waveforms included in that cluster.

Finally, in response to your suggestion, we have provided the full catalog of clustered events as a supplementary data product (Data S1).

Comment 2: My second comment relates to the bubble model - specifically the “bias” in the result that may result from the fact that the model is based on bubbles in water, while yours are in lava (line 240-241). It seems that a bit more discussion of this caveat is warranted?

Response: Thank you for your insightful comment regarding the potential bias introduced by applying a bubble model developed for water to bubbles confined within lava. We agree that the large differences in physical properties between water and lava—particularly viscosity and density—can substantially affect bubble dynamics.

To address this important point, we have expanded the discussion in the main text (lines 256-261) as follows:

“*The much higher density of lava compared to water is expected to lower the natural frequency for a bubble of a given size. As a result, using the Minnaert equation with water properties to infer bubble radius from the observed frequency likely leads to an overestimation. Due to the lack of detailed models for bubble oscillations in lava, our calculated bubble radii and the resulting total bubble volume* ($V\_bubbles = 5.0 \times 10^5\ m^3$) *should be treated as an upper bound estimate.*”

Comment 3: Finally, the intro and conclusions suggest a broad applicability to the analysis for monitoring of the ridge system, and I was left wondering about detectability of these types of events (reviewer 1 touches on this as well) on hydroacoustic and/or OBS systems at larger distances. Some comment seems appropriate.

Response: Thank you for your valuable comment regarding the detectability of impulsive events at larger distances using seismic (OBS) and hydroacoustic networks. We agree that this is a critical aspect for assessing the broader applicability of our approach to ridge monitoring.

To address this point more clearly, we have revised the manuscript to include a more detailed discussion of detection range given the network at Axial Seamount, as well as for several other ridge and volcanic settings. Specifically, we now emphasize that some impulsive events in both the caldera and the North Rift Zone were clearly observable at station AXBA1 which is located ~28 km and ~37 km from these events. We now provide new figures (Fig. S12 and S13) that shows clear detections of impulsive events at such distances at Axial Seamount.

We have also expanded our comparison with prior studies to better contextualize the detection ranges for different deployments. For example, at Fani Maore, hydrophones suspended in the SOFAR channel detected events at distances up to ~58 km. At the East Pacific Rise, densely spaced OBSs recorded events at distances up to ~12 km. At the Gakkel Ridge, seismometers deployed on an ice floe detected events at distances up to ~81 km away. These examples underscore the influence of instrument type, array geometry, and environmental factors on detection capabilities.

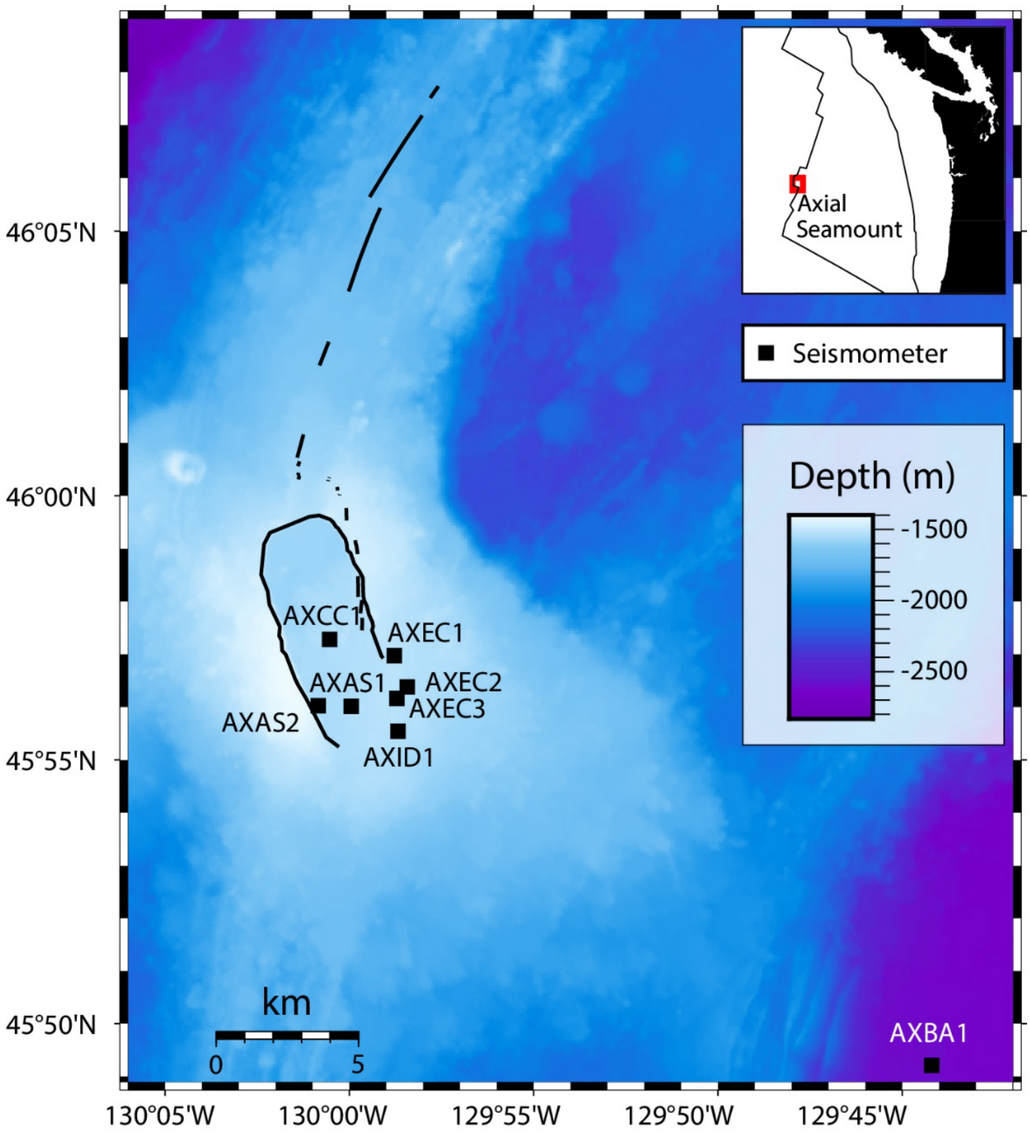

Fig. S12. Distribution of seismic stations around Axial Seamount. Seven stations are located inside the caldera, while AXBA1 is located ~22 km southeast of the caldera.

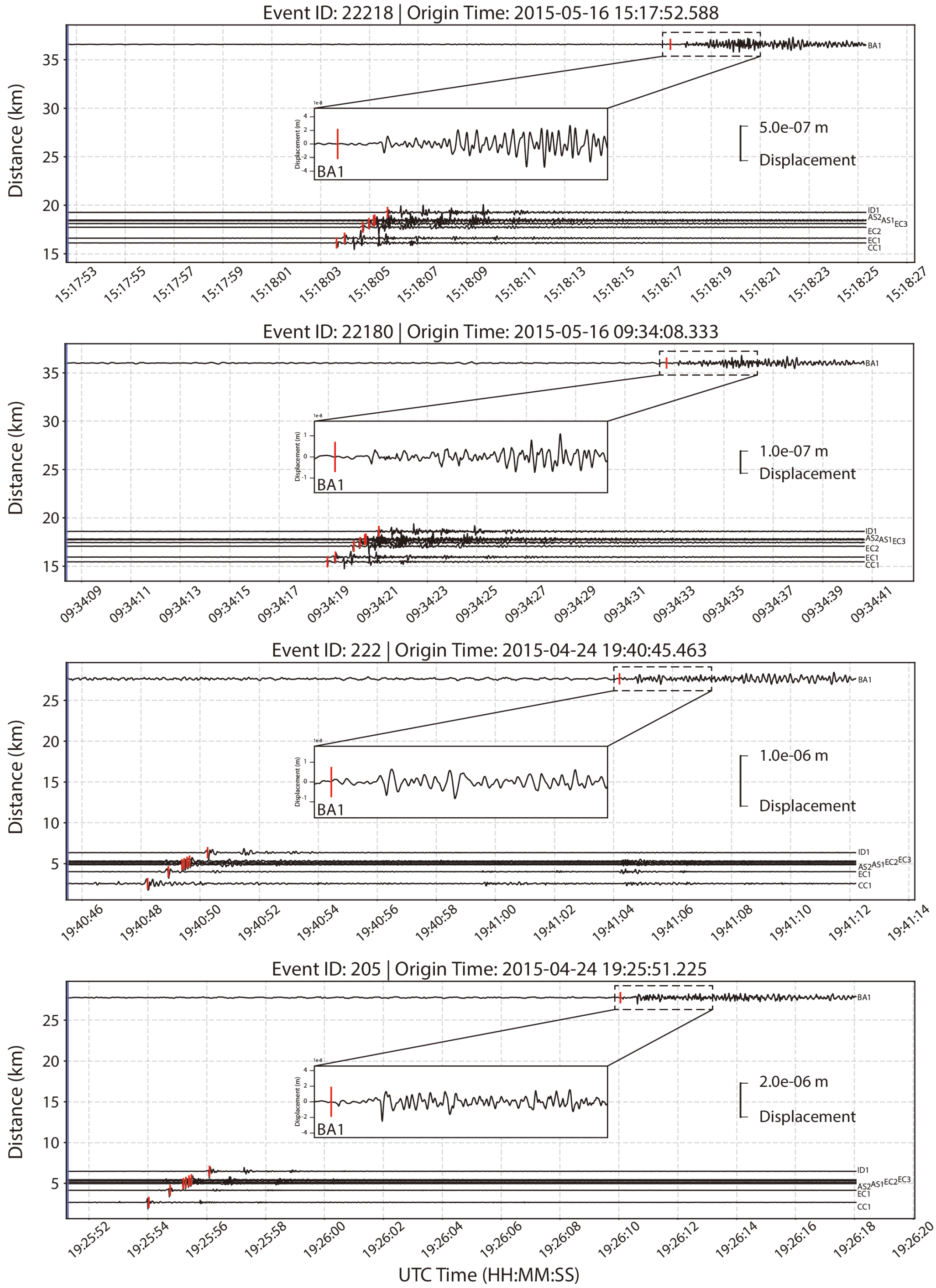

Fig. S13. Distance-time plot of impulsive event waveforms. The top two events occurred in the North Rift Zone, while the bottom two occurred near the caldera. Red vertical lines denote the predicted arrival times assuming a source depth of 1,600 m and a velocity of 1,485 m/s. The predicted arrival times at the farthest station AXBA1 are subject to greater uncertainty due to the use of a simplified velocity model and the increasing influence of path effects with distance. All vertical-component seismograms are band-pass filtered at 4-40 Hz.

Accordingly, we have revised lines 325-341 of the main text to read:

"*The detection range of these seismo-acoustic events is likely influenced by multiple factors,* *including network geometry, bathymetry, background noise, and event size. At Axial Seamount, some events with good signal-to-noise ratio were observed at station AXBA1 located ~28 km and ~37 km from the caldera and north rift zone respectively (Fig. S12 and S13). At the East Pacific Rise, ocean-bottom seismometers near the ridge axis recorded events at up to ~12 km away (14). At Fani Maoré, autonomous hydrophones suspended in the sound fixing and ranging (SOFAR) channel at ~1,300 m depth (~2,200 m above the seafloor) detected events at up to ~58 km away (29). At the Gakkel Ridge, a seismic array on an ice floe detected events at up to ~81 km away (18). These observations suggest that the seismo-acoustic events can be detected at many tens of kilometers away given typical seismic/hydroacoustic monitoring network configurations. However, since the stations furthest away from the lava flows all detected seismo-acoustic events in these studies, the maximum detectable distance of these events remains to be constrained. Nevertheless, given current observations, their use in remote monitoring of submarine eruptions at different tectonic environments for at least the near-regional distances is feasible. This opens the door to improved monitoring and characterization of submarine eruptions using ocean bottom seismic and hydroacoustic data.*"

Reviewer #1

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Comment 1:

This paper uses a variety of techniques, including focal mechanisms and bubble oscillation physics, to develop a model for the source mechanism of impulsive signals recorded seismoacoustically at Axial Seamount in 2015. The authors extend their model to deeper eruptions, considering the role that pressure plays in the generation of these events. The paper is broadly well written, and the analysis is interesting. My concerns focus on the extension of the model to other eruptions, given uncertainties in those data, and I have some suggestion for additional reading, but overall believe the paper could be published with moderate revision.

Response: We sincerely thank the reviewer for the positive and encouraging evaluation of our manuscript. We deeply appreciate your thoughtful feedback and suggestions.

Regarding your concerns about the extension of the model to other eruptions and the associated uncertainties, we agree that this is an important issue. We have carefully considered these points and provide detailed responses and clarifications in our replies to your specific comments below, including revisions to the main text and supplementary materials to better articulate assumptions, limitations, and supporting evidence.

We also thank you for the recommended literature, which we have incorporated into the revised manuscript to strengthen the broader context of our interpretations.

Comment 2:

My first discussion point relates to the estimates of gas volume generated by lava-water contact and steam generation. The authors use the number and size of impulsive events to estimate a total bubble volume (Vbubbles) of 5.07 x 10^5 m^3.

In the supplementary text they consider whether this is a reasonable volume of gas that could be flash heated by the lava flow assuming (a) a single flow and (b) multiple flows, assuming that the total deposit thickness was emplaced in thinner flows. If a single flow was emplaced, the calculations suggest that 2.07 x 10^5 m^3 of gas could be generated, which the authors state “already exceeds” the volume of bubbles.

Unless there’s a typo or I’m misunderstanding something here, this is incorrect: 2.07 x 10^5 does not exceed 5.07 x 10^5. This doesn’t render the argument moot— the authors explain that Vbubbles is a maximum value—but it does require a bit of correction or explanation.

Response: Thank you for pointing out the inconsistency in our comparison between the lower bound estimate of vapor volume and the estimated total bubble volume (V_bubbles). You are correct that the lower bound of vaporization (2.07×10^5 m^3) does not exceed the estimated bubble volume (5.0×10^5 m^3). We apologize for this misstatement and have revised the text to clarify that the lower bound estimate is of the same order of magnitude as V_bubbles, the upper bound estimate of total bubble volume, hence the physical plausibility of our model is still supported.

We have also addressed the issue of lava flow thickness and updated the text with appropriate clarification and citations. In the supplementary text, we discuss whether the observed deposit thickness could have resulted from a single thick flow or from multiple thinner flows. Based on Chadwick Jr. et al. (2016), the northernmost three flows—where most impulsive events were detected—reach total thicknesses of 67-128 meters. These values represent the cumulative thickness of compound lava flow fields, not individual eruptive units. The thickest deposits are interpreted to result from the emplacement of multiple overlapping lava flows (Le Saout et al., 2020).

To better constrain the likely thickness of individual lava flows, we reference several field and experimental studies. Soule et al. (2007) documented that lava flows from the 2005-2006 East Pacific Rise eruption were typically 1-2 meters thick based on direct observations and surface morphology, and adopted 1.5 meters as a representative average for volume calculations, explicitly excluding stacked flows. Schiffman and Lofgren (1982) reported that submarine pillow basalts generally have diameters of less than 1 meter. In addition, experimental and field work by Gregg and Fink (1995) and Gregg and Chadwick (1996) showed that lobate lava flows can be as thin as 20-30 cm. These observations, drawn from both direct measurements and analog studies, support our assumption that individual submarine lava flows are relatively thin.

We have revised the relevant section of the main text (lines 269-279) to the following:

"*The lower bound estimate (2.2×10^5 m^3), considering only the surface layer of lava flows, is of the same order of magnitude as V_bubbles, the upper bound volume estimate of bubbles generating the detected impulsive events. At Axial Seamount, the thickest deposits where most impulsive events occurred exceeds 128 meters in total thickness (26). These deposits were previously interpreted as the result of multiple overlapping flows rather than a single lava flow (20). Field and laboratory studies also suggested that individual submarine lava flows are typically 0.2–2 meters (51-54). Therefore, the total volume of vapor that can be generated is likely closer to the upper bound estimate (2.9×10^7 m^3) which is substantially greater than the bubble volume, indicating that the available heat from the erupted lava is sufficient to generate the observed impulsive events during the 2015 eruption.*"

Note: Some numerical values (e.g., 2.07×10^5 m^3 revised to 2.2×10^5 m^3) have been adjusted to ensure consistency in the use of significant figures. These changes do not affect the accuracy of our conclusions or the core content of the discussion.

Comment 3:

As for the maximum estimate for gas generation, the authors estimate that the overall Axial deposit has an average thickness of 13.3 m, and then go with the assumption that a single flow has a thickness of 1 m. There is no discussion about where the 1 m value came from—many lava flows on land are inflated by internal injection of lava, so flow thickness is not necessarily a simple piling-on of layers. Further, this calculation yields the odd result of there being 13.3 flows. It is a bit nonsensical to consider the possibility of 0.3 flows, so I would recommend revisiting the significant digits described here. Again, the idea isn't a bad one, but there it's a bit sloppy.

Response: Thank you for your valuable comments regarding our assumption of a 1 m lava flow thickness and the calculation of the number of flow layers in our vapor generation estimates.

(1) Clarification of the 1 m Thickness Assumption:

We would like to clarify our use of 1 m as a representative thickness for individual lava flows. This estimate is grounded in multiple observational and experimental studies of submarine lava morphology. For example, Schiffman and Lofgren (1982) reported that submarine pillow basalts are generally less than 1 meter in maximum cross-sectional diameter. Similarly, Soule et al. (2007) documented that lava flows from the 2005-2006 East Pacific Rise eruption were typically 1-2 meters thick based on direct seafloor observations and used 1.5 meters as a representative average in their volume calculations. Additional insights from experimental and field studies of lobate flows suggest that initial flow thicknesses can be even thinner, typically around 20-30 cm (Gregg and Fink, 1995; Gregg and Chadwick, 1996).

In our calculations, we adopt 1 m as a representative and literature-supported flow thickness to estimate the number of distinct eruption units that might contribute to the total deposit thickness. This value allows us to assess the potential number of lava-water interfaces where vapor generation could occur. **We do not imply that all flows are exactly 1 m thick or that the deposit formed by simple layer-stacking; rather, the assumption serves as a practical approximation consistent with observed flow morphologies in submarine settings.** We have now added an explicit explanation and direct quotation in the Supplementary Text S1 to clarify this point.

(2) Revision of Layer Count Expression:

We agree that expressing the number of flow layers as a non-integer (e.g., 13.3) is a mathematical artifact and not physically meaningful. In the revised Supplementary Text S1, we now round this value to "~13 layers" and clearly indicate that this is an approximate average, not a precise count of discrete flows.

(3) Adjustment of Significant Digits:

We have also revised the significant digits throughout this section to reflect the uncertainties inherent in these estimates, ensuring consistency and clarity.

(4) Summary of Changes in Supplementary Text S1:

Added a brief explanation and a direct quotation from Schiffman and Lofgren (1982) to justify the 1 m flow thickness assumption; Revised the number of layers from "13.3" to "~13 layers" and clarified its meaning; Adjusted significant digits in the calculated volumes for consistency.

The revised section now reads as follows:

"*Assuming each lava flow is about 1 m thick (52), the volume of water vapor estimated assuming single layer lava flow should be multiplied by the number of layers (~13 layers):*

- Min Volume: $13 \times 2.2 \times 10^5\ m^3 = 2.9 \times 10^6\ m^3$

- Max Volume: $13 \times 2.2 \times 10^6\ m^3 = 2.9 \times 10^7\ m^3$

The assumed 1 m flow thickness is based on observations of submarine pillow basalts, which are generally less than 1 m in maximum diameter (52). Additional field and laboratory studies show that lobate lava flows can be even thinner, typically 20-30 cm (53, 54). This value provides a representative scale for estimating the number of discrete flow units within the total deposit thickness. "

Comment 4:

The single or multiple layers of lava isn't a huge issue for the Axial data, since the authors make a reasonable case that even a single layer could potentially explain most of the impulsive events. But that's not necessarily the case for the eruptions to which they compare results. Impulsive events recorded during the eruption at Fani Maoré are described in terms of events/m^3 (1000x fewer at FM than at Axial).

But the authors argue that it is only that part of the lava that is exposed to seawater that generates the steam, so the difference between events/m^2 and events/m^3 is critical. Unless we know the thickness of individual flows, it's difficult to connect volume the potential for steam generation.

Response: Thank you for this thoughtful comment. We appreciate the opportunity to clarify our rationale for comparing impulsive event rates normalized by erupted lava volume (events/m^3), and to address the implications of this approach for comparing across different volcanic settings.

First, we agree that impulsive events are generated primarily by lava-seawater interaction, with steam generation likely occurring where lava traps or interacts with seawater. While this interaction is most efficient near the lava surface, our use of events per unit volume (events/m^3)—rather than per unit area—is intended to provide a consistent basis for comparing the overall potential for bubble generation across eruptions of different sizes and styles. This volumetric normalization serves as a proxy for the total energy budget associated with lava emplacement, under the assumption that the mechanism of steam generation is broadly similar (i.e., seawater trapped or entrained in lava flows).

Importantly, both the events/m^2 (1.70×10^{-5}) and events/m^3 (0.83×10^{-7}) at Fani Maoré are substantially lower than those at Axial Seamount, which has events/m^2 (2.02×10^{-3}) and events/m^3 (1.51×10^{-4}). This supports our conclusion that the steam-flash mechanism proposed at Axial is physically plausible at other sites as well. At Fani Maoré, the notably lower event rate per unit lava flow area/volume implies that impulsive events were less frequent, potentially due to greater inhibition of bubble nucleation at depth. This also implies that more energy is available per event at Fani Maoré. Thus, rather than contradicting the plausibility of the mechanism, the data indicate that it remains feasible under varying pressure conditions, albeit with different impulsive event productivity.

We acknowledge the reviewer's point that the difference between surface-based (m^2) and volume-based (m^3) metrics depends on the thickness of individual lava flows. In our analysis, we assume that average lava flow thicknesses are broadly similar among the basaltic eruptions considered (Axial, Fani Maoré, and the East Pacific Rise), which is reasonable given their comparable tectonic and magmatic settings. For the lower events/m^3 value at Fani Maoré to result solely from thicker flows, the flows there would need to be more than 15 times thicker than at Axial—an unlikely scenario based on available bathymetric and geological data. (For example, if Axial flows average ~13 m in thickness, Fani Maoré would require average thicknesses >200 m to match Axial's events/m^2 rate—an unrealistically large value given available bathymetric and geological constraints.)

Nevertheless, we agree that lava thickness and emplacement style introduce uncertainty into these comparisons, and we have explicitly acknowledged this limitation in the revised manuscript (Line 308):

"*... though variability in lava flow thickness and emplacement style further introduces uncertainty to the comparison between different volcanic settings.*"

Comment 5:

I think the argument that bubble expansion is reduced at high pressures is reasonable, but it basically just means that the bubbles themselves would be smaller. This doesn't necessarily mean that the number of impulsive events would be lowered, just that they might be weaker. It isn't clear what the detection threshold might have been for events such as the Fani Maoré and Gakkel eruptions, so it's very difficult to assess whether the apparently low number of impulsive events in those eruptions is real or if it's a product of detection (or other eruption parameters).

Response: Thank you for this thoughtful and important comment. We agree that higher hydrostatic pressure reduces bubble expansion, which would lead to smaller bubbles and potentially weaker impulsive signals. This is a valid point and consistent with our interpretation. However, as you noted, smaller signal amplitudes do not necessarily imply fewer events. To address this, we would like to clarify two aspects:

First, based on physical models of bubble formation and coalescence (e.g., Maicher and White, 2001; Engels et al., 2003), we suggest that higher pressure not only reduces bubble size but may also suppress the number of bubbles that grow, coalesce, and rise to the lava surface. In other words, both the size and number of bubbles capable of generating impulsive events may decrease at greater depths. While direct observational evidence is limited, this interpretation is grounded in the physics of vapor formation under pressure and is reflected in our revised manuscript text and figure captions (see below).

Second, regarding the possibility that the low number of impulsive events at Fani Maoré is due to detection limitations, we agree that this is an important issue to address. However, we find it unlikely that detection thresholds at Fani Maoré were substantially poorer than at Axial Seamount or the East Pacific Rise (EPR). This assessment is based on the number of seismic stations used and the typical event-to-station distances. At Fani Maoré, three OBS stations were used, with event-to-station distances ranging from approximately 5 km to 15 km (Saurel et al., 2022). In contrast, studies of impulsive events at EPR and Axial Seamount were based on 3 stations at EPR (Tan et al., 2016) and 7 at Axial, with most event-to-station distances at these sites often exceeding 10 km. Given the comparable number of stations and distances, the detection capability for impulsive events at Fani Maoré was likely similar to that at Axial and EPR.

We have revised the manuscript to clarify these points:

(1) Line 290: Added the sentence:

"*... (Fig. 3C), where both reduced bubble size and suppressed bubble formation under higher hydrostatic pressures likely contribute to fewer and smaller bubbles reaching the lava surface.*"

(2) Figure 3C caption: Revised to clarify that both the number and size of bubbles may decrease at higher pressures.

(3) Lines 297-301: Added the sentence:

"*The detection capability at Fani Maoré, with three ocean bottom seismometers at ~5 to 15 km distances from the lava flow (16), was likely comparable to that at Axial Seamount and the East Pacific Rise where seven and three ocean bottom seismometers, respectively, were at distances often exceeding 10 km (14). Thus, the markedly lower number of detected events at Fani Maoré is unlikely to be due to differences in detection sensitivity.*"

Line-by-Line Comments:

Lines 71-74: I find this line confusing as it begins by discussing signal recorded at Kilauea and Gakkel, then says that such signals were "subsequently shown in studies (14, 15) to coincide with mapped lava flows" on the EPR and at Axial. I suspect the "subsequently shown" is intended to say that there have been later studies, but overall the sentence is confusing.

Response: Thank you for your helpful comment regarding the clarity of Lines 71-74. We agree that the original sentence structure could be confusing due to the combination of multiple locations and study timelines. To clarify, we have revised the sentence to better reflect the historical progression of observations and interpretations.

Specifically, the impulsive signals at Hawaii and the Gakkel Ridge were among the first to be identified, but in those cases, their association with lava emplacement was only hypothesized, as no coincident mapped lava flows were confirmed. Later studies at the East Pacific Rise and Axial Seamount provided the first clear evidence that such signals coincide with mapped lava flows. The revised sentence at lines 71-75 now reads:

"*These impulsive signals were first identified near Hawaii and the Gakkel Ridge (12, 13) where their association with submarine eruptions was hypothesized. Later studies (14, 15) demonstrated that similar signals coincided with mapped lava flows from the 2006 eruption at the East Pacific Rise 9°50'N and the 2015 eruption at Axial Seamount.*"

Line 93 suggests that this paper uses geochemical data—I don't see that this is the case other than where it cites Clague et al. (2018) to estimate lava temperatures?

Response: Thank you for your insightful comment. We agree that the original wording may have unintentionally suggested that new geochemical data were collected for this study. In fact, we did not generate any new geochemical data. Instead, we used **published geochemical parameters**—such as lava temperature, magma density, and specific heat capacity—from previous studies (e.g., Clague et al., 2018) to constrain our heat transfer calculations (see Supplementary Text S1).

In addition, we cited Clague et al. (2018) not only for thermal parameters, but also to support our interpretation that erupted magmas at Axial Seamount are typically already highly degassed prior to eruption. This observation provides additional support for our conclusion that degassed magmatic volatiles are unlikely to be the dominant mechanism for generating the observed impulsive events.

To clarify this point, we have revised the sentence at Line 92 to:

"*In this study, we integrate seismic data analyses with constraints from heat transfer estimates, supported by published geochemical parameters and geological observations, ...*"

We have also added the following sentence at Lines 188-189 to make this interpretive use of geochemical data explicit:

"*This is consistent with geochemical observations that erupted lavas at Axial Seamount are typically already highly degassed prior to eruption (39).*"

Line 104: "base" should be "based". Also, to clarify that these locations are (I suspect) from the catalog cited above, I'd add another citation for Le Saout et al.

Response: Thank you for pointing out the typographical error. We have corrected "base" to "based."

Additionally, to clarify the source of the event locations, we have added a second citation to Le Saout et al. (2020) in the relevant sentence, as suggested.

Lines 108-109: Clarify that this analysis is being performed on the seismic channels, as the hydrophone will also show first motion down (as it is omnidirectional). I know this is included at the end of the paper, but it should be stated earlier as well.

Response: Thank you for the helpful suggestion. We agree that the distinction between seismic and hydrophone data should be clarified earlier in the manuscript. We have revised the text at Lines 109-110 to explicitly state that the analysis is based on vertical seismic channels.

"*Subsequently, we stacked all the vertical seismic waveforms...*"

Lines 118-119: Since seismic and acoustic energy is leaving the source through the water column, it would seem that the focal mechanisms here are upper hemisphere.

Is that the case? If so, it should be mentioned (and if not, I'm not sure I understand what it shows). It won't matter for an implosion source, but it would be helpful for interpretations of other mechanisms.

Response: Thank you for this thoughtful comment. We have carefully considered the differences between these impulsive events and conventional tectonic earthquakes, particularly the upward propagation of seismic and acoustic energy through the water column and the associated polarity reversal at the sea surface.

Taking these factors into account, we still opted to use the **conventional lower hemisphere projection** for the focal mechanism representations, in order to remain consistent with standard seismological practice, particularly for earthquake focal mechanism studies at Axial Seamount and other mid-ocean ridge settings.

To clarify this in the manuscript, we made the following revisions:

(1) In the main text (at lines 123-124), we added the sentence:

"*The focal mechanisms are plotted using the lower hemisphere projection, as adopted throughout this study.*"

(2) In the caption of Fig. 2: We added the sentence:

"*All focal mechanisms are shown using the lower hemisphere projection.*"

Lines 137-138: The issue of hydrophone versus seismometer polarity is also addressed by Caplan-Auerbach et al. (2017), so could be mentioned here.

Response: Thank you for the helpful suggestion. We agree that the polarity consistency between hydrophones and vertical seismic channels has been previously discussed in Caplan-Auerbach et al. (2017), and we have now cited this reference in the relevant sentence (Lines 140-142) for completeness.

The revised sentence reads:

"*Firstly, the impulsive signals exhibit consistent polarities between hydrophones and vertical seismic channels (Fig. S9), a pattern that has also been noted in a previous study (22).*"

Lines 144-145 suggest that the formation of large cracks did not generate seismic energy (and thus, small ones would not be expected to). It isn't clear to me (a) when these large cracks formed (might they pre-exist the eruption?) or whether we know with certainty that they did not generate seismic energy. Depending on the mechanism of formation, cracks can form under slow rupture, which also might not show up seismically. This particular argument brings up more questions than it answers.

Response: Thank you for your thoughtful comment. We agree that the timing and rupture style of the mapped fissures are important considerations when evaluating possible source mechanisms. The mapped large fissures shown in Fig. 1A were formed during the 2015 eruption, as documented in the *interpreted outlines (version 1) of the 2015 lava flows and eruptive fissures at Axial Seamount, Juan de Fuca Ridge (Chadwick et al., 2016, MGDS, doi:10.1594/IEDA/323598)*. While it is possible that some fissures formed through slow rupture processes—which may not radiate high-frequency seismic energy—we note that these large, eruption-related surface cracks did not produce detectable seismic signals at nearby instruments.

This observation is important because it suggests that even sizable and geologically confirmed tensile cracks did not generate clearly observable seismic energy. Therefore, it is even less likely that smaller-scale cracks—if present—would be capable of generating the distinct, high-frequency, impulsive seismo-acoustic signals observed in our dataset. In other words, the presence of clearly detectable impulsive events, in contrast to the absence of signals from large fissures, argues against a crack-opening origin for these events, regardless of rupture speed.

To clarify this reasoning, we have revised the relevant paragraph in the manuscript (Lines 140-150):
vertical seismic channels (Fig. S9), a pattern that has also been noted in a previous study (22). Secondly, they do not produce observable direct crustal P or S-wave arrivals even for events within the caldera and close to the stations. These absences suggest that the source is strongly coupled with the water column but not with the solid Earth, which is inconsistent with both tensile crack and double-couple mechanisms. Finally, the impulsive event locations do not align with previously mapped large fissures that formed during the eruption (28) (Fig. 1A), and those fissures did not generate detectable seismic signals. Therefore, it is unlikely that similar or smaller-scale cracks would generate the observed seismo-acoustic signals. We conclude that an implosive source mechanism is most consistent with all the existing observations (Fig. 1 and 2A).”

Line 150: Calling these “impulsive events” leaves open the question of whether the other examples (Gakkel, EPR, Fani Maoré) are also water-column signals or whether they might be seismically-derived. Is that the intent? Do we have any known examples in which implosions were captured seismically?

<u>Response:</u> Thank you for this insightful comment. We clarify that the term "impulsive events" is used in our manuscript to describe short-duration, high-frequency signals that are strongly coupled to the water column, as consistently observed in our data at Axial Seamount. We follow this terminology from previous studies (e.g., Gakkel Ridge, East Pacific Rise [EPR], and Fani Maoré), where similarly impulsive signals were observed and described, though their source mechanisms were not explicitly determined.

In all these cases, the events were recorded by seismometers but were interpreted as arising from water-column sources. However, due to limitations such as low signal-to-noise ratio (e.g., at Gakkel) or short signal duration, these studies did not determine first-motion polarity or establish a definitive source mechanism.

At Fani Maoré, for example, Saurel et al. (2022) reported that the hydroacoustic signal duration was typically shorter than 0.05 s, resulting in fewer than 10 samples at a 250 Hz sampling rate. While this rate is sufficient to record signals with frequency content up to 125 Hz, the short signal length and limited SNR made it difficult to resolve the complete frequency content or determine first-motion polarity. They estimated that the signal frequency content likely exceeded 50 Hz, and may be comparable to other high-frequency hydroacoustic signals (50-100 Hz), possibly generated by small bubbles under high-pressure conditions. However, due to these limitations, they could not distinguish between explosive or implosive sources.

We also note that we do not have access to the original waveform data for Gakkel or Fani Maoré, which limits our ability to independently analyze first-motion polarity or perform moment tensor inversions. However, in the case of the East Pacific Rise (EPR), the impulsive events shown in Extended Data Figure 1 of Tan et al. (2016, Nature) display downward first motions, which would be consistent with an implosive source.

In summary, while the prior studies did not definitively identify the source mechanisms, the available evidence—including strong coupling to the water column, impulsive character, and in some cases downward first-motion polarity—is consistent with our interpretation that the events at Axial Seamount represent implosive sources. We use the term "impulsive events" for consistency with these earlier studies, while noting that our data provide stronger constraints on the source mechanism.

Line 159: Since the authors do not believe that the quench fragmentation model is appropriate for Axial events, I'm not sure that this figure is necessary. Also the authors imply that this mechanism would generate collapse pits, and I'm not sure that's always the case. Pillow lavas often undergo implosion (e.g. Moore (1975) and both observed and mentioned in Pele Meets the Sea, the longer version of the videos contained in the supplement to this paper: that video explicitly describes "the implosion of solidified pillows").

Response: Thank you for this insightful comment. We agree that implosions during cooling contraction, such as those observed in pillow lavas (e.g., Moore, 1975; "Pele Meets the Sea" video), do not necessarily produce large collapse pits. Instead, as Moore (1975) describes, these implosions typically involve spalling of fragments from the solidified crust due to thermal contraction and external pressure.

In light of your suggestion, we have removed Fig. S8 to streamline the presentation and avoid overemphasizing the quench fragmentation model.

Line 168: Another appropriate citation for this observation (coincident hydroacoustic and ROV data of a submarine eruption) would be Dziak et al. (2015) (citation below).

Response: Thank you for the helpful suggestion. We have added a citation to Dziak et al. (2015) in the revised manuscript (Line 172) to further support the observation of coincident hydroacoustic recordings and ROV data during a submarine eruption.

Lines 198-201: The authors suggest that the video included in the supplement is sufficient evidence that submarine lava flows in Hawaii do not generate impulsive signals via interaction with seawater. While this may be the case (such events may also require an explosive or implosive mechanism), I'm not sure that the video data are sufficient to make this case.

Response: Thank you for this thoughtful comment. We respectfully maintain that the video observation (Movie S1) provides sufficient supporting evidence for our interpretation. The video represents a shallow-water environment, where hydrostatic pressures are relatively low. If simple lava-water contact were capable of generating widespread impulsive acoustic signals, such signals would likely be more easily observed in shallow settings and across the entire exposed surface of the active flow. However, as shown in the video, no such widespread impulsive activity is observed.

In addition to the video observations, prior studies (e.g., Moore, 1973) provide further evidence supporting this interpretation. Moore (1973) reported that water temperatures only a few centimeters from active lava surfaces rose by just 2.5°C above ambient seawater temperature, suggesting limited thermal exchange and minimal energy release during direct lava-water contact.

To address the reviewer's concern, we have revised the text (Lines 205-209) to clarify this point by adding a brief reference to Moore (1973). The sentence now reads:

"*Observations of active submarine lava flows in Hawaii (25) suggest simple lava-water contact does not generate widespread impulsive events due to the initial steam layer's insulating effect (Movie S1), consistent with temperature measurements near active flows showing minimal heating of surrounding seawater (40).*"

These observations support the need for a more energetic process, such as hydrovolcanic implosion involving trapped seawater or sediments beneath the lava, which we discuss in the following sections.

Line 204: Other evidence that water may percolate through a lava flow comes from Tribble, (1991): observations were made (via SCUBA) of bubbles repeatedly bursting from a common location within a channelized underwater flow. Tribble explicitly states that this could be from steam trapped beneath the flow, and he describes an "audible concussion" associated with the release of these bubbles.

Response: Thank you for the helpful suggestion. We agree that the observations described by Tribble (1991) provide additional support for dynamic seawater interaction with submarine lava flows. Tribble documented frequent underwater explosions, audible concussions, and large releases of hot water through cracks in lava streams during a channelized submarine flow offshore Kilauea, Hawaii, suggesting that seawater can infiltrate and interact within the lava flow.

To address this, we have revised the text at Line 211 to include a reference to Tribble (1991). The revised sentence reads:

"*..., the common presence of pipe vesicles and spiracles in submarine lava flows provides clear evidence that water can enter lava flows from beneath (23). Additional support comes from observations offshore Kilauea, Hawaii, where frequent underwater explosions, audible concussions, and large volumes of hot water escaping through cracks in active submarine lava streams suggest dynamic seawater interaction with lava interiors (25).*"

Lines 205-206: These temperatures are very precise, and the reference within that citation suggests these values are for caldera lavas (the North rift lavas were found to be slightly cooler). I'd simply round this to 1200 oC as the authors do elsewhere in the manuscript.

Response: Thank you for pointing this out. We agree that the originally cited temperatures were overly precise and specific to caldera lavas, whereas the North Rift Zone lavas were slightly cooler. To address this, we have rounded the temperature to 1200°C in the revised text, consistent with how it is presented elsewhere in the manuscript.

Line 221: It might be preferable to cite Roche et al. (2022) for this equation (it's already in the reference list), as that reference cites a version of this equation that is closer to the one shown in the manuscript than Brennen.

Response: Thank you for the helpful comment. While our derivation of the natural frequency formula is based on the general form provided by Brennen (1995), we recognize that Roche, Leighton, White, and Bull (2022) present a simplified version that closely matches the form shown in our manuscript. To clarify this, we have revised the text at Lines 228-230 to acknowledge both sources in the description of the formula.

"*Following the general form derived by Brennen (44) and consistent with the simplified version presented by Roche, Leighton, White and Bull (45), we define the natural frequency...*"

Line 232: Note that this bubble dimension is similar to the size of bubbles observed at West Mata submarine volcano at ~1 km depth, as discussed by Dziak et al. (2015) and Murch et al. (2022). This might be a good place to note that similarity.

Response: Thank you for the helpful suggestion. We agree that highlighting the similarity between our calculated bubble sizes and those observed at the West Mata submarine volcano provides valuable context. Accordingly, we have added a sentence at Line 245 noting this consistency and citing Dziak et al. (2015) and Murch et al. (2022).

"*This inferred bubble size is consistent with observations of similarly sized vapor bubbles (~0.2-1 m in diameter) at the West Mata submarine volcano (~1 km below sea level) (33, 46).*"

Lines 281-204: These suggest that shallow events such as gas bubbles at Kilauea generate sound via explosion versus implosion. While it's true that video of bubbles does show them expanding, that doesn't necessarily mean that sound was generated in the expansion process. This is discussed in detail in Murch et al. 2022, in considering whether hydroacoustic signals at West Mata were caused by bubble explosion or implosion.

Response: Thank you for the insightful comment. We agree that although video observations show bubble expansion, the hydroacoustic signals may not necessarily originate from the expansion process as discussed in Murch et al. (2022). To clarify this, we have modified the text at Line 314-317 by adding a sentence after "Movie S2" noting that the acoustic signals could result from either bubble expansion or collapse, and citing Murch et al. (2022).

"*This has been observed at the coastline of Kīlauea (Movie S2) and is likely the case for the impulsive events related to lava ocean entry and offshore lava emplacement during the 2018 Kilauea eruption (58, 59), although it remains uncertain whether the hydroacoustic signals originate from bubble expansion or collapse (46).*"

Line 282 cites the movie shown in the supplementary data. The two movie clips are given what I think is an incorrect citation: it links to an AGU abstract in 1990 (Sansone et al., 1990) instead of the copyright to the file Pele Meets the Sea, from which these clips were taken. The correct citation is below.

Response: Thank you for highlighting the citation issue. We have corrected the reference to the movie clips in the Supplementary Materials, citing Pele Meets the Sea appropriately.

Figure 1: Event #179, shown in figure 1D is said to be "marked as a white circle in" 1A. From

my view there are several white circles in A, so I'm not sure which one is shown here. The direct and surface-reflected arrivals seem long for the north rift: those are typically about ~0.6 second apart (e.g. Le Saout et al., 2020, and Caplan-Auerbach et al., 2017), but these appear much longer on AXEC2 and AXCC1. Also, on 1B I would add West Mata to the map—impulsive (albeit low frequency) events were recorded there as well (see references below).

Response: Thank you very much for your careful reading and helpful suggestions.

We agree that the original use of white circles in Fig. 1A could cause confusion, particularly because white diamonds were also used to mark the three event locations shown in Fig. 2. To address this, we have changed the symbol for Event #179 in Fig. 1A from a white circle to a white X, making it clearly distinguishable from the other markers.

Regarding Fig. 1B, our intention was to show only sites where impulsive events have been confirmed through seismic detection, as stated in the figure caption: "*(B) World map displaying confirmed submarine eruptions with impulsive events detected (triangles) and Axial Seamount (star), all color-coded according to their water depth (12, 14, 16, 20).*" Since the impulsive events at West Mata were primarily observed through visual ROV observations and not through direct seismic detection, we have opted not to add West Mata to Fig. 1B in order to maintain consistency with our selection criteria.

Figure 3 suggests that there are fewer gas bubbles generated at higher pressures, but it seems more likely to me that the bubbles generated are simply smaller (they don't expand as much). Perhaps this should be reflected in the figure?

Response: Thank you very much for your thoughtful comment. We agree that at higher hydrostatic pressures, bubbles are less able to expand, resulting in smaller bubble sizes. However, based on the physical processes involved, we believe that the number of bubbles also decreases under higher pressures. Specifically, after water is trapped by hot lava, bubbles form, ascend, and undergo coalescence and accumulation. Higher ambient pressures suppress these processes, leading to fewer bubbles ultimately reaching the lava surface.

To address this point and clarify our interpretation, we have added the following sentence after the first mention of Fig. 3C (line 290):

"*... (Fig. 3C), where both reduced bubble size and suppressed bubble formation under higher hydrostatic pressures likely contribute to fewer and smaller bubbles reaching the lava surface.*"

Citations:

Dziak, R. P., Bohnenstiehl, D. R., Baker, E. T., Matsumoto, H., Caplan-Auerbach, J., Embley, R. W., ... & Chadwick Jr, W. W. (2015). Long-term explosive degassing and debris flow activity at West Mata submarine volcano. Geophysical Research Letters, 42(5), 1480-1487.

Moore, J. G. (1975). Mechanism of formation of pillow lava: pillow lava, produced as fluid lava cools underwater, is the most abundant volcanic rock on earth, but only recently have divers observed it forming. American Scientist, 63(3), 269-277.

Murch, A. P., Portner, R. A., Rubin, K. H., & Clague, D. A. (2022). Deep-subaqueous implosive volcanism at West Mata seamount, Tonga. Earth and Planetary Science Letters, 578, 117328.

Sansone, F. J., Culp, J. B., & Pyle, R. L. (1990). Pele meets the sea. Lava Video Productions.

Tribble, G. W. (1991). Underwater observations of active lava flows from Kilauea volcano, Hawaii. Geology, 19(6), 633-636.

Reviewer #2

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Comment 1: This study presents an exciting leap forward in our ability to remotely characterize submarine volcanic eruptions. I particularly commend the thoughtful integration of seismic and hydroacoustic data with physical modeling of heat transfer and vapor bubble dynamics. The paper is both methodologically rigorous and conceptually innovative. The identification of implosion-driven signals from seawater vaporization as the dominant source mechanism is both compelling and significant. The analysis sets a new standard for how these elusive acoustic events can be used to track lava emplacement on the seafloor.

Response: Thank you for your positive comments and recognition!

Comment 2: Highlighting Implications for Future Monitoring

The discussion touches on the potential for tracking eruptions in remote regions. Consider adding a concluding paragraph or bullet points that more explicitly frame how this work could inform future observing network designs (e.g., Ocean Observatories, global hydroacoustic systems, or even autonomous ML detection algorithms).

Response: Thank you for your helpful suggestion. In response, we have added a new concluding paragraph at the end of the Discussion section to more explicitly highlight the implications of our findings for the design of future submarine eruption monitoring networks. This new paragraph focuses on the observed detection ranges of seismo-acoustic events across different tectonic settings and discusses the feasibility of using such signals for remote eruption monitoring over near-regional distances.

The revised paragraph reads as follows:

"*The detection range of these seismo-acoustic events is likely influenced by multiple factors, including network geometry, bathymetry, background noise, and event size. At Axial Seamount, some events with good signal-to-noise ratio were observed at station AXBA1 located ~28 km and ~37 km from the caldera and north rift zone respectively (Fig. S12 and S13). At the East Pacific Rise, ocean-bottom seismometers near the ridge axis recorded events at up to ~12 km* *away (14). At Fani Maoré, autonomous hydrophones suspended in the sound fixing and ranging (SOFAR) channel at ~1,300 m depth (~2,200 m above the seafloor) detected events at up to ~58 km away (29). At the Gakkel Ridge, a seismic array on an ice floe detected events at up to ~81 km away (18). These observations suggest that the seismo-acoustic events can be detected at many tens of kilometers away given typical seismic/hydroacoustic monitoring network configurations. However, since the stations furthest away from the lava flows all detected seismo-acoustic events in these studies, the maximum detectable distance of these events remains to be constrained. Nevertheless, given current observations, their use in remote monitoring of submarine eruptions at different tectonic environments for at least the near-regional distances is feasible. This opens the door to improved monitoring and characterization of submarine eruptions using ocean bottom seismic and hydroacoustic data.*"

Comment 3: Figure 4 Clarification

The bubble radius histogram (Fig. 4B) is very compelling. You might consider briefly elaborating on the potential implications of bubble clustering and interaction (as mentioned in-text) in the figure caption itself to reinforce interpretation.

Response: Thank you for your suggestion. We have revised the caption of Figure 4 to briefly address the potential influence of interactions within bubble clouds on the inferred size distribution, consistent with our discussion in the main text.

Comment 4: Depth-Dependence Framing

You present a nuanced view of how depth influences the viability of vaporization-based implosions. A schematic timeline or depth-threshold summary graphic (or even an inset to Fig. 3C) could help clarify the generalizability of this mechanism across diverse volcanic settings.

Response: Thank you for your helpful suggestion regarding the schematic illustration of depth-dependent processes. In the revised Figure 3, we have clarified the transitions by: (1) Indicating only 0 m and ~3,000 m as reference depths, (2) Using a wavy line between the explosion-dominated (panel A) and implosion-dominated (panel B) regimes to emphasize the uncertain nature of this boundary, and (3) Explicitly noting in the caption that implosions were observed as shallow as ~1,600 m in our case study, but that the exact depth of transition is unconstrained.

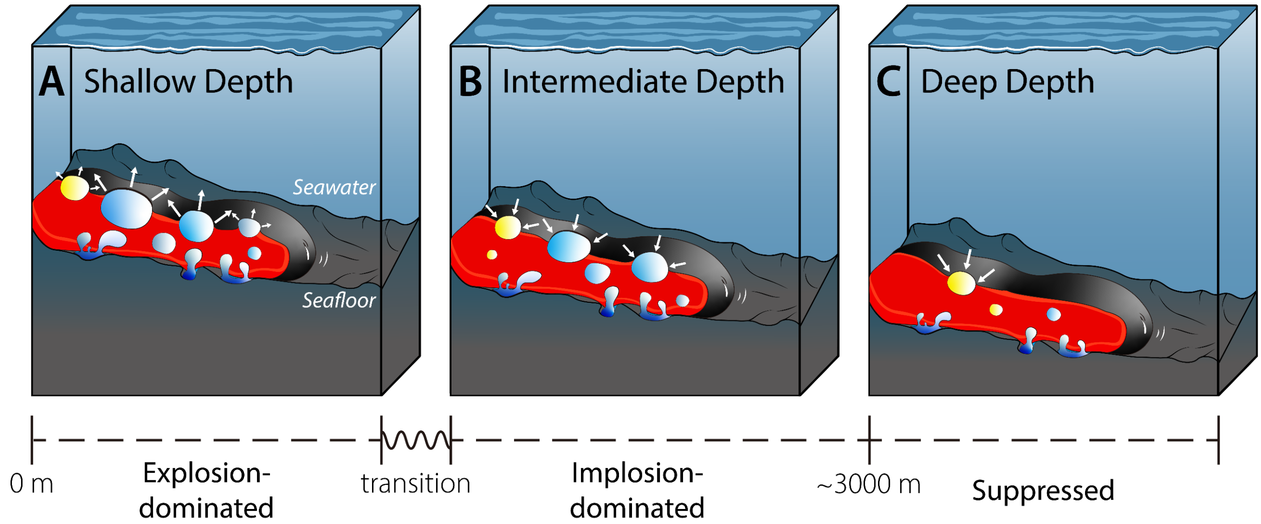

Fig. 3. Schematic diagram of the inferred processes underlying impulsive seafloor events. (**A**) At shallow depths, rapid expansion of bubbles formed from vaporized seawater (blue) by hot erupted lava and volatiles exsolved from magma (yellow) can generate explosive signals. (**B**) At intermediate depths, implosions of predominantly vaporized seawater and some exsolved volatiles due to high hydrostatic pressure generate implosive signals. The depth boundary for transition (wavy line) from explosion-dominated to implosion-dominated regime remains to be determined though implosions were observed as shallow as ~1,600 m in this study. (**C**) At > ~3,000 meters depths, vaporization of seawater by hot erupted lava is inhibited so proportionately fewer and smaller bubbles reach the lava surface, resulting in fewer detectable implosive events.

Comment 5: Minor Stylistic Polishes

The phrase "we decide to assume" might read more smoothly as "we assume" (Results section).

Response: Thank you for your suggestion regarding stylistic clarity. We have revised the sentence in the Results section to read "we assume that all detected events possess similar focal mechanisms..." as recommended, instead of "we decide to assume." We appreciate your careful review and constructive feedback.

In several places, "implosion of bubbled volatiles" might be more clearly phrased as "implosion of gas-rich bubbles" or similar for flow.

Response: Thank you for this helpful suggestion. We agree that "implosion of gas-rich bubbles" is clearer and improves the flow of the text. We have revised the relevant sentence to read: "these large-amplitude impulsive events are possibly related to the implosion of gas-rich bubbles released from magma."

Occasionally the flow of paragraphs could benefit from a topic sentence or framing statement—particularly when shifting between hypotheses in the Discussion.

Response: Thank you for your thoughtful suggestion to improve the paragraph flow in the Discussion section by adding topic sentences or framing statements, especially when shifting between different hypotheses.

In the revised manuscript, we have incorporated clear topic sentences and framing statements at the following positions:

(1) At the beginning of the section discussing physical processes (Lines 158-159): We added "*Below, we summarize and assess each hypothesis in turn.*" after introducing the three main hypotheses.

(2) When transitioning to the second hypothesis (Lines 170-171): We revised the opening to "*A second hypothesis relates the observed implosive events to volatiles degassed from the underlying magma/erupted lava.*"

(3) When transitioning to the third hypothesis (Lines 204-205): We revised the opening to "*A third possible mechanism is hydrovolcanic implosion, in which interactions between lava and seawater generate implosive events.*"

These additions help clarify the structure of the Discussion and improve readability as you recommended.

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Once again, we thank the editor and reviewers for their valuable suggestions and comments, which have helped us to improve our manuscript.

Sincerely,

Peifeng Wang (on behalf of all co-authors)