Integrated seismic monitoring reveals subsurface evolution during volcanic inflation at Askja volcano, Iceland

Brenot et al., 2026

Answers to the reviews

To the editor,

I am attaching the revised version of the article 'Integrated seismic monitoring reveals subsurface evolution during volcanic inflation at Askja volcano, Iceland'. This includes a version without changes shown and a version with changes shown.

We would like to thank the two reviewers for their constructive comments, which we have considered carefully and in response have made changes as appropriate. This has certainly improved the manuscript. We added earthquake catalog data to Figures 1 and 2, the volcanic settings section, and the discussion, as requested by both reviewers; corrected the GNSS data of the station OLAC's north component; added a moving reference analysis for the dv/v computation; and finally improved the DSAR discussion and nuance throughout the text, particularly regarding data gap and interpretation.

Specific responses to each comment can be found following this cover letter, where we describe the major changes in the new version. I have referred to line numbers corresponding to the version with changes shown. Note that in the "changes shown" version, most of the figures appear after the References section (not in the clean version).

The manuscript recognizes the work of the reviewers in the Acknowledgments section L800-801.

Thank you in advance for considering this revised manuscript, and we look forward to your response.

Sincerely,
Laure Brenot

Reviewer A:

This paper present interesting methods to track changes at an active volcano. While it is well written and I would recommend publication, I strongly suggest that the authors nuance their interpretations as their results are not as evident as their wording make it feel, particularly considering the large gaps in data in some places. I have listed my specific comments in the following text but I recommend spending some time mentioning the gaps in data as early as possible and explaining exactly why you think you can interpret some results taking into accounts those measurements gaps.

Main text:

- lines 106-114: microgravity is not mentioned anywhere else in the paper, and here the authors present a work that did not show results, so I really don't see why it is mentioned ?

L122-136: We rephrase the paragraph to clarify that there is a microgravity increase between 2016 and 2021. But no constraint on how this increase occurred over time. Microgravity studies are rare and valuable, which is why we found it useful to mention in the introduction and also because it helps create a niche for our study that has results for the time period not covered by other geophysical methods. In addition, we added Sepúlveda-Araya et al. (under review) work that measured a mass increase with deformation and microgravity data from 2022 and 2023.

- lines 124-130: it could be interesting to see the seismicity in one of the map. Could you add it to the first figure maybe ?

L114-121: Yes, seismicity has been added to Figures 1 and 2. As the network detection of the events is seasonally impacted by the stations' availability (daylight decrease and snow panel covered by snow), we also show the number of seismic events above 0.65 magnitude, to account for varying magnitude of completeness.

- lines 270- 272. This sentence is a little confusing. You say there are signals in 3-7 Hz both during winter and summer so just say that in summer there are signals at 1-3 Hz ?
  Also the green highlight on the figure is at 1-3 Hz so your sentence should read « range from 1-3 Hz **(green highlight on figure 4)** with intensified activity in the 3-7 Hz range. »

L308-313: We have rewritten both sentences as: "Persistent sources concentrate in the frequency range between 3 and 7 Hz (Figure 3B). In summer (April–October), signals extend to lower frequencies of 1–3 Hz (green highlight on Figure 3B), with lower spectral width values in the 3–7 Hz range."

- lines 277-281: this a paragraph that needs nuance. I don't see the significant signals you mention.

L318-327: We agree, we have rephrased and removed some statements. The changes to the text are provided in the response after the next four questions below, which cover the same section.

- First, I don't understand the two sub-groups: STAM is as much east than JONS so why are the two separated ? If the separation is done because the observations differ then explain why. If not, I would move STAM to the West group. Or I would not say East and West but rather Northwest and Southeast and explain why you separated like this.

We have changed the named groups to Northwest and Southeast and clarified the definition of the groups relative to the caldera. These two groups are created based on their distance from the power station dam.

- I think the reference to the figures are wrong ? The west group is shown on Fig SI 2C, not B ?

Fixed

- I cannot see any significant signal at 5Hz in fig 2C that is continuous from 2021. I see a seasonal signal closer to 4Hz in Fig 2B or a seasonal weak signal at 2Hz in Fig 2C. Everything else seems weak and not continuous in 2B.

Yes, the 5Hz signal in Figure SI.2B (before C) starts from August 2021 but ceases with the onset of the 2.5Hz signal in early 2022, but it is not continuous. We rephrase and clarify the signal descriptions and remove some statements.

- You mention the small signal at 2.5 Hz in 2C but, as far as I saw, you forget about it later ?

We removed mention of this small signal, because we first thought it could be weather-related, but in the end, we didn't discuss it, since it doesn't appear in meteorological measurements.

In the end, this paragraph (L318-327) becomes:

" To better characterize the source signal, we run an analysis of geographic sub-network groups based on their position relative to the caldera. In particular, there is a Northwest group with JONS, KATT, and BRUN stations and a Southeast group with ASK, LIND, and STAM stations positioned closer to the Kárahnjúkar power station dam located Southeast of Askja (Figure 1). The Northwest group showed mostly high spectral width values with a few sources with narrow frequency band (Figure SI.2B). The Southeast group, closer to the Kárahnjúkar power station dam, which could be a potential source, showed a continuous signal between 3.5–5 Hz, accompanied by a weaker signal at 1.8 Hz (Figure SI.2C)."

- line 301: the stronger amplitudes start BEFORE your inflation line. Explanation ?

L551-570: KATT's greater amplitude could reflect a superposition of an early hydrothermal or fluid-pressure response with the seasonal pattern, making it appear stronger. But KATT is further from the inflation source than either JONS or STAM, so it is most likely due to local attenuation (site/local factors, possibly meteorological). This is now mentioned in the discussion section. The changes to the text are provided in the response to the next question below, which covers the same section.

- line 303: why would RIFR start changing in 2024 ? That should be added in the discussion or mentioned here.

L349, 558-559: RIFR is located within a fault swarm area. However there is no seismic activity during this period, so we also related it to local factors. This is now mentioned in the discussion section.

For the two previous comments, the paragraph (L551-570) in the discussion is now:

"Several processes could be responsible for the seasonal signal observed in the Displacement Seismic Amplitude Ratio (DSAR). Snow cover can attenuate high-frequency ground motion more than lower frequencies, potentially biasing DSAR during winter months. This effect has been explored at Mount St. Helens (Köpfli et al., 2024) and may contribute to seasonal variability observed across stations. The seasonal DSAR signal varies between stations depending on elevation, local slope, and distance to the hydrothermal system, linking differences in infiltration timing of snowmelt and groundwater dynamics rather than a regional signal (Köpfli et al., 2024). Local attenuation associated with discontinuous permafrost and buried ice (Etzelmüller et al., 2020; Shoemaker Thackston et al., 2024) may further contribute to differences between stations. Most stations show some timing differences and amplitude variations in DSAR likely related to these local effects (e.g., KATT in 2021 and RIFR in 2024, Figure 4). The dual-peak DSAR pattern observed at station ASK (Figure 4) could reflect both the combined influence of seasonal snow loading and enhanced groundwater recharge. These two processes could be enhanced at the ASK station location because of the steep slopes and drainage networks surrounding it. These topographic controls may increase subsurface fluid saturation, thereby intensifying seismic wave attenuation (Zhang et al., 2022), compared to flat-terrain stations. As seen, local seismic noise sources such as rivers, wind, and snow cover can radiate energy in restricted frequency bands and thus impact the amplitude ratio (Köpfli et al., 2024). Here, DSAR measurements cover the 6–16 Hz frequency band, while spectral width results show a source signal extending to 7 Hz. Therefore, the higher DSAR values in summer could be contaminated by this source signal and its seasonal variation. However, the long-term DSAR changes observed after the inflation onset, especially at STAM station (Figures 4 and 8C), and the absence of comparable changes prior to the inflation onset, suggest that these changes are unlikely to be only caused by local or meteorological factors."

- lines 304-310: this needs to be much more nuanced. The DSAR pattern at JONS cannot be discuss when there is basically no data prior to 2021. STAM seems to change, I would agree on this only. JONS value of 2.2 in 2021 is the value in 2018, the only time you have measurements.

L350-359: We changed the paragraph by distinguishing between STAM and JONS variations, and added a mention of the data gap that does not allow a clear comparison in time for JONS.

"Since the inflation onset in August 2021, DSAR at STAM shows an increase to 2.0, with this level maintained through June 2022. JONS also reaches high values following the inflation onset, but the data gap in 2019 and 2020 does not permit a clear comparison. Notably, both stations show divergence from the seasonal pattern, and the increase from the inflation onset occurs earlier, out of phase with the seasonal patterns visible before 2021 at STAM and in 2018 and 2023 at JONS. This contrasts with the concurrent 328 seasonal decrease observed at other stations. Following June 2022, both stations exhibit values below the median, with STAM at its minimum value of approximately 1.5 by 2024. From mid-2023 onwards, divergent behavior emerges: JONS gradually returns to higher values (2.0–2.2) while STAM remains at reduced levels (1.5)."

- Line 309 « Following June 2022, both stations exhibit marked decreases, with STAM declining to approximately 1.5 by 2024. » There is an increase in between, so this sentence is kind of wrong.

L357-358: We changed the passage in question by describing the variations from June 2022 with respect to values below the median. DSAR values are consistently lower than the median during this period for STAM and until the end of 2023 for JONS.

" Following June 2022, both stations exhibit values below the median, with STAM at its minimum of approximately 1.5 by 2024."

- Lines 311-312: while ASK do not change, KATT present a marked change in 2021 with stronger amplitudes that start before the inflation line. That is interesting.

L361: This is now more clearly described.

"KATT, in particular, exhibits the highest values just before the start of the inflation."

- line 319: « a drop occurred in August 2021 » the abrupt is true for JONS and STAM, but ASK is not abrupt. And for me STAM show the change in 2020, not 2021.

L369-370: The word "drop" has been replaced by "decrease" except when mentioning the JONS-STAM pair. The 2020 STAM-KATT change is also added.

"A decrease occurred in 2020 for the STAM-KATT pair and in August 2021 for the other pairs shown, including the largest drop of 60% for the JONS-STAM pair."

- line 331: there is no data for half of 2015, so you should nuance at least your interpretation of the 2015 minimum. Then you have a slight change for 2 years, true but it does not feel very strong as a result.

L384-386: We agree and have reorganized and modified the paragraph to acknowledge that the year 2014 (2015 has measurements), has a data gap. Also, we removed the minimum red dot visualization for this year on figure 5 (previously 6).

- Lines 339-340: can you explain how this does not bias the results ? That is a strong coincidence.

L394-397: We reword this passage to explicitly state that it is the dv/v yearly-amplitude values that are changing through time, which are not necessarily linked to the seasonal effect. The main focus of the study is the inflation onset in August 2021. The network change are administrative, so there is no bias in the results from 2016 to 2024. The reference was also selected in this period.

"The dv/v yearly amplitude decreased from 0.38% (2008–2011) to 0.29% (2011–2016) to 0.27% (since 2016), coinciding with a progressive increase in the number of contributing station pairs (Figure 5, network-code change from 4F to Z7 to 8K is only administrative)."

- Line 358: « evident » is a strong term here…

L414: We changed the word to "apparent".

- Line 366: I don't care overmuch but, in order, this should be Figure 8.

We changed the figure order so it is now figure 7 (since figures 2 and 3 have been merged).

- line 381; I find interesting that STAM has a drop before on EZ, around 2020

L618-623: Yes, there is lower seasonal dv/v in 2020, which coincides with increased precipitation and the frequency-dependent response is consistent with the observation during previous seasonal variations. We added a sentence in the discussion/seasonal variations section: "Additionally, we observe higher precipitation in 2020 (Figure SI.5B), which results in pore pressure changes lowering dv/v values (Hillers et al., 2014; Feng et al., 2021; Bruland et al., 2025) (Figures 5, 6, and 9)."

- Line 384-392: another paragraph that is over interpreted in my view. The changes are quite subtil here for ASK, particularly on EZ.

L461-468: We have reworded the paragraph to moderate our interpretation and focus on concrete observables, following your comment. The revised paragraph distinguishes component behavior at 3.5–5 Hz more explicitly. EZ retains a seasonal signal while EN shows episodic negative values without a clear seasonal structure. The post-inflation response is also described more precisely: EZ shows a shorter but more pronounced dv/v decrease, while EN exhibits a smaller amplitude decrease that persists longer, remaining below pre-inflation values even during intervals of positive dv/v. The changes to the text are provided in the response to the next three questions below, which cover the same section.

- « then the 0.5-1 Hz band trend reverse with the higher frequency bands(June 2022-mid 2023) » I don't understand this sentence.

L452-453: We have changed the text to "The 0.5-1 Hz band then shows a decreasing dv/v trend while the higher frequency bands show increasing trends (June 2022- mid 2023)."

- Line 386: the changes are not exactly regular either on EZ.

L461-463: We have modified the text accordingly.

- Line 387 I definitely don't agree with the weak impact on ASK EN when there is a relatively strong drop at high frequency.

L461-468: We have reworded this section accordingly.

For the description of the single-station dv/v at ASK, the paragraph L461-468 becomes:

"Pattern intensity and consistency vary between cross-components for each station. ASK cross-component dv/v (Figure 9) shows different behavior at 3.5–5 Hz, EZ keeps showing partial seasonal variations (not from 3.5–4.5 Hz in 2019 and 2020) while EN shows mostly negative values with episodic negative dv/v. Following the inflation, EZ dv/v shows negative dv/v values across frequencies for a couple of months. In contrast, EN values exhibit a stronger decrease in dv/v from October to June 2022. Under 3.5 Hz, the negative dv/v values remained below pre-inflation values even after an interval of positive dv/v values."

- Lines 388-391: JONS has a very large gap of data prior to 2021. You need to nuance your results considering this, you compare with non-existent values.

L469-470: Yes, the 2019-2020 data gap prevents direct visual comparison to pre-2021 conditions; however the dv/v values are computed using a reference period from 2018 to 2019 that is not impacted by this gap.

"JONS cross-component dv/v results have a 2019–2020 data gap that prevents direct visual comparison to pre-2021 conditions" (L469-470)

- Line 398-299: the negative values show up before your inflation line.

L634-636: The inflation line has been defined using the GPS data. The results are being influenced by seasonal variations that demonstrate decreasing dv/v during the period of the inflation onset. We cannot interpret the negative values before the onset in terms of the inflation because of the seasonal variations. And since the pattern of the change is similar to the previous seasonal patterns, it is most likely caused by seasonal variations. We have added this sentence to the discussion/inflation section (L634-636).

“The inflation started while seasonal processes were already likely inducing a decrease in dv/v. It is therefore challenging to ascribe the negative dv/v before August 8th, 2021, as a definite precursor to the inflation.”

- Lines 419-423: can you precise what happened when using data with large gaps ? JONS has no data in 2019-2020 for exemple but you use these dates here.

L290-292: Gaps are considered in the code. Each time segment is processed independently, with no interpolation or smoothing to fill the gaps. The station contributes measurements only during periods when data are available. The spatial resolution varies over time and is accurately reflected in our resolution matrix calculations. Indeed, stations with large gaps provide less information and have reduced influence on the final map, so it is necessary to identify these gaps before interpreting spatial change over time in the results. Areas with insufficient data coverage are masked based on the resolution threshold. If the gap were to be consequential to give reliable results, the map would show a blank square in the area without sufficient data, like in Figure SI.1 panels b and e.

We add the following sentence in the method/dv/v location section (L290-292):

“The station contributes to the results only during periods in which it recorded data. Therefore, the spatial resolution can vary over time and areas with insufficient data coverage are masked based on the resolution threshold”.

- Lines 453-457: this a very long sentence

L587-590: This sentence is now split into two sentences.

- Lines 489-490. It is still not clear for me why the inflation has no effects network-wide.

Figure 1 : The inflation is very shallow and localized, and only a subset of stations have data covering the inflation period that are close enough to the source to have its wave field impinging on the inflated area. The sill outline modeled by Parks et al. (2024), has been added to Figure 1 to give a better idea of the inflation size at the network scale. Additionally, inflation is causing a decrease in dv/v, which is already induced by seasonal variations. Thus, the more distant stations that could be impacted have this signal hidden in the seasonal variations that are more dominant.

- line 545: « FromJune 2022 to mid-2023, the dv/v trend reversal with low frequencies decreasing and high frequencies increasing could be associated to a deeper recharge » Still can’t see what you mean here.

L746-763: By using the moving reference, we observe other changes in dv/v during this period, compared with the static reference. Therefore, we remove this previous observation and remove the interpretation from the static reference results.

L742-759: “The stabilization of dv/v between October 2021 to February 2022 (Figure 7B), occurs at higher frequencies (1–5 Hz, sensitive above 2 km depth) before lower frequencies (0.5–1 Hz, sensitive up to 2–3 km depth). We interpret this frequency dependent response as reflecting a perturbation persisting at deeper levels and the impact of the intrusion body. We observe a similar behavior from January to April 2023 with the 1–3Hz dv/v showing a stabilization with increasing dv/v. Seasonal variations may contribute to these changes, since they occur around the same period of time and can cause large dv/v fluctuations, as shown previously. However, the dv/v values differ between frequency bands and expected seasonal trends, especially at low frequencies, consistent with the inferred location of the intrusion. The sustained higher DSAR values of JONS and STAM (Figure 4) until June 2022 align with the scenario in which pressure increases at 2.7 km depth from the initial magma migration, modifying fluid circulation and gas distribution near the surface. This could be associated with a recharge into the sill at 2.7 km depth (Parks et al., 2024; Fone et al., 2025), destabilizing the hydrothermal circulation.”

- line 548: how come this cessation of inflation appears only here ? This should be made clear in the introduction and discuss throughout the text !

L108-111, 761: We replaced the mention of cessation of inflation with “reduced rate of inflation”, and added more about the inflation evolution in the volcanic settings section. The uplift from sill inflation is superimposed on the seasonal signal; so where the inflation rate is much lower, the background seasonal signal masks it (less effect of the seasonal signal at KASC and JONC than OLAC).

- Lines 551-553: not really convinced about this equilibrium. There might be a small convergence, for some stations that have data.

L770: We remove the idea of equilibrium, since the main point is that the dynamic remains different from the onset of the inflation.

L769-772: “From September 2023, at JONS-STAM pair (Figure 7B), dv/v trends across frequency bands begin to converge, which could indicate that the system reaches a new state with established hydrothermal and magmatic pathways at multiple depths as inflation continues with a reduced rate.”

- Lines 560-563: I am surprised there were not one study on anisotropy for the region done for any time periods that you use ?

L695-700: We have rewritten the paragraph to include reference to Bacon et al. 2021 and Fone et al. 2025 who study azimuthal (from local earthquake shear wave splitting) and radial (from ambient noise tomography) anisotropy in and around Askja, respectively.

“Bacon et al. (2022) found seismic anisotropy associated with stress-aligned vertical fractures perpendicular to the plate spreading orientation with localized disruptions around Askja attributed to stress perturbations from magmatic activity. Fone et al. (2025) identified positive radial anisotropy beneath Askja at around 2 km depth, contrasting with predominantly negative values in the NVZ, which they interpreted as evidence of magma storage in a horizontal sill. In this study, component-dependent responses between horizontal (EN) and vertical (EZ) cross-correlations reveal different behavior after the inflation onset (Figures 7 and 9).”

- line 575: you mention increase in seismicity in your conclusion as if it was one of your results but you do not show the seismicity in any figure in association with your result and even do not talk about in the discussion at all. I agree it should be discussed, though.

L114-120 : We have removed mention of the seismicity in the Conclusion and added it to the main body of the article, in the volcanic settings section, and Figure 2.

L114-120: “From the Veðurstofa Íslands (2023a) earthquake catalog, we observed an increase in seismicity at the onset of the inflation with 68, 109, and 55 events in August, September, and October 2021, respectively, after filtering to only include events with ML > 0.65, to account for varying magnitude of completeness (Figure 2A). On a longer timescale, the inflation was accompanied by increased seismic activity, with a pre-inflation median of 15 events per month, which increased to 26 events per month from November 2021. Winder et al. (2023) showed that the seismicity rate increase at the onset of inflation is concentrated around the Öskjuvatn ring fault, close to the inferred sill (Figure 2B). Focal mechanisms indicated consistent reverse faulting parallel to the inner-caldera rim (Winder et al., 2023; Siggers et al., 2024).”

Figures:

- Fig ~~1~~: OLAC shows changes in the direction of horizontal displacement, as well as a relatively important gap in measurements. Can you discuss this a bit, particularly if the direction changes play a role in your different calculations ?

Figure 2B: The direction change has been corrected by IMO with the updated dataset at https://brunnur.vedur.is/gps/timeseries/timeseries/OLAC-plate.NEU. The associated figures have been updated.

- Fig 3: the seasonality correlates with the low number of trace counts, can it be a bias of calculations then ? Can you add a panel for trace counts on figure SI 5 to show if this is true or not, please.

L536-540: We have added panel F to Figure SI5 to show the trace counts used in spectral width computation. When the number of stations influences the visualization, it affects all frequencies, not just a single frequency band, as in the case from 2009 to 2011, this is not the case for the seasonal variations around 2021. We added the following text to the discussion: “The yearly variations of spectral widths (Figure 8B) follow the number of trace counts for a few specific years (2021, 2022, 2023; Figure SI.5F). Specifically, more stations are available in summer than in winter, most likely due to greatly reduced sunlight hours and snow-covered solar panels. However, in the other years, the spectral widths show clear seasonal variations without corresponding changes in trace counts. We therefore do not attribute the seasonal variations in the results to changes in station availability."

- Fig 6: how come nothing changes from 2021 ?

The inflation is relatively shallow and localized at the network scale. The inflation area covers 4.5*2.9km², while the network spans 40*35km². We can also see that the dv/v shown in Figure 10 aren't impacted when distant from the inflation source, noting that all station pairs are involved and cover a much wider region. We have added to Figure 1 the sill outline modeled by Parks et al. (2024), to give a better impression of the size of the inflation source at the network scale.

Recommendation: Revisions Required

Reviewer B:

This paper, "Integrated seismic monitoring reveals subsurface evolution during volcanic inflation at Askja volcano, Iceland" by Brenot et al. describes their work in looking at multiparametric data leading up to, during, and after an inflationary deformation event at an active volcano. This work is unique in that it combines several complementary ways of processing continuous seismic data that I have seen individually—but not together like this—into a cohesive story. The writing is overall excellent, the methodology is both well-described and well-founded, and the authors plan to release the full workflow in a public archive for reproducibility. I have a few small editorial comments and some musings about places for incremental improvement or additional discussion, but I think the manuscript is in great shape already.

Please find below a few comments that I hope will further improve an already excellent paper:

Line 35 (and throughout): Perhaps this will be better when read in the future if put as past tense ("…Askja volcanic system experienced rapid inflation […] (GNSS) station OLAC, which continued inflation beyond this study period…"), same in a few other places where references are presented in current rather than past tense.

Changed on lines 35, 36, 37, 131, 135, and further on.

Figure 1: Consider directly labeling the eruption location and power station on the map, rather than including unique labels. It might also be useful to separate the symbols by whether they are in the primary map or inset.

Figure 1: Done: labels now on the map and split legend.

Figure 2: You mention earthquakes in the section containing this figure, and it might be useful to show the counts of earthquakes with time alongside Figure 2A. In 2A, is zero displacement relative to elevation in 2015? Is there negative displacement, and if so, why is it not shown? I think also it could be useful to show line length change between OLAC and KASC since they cross the inflation source and could be useful as a proxy for horizontal strain in that direction. Is the northeastern motion of OLAC robust? What's the scale for the data shown in 2B (i.e., how much horizontal deformation is shown)?

Figures 1 and 2: We added the seismic data to the figures as requested. We allow the visualization to show negative GNSS values (relative to 2015) and added the distance change between OLAC and KASC (dashed black line). GNSS data have been redownloaded from https://brunnur.vedur.is/gps/timeseries/timeseries/OLAC-plate.NEU, and the data for the northeastern motion were corrected, which is now robust. Finally, we have added a scale to the bottom left of the figure associated with the horizontal displacements.

Line 151: Have you considered using the ERA5-land dataset instead? It has finer spatial resolution than the ERA5 dataset. I don't think it will change your findings, but potentially worth a look to check.

L174 : We changed the weather dataset to ERA5-land, thank you for the suggestion.

"From ERA5-land (Muñoz Sabater, 2019), we extracted temperature, air pressure, precipitation, and snow depth water equivalent measurements."

Line 184 (and possibly throughout): Add "e.g.," to the list of references when that list is far from exhaustive.

We added it to several lines, including L208, which corresponds to the previous line 184.

Line 263: This might be a good place to specifically mention where to find your full workflow for doing this analysis (I saw later in the data statement that you will release this on Zenodo).

L304: Added to method section

"The complete workflow can be found on Zenodo (see Data and code availability)."

Figures 3, 4 (maybe others): Consider making sure that these are full page width figures; they're quite small to read at single column width.

Figures 3 and 4 become Figure 3 A and B and change to full page width. The number of figures is limited to 10, so we merge the plots to add a tidy version (Figure 8) of the yearly seasonal variation plot (Figure SI.5).

Figure 5/DSAR in general: Is the 90-day median causal or centered? Just wondering how much your smoothing parameter here affects the phase. It's quite interesting how seasonal some of these are, but that the phase of the variations across stations isn't in sync. Why might that be?

Figure 4: The 90-day median is causal (backward-looking), not centered. We add this information in the caption of the figure.

L551-559: Each station essentially records the same seasonal forcing but filtered through its own local hydrological transfer function with the same driver, different timing and amplitude (elevation, local drainage, distance to hydrothermal system). We refer to this observation in the context of the work done by Köpfli et al. (2024) in the discussion.

"Several processes could be responsible for the seasonal signal observed in the Displacement Seismic Amplitude Ratio (DSAR). Snow cover can attenuate high-frequency ground motion more than lower frequencies, potentially biasing DSAR during winter months. This effect has been explored at Mount St. Helens (Köpfli et al., 2024) and may contribute to seasonal variability observed across stations. The seasonal DSAR signal varies between stations depending on elevation, local slope, and distance to the hydrothermal system, linking differences in infiltration timing of snowmelt and groundwater dynamics rather than a regional signal (Köpfli et al., 2024). Local attenuation associated with discontinuous permafrost and buried ice (Etzelmüller et al., 2020; Shoemaker Thackston et al., 2024) may further contribute to differences between stations. Most stations show some timing differences and amplitude variations in DSAR likely related to these local effects (e.g., KATT in 2021 and RIFR in 2024, Figure 4)."

Section 4.3.4: Perhaps I glossed over it in the discussion, but what's the interpretation for there being such a difference in behavior between the different cross-channel results?

Different components preferentially record different wave types, with Rayleigh waves dominating the radial-vertical components and Love waves the transverse components (Lin et al. 2008, Nayak et al. 2020, Savage et al. 2013), leading to different sensitivities between vertical and horizontal correlations. The EZ component demonstrates more localized but strong responses in specific frequency bands, whereas the EN component exhibits broader frequency responses and recovers seasonal behavior from 2023 onward. These differences likely reflect varying contributions of vertically and horizontally polarized wavefields, which may interact differently with the anisotropic crustal structure beneath Askja, where stress-aligned fractures and radial anisotropy have been documented at comparable depths (Bacon et al. 2022, Fone et al. 2025). The origin of these component-dependent differences remains uncertain, but their consistency across stations supports the observed velocity changes associated with the 2021 inflation. We have clarified this in the discussion L695-710.

Figure 9 (and general for this section): The drop in cross-correlation across the intrusion on the JONS-STAM ZZ pair is striking. Would it be useful to have a second reference from after the intrusion to help better resolve the changes from mid-2021onward? This is one of my personal issues with using a single reference (i.e., what happens when your single reference is no longer useful).

We added the results of a moving reference in what is now Figure 7B. One paragraph is added at the end of section 3.4 (L236-247), the end of section 4.3.3 (L432-440), and in the discussion (583-587, 712-725, 746-758) to describe this additional analysis.

The moving reference is computed on a 10-day moving window that ends 10 days before each processed date. The computed dv/v are then stacked over 10 day intervals to give delta-dv/v and summed to retrieve dv/v.

Across all the references used, the impact of inflation on dv/v is significant. The moving reference dv/v definitely shows more reliable results during the inflation than the pre-inflation static reference.

Lines 453-461: You're probably right that it's dominantly snow causing the seasonal variations. I'll mention that another possibility is the presence/absence/percentage of frozen ground. Hotovec-Ellis (2024) showed that at Mauna Loa where there is no continuous snowpack that strong seasonal variations with higher velocities in winter at high elevation stations still existed, with the only logical cause being the presence of ground ice. I don't know if you have enough evidence to be able to tell the difference, but it could be worth consideration.

L590, 614, 623-625: We have added mention of ground frost and now reference Hotovec-Ellis (2024). With our observations, it is challenging to unravel the influence of ground frost. The elevation difference within the network is 664m. The region has discontinuous permafrost, especially at elevations > 1 km (JONS 1174m, STAM 1171 m) (Etzelmüller et al., 2007, 2020). Additionally, there is buried ice in some areas (Shoemaker Thackston et al., 2024), that will most likely not influence the seasonal variations, but the hydrological and thermal consequences of its presence could influence the seismic waves. Air temperatures are below 0°C during 8 months of the year (Figure 8A), and all the stations are impacted by the ground frost as well as the snow mass (Figure SI.5D). Donaldson et al. (2019) show that at comparable stations, ground warming occurs later than the annual decreases in dv/v, suggesting the frost thaw is unlikely to be the dominant driver of seasonal variations. Even if the elevation range is sufficient to impart some differences in the amplitude and timing of seasonal signals between stations, it is likely not sufficient to produce enough evidence due to the spatial discontinuity of the ground frost (in contrast to Mauna Loa). There is partial permafrost and buried ice, but the entire network is exposed to snow and freezing temperatures.

L613-629: "Therefore, to explore the impact of seasonal variations, it is critical to consider the influence of snow thickness (Wang et al., 2017; Yates et al., 2024), ground frost (Hotovec-Ellis, 2024), atmospheric pressure (Silver et al., 2007), groundwater level (Clements and

Denolle, 2018), and temperature (Richter et al., 2014; Bruland et al., 2025). Snow mass shows a clear positive correlation (Pearson r ratio is 0.75 from 2016 to 2020) with dv/v (Figure SI.5D), having at least twice the influence on the annual cycle amplitude change compared to atmospheric pressure (Donaldson et al., 2019). Additionally, we observe higher precipitation in 2020 (Figure SI.5B), which results in pore pressure changes lowering dv/v values (Hillers et al., 2014; Feng et al., 2021; Bruland et al., 2025) (Figures 5, 6, and 9). Donaldson et al. (2019) showed that the annual decreases in dv/v occur prior to the ground warming, suggesting ground frost does not dominate the seasonal variations. Also, as the entire network is exposed to freezing temperatures (Figure 8A) and seasonal snowmass (Figure SI.5D), stations above an elevation of 1 km may also be influenced by discontinuous permafrost (Etzelmüller et al., 2020; Shoemaker Thackston et al., 2024). Disentangling at each station the respective contributions of snow loading (Wang et al., 2017; Yates et al., 2024), buried ice (Shoemaker Thackston et al., 2024), and other meteorological factors remains challenging."

Lines 499-503: Do the DSAR changes have to be associated with changes in volatiles? Could they also not be related to the same changes in the medium (scattering properties, velocity change, etc.) as the velocity changes (i.e. ground strain)? Those would also be mostly shallow. Perhaps I'm just missing some additional context in the discussion.

L741-745: We agree and mention it in the discussion section by adding the following paragraph: "The observed surface inflation confirms that deformation from the deeper magmatic intrusion reaches the surface. It could drive shallow processes in the upper 1 km that are observed in the DSAR changes, including crack opening, stress field reorganization, scattering property changes (Köpfli et al., 2024), and volatile redistribution (Caudron et al., 2021). The sustained and spatially heterogeneous DSAR anomalies at JONS and STAM persisting beyond the initial inflation onset indicate complex subsurface process interactions, with variable network response reflecting local effects."

Line 533-537 and Figure SI.4: Is it not possible that all the changes are occurring shallowly? I know that studies like to point out in the depth sensitivity kernels that there is a maxima at certain depths, but there is non-zero sensitivity at the surface for most frequencies. Certainly, the amplitude might be reduced, but these waves are still sampling the shallowest volume, right? Unless there's a very robust and well-defined difference in the behavior at the different frequency bands, I guess I'd just like to challenge the assumption that they need to be due to separate processes.

L717-719: We agree with the reviewer that all changes could occur at shallow depth, given the non-zero sensitivity of all frequency bands near the surface. We have reworded the sentence to acknowledge that frequency-dependent signatures could reflect varying shallow responses to a single process or involve separate processes at different depths. Nevertheless, some changes occur at different times and are not always visible across all frequency bands. For example, when changes are restricted to low frequencies, it is most likely that the change occurs deeper compared to what is sampled by the higher frequencies. This is what we observe with the moving reference dv/v (Figure 7B), where some changes are shifted in time from one frequency band to another and don't always follow the same trends. Additionally, the most significant and sharpest change at low frequency occurs only at the inflation. Seasonal variations have less impact on the 0.5-1 Hz frequency band in contrast to higher frequency bands (Figure 7).

L717-719: "These observations could be associated with varying responses to a single process across depth, as well as frequency-dependent signatures involving separate processes at different depths."

Lines 557-559: Calling back to my note about using a second reference, I think it would be useful to demonstrate the permanence of the amplitude change of the seasonal changes with a different reference. If it's still less using a reference after the intrusion, I'd be more inclined to believe this.

L236-247, 432-440, 583-587, 712-725, 746-758: We added the results of a moving reference in Figure 7B.

Figure SI.3: Saturate the amplitude scale at a lower threshold. The coda window is basically invisible as shown here.

Done Figure SI.3

Figure SI.5: With a bit of cleaning up, I think this would serve better as a figure in the main text. It lays out nicely the timeseries that are seasonal (and several that are not). There are a lot of lines; it might be worthwhile to add some line patterns (dashed, dotted, bold) in addition to the color to make specific time periods easier to find.

We have included a tidy version of Figure SI.5, with the temperature, minimum spectral width, DSAR and dv/v in the main text (see Figure 8). We add some line patterns and a marker for the onset of the inflation and data gaps are also not filled by connecting lines if the gap is longer than 14 days. We keep SI.5 with the original panels (and added a panel with spectral width trace counts).

I hope you find these comments useful, and I'm looking forward to seeing this in print!

Recommendation: Revisions Required

Thank you both very much for your constructive and stimulating comments that massively improved the manuscript.