Rebuttal letter

We thank the editor and both reviewers for their assessment and very constructive feedback, which has greatly improved the manuscript! **All line numbers below refer to the document 'Track_changes.pdf'.**

Reviewer #1:

We have incorporated all the requested minor changes in the PDF.

- Citations and typos: **lines 65, 68, Caption Fig. 1 (p. 4), Caption Fig. 2 (p. 7)**
- Manually refined absolute locations from SeisComP: **lines 114-118**
- Added depth for the dipping seismicity: **line 431**

I find all your figures excellent and easy to navigate/understand. However, for figure 8 I find it a bit hard to always have to rotate the depth cross-sections in my mind. Everything is clearly labeled, but I'm wondering if it's possible to have them rotated to lie in the same direction as on the map? This is most confusing for the side views - however easier with every read. Perhaps this issue could be solved by projecting the IDDP on to the depth sections?

Thanks for pointing this out! – We agree that the spatial orientation in the original figure was somewhat confusing. We decided to remove the azimuth positions from the images and instead mark the observers' viewpoints for the side views on the corresponding maps. This makes it clear from which direction the cross-sections are being viewed. We hope that these changes improve the clarity and readability of the figure!

'The observer viewpoints for these panels are indicated on the map by arrows' (**Caption Fig. 8, p.16**)

I'm wondering how much the earthquake catalogue is influencing you results. It is a SeisComP automatic catalogue which is manually reviewed. If you would have a denser (more complete) earthquake catalogue with perhaps 50% more events by using another detection algorithm, like a migration based on. How much would that affect the results/imaging?

We have added a section discussing this in the main manuscript (**lines 364 – 371**):

'Spatial illumination could be improved by using a more complete earthquake catalogue. Although we rely on manually reviewed event locations, ensuring a high level of reliability in phase picks, smaller events may not have been fully captured. In future work, advanced detection and location techniques—such as template matching (e.g., Gibbons et al., 2006, Li et al., 2020) and Deep Learning-based methods (e.g., Dokht et al., 2019)—could be used to recover additional events. This could fill data gaps and increase the signal-to-noise ratio during the stacking process, improving the detectability of weak or discontinuous reflectors.'

I would like to know more about what you mean by eq mislocation, if it isn't due to the velocity model?

The mislocations refer to uncertainties in hypocentre locations (**lines 375 -377**). We modified the sentence for clarity (**lines 388 -391**): 'We therefore speculate that, at least for shallow reflectors, degradation of the image quality is more likely caused by earthquake mislocations than by inaccuracies in the velocity model; that is, by errors in the estimated hypocenter positions (both lateral and depth), which lead to mispositioned inferred reflection points and therefore a distortion or smearing of coherent structures in the final image.'

<u>Reviewer #2:</u>

Given the level of coherency of reflected arrivals in their data, the authors could try extending the migrated image to larger depths without muting the S-wave coda (only mute the direct S-wave). There is a possibility that the Moho appears from either P-P, or P-Sv, or Sv-P, or Sv-Sv phases. I don't exactly know how deep the Moho is underneath Krafla, and if it is gradiational or not (I would expect a clear contrast for thickened oceanic crust), but if a clear Moho is visible it would be a meaningful result for this methodology. I have looked for Moho reflections in the past with RVSP (Fig 9 & 10 in Quiros et al. 2015 - Aftershock Imaging with Dense Arrays Following the Mw4.0 Waterboro Earthquake of 16 October 2012 Maine, U.S.A.) using a single microearthquake, I think it is worth exploring, and this dataset might be just right for it, as the reflection coefficients might be sufficiently large.

Extending the migrated image to greater depths and exploring the potential for Moho reflections would be very interesting. In the present study, we deliberately avoided including the S-wave coda in the migration. The reason is that we don't want to overinterpret phases or spurious reflectors in the images, as the wavefield becomes progressively more complex with increasing traveltime due to interfering and overlapping phases. That said, reflections from the Moho are expected to be comparatively strong, and we agree that investigating such deeper signals would be a valuable extension of this approach, and a good direction for future work.

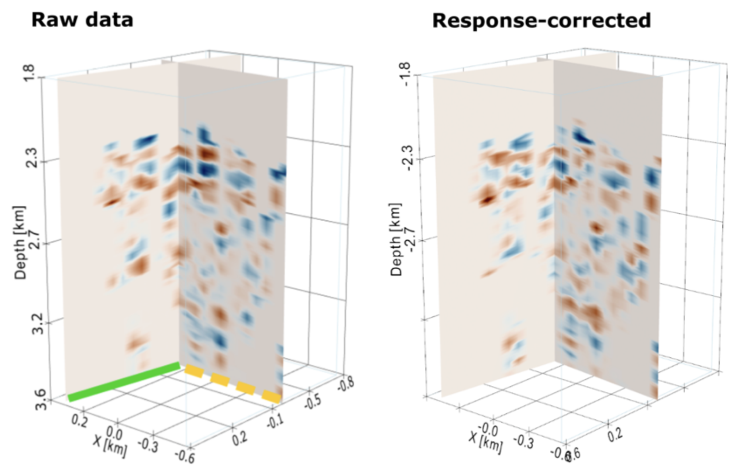
Another point I think is worth making is that a single event could be potentially really useful, if the reflections/conversions have high SNR, stacking is not needed, and avoids the issues with polarity from multiple events, and errors in the stack due to hypocenter uncertainty. Similarly, if multiple events which show the same reflector are used to create separate single fold images, the differences in the depth of this reflector can be used in fine-tuning the relocation of the events, to improve the stacked image.

We agree with that! Analyzing single events avoids some of the challenges associated with stacking, such as polarity differences between events and errors related to hypocenter uncertainty. This is to some extent illustrated in Fig. 5, where individual events are compared with synthetic waveforms and already show good agreement. In future work, it would also be useful to revisit individual events to better constrain focal mechanisms and to include amplitude information, which we have not considered here. This could help us to constrain seismic impedances and thus provide further insights into the properties of the reflector.

In terms of the preprocessing described in section 4.2.1, I think the response removal (i.e., response-corrected) is not necessary and runs the risk of introducing artifacts (depending on how it is done). The nodes appear to have a corner frequency of 5 Hz, and one would assume the instrument response if flat above the corner frequency, specially since there is a bandpass filtered applied between 5 - 25 Hz. So perhaps the authors might want to consider exploring running their workflow without instrument response correction.

Upon revisiting the code, we realized that results presented in the paper were based on raw data rather than instrument-response–corrected data. During processing, we tested both corrected and uncorrected datasets and found that the results are very similar with some minor differences, with the same reflectors clearly resolved (see figure). Given the flat instrument response in the 5–25 Hz band, we ultimately opted to use the raw data to minimize processing. However, we did not remove the sentence about instrument correction in the original version of the manuscript. We have now corrected this (**line 206**). Thank you for bringing this to our attention!

Figure 8g from main manuscript:



Lastly I think it is worth mentioning in the discussion the potential these types of datasets have for transient source interferometric imaging (i.e., treating the events as controlled sources). Potential uses of this type of processing would be to redatum the surface receivers to hypocenter locations. This can be particularly useful for avoiding a complicated overburden (imaging horizontal targets such as a magma sill) or for imaging of vertical targets such as vertical migration of magma. Myself and colleagues have looked at this imaging method in Chamarczuk et al 2021 - Towards adapting reverse vertical seismic profiling for ambient-noise imaging with transient sources (e.g., Fig 1 & 3), and the potential for imaging in volcanic settings with microearthquakes is there, this would function as an extension of the standard RVSP methodology when imaging subvertical targets that are not illuminated by the standard RVSP microearthquake method.

We have added a section in the discussion (**lines 497 - 501**): 'Moreover, the high station density of the seismic network enables the application of interferometric techniques to microearthquake data, in which seismic wavefields are redatumed from surface receivers to earthquake hypocenters using cross-correlation approaches (e.g., Curtis et al., 2009, Tonegawa et al., 2010, Chamarczuk et al., 2022}. Such redatuming can reduce the effects of complex near-surface structure and thus represents another promising direction for future work.'