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Warszawa, March 16, 2026

Prof. Puy Ayarza
Handling editor

Dear Prof. Ayarza,

I hereby submit revised version of our article "*Lower Carboniferous igneous intrusions within the crystalline basement of the Baltic Basin (SW edge of the East European Craton, Poland) – insight based on seismic data interpretation and seismic forward modelling*" (P. Krzywiec, Ł. Słonka, P. Poprawa). The paper is intended for publication in the Solid Earth Special Issue „*Seismic imaging from the lithosphere to the near Surface*", guest edited by Christopher Juhlin, Puy Ayarza, Ayse Kaslilar, Michal Malinowski and David Snyder.

First of all, we'd like to thank the reviewers for their very constructive and insightful comments, which helped us to strengthen and clarify the manuscript.

Please find below our comments and replies to their main comments and questions. We provide the following files:

- pdf file with the revised text with figures
- pdf file with revised text with figures, redlined (with tracked changes)
- author responses to the reviewers' comments (also provided as PDF supplements attached to the individual replies)

We hope our paper may be accepted for publication in the Solid Earth Special Issue.

sincerely yours,

Piotr Krzywiec, on behalf of the co-authors

I. COMMENTS FROM REVIEWER 1 (R. BUTLER)

Comment 1: *Seismological imaging of igneous intrusions within the continental crust is an area of extensive, global, continuing research. Much of this effort globally has used passive seismic methods applied to teleseismic arrivals. The data from deep seismic reflection profiling, much of it legacy, is an under-used resource and so I welcome this contribution by Krzywiec and co-workers. It is an interesting contribution that interprets and models results of deep seismic reflection profiling in Poland (PolandSPAN), focusing on discontinuous mid-crustal reflection packages. The authors have an opportunity here to make this study far more useful to the global community of earth scientists by drawing out the general application of their methods and making broader comparisons with other examples. It is this recommendation that underpins many of the comments in this review. As written, the paper tends to the parochial – the authors have a great opportunity here to do something more! So while this could be published with only minor modifications, I encourage the authors to reframe the text in this light.*

Response: We welcome comment that results of our study could be useful to the global community of Earth scientists. However, scope and focus of our paper submitted to the Solid Earth Special Issue „Seismic imaging from the lithosphere to the near Surface" is part of our overall strategy on publication of our results obtained for the newly defined Lublin-Baltic Igneous Province. Our first paper (Poprawa et al., 2023, Alkaline magmatism from the Lublin-Baltic area of Poland (SW slope of the East European Craton) – Manifestation of hitherto unrecognized early Carboniferous igneous province, Terra Nova, 36 (1), 77–88, doi:10.1111/ter.12681) contained summary of geochemical and geochronological results together with briefly mentioned seismic examples of magmatic intrusions from the Baltic Basin (N Poland) and Lublin Basin (SE Poland). In our paper submitted to SE we focused on geophysical aspects (seismic imaging and modelling) of deep magmatic intrusions within the basement of the Baltic Basin. It is also important to note that this paper is based on presentation that was given during the Seismix 2024 conference, i.e. for the seismic community, to a large degree interested in strictly geophysical aspects of seismic imaging, seismic modelling etc. Next paper will be devoted to similar modelling study of the Lower Carboniferous intrusions but developed within the sedimentary cover, and then we plan to prepare paper dealing with various regional geological aspects of Early Carboniferous magmatism, using PolandSPAN but also other industry data available to us. Taking this into account, we'd highly appreciate if structure of our paper could remain unchanged.

Comment 2: *Given then opportunities noted above, it would be useful to clarify the aims of this contribution. Of course, large igneous intrusions are exposed at outcrop, exhumed from various levels (including deep). The point here is detecting them in the subsurface, potentially linking them to coeval extrusives at outcrop. But why does this have importance for understanding anything other than the evolution of some local continental crust (i.e. beneath Poland)? As noted in the geological setting... the region has igneous rocks at outcrop. Is the point here to establish their extent at depth? If yes, say so!*

Response: Entire Paleozoic succession of the Baltic Basin in Poland is covered by Permo-Mesozoic and Cenozoic sedimentary cover so there are no Lower Carboniferous igneous rocks at outcrop, hence it is not possible to link seismically imaged sills to their outcrop equivalents.

Comment 3: *Or is it about establishing the geometry of intrusions? Do sills emplaced in the basement have the same propensity for steps and splays as they do when emplaced in layered sedimentary rocks, nearer the syn-volcanic earth's surface? The abstract does a better job at*

setting the scene. Likewise, the general importance of this study, the opportunities provided by deep seismic reflection profiling, the methods used here, and the general interpretations could be developed much more fully in the discussion.
Response: As stated at the end of Introduction chapter, "*Main aim of this study was to constrain lithology, thickness and overall geometry of inferred igneous intrusions recognized at the depths of c. 6-7 – 19-20 km, well beyond the reach of even deepest wells drilled in this area. Obtained results significantly improved understanding of the recently recognized Lublin Baltic Mississippian Igneous Province (Poprawa et al., 2024) and also deep igneous systems in general, including also geophysical approaches towards determining their petrophysical and geometrical characteristics using seismic modelling techniques.*"

Comment 4: *At various points in the text, the authors' approach is described as "educated guessing". While this is very self-effacing, it is of course common to all interpretation – which naturally always carries uncertainty and assumptions. It would be helpful to be explicit about the limitations imposed by the specific assumptions adopted here*
Response: Paragraph explaining this has been added at the beginning of section 3.3.

Comment 5: *Section 3.1. It's good to see the information on the acquisition and processing parameters for the PolandSPAN survey. Presumably it is the quality of these that has allowed the detailed work here (as the authors say, these are unparalleled). But if the authors choose to broaden the vision of this paper to promote the greater use of legacy surveys elsewhere – it would be worth comparing the PolandSPAN parameters with other, historical, onshore surveys (ECORS, FIRE, various Iberian experiments etc).*
Response: Information on acquisition parameters of "classic" deep reflection seismic data (ECORS and POLCRUST) has been added in section 3.1 and compared to PolandSAN acquisition parameters

Comment 6: *Section 3.3. Introductory paragraph (lines 181-187). The arguments for adopting synthetics rather than ray-tracing methods needs referencing to justify these statements – especially as the only citation (Slonka et al) is to a study of sedimentary rocks rather than as a tool to enhance crustal imaging.*
Response: Our intention was to briefly explain the rationale for using full-wave forward seismic modelling (not synthetic seismograms) rather than ray-tracing approaches for analysis of seismic response of deep igneous intrusions. One of the key advantages of full-wave modelling is that, in contrast to ray-tracing methods, it simulates the complete seismic wavefield, which is particularly useful for analysis of interference effects, reflection amplitudes, and tuning phenomena associated with thin bodies such as, for example, magmatic sills. In the context of the intrusions interpreted in this study, where seismic wavefield characteristics were investigated using amplitude tuning analysis and forward seismic wedge modelling in order to estimate intrusion thickness, the use of this full-wave modelling approach was therefore fully justified. In the revised manuscript, we have clarified this paragraph and expanded the reference list to include studies that used seismic forward modelling to seismic reflection data analysis in order to investigate the seismic expression of igneous intrusions and related structures (e.g., Hansen et al., 2008; Magee et al., 2015; Eide et al., 2017; Wrona et al., 2019; Köpping et al., 2022). These studies demonstrate how forward seismic modelling and detailed analysis of seismic wavefield characteristics can be used to interpret intrusive bodies and their geometries, including those occurring within the lower crust. The text in Section 3.3 has been revised accordingly.

Comment 7: *3.3.1. This discusses the selection of modelling parameters. It would be helpful to state explicitly the values (and their ranges) as used in this study (velocities, densities). And be specific in the comparisons made with the geology (end sentences of this section).*
Response: The velocities and densities used in the modelling were already presented in Table 1 and discussed in Section 4.2, where the three tested lithological scenarios (granite-granodiorite, basalt and dolerite) and their petrophysical parameters are described in detail but for clarity, we have indicted representative velocity and density values used for the tested lithologies and by adding a direct reference to Table 1 and Section 4.2, where these parameters are summarized. We have also slightly clarified the final sentences of this section to better explain how the comparison between synthetic and observed seismic responses was used to evaluate the most plausible lithological scenario. The text in Section 3.3.1 has been revised accordingly.

Comment 8: *Section 4. Line 253 – why was 500 ms used as the wavelet extraction window? What are the implications of changing this?*
Response: The 500 ms extraction window was selected as a compromise between statistical robustness and interval specificity of the estimated wavelet. Shorter windows provide fewer samples and tend to produce wavelets that are more sensitive to local noise and amplitude fluctuations, whereas longer windows may incorporate reflections from adjacent stratigraphic intervals and reduce the representativeness of the wavelet for the target intrusion zone. Tests performed using different window lengths indicated that a 500 ms window provides stable and repeatable wavelet estimates in the studied depth range. Increasing the window length did not significantly improve spectral stability, while shorter windows resulted in less consistent bandwidth and phase characteristics. Changing the window length primarily affects the bandwidth and phase stability of the extracted wavelet: smaller windows reduce statistical reliability, whereas larger windows tend to smooth the spectrum and may obscure interval-specific features.

Comment 9: *Line 260-2. Please be explicit that the tuning thickness chart is Fig. 6c (it's in the caption but it makes the text easier to follow if in there too). Maybe note that tuning thickness charts are also provided for Fig 7 and 8.*
Response: Information that Figures 6C, 7C and 8C contain tuning thickness chart is now clearly specified in the text

Comment 10: *2-D seismic modelling. It's good to contrast acid/intermediate composition (granite-granodiorite) with basic-composition igneous rocks. However, it's curious to use parameters from both basalt and dolerite. Presumably the basalt ones relate explicitly to lavas while dolerite are for intrusive rocks (the difference not being compositional but textural – as basalt may have porosity). Surely only dolerite is relevant here (e.g. rhyolite hasn't been modelled along with granite). But more critically – given the thickness of the interpreted igneous bodies (up to 200m for the wedge approximations) – these are likely to be layered – and could include ultrabasic compositions… (And indeed the basic component is likely gabbro). So over-emphasising specific values on seismic velocity and density is un-necessary – beyond eliminating those for acid/intermediate composition rocks. Simply setting this up as a two-comparison acid/intermediate vs basic (dolerite/gabbro) test – would suffice.*
Response: The purpose of including both basalt and dolerite scenarios in the modelling was not to distinguish between extrusive and intrusive volcanic rocks in a strict petrological sense, but rather to test a plausible range of petrophysical parameters for basic igneous compositions. In particular, basalt and dolerite represent commonly reported end-member values of seismic

velocity and density for mafic rocks in the literature (cf. Brown and Kim, 2020). In this context, the basalt parameters were used as representative values for a lower-velocity mafic end-member, while dolerite represents a higher-velocity intrusive equivalent typical of mafic sills and dykes. Testing both parameter sets therefore allowed us to evaluate the sensitivity of the synthetic seismic response to variations in acoustic impedance within the plausible range of mafic compositions. In the revised manuscript, we have clarified that the basalt parameter scenario was introduced to test a plausible range of petrophysical properties for mafic rocks rather than to imply the presence of volcanic basaltic lithologies at the depth of the interpreted intrusions. We also note the Reviewer's comment that intrusive bodies of the thickness considered here could potentially include gabbroic lithologies. However, the seismic velocities and densities reported for gabbro overlap strongly with those of dolerite in published laboratory measurements. Therefore, the dolerite scenario used in our modelling can be regarded as representative of dense mafic intrusive rocks in general.

Comment 11: *I'd expect to see justification for the host rock parameters ("Precambrian basement") – as of course it is the contrast with this that is important for investigating the intrusions.*
Response: General description of basement geology is given in chapter 2 (Geological background), section 4.2 and Table 1 contain information on wells used to determine basement petrophysical parameters (velocity, density).

Comment 12: *This aside apart – the modelling section reads well. The illustrations are nice (Figs. 10 and 12, 11 not needed if you drop the basalt!)*
Response: As explained above, we'd like to keep the basalt as one of petrophysical scenarios

Comment 13: *The wedge model and the deductions of the thicknesses of the sill, together with its steps – are great (Figs. 14, 15, 16). Although the authors have chosen to reserve further comparison with outcrop "analogues", choosing only to compare with seismic examples of Cartwright et al. – I think again that this is a missed opportunity. Stepping and branch sills have been known and illustrated in the geological record for over 200 years (yes, really – Macculloch 1819) ... which are well-known to the igneous geology community... Simply citing work from the 2000s onwards isn't great!*
Response: We added references to papers with field examples of stepping of sills etc.

Comment 14: *The Discussion focusses on Poland and its geology. As noted above, this would be far more valuable if these discussions were broadened out. The final paragraph hints at this. More discussions should be made about the underpinning assumptions*
Response: Yes, this is the focus of the Discussion, but, as explained above, this was intentional, this paper is meant to deal with more geophysical aspects of seismic modelling of magmatic intrusions, we'd like to save more extensive discussion of various geological aspects, including global analogues, for another paper, we sincerely hope that this is justified and could be approved

Comment 16: *Seismic Interpretations. The seismic interpretations (figs 3, 4, 5) should include uninterpreted (clean) images.*
Response: Panels with uninterpreted seismic profile have been added to these three figures

Comment 17: *As presented – the images have very high vertical exaggerations. Is the deep normal fault in Fig 5 really dipping at just 25 degrees (as implied by this interpretation when at ~v=h...)?*

Response: It should be noted that this regional profile is almost parallel to the Teisseyre-Tornquist Zone that separates East European Craton and the West European Platform so indeed at such intersection deep crustal-scale faults of this zone might have such geometry

Comment 18: *Figure 1 – please change the colours between extrusive and intrusive igneous rocks – they're too similar at the moment.*
Response: Corrected

II. COMMENTS FROM REVIEWER 2 (L. BROWN)

Comment 1: *In general the paper is well written with only a few grammatical glitches. The discussion of the geological setting is relatively clear and thorough. The figures are generally of excellent quality. However the figures purporting to show the results of the seismic wavelet analysis generally lacking sufficiently detailed depictions of the actual data that are needed to evaluate the validity of some of the modeling conclusions.*
My biggest criticism is that the discussion of the wavelet analysis, intended to reveal primarily the thickness of the interpreted intrusions is presented in a confusing manner, and lacks any clear comparison of the modeled waveforms with the observed waveforms on a common scale that allows for the assessment of the modeling results. Moreover important details of the analysis are not explained.
For example, a first step in the analysis as described by the authors as "wavelet extractions" within a window encompassing the deep reflections of interest. What does this mean? Deconvolution? How can the wavelets be "extracted" without making some a priori assumption of their waveform? Since the analysis all about interpreting these waveforms, how were they extracted without altering them. Moreover, to what extent are the wavelets of interest "contaminated" by multiples generate in the overlying sedimentary sections Was deconvolution use, and could such deconvolution have distorted the intrusive reflection wavelets?
Response: We agree that the original description of the wavelet extraction procedure was insufficient and potentially confusing. We have now clarified that the wavelets represent effective statistical wavelets estimated from fully processed pre-stack time-migrated (PreSTM) seismic data provided by the PolandSPAN project.
The seismic data used in this study were obtained as final PreSTM and PreSDM datasets, acquired, processed, and migrated prior to our analysis following regional workflows described by Mężyk et al. (2019). It should be noted that the original processing workflow included deconvolution before pre-stack time migration, as well as multiple attenuation procedures. However, we were unable to determine the direct impact of these steps as we did not have access to field data. We did not perform or modify any processing steps ourselves; we worked exclusively with the fully processed datasets that were available for interpretation.
The wavelet was estimated directly from the already processed seismic data and was not modified or altered by the authors.
Wavelets were derived using a statistical frequency-matching approach, which estimates a zero-phase wavelet whose amplitude spectrum matches that of the seismic data within the selected time window. This method does not impose an a priori waveform shape and reflects the combined source signature and propagation effects rather than a true source wavelet.
No dominant multiple energy was observed within the analysed time windows used for wavelet estimation. Consequently, the extracted wavelets are considered representative of the primary reflection response from the intrusion interval.
The manuscript has been revised accordingly.

Comment 2: *A related concern is modeling the waveforms using the Ricker wavelet. The Ricker wavelet is usually associated with impulsive sources such as explosives. However the data shown here is implicitly from Vibroseis sources, although that is never explicitly stated in the paper. Vibroseis correlation results in a Klauder wavelet. How do the authors justify using the Ricker wavelet for modeling of what are presumably Klauder wavelets in the actual data? Were the data subjected to a Klauder to Ricker transform.*

Response: We thank the reviewer for raising this important point. The seismic data used in this study were obtained as fully processed and migrated PreSTM and PreSDM datasets, and we did not work with field records or perform any additional signal processing, including any Klauder-to-Ricker transformation.

Our modelling was not intended to reproduce the original Vibroseis source signature or the detailed shape of the correlated Klauder wavelet. Instead, the objective was to investigate tuning effects and vertical resolution using simplified forward modelling based on the effective wavelet present in the fully processed seismic data.

We first extracted an effective statistical wavelet from the pre-stack time-migrated data using a frequency-matching approach. This extraction yielded dominant frequencies in the range of approximately 29-30 Hz, depending on the analysed polygon. Based on this result, we adopted a zero-phase Ricker wavelet with a dominant frequency of 29 Hz as a simplified approximation of the effective seismic bandwidth.

The Ricker wavelet was therefore not used as a replacement for the true Vibroseis wavelet, but as a deliberate and controlled simplification suitable for analysing tuning effects and thickness-dependent waveform interference. This approach is commonly applied in wedge and tuning studies, where relative waveform behaviour is of primary interest, rather than precise reproduction of absolute amplitudes or source signatures.

We acknowledge that this represents a simplification of the true seismic wavelet. However, given the objectives of this study and the use of fully processed migrated data, we consider this approach appropriate and sufficient for the first-order analysis presented here.

The manuscript has been revised accordingly.

Comment 3: *The modeling of the data appears to consist of three parts. The first is essential 1D modeling to determine the thickness of the presumed intrusion layer by matching amplitudes peaks and wavelet peak to trough measurements of real data with synthetics for a simple high impedance layer. However there is no explicitly explanation of how the apparent peak to trough time measurement of the composition waveform relates to the impedance model.*

Response: We agree that the relationship between apparent peak-trough separation and layer thickness was not sufficiently explicit in the original manuscript and may have led to confusion.

We would like to clarify that Section 4.1 does not involve impedance-based forward modelling. This section presents a data-driven statistical tuning analysis based on wavelets extracted from the fully processed seismic data. Its purpose is to estimate dominant frequency and vertical resolution directly from observations, without assuming any specific impedance model.

In Section 4.1, the apparent peak–trough separation is interpreted using tuning curves derived from the extracted effective wavelets. These curves describe the expected interference behaviour of reflections from thin layers as a function of thickness and wavelength, and provide an empirical link between observed waveform characteristics and vertical resolution.

The physical relationship between waveform interference, impedance contrasts, and true layer thickness is subsequently addressed through wedge modelling in Section 4.3 (Fig. 13), where explicit impedance-based forward modelling is performed. This modelling provides the theoretical framework that links the tuning behaviour observed in Section 4.1 to layer thickness.

We have revised the manuscript to more clearly distinguish between these analytical stages and to explain the role of tuning curves and wedge modelling in relating peak–trough separation to intrusion thickness.

Comment 4: *Only once the wedge modeling is subsequently described is this relation depicted graphically, though still not explicitly explained.*
Response: We thank the reviewer for this helpful comment. We have expanded the description of the wedge modelling and clarified its role in explaining the physical basis of the tuning behaviour. In particular, we added explicit cross-references and text explaining how the wedge model demonstrates the relationship between peak-trough separation, amplitude variations, and layer thickness. This improves the linkage between the statistical tuning analysis and the physical forward modelling results.

Comment 5: *Moreover, is not clear but it seems that the authors are trying to use an average waveform for the intrusion response in a large window without considering possible later various in intrusion thickness (although lateral variations are address later in the analysis. The authors should be clear on this point.*
Response: We agree that this aspect required clearer explanation. Wavelets were extracted independently for the three interpreted polygons located in different parts of the intrusion in order to evaluate potential lateral variations in frequency content and tuning behaviour. The extracted wavelets showed very similar dominant frequencies (approximately 29-30 Hz), indicating limited lateral variability in effective bandwidth.
This justified the use of an average dominant frequency in the subsequent wedge modelling. In our view, this approach ensures internal consistency of the modelling results while remaining representative of the observed seismic response. The manuscript has been revised to clarify this procedure.

Comment 6: *In fact the modeling effort would be much clearer if they started with the wedge modeling discussion, which shows how distinct reflections from the top and bottom of a thin layer "merge" as the layer thins. This graphic could be used to clearly indicate what the various parameters are in the graphics in the previous modeling section (e.g. actual time travel time difference, observed peak to trough time, maximum amplitude and resonance frequency etc.).*
Response: We thank the reviewer for this constructive suggestion. We would like to clarify that tuning analysis and wedge modelling represent two complementary stages of the workflow. The tuning analysis is data-driven and provides empirical estimates of dominant frequency and vertical resolution, whereas wedge modelling illustrates the physical basis of tuning effects and reflection interference.
In our study, the tuning analysis is intentionally presented first, as it is derived directly from the observed seismic data and forms the basis for subsequent forward modelling. The dominant frequencies obtained from the tuning analysis were then used to constrain the wedge modelling parameters.
We agree that this relationship was not sufficiently clear in the original manuscript. We have therefore revised the text to explicitly describe the workflow and to strengthen the link between Sections 4.1 and 4.3, emphasizing how the wedge model provides a physical framework for interpreting the statistical tuning results.
The manuscript has been revised accordingly.
Comment 7: *In none of the discussions do the authors show their modeled waveforms next to actual recorded data at the same scale and in the same display format. The "zoomed" versions of the color section in Figures 6-8 simply to not show the observed waves in sufficient detail*

nor in a comparably format (eg. Variable area wiggle) to make in useful judgment of how well the modeling matches the observations.
Response: Thank you for this helpful suggestion. Following this comment, we have added a new figure that directly compares observed and synthetic data displayed at identical time windows, consistent amplitude scaling, and in the same variable-area wiggle format. The new figure presents (i) the location of the selected trace on the PL-5400 seismic profile (polygon 2), (ii) a zoomed view of the real seismic trace extracted from the intrusion interval (100 ms TWT window), (iii) the corresponding synthetic trace extracted from the wedge model using the same time window and a thickness of ~58 m (i.e., close to the estimated tuning thickness), and (iv) their overlay. The selected trace was taken from the same polygon used for the tuning analysis (Fig. 7), from a location where the intrusion is well imaged and its thickness is close to the tuning thickness, making it representative for this comparison.

This figure allows a direct visual assessment of how well the modeled response matches the observed data and addresses the reviewer's concern regarding scale, display format, and waveform comparability. The manuscript and figure captions have been updated accordingly.

Comment 8: *Also the color scale wavelets appear to exhibit way more wiggles (side lobes?) than explained by the modeling. Is this an artifact of displaying the modeling and data with different display modes or gain functions (eg. AGC)? Does such "additional" wavelet complexity indicate finer layering with the intrusion, or do they represent of multiples generated in the sedimentary column above. Were such multiples removed by processing (deconvolution? wavelet extraction?)?*
Response: We would like to clarify that in the original manuscript we used schematic, wavelet-like color-scale symbols solely as explanatory elements. These symbols were included to illustrate the seismic amplitude color scale in several figures (Figs. 6, 7, 8 and 14, 15, 16 in the previous manuscript version). They were not intended to represent actual seismic traces or data, but rather to provide a visual guide to the color–amplitude relationship used in the corresponding figures. These symbols appeared in the explanations of the colour amplitude scales and were intended solely as illustrative amplitude indicators. They did not represent extracted or modelled seismic wavelets. These elements were added for graphical clarity and aesthetic purposes only and were not related to the actual seismic signal.

Consequently, the apparent complexity and additional side lobes visible in these schematic symbols do not reflect the character of the effective statistical wavelets extracted from the fully processed seismic data, as they are not related to each other. As mentioned above, the sole purpose of these color-scale wavelet-like symbols was to illustrate the amplitude scale, not the real wavelet shape. We acknowledge that their presence may have led to misinterpretation and that this was potentially confusing. To avoid any ambiguity, these schematic elements have been removed and replaced by simplified amplitude scales in the revised figures (Figs. 6–8 and 15–17).

As discussed earlier, all analysed seismic data were obtained after complete regional processing conducted by the data provider (it included multiple attenuation and deconvolution procedures, done by the company, but detailed workflow remains classified). The present study is based on fully processed and migrated seismic data, and the authors did not apply any additional processing to the field records. No dominant multiple energy was observed within the time windows used for wavelet extraction, and we did not apply any additional gain functions, deconvolution, or AGC during wavelet estimation.

The extracted wavelets represent effective statistical wavelets that combine source signature, propagation effects, scattering, and residual processing imprint. Consequently, they are inherently more complex than an idealized Ricker wavelet used in forward modelling, which

was deliberately adopted as a simplified approximation for tuning analysis and thickness estimation using the wedge model.
To further minimize any display-related effects and to allow direct waveform comparison, we have added a new figure (Fig. 14) in which observed and synthetic traces are shown using identical time windows and consistent amplitude scaling.
We note that some additional waveform complexity may also reflect small-scale heterogeneities or fine internal layering within the intrusion and surrounding rocks. However, resolving such features is beyond the scope of the present study and does not affect the first-order tuning and thickness estimates presented here.

Comment 9: *In any case, at least a sample of the data needs to be shown in the same wiggle format and at the same display scale as the modeling. And these more zoomed versions of data should show your picks for peaks and troughs used to deduced thickness.*
The second part of their analysis they use full 2D modern of the complete geological column including the sedimentary section for several different proposed lithologies for the intrusion in an attempt to see which seems to match the relative amplitudes of the intrusions compared with the shallower reflections. The result is rather predictable: the model with the largest impedance contrast the between intrusion and host rock gives the strongest reflection.
Response: Regarding the identification of peaks and troughs, this issue has now been addressed by the newly added Fig. 14. The figure presents observed and synthetic traces displayed in identical wiggle format and at consistent scales, with the main peak and trough corresponding to reflections from the intrusion top and base, respectively, as indicated in the figure caption. This demonstrates that thickness estimates were derived consistently from peak–trough separation, following the same criteria used in the tuning analysis, rather than from subjective visual picking.
We agree that reflection amplitude is primarily controlled by impedance contrast and that models with larger contrasts produce stronger reflections. However, the forward modelling performed in this study quantifies this relationship and provides constraints on the range of plausible lithologies consistent with the observed amplitudes. To clarify this point, an explanatory sentence has been added to the manuscript.

Comment 10: *Using the overall appearance of the seismic section to demonstrate this is rather qualitative. A more quantitative approach would be to compare amplitude vs travel time curves for traces that sample the intrusive. Also any amplitude analysis begs the question of what attenuation values were used for the geologies traversed by the seismic waves.*
Response: We agree that a fully quantitative amplitude–versus–travel-time analysis could, in principle, provide additional constraints on the seismic response of the intrusion. However, the PolandSPAN® dataset used in this study consists of fully processed and migrated regional seismic data, and data version we used not processed with strict amplitude preservation as the primary objective. As a result, absolute amplitude values are influenced by processing, scaling, and migration effects, which limit the reliability of purely quantitative analysis.
Instead, our approach combines statistical tuning analysis, wedge-based forward modelling, and direct waveform comparison (Fig. 14) using identical display parameters. This provides a semi-quantitative validation of the relationship between layer thickness, impedance contrast, and observed reflection amplitudes under consistent conditions.
Regarding attenuation, reliable Q estimates are not available for the deep crustal levels investigated in this study. Introducing assumed attenuation values would therefore be highly uncertain and potentially misleading. For this reason, attenuation was kept constant in the modelling, and the analysis focused on relative amplitude behavior rather than absolute

amplitude calibration. We have clarified these limitations and methodological choices in the revised manuscript.

Comment 11: *As mention earlier the wedge modeling in the next section of the paper clarifies some of the ambiguities in the discussion of the earlier modeling and should precede the other waveform analyses. The quantification of lateral variations in thickness is one of the most outstanding contributions of this study. However here too the presentation would have been more effective by zooming into the wavelets to make their details clearer.*
Response: We thank the reviewer for highlighting the importance of the wedge modelling results in clarifying tuning effects and thickness variations. We note that this comment is closely related to an earlier suggestion regarding the presentation order and the role of wedge modelling in the interpretation workflow. As explained in our previous response, the manuscript is intentionally structured to reflect a data-driven workflow. The tuning analysis (Section 4.1) is derived directly from observed seismic data and provides empirical estimates of dominant frequency and vertical resolution, which are subsequently used to constrain the wedge modelling (Section 4.3). The wedge model then provides a physical framework for interpreting the statistical tuning results and for validating thickness estimates.
We have significantly revised the manuscript to explicitly describe this relationship and to strengthen the link between Sections 4.1 and 4.3.
Regarding the request to zoom into the wavelets, this has now been addressed by the newly added Fig. 14, which presents observed and synthetic traces in identical wiggle format and at consistent display scales. This figure enables detailed inspection of waveform characteristics, including peak-trough separation, tuning effects, and reflection interference, and we hope it now better clarifies how thickness estimates were derived.
We believe that these revisions improve the clarity of the modelling workflow and its connection to the wavelet analysis. The manuscript and figures have been revised accordingly.

Comment 12: *Also, could amplitude processing (eg lateral variations an spherical divergence corrections) have distorted the intrusion election amplitudes?*
Response: All analyses were performed on consistently processed regional seismic data provided by the PolandSPAN project. The processing workflow implemented by the data provider included true-amplitude gain recovery (TAR), aimed at compensating for propagation-related amplitude decay and preserving relative amplitude variations. We would like to emphasize, however, that the present study is based on fully processed and migrated seismic data, and the authors did not apply any additional amplitude scaling, balancing, or gain functions. Therefore, any potential effects of amplitude-related processing are uniform across the dataset and do not preferentially affect the intrusion reflections. Moreover, our interpretation relies primarily on relative amplitude behavior and internal consistency between observed and synthetic data, rather than on absolute amplitude values. This approach minimizes the influence of possible residual processing effects. The close correspondence between observed and modelled waveforms shown in Fig. 14 further supports that the main amplitude characteristics of the intrusion reflections are not dominated by processing artifacts.
We therefore conclude that amplitude-related processing is unlikely to have significantly distorted the relative amplitude response of the analyzed intrusions. While minor residual processing effects cannot be entirely excluded, they do not affect the main conclusions of this study.

Comment 13: *Lastly the authors attribute all of the lateral waveform variations to changes in intrusion thickness, but give no consideration to the a possible alternatives: lateral variations*

in composition of either intrusions or country rock, or the possibility of finer layering with the intrusion.
Response: We agree that lateral variations in seismic waveforms may, in principle, also reflect changes in intrusion composition, internal layering, or variations in the surrounding host rock. These factors cannot be entirely excluded, particularly given the lack of other precise geological or geophysical data at the analyzed depths.

In this study, we interpret thickness variations as the primary control on lateral waveform variability because (i) the observed changes are systematic and consistent with tuning and interference effects predicted by wedge modelling, (ii) they are generally reproduced by forward modelling under constant lithological parameters, and (iii) independent geological and geophysical constraints from other studies (also discussed in Section 5) suggest a relatively homogeneous doleritic composition at the regional scale.

Nevertheless, we acknowledge that compositional heterogeneity, fine-scale internal layering, or lateral variations in basement properties may locally influence the seismic response. This limitation has now been explicitly stated in the revised manuscript.

Comment 14: *However, in summary the authors present some beautiful new seismic data depicting likely intrusions in the sub-sedimentary crust. However the discussion of the waveform analyses could be improved for clarity of technique and results and completeness of interpretational possibilities.*
Response: We thank the reviewer for this constructive summary and positive assessment of the presented seismic dataset. We agree that improving the clarity of our waveform analysis and the completeness of interpretation was essential.

In response to reviewer's comments and suggestions, we have substantially revised the manuscript to improve the transparency of the applied methodology and the presentation of results. The workflow has been clarified, particularly the relationship between tuning analysis, wedge modelling, and forward modelling (Sections 4.1–4.3). Additional explanations have been introduced in Sections 3 and 4 to better describe the modelling approach and amplitude interpretation.

Furthermore, we have added a new Figure 14, which presents observed and synthetic traces at consistent scales and in identical display format, allowing detailed inspection of waveform characteristics and improving the clarity of thickness estimation and tuning effects.

Finally, we have explicitly acknowledged alternative interpretations, including possible effects of compositional heterogeneity, internal layering, and variations in the surrounding host rock, and to emphasize the limitations of the current dataset. We believe that these revisions significantly improve the clarity and completeness of the waveform analysis, its interpretation, and the overall manuscript.