

Warsaw, April 27, 2026

## COMMENTS FROM REVIEWER (PROF. LARRY BROWN)

Reviewer comments are reproduced in italics, followed by the authors' responses.

**Comment 1:** *I appreciate the authors responses to my original critiques which address, at least in part, some of my concerns.*

*I still think there needs to be some clarity of the what the “extracted wavelets” represent. As written it implies this are the wavelets that are impinging on the intrusions and therefore to appropriate us to explore the tuning effects. How is this done is a still a mystery. On the other hand, if these extracted wavelets are presumed to include the tuning effects, it makes the modeling seem rather circular, e.g. merely showing that amplitude maxima from the modeling correspond to the peak frequencies in the spectra of the “extracted wavelet”. Something seems to missing here.*

*The same concern applies to the wedge modeling. This should be done with an estimate of the wavelet not yet involving the tuning effects.*

*The Ricker vs Klauder issue is only relevant when comparing the synthetics with the real Vibroseis data, which is done in Figures 14-17.*

**Response:** We thank the Reviewer for this important comment and for highlighting the need to clarify the role of the extracted wavelet in our workflow. We acknowledge that the wavelet was extracted from a window that includes the interpreted intrusion, and therefore it may contain some influence of interference effects associated with tuning. However, this extracted wavelet was not used directly in the modelling. Instead, it was used solely to estimate the dominant frequency content of the seismic data (~29–30 Hz). The forward and wedge modelling were performed using an idealized zero-phase Ricker wavelet with the same dominant frequency. This approach ensures that the modelling is based on a generalized representation of the seismic bandwidth rather than on a potentially tuned wavelet extracted from the data. Importantly, the tuning behavior investigated in the modelling depends primarily on the dominant frequency and layer thickness, and only secondarily on the specific shape of the input wavelet. Therefore, the use of a Ricker wavelet with a frequency equivalent to that of the extracted wavelet provides an independent and physically consistent basis for analyzing tuning effects. Consequently, we think that the modelling is not circular, as the input wavelet does not encode the specific interference patterns observed in the data, but instead allows them to be reproduced as a function of the modelled geometry and wavefield interference that were central to this modelling study. We have clarified this point in the revised manuscript by adding additional explanations in Sections 3.2 and 3.3.2, where we explicitly distinguish between the role of the extracted wavelet (used for frequency estimation) and the idealized Ricker wavelet used in forward and wedge modelling.

**Comment 2:** *Below are some pickier issues, most grammatical that I came across that would clean up the grammar a bit. A good spell checker should be use on the final version to remove these types of small errors.*

*Line 19. Better sentence “We applied a methodology that combines quantitative assessment of seismic data resolution and estimation of tuning thickness with 2D seismic forward modelling based on geological constraints that may be used to better understand, or estimate, parameters characterizing deep intrusions that are beyond reach of deep wells.”*

*Line 26. “in Devonian” should be “in the Devonian”*

*Line 40. delete “studied”*

Line 41. "result" should be "results" and "regionally extensive system" should read "a regionally extensive system"

Line 43. "Main" should be "The main" and

Line 60. Delete "an"

Figure 1:(A) caption "at the background of" should be "on the background"

Line 10. "small" should be "shallow"

Line 116: "constrain period" should be "constrain the period"

Line 127. "analyzed" should read "the analyzed"

Line 145. What do PreSTM and PreSDM mean?

Line 146. "Could" should be "Can"

Line 148. "rather unique" should be "the rather unique"

Line 155. "data has" should be "data have" data is plural

Line 222. "role of" should be "the role of"

Line 223. "is another important issue" should be "are other important issues"

Line 225: "tuning effect" should read "the tuning effect"

Line 227. "by very" should read "by a very"

Line 231. "single composite seismic horizons" should be "a single composite seismic horizon"

Also "Similar situation" should be "A similar situation"

Line 233. Delete one of the "modelling" words"

Line 234. "could be indirectly estimated" should be "must be indirectly estimated"

Line 236. "High degree" should read "A high degree". Also "that final" should be "that the"

Line 238. "guess" should be "guesses". The term "educated guess" overused in this paper. A better term is simply "estimate"

Line 251. "used zero-offset" should be "used a zero-offset"

Line 261. "intrusion" should be "intrusions". "interpreted" should be "the interpreted"

Line 263. "layer" should be "layers"

Line 264. "vertical" should be "a vertical" "

At this point I will refrain from pointing out further examples of missing articles "the" "a" etc. as they should be caught by any decent English spell checker, as should error is using singular vs plural nouns.

**Response:** All suggested corrections have been implemented in the revised manuscript. The sentence in the Abstract (Line 19) has been revised accordingly. The abbreviations PreSTM and PreSDM have now been defined at their first occurrence. The usage of "educated guess" term has been reduced, as suggested. In addition, the entire text has been thoroughly proofread and further improved to ensure consistency in grammar, use of articles, and singular/plural forms throughout the manuscript.

**Comment 3:** Line 274. Why is basalt being considered since it is a volcanic not a plutonic rock. Its plutonic equivalent is dolerite. If the reflectors are indeed intrusions, basalt would by definition be excluded. Its use in this study is basically redundant with dolerite.

**Response:** The purpose of including both basalt and dolerite scenarios in the modelling was not to distinguish between extrusive and intrusive rocks in a strict petrological sense, but rather to represent a plausible range of petrophysical parameters for mafic compositions. In this context, basalt and dolerite were used as end-member models to test the sensitivity of the seismic response to variations in acoustic impedance. We agree that the term "basalt" may be misleading in the context of deep intrusions, and we have clarified in the revised manuscript that this scenario is intended to represent lower-bound petrophysical properties of mafic rocks rather than to imply the presence of volcanic lithologies.

**Comment 4:** Line 279. *“volcanic lithologies” should be “plutonic lithologies” If you are considering intrusions, volcanic materials are irrelevant.*

**Response:** We agree and have revised the wording in the manuscript to avoid the use of the term “volcanic lithologies” in this context.

**Comment 5:** Line 317. *“could” should be “may”*

**Response:** Corrected.

**Comment 6:** Line 429. *“Because the dataset does not represent strictly amplitude-preserved data and reliable attenuation (Q) estimates were unavailable at these depths, the analysis focuses on relative amplitude behavior rather than absolute amplitude calibration.” Too true. The result simply confirms what is obvious from the model parameters, e.g. that strongest reflections are associated with the high impedance contrasts. The only value to such modeling would be to compare the relative amplitudes calibrated against the known impedance contrasts determined for shallow reflections whose impedance contrasts can be at least estimated from the drilling data*

**Response:** We agree that, in general, reflection amplitudes are primarily controlled by acoustic impedance contrasts. As noted above, our modelling was not intended to perform absolute amplitude calibration, but rather to evaluate whether the observed amplitude characteristics of deep reflections are consistent with plausible petrophysical scenarios. Given that the dataset is not strictly amplitude-preserved and that reliable attenuation (Q) estimates are unavailable at these depths, the analysis was intentionally limited to relative amplitude behaviour. We acknowledge that calibration against shallow reflections with better-constrained impedance contrasts could, in principle, provide additional constraints. However, such an approach would require amplitude-preserved data and a consistent processing workflow across the entire section, which are not available for the present dataset. Therefore, the aim of the modelling is not to reproduce absolute amplitudes, but to test whether the observed seismic response of deep reflections is compatible with realistic ranges of acoustic impedance contrast. In this context, forward modelling provides a useful framework to assess whether the observed seismic response can be reasonably reproduced by different petrophysical models. This point has been clarified in the revised manuscript.

**Comment 7:** *Assuming the synthetic and real data in figures 10-12 were processed exactly identically as far as amplitude treatment, then the relative amplitudes of shallow vs deep reflections is inherent in the sections, but too obscure to judge in the full section displays. It would be more useful to show a single representative trace at shallow and at intrusion levels with zoomed waveforms for the 3 cases, with each trace normalized to the amplitude of a “known” shallow event.*

**Response:** This is a valuable suggestion and we agree that comparing representative traces from shallow and deep levels could provide additional insight into amplitude relationships. In the present study, the comparison between synthetic and real seismic data was carried out at the section scale (Section 4.2), with particular focus on the relative amplitude characteristics of reflections associated with the interpreted intrusions. We acknowledge that a trace-based, amplitude-normalized comparison could further refine this analysis. However, implementing such an approach would require additional processing steps and consistent amplitude calibration, which are beyond the scope of the present study. We therefore consider this useful suggestion as a potential direction for future work.

**Comment 8:** *Figure 16. The mapping of lateral variations in intrusion thickness (and layering?) is one of the interesting parts of the analysis. However, the scale at which the seismic*

*section is shown makes it difficult to judge the alleged matches to the corresponding wedge model.*

**Response:** The scale of the seismic sections may to some extent limit detailed, trace-by-trace comparison with the corresponding wedge models. We agree that more detailed, zoomed comparisons (e.g., representative traces) could provide additional insight into amplitude relationships. However, the figures presented here (Fig. 16 and Figs. 15, 17) are intended to illustrate relationships between the observed seismic response and the modelled tuning behaviour, and to provide a basis for estimating the range of possible intrusion thicknesses and their geometry, rather than to provide an exact one-to-one match. This is further supported by the semi-transparent overlays shown on the wedge model panels, which indicate the range of thicknesses consistent with the observed seismic amplitudes. To clarify this, we have added an additional explanation to the figure captions (Figs. 15–17) emphasizing that the comparison is qualitative and intended to illustrate general trends, i.e. the range of thickness variations rather than exact values. We think that even such quantitative analysis aimed at deep seismic events that were imaged by seismic data within the basement of the Baltic Basin represent significant step – of course, not the final one – towards better understanding of the deep subsurface of this part of Europe, with important implications for regional geology, existence of so-far unknown LIP etc. Future work that might include also reprocessing of the PolandSPAN<sup>®</sup> data with amplitude preservation etc. might help to tackle at least some of the issues mentioned above.

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On behalf of all the authors