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Reply on RC4

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Author comment on "Late Cretaceous – early Palaeogene inversion-related tectonic structures at the NE margin of the Bohemian Massif (SW Poland and northern Czechia)" by Andrzej Głuszyński and Pawel Aleksandrowski, Solid Earth Discuss., https://doi.org/10.5194/se-2021-99-AC2, 2021

Response to Anonymous Referee #3

We appreciate the careful reading of our manuscript by Anonymous Referee and picking up several issues that, in their opinion, are controversial or deserve criticism. However, we approach with caution the Referee's general idea of how our article should look like, as it is fundamentally different from our own concept.

At the beginning , we are criticized by Anonymous Referee for concentrating on the "late Cretaceous–early Palaeogene, several macro-scale tectonic structures [...] formed in Permo-Mesozoic cover, mainly due to shortening in NE-SW/NNE-SSW direction", while not on "an earlier extensional stage which preceded inversion". However, we intentionally, limited the scope of our paper to the "Late Cretaceous – Early Palaeogene inversion-related tectonic structures", i.e. those formed due to contractional tectonics. This intention is expressed in (1) the paper's title, (2) the very first lines of the abstract, as well as (3) in the Introduction, as the paper's goal "to briefly overview the wide spectrum of structural effects produced by the Late Cretaceous to Early Cenozoic trans-European compressional event at the NE margin of the Bohemian Massif". Therefore, in our opinion, the question of "an earlier extensional stage which preceded inversion in a subsequent compressional stage" lies beyond the scope of our paper. Possible extending of the paper's scope as suggested by the reviewer, would necessitate much more space in our short paper and, in particular, undertaking a new study on the Permo-Mesozoic extensional tectonics of the region requiring years of work. Actually, so far – to our knowledge - there is no holistic interpretations of the Sudetic extensional tectonics that might be at least roughly in agreement with the up-to-date results of low temperature geochronology and based on reliable sedimentological or structural data. Last but not least, on the newly reprocessed seismics, whose interpretation we present in the manuscript, unequivocal cases of faults ,that may have been once normal and later became inverted turned out to be uncommon and difficult to detect in spite of our sincere efforts.

Of course, we are fully aware of the importance of the dominantly extensional tectonics during the post-Variscan evolution of SW Poland and northern Czechia, together with

much wider areas around, which created space for deposition of the Permo-Mesozoic succession over those territories. However, as said before, the subject of our paper was never intended to include the complex issue of the pre-inversion evolution of NE Bohemian Massif, the more so it is still far from being well understood. Nevertheless, when necessary, our manuscript refers each time to the ideas concerning the extensional or rifting evolutionary stages expressed by earlier authors (as in case of the Wleń Graben and the Upper Nysa-Kraliky Graben; in the latter case we state openly that we do not agree with the ideas we refer to as based on doubtful premises) and – so far – we considered these references as sufficient, taking into account the size and main subject of our paper. However, in order to (at least partly) comply with the Anonymous Referee's remark on our insufficiency in dealing with the earlier authors' opinions on the extensional tectonics, we propose complementing our discussion on particular structures in our revised manuscript – wherever applicable - with additional short information on previous authors' relevant views. In this way we will also partly address another point of the Anonymous Referee's criticism concerning our "rather incomplete review of the existing literature". We, of course, agree that our literature review is "incomplete", but we ourselves stressed in the manuscript (line 79) that our review paper is "concise" and, thus "not fully systematic" and – let alone having the paper's size in mind – we found ourselves as being obliged to select the literature cited.

The further Anonymous Referee's critical remark that our "interpreted time-migrated sections are the only new element in the ms" is to some degree true (in terms of the data, but not the interpretations), but probably unnecessary, since we ourselves nowhere try to hide this fact, clearly stating that our paper is "a brief review" (which we understand by default as: "of ideas mostly by other authors") "complemented with results of new seismic studies" or even we more clearly write that our manuscript contains "review of the tectonic structures of likely Late Cretaceous – Early Palaeogene age [...] based (1) on authors' own analysis and structural interpretation of the newly reprocessed reflection seismic data [...] and (2) on structural field data of the present authors or (3) coming from critically assessed literature".

The successive complaint by Anonymous Referee concerns our interpreted seismic sections, which they consider as "poorly discussed while offering only colored arbitrary interpretations". In the review paper like ours, there is generally little space for detailed analysis and descriptions of several presented new seismic sections (this will possibly be the subject of a separate future publication) – instead, we tend to think the presented graphic material is self-explanatory, but, also, we comment in the text on the general structural features seen on the seismics and – in some cases – also explain selected details. As to the Anonymous Referee's remark about the lack of "any verification" due to the paucity of boreholes", we can assure that the calibration of the interpreted seismics with drillhole data was made wherever possible and that the density of the boreholes in the areas in question is not significantly lower than in other areas typically explored with oil industry-related seismics. We probably also should disclose that we have previous experience in structural interpretation of industrial seismics in terrains of fold-and-thrust tectonic style, in many respects similar to those illustrated in our manuscript. At the same time, we, of course, agree that structural interpretation, involving recognition, tracing, interpolation, extrapolation etc. of seismic reflectors, combining it with knowledge of tectonic styles, requires also some degree of arbitrary choice and speculation, but applying these within reasonable limits perhaps should not be the reason of serious objections.

From the Anonymous Referee's critical discussion on our interpretation of the Zieleniec Thrust and Upper Nysa - Králiky Graben, the reader might gain an impression that (1) the Zieleniec Thrust is but "one of ambiguous features in the Sudetes" over-interpreted by us in disagreement with all other authors involved in studying it and (2) that the Upper Nysa – Králiky Graben (and also the Wleń Graben), had experienced important 'fault-controlled syndepositional subsidence of the graben floor" during the Late Cretaceous extensional stage of its evolution that we disregarded "without justification", together with neglecting all references to the proponents of this idea, who were cited by Anonymous Referee.

Actually, however, we have referred in our manuscript to the papers cited in the review and interpreted the Zieleniec Thrust in agreement with Cymerman (1990), who mapped in detail the neighboring area. Meanwhile, Cymerman (1990) was cited by Anonymous Referee in his review, but in a different context: that the thrust in question "is nowhere exposed". Anonymous Referee, instead, relies in their views on the Zieleniec Thrust rather on the short paper by Kozdrój (2014), mostly concentrated on minor structures and petrographical data related to a drillcore from a borehole that penetrated the Thrust. Kozdrój (2014) maintains to have learned the orientation of the thrust from the drillcore alone and – imprudently in our opinion - extrapolates it beyond the borehole, which leads him to expressing the three mutually excluding hypotheses as to the origin and age of this map-scale structure. In our opinion, however, the Zieleniec Thrust, whose surface trace trend (Fig. 12 in our ms), which we consider in the context of the most probable regional contraction direction during the Late Cretaceous – Early Palaeogene compressional event, can be rightly, though still hypothetically, interpreted as a reverse fault of that age. At the same time, Figure 17 of our manuscript (not Fig. 12 to which Anonymous Referee's refers by mistake) should not be described as "a conceptual ideograph but not a real, geometrically rigorous cross-section" – as we find in the review. This cross section, although simplified, maintains the real angular relationships, provided by Cymerman (1990) and – partly – also by Kozdrój (2014).

As to the the Upper Nysa – Králiky Graben, we have also cited in our manuscript all the papers mentioned by Anonymous Referee (we have cited also some more papers in this context) and we clearly stated that their authors proposed pre-inversion, Late Cretaceous extension affecting the graben, but because "the early rifting was inferred to have occurred **on stratigraphic and sedimentological premises – not convincing in our opinion** – and most often explained as due to compression- or extension-driven updoming of the Orlica-Śnieżnik Massif during the Cretaceous or, still, by pull-apart graben formation due to strike-slip displacements on NW-SE trending structural discontinuities in the crystalline basement". To dispel, however, or at least weaken the doubts expressed by Anonymous Referee, in the revised manuscript, we plan to add a short additional information explaining why we consider as not convincing the stratigraphic and sedimentological premises for the conclusions of earlier authors concerning the possible importance of fault-controlled syndepositional subsidence of the Upper Nysa – Kraliky Graben in the late Cretaceous.

The consecutive criticism of Anonymous Referee goes to our interpretation of the North-Sudetic Synclinorium, following the information we extracted from the reprocessed reflection seismics, as well as to our comments concerning the interpretation of the Wleń Graben by Kowalski (2020), using Kowalski'is own serial cross-sections based on his field mapping study. The conclusion of Anonymous Referee that we "see problematic (without any argument) interpretation by Kowalski because [of] his remark that seismic survey

might help to better understand geometry of that structure" seems to be a gross misunderstanding. On the contrary to this conclusion, we welcome the initiative of Kowalski to verify the geometry of the Wleń Graben using seismic techniques. We see the interpretation of the Wleń Graben by Kowalski (2020) as problematic **not at all because of his remark about the usefulness of the planned seismic survey, but because his interesting and compelling cross sections (our Fig. 11) show a regular syncline structure instead of that of a tectonic graben. Kowalski's (2020) cross sections, actually, contain boundary faults of the graben, either (sub)vertical or normal ones. However, from the point of view of the structural geometry, they seem to have contributed very little – if at all – to the "graben's" formation. They are practically "useless" in this respect and, hence, our doubt where the Wleń Graben is actually a graben or a syncline.**

Independently, it is interesting, that our analysis of the reprocessed seismic sections from the North Sudetic Synclinorium led us to very similar results as those stemming from the Kowalski's (2020) cross sections for the Wleń Graben, but on a somewhat bigger scale. Our analysis points to a roughly synclinal geometry (though somewhat complicated with local internal thrusting) of the North-Sudetic Synclinorium.

The seismic sections we analyzed seem to show the geometry of a downwarped (downfolded) top surface of the low-grade basement metamorphics in the Synclinorium, as concordant with that of a large syncline (with complications due to reverse faults cutting across the basement/cover interface) in the Permo-Mesozoic cover. The complications, such as local thrusts and smaller scale folds can be partly explained by e.g. the model of flexural-slip folds, where contractional structures tend to form within folds' inner arcs. In such a scenario a detachment at the interface between the epi-metamorphic downwarped basement and layered sedimentary cover is more than certain to have developed, though it is not directly visible on the seismics. The Anonymous Referee's assertion that "no small-scale folds and reverse faults [are] reported from the field, whereas the concave base of Permo-Mesozoic strata might still be affected by early normal faulting in the basement" [of the North Sudetic Synclinorium] is at odds with the content of our manuscript, as the spectacular examples of mesoscopic folding within the Muschelkalk strata are presented in our Fig. 8, whereas minor reverse faults do occur in the same quarry (Leśniak 1979; Solecki 1986, 1994; Cymerman 1998 - all cited in our manuscript).

Further criticism is focused on us as "the authors providing no verification of their interpretation" understood by us as (no) checking our seismic interpretation with drilling results, which – of course – is often difficult in case of seismic data, due to the lack of suitably dense network of deep drillholes and – usually, accepted in the practice of seismic interpretation. We were able only to compare/calibrate the seismics with the existing boreholes (which, nevertheless, as we pointed out earlier, are not distributed much less densely than in typical areas explored with seismic method while prospecting for hydrocarbons) and rely on the surficial maps and – not of least importance – on our experience with structural geology and seismic interpretation, which all we did. Then, our paper is compared by Anonymous Referee in the context of the quality of collected documentation with that of Coubal et al. (2014) concerning the spectacularly exposed Lusatian Fault. This paper we cite in our manuscript and very briefly refer to its results, which we have redrawn in a simplified form, while allowing the reader to check the details at the original source. This comparison seem to us rather not to be quite fair, as our short

paper is intended to convey general information on the style of structures known to various degree over a large, little exposed area, whereas the excellently illustrated paper by Coubal and coauthors concentrates on an exceptionally well exposed single major structure.

Criticized has been also our short reference to the known jointing pattern in the Permo-Mesozoic sedimentary rocks of the Sudetes and some Carboniferous granites from the crystalline basement, that, in general, showed a simple orthogonal subvertical joint system with joint strikes approximating NW-SE to NNW-SSE and NE-SW to ENE-WSW. Anonymous Referee advised us to conduct "a more meticulous study" and, also, as we understand, to concentrate on "reactivation of fracture pattern in the crystalline basement during late Cretaceous-Cenozoic time and how much such process has controlled /influenced development of joint and fault systems in the Permo-Mesozoic cover". These topics, in particular the latter one, are extremely interesting, however, in spite of numerous local descriptive studies of joints made at various areas of the Sudetes since the 1950s, not such research has been done as yet. We know, nevertheless, papers reporting very complex joint networks from some crystalline Sudetic areas (such as, e.g., the Orlica or Kaczawa metamorphics, studied, respectively, by e.g. Żelaźniewicz (1977) and Teisseyre (1976) and we also know areas where the joint pattern in the crystalline basement is generally simple, such as that in parts of the Karkonosze pluton, which we use as a hypothetical example of joints initiated during the Late Cretaceous –Palageogene compression. From the recent low-temperature geochronology (Migoń and Danišík 2012, Sobczyk et al. 2016) it follows that the present-day exposure level of same parts of the Karkonosze pluton was still at a depth of a few kilometres at the end of Cretaceous, and, hence the joints we observe recently at the surface can, indeed, be Late Cretaceous-Cenozoic rather than, e.g., Carboniferous as concerns their initiation and formation.

In his conclusion, Anonymous Referee considers our manuscript as "at the moment [...] rather immature for publication" [...]and suggests limiting the scope of its future rewritten version to a thorough analysis of the seismics and "collecting small-scale field observations to support the assumed interpretation" as "no new deep drillholes are likely planned in the area in the near future".

Our possible following this suggestion would entirely change the content and purport of our paper, depriving it of the intended value of a regional, though brief and not comprehensive, review of the present day knowledge on "the Late Cretaceous – Early Palaeogene inversion-related structures" and would require at least months if not years of field work with no guarantee of a success, as there is no reason to expect that the minor structures possibly found in the most often flat-lying Mesozoic sedimentary strata in a generally poorly exposed area, would furnish us with analogues of the structures, having much different scale and interpretable from the seismics at or near to the interface of the sedimentary cover with its metamorphic basement. In our eyes, this advice of the Anonymous Reviewer shows their general distrust of the reflection seismics method as a source of reliable (though – of course – within certain limits and degree of precision) structural geological information – an opinion which we do not share.

Nevertheless, a possible trial of us to comply with nearly all the areas of criticism expressed by Anonymous Referee in his review (e.g. incorporating the question of the earlier extensional tectonics, referring to the more full spectrum of the existing literature, though perhaps not a "more meticulous study and consideration" of joints, including the problem of reactivation of joint pattern in the basement, etc.) will be undertaken, while preparing the revised version of our manuscript.

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