Dear Editors,

The following report contains our response to the general comments of the three reviewers and to one comment from the community.

In the text below, we successively cite entire reviews, using blue fonts for the texts by the referees and the community members. Our published responses to the reviews are written in black fonts, whereas our current comments on how we improved our revised manuscript following the referees' suggestions are mainly contained in the MS Word Comments at the margins and are written in brownish fonts.

Comments by *Jonas Kley (Referee) - RC1*

General comments

Dear authors,

this is a highly welcome contribution on an area for which no easily accessible information was available before. It adds an interesting facet to the panorama of tectonic inversion phenomena presented in the Special Issue. I like your approach of combining the reprocessed seismic data with field observations and glimpses at earlier interpretations, deriving a new, unified model in the end. The manuscript is in a very mature stage already, well written, easy to follow and beautifully illustrated. Most of my annotations refer to very minor issues such as typos (of which there were very few). In some instances I made suggestions for alternative phrasing which you may follow or not.

A formal thing I would like to mention upfront is the absence of a discussion section. The discussion of own and other authors´ interpretations is instead distributed in bits and pieces throughout the paper. Counter to first intuition, this makes a lot of sense. As the paper deals with different areas and geologic situations one by one, a bulk separation of description and interpretation would require dealing with each structure twice: First, going through all structures for description, and then again for discussion. This would make for very awkward reading. Within each paragraph, there is a clear separation of descriptive and interpretative elements.

Specific comments

As regards contents, I only have two remarks, a minor and a more substantial one:

(1) In l. 33 ff. you describe folds 1000 km long, 150-250 km wide and up to 3 km high (equal to 1.5 km amplitude). This would put them in the realm of mantle (not crustal) folding as modelled by, e.g., Cloetingh et al. 2005, implying that they should be seen to affect the Moho. I don´t think that is the case. (Age in our Central European case is considered to be around 300 Ma).

I think that all of these structures are single folds only to a very first approximation; in fact, they are overgrown by many smaller-scale structures. The interesting question then becomes how much different mechanisms contribute to uplift. I have discussed this a little bit in the 2018 Geol.Soc. paper you cite and in more detail we have done so in the von Eynatten et al. and Hindle and Kley contributions to this Special Issue. (I don´t mean this as a nudge to cite those). We find ourselves compelled to invoke a role for mantle processes and brealking/flexing the plate, whereas other authors (e.g., Jef Deckers in the Special Issue and earlier publications) are inclined towards lithospheric or crustal folding. Maybe you can elaborate in a few sentences on this unsettled debate. Whether you want to take sides or just state there is no consensus is of course your choice.

(2) The paragraph on joints relies entirely on matching orientations but is mechanically problematic. Joints are opening-mode fractures that form with the smallest principal stress being tensile (Mohr circle touches the Griffith fracture criterion to the left of zero normal stress). This can occur either at shallow levels in the crust or with elevated fluid pressures, but always at low differential stress (s_1-s_3). The inversion structures, however, certainly formed at a peak of differential stress. In addition, joints or veins open in the s_1/s_2 Multiple (e.g., orthogonal) sets of vertical joints are therefore indicative of a (mildly) extensional regime with s_1 vertical. Joints forming in a thrust regime should be horizontal. My advice would be to drop this entire short section with the accompanying figure. This would not weaken the paper in the slightest.

Technical corrections:

See annotated pdf

I am looking forward to seeing this contribution published!

Jonas Kley

Our response to Jonas Kley (Referee) - RC1

Dear Professor Kley,

Thank you very much for the review, in which you have found quite a number of positive sides of our manuscript. Thank you, as well, for the annotations you made on our text, which will help us greatly in preparing the revised version of our paper.

We find very interesting both your critical remarks: the one on the crustal-scale folding and that on the possibility of producing the pattern of joints observed in the Cretaceous and older rock of the region by the Late Cretaceous – Early Palaeogene compressional episode. Both are, unfortunately, not easy to address, but are, definitely, worth of discussion.

(1) As concerns the "gentle folds up to 1000 km long, 150-250 km wide and up to at least 3 km high" we mention in the introduction to our manuscript, they diagonally cross-cut entire Poland in the NW-SE direction, subcrop at the base Cenozoic level and, indeed, show an extremely long wavelength. As you rightly noticed, this may imply lithospheric-scale folding, involving the Moho and uppermost mantle, if we rely on the modelling results by Burov and Cloetingh (e.g. Burov et al. 1993, Cloetingh et al. 1999, 2005, Cloetingh and Burov 2011). It happened that one of us (P.A.) was once the person

responsible for the tectonic interpretation of the international deep seismic wide-angle reflection/refraction (WARR) profile DOBRE-4 (Starostenko et al., 2013), extending SW-NE along the NW coast of Black Sea, from Dobrudja in Romania to the interior of the Ukrainian Shield, across the Teisseyre-Tornquist Zone. A specific feature of the velocity model resulting from that project were considerable waveform undulations of the Moho, with a wavelength of the order of 125-150 km and the amplitude attaining 4–8.5 km. Similar wavy aspect were shown by the upper mantle seismic velocity contours and those in the upper crystalline crust, with somewhat shorter (but still much too long in terms of the "wavelength vs thermotectonic age" graph of Burov-Cloetingh) wavelength of ~75-80 km in the latter (except for the uppermost, 2-4 km thick sedimentary layer of the crust with much shorter wavelength, 8-30 km, folds within the foredeep to the Cimmerian Dobrudja fold belt, the Dobrudja Trough, being "itself geometrically a large-scale syncline, ~120 km wide and more than 3.5 km deep". The origin of all these undulations was then explained by us (Starostenko et al. 2013) as the result of compressional lithospheric-scale buckling, and tentatively ascribed to Late Jurassic–Early Cretaceous and/or end Cretaceous collision-related tectonic events associated with closing of the Palaeotethys and Neotethys oceans in this part of Europe. This age assignment with respect to the folds in the lower crust, Moho and uppermost mantle was roughly in agreement with the Burov-Cloetingh graph.

The coexistence of various wavelength folds in particular crustal layers and a (partial) lack of correlation between the wavelength values and the position in the middle and upper crust was ascribed by Starostenko et al. (2013) to the likely "presence of several detachment horizons in the folded crust, [...] compatible with the existence of fold systems with various dominant wavelengths at successive crustal levels, [...] considered as typical of lithospheric-scale folding (*cf.* fig. 1 in Cloetingh & Burov 2011)". Further on, we pointed out to "deformation partitioning between particular structural levels, in which competent layers separated by ductile ones are buckled independently, showing wavelengths controlled mostly by their thicknesses (Cloetingh *et al.* 2002; Jarosiński & Dąbrowski 2011; Jarosiński 2012)." This model and its tentative dating and "regionally nested" explanation were very well received and accepted by Sierd Cloetingh, the referee of the paper by Starostenko et al. (2013), in spite of the fact, that – due to some thermomechanical difficulties we encountered, which, we are not considering here - we had to concede that, finally, "a possibility should [also] be considered that the Moho folds recorded by the DOBRE-4 profile did not behave during deformation according to the regular, linear (Biot's 1961) model, with the implication that the resultant fold wavelength may not be directly dependent on the thermomechanical lithospheric age/competent layer thickness" (Starostenko et al. 2013). Needless to say, the Biot's (1961) model is one of the foundations of the "wavelength vs thermotectonic age" diagram of Burov and Cloetingh and coworkers, central to our discussion.

All this we write here, in order to show that "in practice", the otherwise correct theoretical solutions not always work, or that our knowledge of all the aspects of the 'real' examples is seldom as complete as to effectively apply the purely theoretical models.

Coming back to the Mid-Polish Swell (Anticlinorium) and its neighbouring synclinoria, we are convinced that the reason for its oversize wavelength was the fact that it formed out of the Mid-Polish Trough of roughly the same location and size. The Mid-Polish Trough (e.g. Dadlez et al., 1995) was a linear, highly elongated depocentre of the Polish-German Basin, in which the total thickness of the accumulated Permo-Mesozoic sedimentary strata locally exceeded 12 km and was much greater than in the flanking areas (e.g. Wagner et al. 2002, Dadlez 2003). A sedimentary prism of that size and thickness could not have avoided exerting an overwhelming influence on the (local) value of the dominant wavelength (cf. Biot 1961) of the growing buckle(?) folds, and during this process also numerous local inhmogeneities of the sedimentary succession were amplified to form lower order folds superimposed on the growing anticlinorium. At the same time, during the Late Cretaceous, the slowly evolving anticlinorium underwent synkinematic erosion, whose products were dispersed extensively sideways (Krzywiec and Stachowska 2016). This, most probably, also infuenced the width of the synclinoria that were being formed on both sides of the Mid-Polish Swell.

Recently, more and more geophysical evidence points to the continuation of the attenuated crystalline basement of the East European Craton to the SW below the Trans-European Suture Zone and the Palaeozoic Platform (e.g. Mazur et al., 2015, 2018, 2020), possibly as far as up to the Middle Odra Fault Zone (Zhu et al., 2015) and this phenomenon can probably limit the extent, or at least severely modify at depth the geometry of the large-scale folds under discussion. Also, the present-day knowledge on the actual detailed configuration of the Moho below central Poland, based in much part on the refraction seismics is insufficient (in terms of the resolution of the relevant maps; Grad et al. 2009) to responsibly judge, whether it is involved or not in gentle NW-SE folds on its SW-ward raising slope due to the thinning crust at the transition from the East-European to the Palaeozoic platform. In these circumstances, we tend to comply to your criticism regarding the possibility of the crustal-scale extent of the Mid-Polish Swell and the associated structure and we are ready to get rid of our bracketed comment "(crustal-scale?)" in our revised manuscript.

Komentarz [Authors1]: This has been done in line 36 of our manuscript (in the „track changes" mode file)

(2) In regard of our conviction about the genetic relationship between the joint pattern existing in the Upper Cretaceous strata of the Sudetes and the Late Cretaceous to Early Palaeogene compressional event, we think that such a relationship can be intuitively felt as rather obvious. This is in view of the fact that the mentioned event was, most probably, the last known regional-extent significant compressional episode in SW Poland and beyond (outside the Alpine belt), and of another fact that the mutually perpendicular subvertical joint sets in the Cretaceous rocks show strikes parallel and perpendicular to the other structures described in our manuscript and, as we expressed in our manuscript, "to the inferred Late Cretaceous-Early Palaeogene shortening direction (cf. e.g. Solecki, 2011, Nováková, 2014)". Our information in the manuscript on the genetic relationship in question is worded in a cautious, hypothetical way: "regional jointing pattern: a **likely** product of Late Cretaceous – Early Palaeogene deformation" or: "**a conclusion** that they [=steep joints] are genetically related to that shortening event **seems reasonable**", or still: "actually, **we believe** that the formation of the dominant jointing pattern...". We think that the way we refer to our idea of the jointing formation as to an (otherwise probable) hypothesis is fair and makes the idea acceptable in the paper. As far as we know, this pattern of jointing prevails in the Mesozoic formations all over SW Poland.

On the other hand, it is true that mechanical aspects of the **regional** joint network origin are not clear and simple in our study area. In fact, these are perhaps everywhere in the world not sufficiently understood, particularly the orthogonal systems of ~vertical joints, which are often observed over significant areas in apparently little deformed subhorizontal sedimentary formations, like e.g. in platform covers. Still more enigmatic, in their mechanical aspect, are systems of diagonal steep joints, defined by sets of joints intersecting at an acute angle, which are preferably developed over significant areas in fold belts, as in the Polish external ("flysch") Carpathians. In this region, the bisector of the obtuse angle is systematically parallel to the axes of folds developed along-strike of the orogenic belt. Thus, the geometry of the joints is analogous to that formed in conjugate sets of shear fractures with horizontal σ1 at right angle to the fold axes, however, the joints show the morphology typical of extension fractures (e.g. Aleksandrowski 1985, 1989; Zuchiewicz 1998. Mastella et al. 2002).

Generally, it seems that the great progress made during the last 50 or 60 years in the understanding of the physics of rock fracturing processes (e.g. Atkinson 1987, Bahat et al. 2005, Gudmundsson 2011) was, so far, not particularly successful, when applying its achievements on a regional scale to the field. Though not clearly stated, this is rather obvious from many recent structural geology manuals.

A way out of similar self-inconsistencies as mentioned before with respect to the Flysch Carpathians or the Sudetes, seems to be an assumption, following Price (1959, 1966), that in the process of formation of regional joint systems one should distinguish between the initiation of joints and their opening, which may happen much later, under different stress conditions and in a different tectonic scenario (Jaroszewski 1972, 1994). The initiation can be related to tectonic episode of compressional (or perhaps even extensional) nature, applied to a rock massif resting at certain depth and may produce a hidden mechanical feature of rock, the fracture anisotropy (e.g. Suppe 1985, mentioning also traditional mining terms, such as "rift" and 'grain"). The fracture anisotropy can be formed through preferentially oriented subcritical crack growth (e.g. Atkinson and Meredith 1987) and stress corrosion (Heinisch 1992), which may be assisted by tectonically induced residual stresses left in the rock mass (e.g. Price 1966, Engelder and Geiser 1984). The fracture anisotropy, once acquired, may subsequently be the reason for opening of the joint network in later extensional/uplift episodes, consisting of joints orientated in a regular way with respect to the much earlier formed tectonic structures (folds, faults, boundaries of rigid massifs etc). This is in short a hypothetical mechanism, which we tried to apply to the Late Cretaceous and later (opening) times in the Sudetes.

"In our imagination" the vertical NE-SW & NW-SE joint sets may have been initiated as "fracture anisotropy" in the Cretaceous and older rocks through subcritical crack growth under a strike-slip tectonic regime (~NE-SW-directed horizontal σ1; one set of vertical 'proto-joints' anisotropy may have formed then, in case of a small differential stress and, somewhat later, another, NW-SE set may have been initiated under localised 'radial' NE-SW extension within and above the buckled basement/cover interface, all this combined with the conditions of abundant pore fluids pressurized by the tectonic compression plus overburden weight). After the cessation of the NE-SW compression, in a (much?) later episode of extension, e.g. during a late Miocene uplift, the initiated, only "partly developed" joints (as trains of preferentially oriented microcracks) may have massively opened on a regional scale under extensional tectonic regime (σ1 vertical) and again at presence of abundant pore fluids, this time pressurized mostly by the weight of the rock overburden.

In conclusion, we would prefer retaining our short passage on the jointing, while, referring to its "possible genetic relationship with the Late Cretaceous compression" and completing it with information that we distinguish between the joint initiation and opening. Following a suggestion of another referee, we would also like to add to this passage a short info on deformation bands, which supply a more direct information on (at least one stage of) the stress regime during the Late Cretaceous to Early Palaeogene deformation. Thank you, once again for your review and the interesting issues you raised.

Komentarz [Authors2]: We have completed our revised manuscript in lines 404-414 with information and discussion concerning our opinion on the origin of (some) regional joint networks, including that in the Permo-Mesozoic of the Sudetes.

Komentarz [Authors3]: This has been done in lines 422-427 of our revised manuscript

Komentarz [Authors4]: Nearly all of the detailed lingustic and technical comments of Professor Kley included in his annotated PDF file, were incorporated by us into the revised manuscript.

References:

Aleksandrowski P. (1985) - Structure of the Mt. Babia Góra region: an interference of West and East Carpathians foId trend (in Polish with English summary). Ann. Soc. Geol. Pol., 55: 375-423.

Aleksandrowski P. (1989) - Structural geology of the Magura Nappe in thc Mt. Babia Góra region, Western Outer Carpathians (in Polish with English summary). Stud. Geol. Pol., 96.

Atkinson B.K., Meredith P.G: The theory of subcritical crack growth with applications to minerals and rocks. ln: Fracture Mechanics of Rocks (ed. B. K. Atkinson): 111-166. Academic Press. London, 1987.

Bahat D., Rabinovitch A., Frid V., Tensile fracturing in rocks. Tectonofractographic and electromagnetic radiation methods, Springer, 2005.

Biot, M. A..: Theory of folding of stratified viscoelastic media and its implications in tectonics and orogenesis, Geol. Soc. Am. Bull., 72, 1595-1620, 1961.

Burov, E.B., Nikishin, A.M., Cloetingh, S., Lobkovsky L.I.: Continental lithosphere folding in central Asia (Part II): constraints from gravity and tectonic modelling. Tectonophysics, 226, 73-87, 1993.

Cloetingh S., Burov E.: Lithospheric folding and sedimentary basin evolution: a review and analysis of formation mechanisms. BasinResearch, 23, 257–290, 2011.

Cloetingh, S., Burov, E., Poliakov, A.: Lithosphere folding: primary response to compression? (fromCentral Asia to Paris Basin).Tectonics, 18, 1064-1083, 1999.

Cloetingh S., Ziegler P.A., Beekman F., Andriessen P.A.M., Matenco L., Bada G., Garcia-Castellanos D., Hardebol N., Dèzes P., Sokoutis D.: Lithospheric memory, state of stress and rheology: neotectonic controls on Europe's intraplate continental topography. Quart. Sci. Rev., 24, 241-304, 2005.

Dadlez R.: Mesozoic thickness pattern in the Mid-Polish Trough. Geological Quarterly, 47 (3), 223–240, 2003.

Dadlez, R., Narkiewicz, M., Stephenson, R.A., Visser, M.T.M., van Wees, J-D.: Tectonic evolution of the Mid-Polish Trough: modelling implications and significance for central European geology. tectonophysics, 252 (1-4), 179-195, 1995.

Engelder T.: Loading paths to joint propagation during a tectonic cycIe: an example of the Appalachian Plateau, USA. J. Struct. Geol., 7, 459-476, 1985.

Engelder T.: Joints and shear fractures in rocks. ln: Fracture Mechanics of Rocks (ed. B. K. Atkinson), 27-65, Academic Press. London, 1987.

Engelder T., Geiser P.: Near-surface in-situ stress. 4. Residual stress in the Tully Limestone, Appalachian Plateau, New York. J. Geophys. Res., 89, 9365-9370, 1984

Grad M., Tiira T. and ESC Working Group: The Moho depth map of the European Plate. Geophysical Journal International, 176, 279–292, 2009.

Gudmundsson A.: Rock fractures in geological processes. Cambridge Univ. Press, 2011.

Jaroszewski W.: Drobnostrukturalne kryteria tektoniki obszarów nieorogenicznych na przykładzie północno-wschodniego obrzezenia mezozoicznego Gór Świętokrzyskich (Mesostructural criteria of non-orogenic areas: an example of the NE Mesozoic margin of the Holy Cross Mountains), Studia Geologica Polonica, 38 ,1972.

Jaroszewski W.: Tektonika. Wydawnictwo Naukowe PWN, Warszawa, 1994.

Krzywiec, P., Stachowska, A.: Late Cretaceous inversion of the NW segment of the Mid-Polish Trough – how marginal troughs were formed, and does it matter at all? Zeitschrift der Deutschen Gesellschaft für Geowissenschaften. (German J. Geol.), 167 (2/3), 107–119, 2016.

Mastella L., Konon A.: Jointing in the Silesian nappe (Outer Carpathians, Poland) – paleostress reconstruction. Geologica Carpathica, 53, 5, Bratislava, 315—325, 2002.

Mazur S, Mikołajczak M, Krzywiec P, Malinowski M, Buffenmyer V, Lewandowski M: Is the Teisseyre-Tornquist Zone an ancient plate boundary of Baltica? Tectonics 34(12):2465–2477, 2015.

Mazur S., Krzywiec P., Malinowski M., Lewandowski M, Aleksandrowski P., Mikołajczak M.: On the nature of the Teisseyre-Tornquist Zone. Geology, Geophysics & Environment, 44 (1), 17-30, 2018.

Mazur, S., Aleksandrowski, P., Gągała, Ł., Krzywiec P., Żaba J., Gaidzik K., Sikora R., 2020. Late Palaeozoic strike‑slip tectonics versus oroclinal bending at the SW outskirts of Baltica: case of the Variscan belt's eastern end in Poland Int J Earth Sci (Geol Rundsch) 109 (4): 1133-1160, 2020.

Price N.J.: Mechanics of jointing in rocks. Geol. Mag. 96, 149—167, 1959.

Price N.J.: Fault and joint development in brittle and semibrittle rock. Pergamon Press, 1—176, 1966.

Starostenko V., Janik T., Lysynchuk D., Środa P., Czuba W., Kolomiyets K., Aleksandrowski P., Gintov O., Omelchenko V., Komminaho K., Guterch A., Tiira T., Gryn D., Legostaeva O., Thybo H., Tolkunov A.: Mesozoic(?) lithosphere- scale buckling of the East European Craton in southern Ukraine: DOBRE-4 deep seismic profile. Geophysical Journal International, 195, 740-766, 2013.

Wagner R., Leszczynski K., Pokorski J., Gumulak K.: Palaeotectonic cross-sections through the Mid-Polish Trough. Geological Quarterly, 46 (3), 293–306, 2002.

Zhu H., Bozdağ E., Tromp J., 2015. Seismic structure of the European upper mantle based on adjoint tomography. Geophysical Journal International, 201, 1, 18–52, 2015.

Zuchiewicz W.: Cenozoic stress fieId and jointing in the Outer West Carpathians, Poland. J. Geodynamics, 26: 57-68, 1998.

Comments by *Andrzej Solecki (Referee) – RC2*

General Comments

The assessed paper preprint constitutes a concise, deliberately selective review of effects of the Late Cretaceous – Early Palaeogene deformation in the Sudetes and their foreland at the NE margin of the Bohemian Massif. The review is enriched with new data yielded by newly reprocessed archival seismics of 1970s to 1990s. Most of the described structural examples come from SW Poland, while some others - from northern Czechia.

The topic is appropriate for the special issue of the Solid Earth devoted to the Late Cretaceous -Early Palaeogene inversion in central Europe. The paper seems to partly fill the gap consisting in the lack of a holistic approach and easily accessible information on the „Laramide" or „Saxonian" tectonics affecting the Sudetic fragment of the Variscan belt. The paper is, in general, well written and nicely illustrated and presents well-suited tectonic examples, some of which come from the authors' own collection of data and the others are borrowed from the literature, though each time with critical discussion on their interpretation. The hitherto state of the knowledge is complemented with new seismic structural interpretations supplied by the authors. The presented examples are shortly explained and discussed as to their origin, mostly in a reasonable way and to the extent possible in a relative short publication.

The work of the past generations of geologists on the post-Variscan deformation structures in the Sudetes is rather decently acknowledged, in the right proportion to the modest size of the paper. Some completion in this respect seems, nevertheless, to be adviced, which has been indicated on the text below. Similarly, the paper would benefit from being completed with information on such interesting tectonic structures as the Late Cretaceous deformation bands that were described from the area of interest of the paper.

Specific Comments

Taking into account the abundance of published works, it was nearly impossible for the authors to avoid some simplifications and omissions. In this respect, I would like to point out four issues.

The first is the existence of a Triassic-Upper Cretaceous stratigraphic gap . The deposition of Late Cretaceous sandstones on substratum of various age caused long-term discussion on the Cimmerian phase of deformation. The most far-fetched interpretation was expressed by Beyer (1934), assuming the existence of the Cimmerian basemen folds transverse to the present North-Sudetic synclinorium structure.

The second issue is the possible role of salt tectonics especially in the north-eastern margin of the Sudetes in the transition zone to the Fore-Sudetic Homocline described by Markiewicz and Becker (2009). The presence of casts after halite crystals in the Zechstein deposits in the southernmost part of the North-Sudetic Synclinorium (WleÅ Graben) described by Kowalski et al. (2019) indicates that not only anhydrite (mined in the central part of the North Sudetic Synclinorium) but also rock salt may influence its deformation. One should take into account that according to Kley (2013) both extensional and contractional Saxonian" structures are often strongly modified by salt movement

The third is description of historical concepts of North Sudetic synclinorium development. In lines 193-196 authors point out "the necessity for reinterpretation of the hitherto widely held concept of the internal structure for the North-Sudetic Synclinorium, assuming the dominance of high-angle block tectonics. In a new concept, the significance of low-angle thrust faults, of compressional down-warping of the top basement surface, and of the well-developed detachment folding pattern should be taken into account."

It would be nice to mention, that the author's new concept confirms ideas of Solecki (1986, 1994) who contradicted the then dominant views and wrote in his 1994 paper at page 37:

"Deformation process of the North Sudetic Synclinorium was connected with reactivation of ancient faults which have been at least active in Permian (northern fault of Swierzawa Graben) or Triassic-Cretaceous times (Jerzmanice Fault) … In the pre-Cenomanian times the northern limb was the downthrown one, while during Laramian phase …was ovethrusted on the Cretaceous strata. These facts support J.A. Jackson (1980) model where the basin develops due to extension of the listric faults of basement and next due to basement compression the sedimentary cover is deformed in Saxonian style. As a result the North Sudetic Synclinorium may be described as an inverted basin (P.A. Ziegler 1987 and references contained therein)".

In my opinion, the discussion section suggested by Kley (https://doi.org/10.5194/se-2021-99-RC1), in his recent comments to the reviewed paper by GÅ uszyÅ ski and Aleksandrowski, would be a good solution to tackle the four issues mentioned above.

The forth problem is the paragraph on joints in the reviewed paper, which I, in contrast to Kley, appreciate. It is true as claimed byKley, (https://doi.org/10.5194/se-2021-99-RC1) that the authors rely entirely on matching orientations but in my his opinion this approach does not seem to be mechanically problematic. Although "joints are opening-mode fractures that form with the smallest principal [effective] stress being tensile", one should remember that the opening is but the last phase formation of joints, which takes place during decompression of a rock massif. The orientation of joints is determined by the evolutionary history of a given rock, that often includes accumulation of residual stresses (cf. Price 1959,1966) and/or preferentially oriented chains of microcracks developed well before the decompression, during earlier compressional phases. Therefore, the Polish traditional mining term "cios" (Eng. blow, stroke) used for the joints rightly emphasizes a tendency of apparently intact rock to break and form fractures in regular way when hit, thus reflecting the presence of a hidden mechanical anisotropy acquired by the rock under (usually horizontal) compression during the initiation of a joint network, as opposed to their opening during the late phase of decompression (usually related to regional uplift).

The rose diagrams in the paper's figure 20, derived from Fig. 9 of Solecki (2011) (see Fig.1 below) are welcomed. Their more detailed description and interpretation supporting their Late Cretaceous-Paleogene age can be found in Solecki (2011, in Polish), whereas the English description can be found in Solecki's (1994) paper.

Fig. 1. Strikes of joints (all), derived from Fig. 9 Solecki (2011).

P1–T2 – Permo–Triassic strata orientation; K2 – Cretaceous strata orientation; WG – WleÅ Graben; SG – Å wierzawa Graben; LS – Leszczyna Syncline; BS – BolesÅ awiec Syncline; TB – Tertiary basalts; PV – Permian volcanites; Pz – epimetamorphic basement; P1 – Rotliegend sediments; P2 – Zechstein sediments; T1 – Buntsandstein sediments; T2– Roet and Muschelkalk sediments; Cr2 – Late Cretaceous sediments.

More details of joints running transverse to the folds are visible in rose diagram Fig. 10 of Solecki (2011), where only vertical joints were included, (see Fig.2 below).

Fig. 2. Strikes of joints (vertical only), derived from Fig. 10 Solecki (2011).

Other explanations as in Fig.1.

Fig. 3. Orientation of strata (contours of plane poles, upper hemisphere) and deformation bands (circles ,upper hemisphere); derived from Fig. 2 Solecki (2011).

Other explanations as in Fig.1.

It would be nice to have your paragraph about the joints completed with information about deformation bands described by Solecki (2011) (cf. Aydin 1978, Fossen et al. 2007).

A comparison of the cataclastic bands orientation (Fig.3) with that of the faults and strata indicates their original relationship with the main North-Sudetic synclinorium compressive deformation during the Late Cretaceous – Early Paleogene. N-S system of joints seems to be related with north-south Paleogene age compression near significant fault lines as described by Cobal (1990) from the Bohemian Cretaceous Basin.

Recommendation

Irrespective of the above disputable issues and remarks, in much part addressed to the reservations expressed by the other reviewer of this paper, the paper itself deserves to be published in the Solid Earth.

References

Aydin A.: Small faults formed as deformation bands in sandstone. Pageoph., 116, 913-930, 1978.

Beyer K.: Zur kimmerischen Faltung in den Nordsudeten. Zeitschr. d. deutsch. Geol. Ges.. 86, 702-702, Berlin,1934.

Coubal M.: Compression along faults: example from the Bohemian Cretaceous Basin Mineralia slovaca (1990), 139-1444, 1999. Centrum24.pl/centrum24-web/multi/dashboard

Fossen H., Schultz R.A., Shipton Z.K. and Mair K.: Deformation bands in sandstone: a review. Journal of the Geological Society, London, Vol. 164, pp. 755–769, 2007.

Jackson J.: Reactivation of basement faults and crustal shortening in orogenic belts. Nature 283, 343–346, 1980. https://doi.org/10.1038/283343a0

Kley J.2013 , "Saxonian tectonics" in the 21st century. Zeitschrift der Deutschen Gesellschaft für Geowissenschaften Band 164 Heft 2 (2013), p. 295 - 311published: Jun 1, 2013

Kowalski A., Durkowski K. and RaczyÅ ski P.: Zechstein marine deposits in the WleÅ Graben (North Sudetic Synclinorium, SW Poland)– new insights into the palaeogeography of the southern part of the Polish Zechstein Basin. Annales Societatis Geologorum Poloniae (2018), vol. 88: 321–339,2018.

Markiewicz A. and Becker R.: The original extent of the Oldest Halite (Na1) in the southern part of the Fore-Sudetic Monocline (SW Poland) GEOLOGIA 2009 Tom 35 Zeszyt 3 327–348,2009.

Price N.J.: Mechanics of Jointing in Rocks. Geological Magazine , Volume 96 , Issue 2 , April 1959 , pp. 149 – 167, 1959 DOI: https://doi.org/10.1017/S0016756800060040

Price N.J.: Fault and joint development in brittle and semibrittle rocks. Pergamon Press, Oxford-London. 176 pp, 1966.

Solecki A.T.: Tektonika dysjunktywna i jej wpÅ yw na warunki wystepowania kopalin w synklinorium pólnocnosudeckim, PhD thesis, University of WrocÅ aw, 1-152 (in Polish), 1986.

Solecki, A.: Tectonics of the North Sudetic Synclinorium, Acta Universitatis WratisIaviensis No 1618, Prace Geologiczno-Mineralogiczne, 45, 1-60, University of Wroclaw, 1994.

Solecki, A.: Rozwój strukturalny epiwaryscyjskiej pokrywy platformowej w obszarze synklinorium póÅ nocnosudeckiego (Structural development of the epi-variscan cover in the North Sudetic Synclinorium area). In: Å»elaÅºniewicz, A. et al. (eds), Mezozoik i kenozoik Dolnego Å lÄ ska, Polskie Towarzystwo Geologiczne, WrocÅ aw, 19-36, (in Polish with English summary), 2011.

Ziegler P.A.: Compressional intraplate deformations in the Alpine foreland. Introduction. Tectonophysics V. 137, Issues 1-4, Pages 1-5,1987.

Response to Andrzej Solecki (Referee) – RC2

Dear Professor Solecki,

We appreciate your positive opinion on our manuscript and the suggestions to complete our paper with additional material and lacking references, which we will seriously take into consideration.

As to your specific comments, our position is as follows:

(1) The existence of a Triassic-Upper Cretaceous stratigraphic gap and the question of a possibility to have the effects of the Cimmerian deformation in the North Sudetic Synlinorium is important to understand all the aspects of the Sudetic geology. This problem, however, lies beyond the thematic scope of our intended paper. On our studied seismic profiles we were not able to detect the Triassic/Upper Cretaceous unconformity. The profiles showed us only an erosional unconformity at the boundary between the Triassic and Late Cretaceous. We did not observe, either, "the Cimmerian basement folds" interpreted by Beyer (1934), you refer to. We saw only faults that cut across the Zechstein strata, while, upwards, end up in the Triassic, which makes it possible to hypothetically relate them to the Cimmerian tectonism.

(2) The possible role of salt tectonics should be, indeed, taken into account, but only on the Fore-Sudetic Homocline, and following your remark, we will add a short comment to our text It will contains information that in the neighborhood of the seismic profiles presented in the paper there may occur salt-tectonic phenomena, in particular such as normal/listric faults rooted in Zechstein salts.. Nevertheless, in case of the seismic profiles from the Sudetic Homocline we show in the manuscript, the Permo-Mesozoic strata are (very gently) folded together and concordantly with the Rotliegend and older substratum, which points to the lack of salt tectonics in these particular cases. (3) Your suggestion ("it would nice to mention") of confirming earlier ideas of Solecki (1986) by our paper, will be considered by us, and we tend to include in our text an appropriate short mention.

(4) In the the question of the genetic relationships of the joint pattern with the Late Cretaceous deformation we, of course, agree with you. It seems that both you and us were influenced by the books and papers of the late Professor Wojciech

Komentarz [Authors5]: A relevant comment was included by us in the revised manuscript in lines 132-136.

Komentarz [Authors6]: In the meantime, we learned about some sporadic occurences of rock salt also in few boreholes in the North Sudetic Synclinorium. In the revised manuscript we added a short comment on this in lines 193-194.

Komentarz [Authors7]: A relevant comment was added by us to the revised text in lines 219-221.

Jaroszewski of Warsaw University, who promoted the ideas of separate initiation and opening of joints, which turned out to be – in much part - convincing to us.

(5) The captions and comments to the rose diagrams and other illustrations borrowed from your publication (Solecki, 2011), will be improved taking into account your suggestions.

Komentarz [Authors8]: This has been verified

(6) The idea of including in our paper the information on the deformation bands you described from the North Sudetic Synclinorium (Solecki 2011) was clear to us from the very beginning of our writing the paper. The reason we have not done this yet, was quite prosaic: we had to stick to the deadline for the submission and did not manage to include an appropriate short passage on the deformation bands. This will be rectified in the revised manuscript.

Komentarz [Authors9]: We have included information on the deformation bands in lines 422-427 of the revised manuscript.

Comments by Anonymous Referee – RC3

The Authors claim that in the Sudetes, during the late Cretaceous–early Palaeogene, several macro-scale tectonic structures were formed in Permo-Mesozoic cover, mainly due to shortening in NE-SW/NNE-SSW direction, which also affected a crystalline basement. However, most previous papers on that issue indicate an earlier extensional stage which preceded inversion in a subsequent compressional stage. This earlier stage is not considered by the Authors who focus on structures that apparently developed only by contraction. Consequently, the ms. is based on rather incomplete review of the existing literature and on reprocessed seismic reflection data once acquired by industry in two Sudetic synclinoria. The interpret-ted time-migrated sections are the only new element in the ms.

Eventually, the literature-based description of the reviewed tectonic units is accompanied by the cross-sections to geological maps of those units and by the seismic profiles of two of them. However the seismic profiles are poorly discussed while offering only colored arbitrary interpretations. Any verification is hampered by the paucity of boreholes in the region on one side and by the scarcity of Authors' own field studies on the other. The only exception is a single cataclastic shear zone observed near Lewin KÅ odzki (Fig. 18b), which is speculated to be part of a strike-slip transition between the PoÅ ici-Hronov reverse fault and the Zieleniec thrust or just a continuation of the latter. Unfortunately, the Zieleniec thrust is one of ambiguous features in the Sudetes and it is nowhere exposed (Cymerman, 1990). Although there is no doubt that mica schists have been drawn easterly over Cretaceous sedimentary rocks along the contact of the two lithologies, their structural relationships might be variously interpreted in view of both the recently drilled borehole Zieleniec PIG-1 (Kozdrój, 2014; in the ms., Fig. 12 is a conceptual ideograph but not a real, geometrically rigorous cross-section) and detailed field observations along strike. Kozdrój (2014) mentioned three feasible options: thrusting of unknown exact age, rejuvenate faulting with reverse motion over earlier deformed footwall, or landslide down the steep slope during later Neogene extension, Besides, he pointed to multi-stage evolution of the Nysa KÅ odzka Graben under extensional and then under compressional regime with strike-slip component. Such conclusion is in line with other views on the graben (e.g. Don & GotowaÅ a, 2008;

Badura & Rauch, 2014). The present Authors neglect (without justification) evidence of fault-controlled syndepositional subsidence of the graben floor, strikingly do not comment on subsequent open folding of its infill and entirely omit the data presented by Badura & Rauch (2014) except for copying their blockdiagram (Fig. 17). Instead, they emphasize later E-W extensional rejuvenation and thus reshaping of the graben. Such an approach clearly disagrees with the overall message of the ms. stating that ‘all the reviewed structures are due to the Late Cretaceous-Early Palaeogene tectonic shortening episode”.

The above message is also inconsistent with the multi-stage (extension→compression→extension) formation of the WleÅ graben (SW edge of the North-Sudetic Synclinorium”) recently described by Kowalski (2020), which otherwise is similar to the tectonic evolution of the Nysa Graben. Again, the ms. Authors see problematic (without any argument) interpretation by Kowalski because his remark that seismic survey might help to better understand geometry of that structure. The North-Sudetic Syncli-norium itself has been long considered as due to consecutive operation of the extension→compres-sion couple. The ms. Authors, based on the reprocessed seismic profiles, see this unit as an exclusively compressional structure being effected by down-warping of the basement-cover interface, low-angle thrust faulting and detachment folding. Although all these features may quite feasibly occur, no detachment is indicated, no small-scale folds and reverse faults reported from the field, whereas the concave base of Permo-Mesozic strata might still be effected by early normal faulting in the basement. The Authors provide no verification of their interpretation, which leaves the reader dissatisfied. Similar uncertainty comes from their consideration of the Intra-Sudetic Synclinorium.

An appealing structure of the Lusatian Fault (Coubal et al., 2014) has received meticulous field documentation. This is badly missing in the reviewed ms. The Lusatian feature shows high structural variability along strike, which calls for caution in simple duplication of that example, no matter how much it is attractive. Also caution has to be exerted while orthogonal (sub)vertical jointing in Cretaceous sandstones is attempted to be related to contraction imposed on a sedimentary infill in the Sudetic synclinoria and arbitrarily assigned to the same stress field. This is another weak point of the ms., which requires more meticulous study and consideration. Another interesting topic, only skimmed in the ms., is the reactivation of fracture pattern in the crystalline basement during late Cretaceous–Cenozoic times and how much such process has controlled/in-flu--enced development of joint and fault systems in the Permo-Mesozoic cover.

At the moment, all those failures and omissions render the ms. rather immature for publication. A resubmission would be welcome with focus on the seismic profiles, their decent discussion (with alternatives) and on collecting the small-scale field observations to support the assumed interpretation, especially in the face of likely high improbability of any drilling project in the region in not too distant future.

Our response to Anonymous Referee – RC3

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

Komentarz [Authors10]: We were not successful in finding out any additional significant papers that would broaden or deepen our knowledge on tectonic aspectsof the pre-late Cretacous extensional development of any of the structural units we described in our manuscript, and the referee was not helpful in this respect, either, as they did not specified themselves any such lacking sources in our manuscript. In these circumstances, we had to constrain ourselves to adding references to only two short papers from conference field guides (Śliwiński et al., 2003; Chrząstek and Wojewoda, 2011 – in lines 165-166 and 172), which to some degree fill the indicated gap.

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.

Komentarz [Authors11]: We have done this in lines 320-322 of our revised manuscript.

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

Komentarz [Authors12]: In the revised manuscript we slightly expanded our argument – in lines 243-246 - for considering the Kowalski's (2020) solution as problematic 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.

Komentarz [Authors13]: We complemented the text with respective comments, except for one instance, already mentioned in Comment AP10, when we were not able to find out any important allegedly lacking references concerning the extensional tectonics pre-dating the Late Cretaceous inversion

References:

Migoń, P., Danišík, M.: Erosional history of the Karkonosze Granite Massif –constraints from adjacent sedimentary basins and thermochronology. Geological Quarterly, 56, 441–456, 2012

Sobczyk A., Danišik M., Aleksandrowski P., Anczkiewicz A.: Post-Variscan cooling history of the central Western Sudetes (NE Bohemian Massif, Poland) constrained by apatite fission-track and zircon (U-Th)/He thermochronology. Tectonophysics 649, 47–57, 2015.

Teisseyre, H.:Spękania skalne w południowo-wschodniej części Gór Kaczawskich i w północnej części depresji Świebodzic (Jointing in rocks of the soutwestern part of the Kaczawa Mts and in the northern part of the Świebodzice Depression), Geologia Sudetica 11, 9-55, 1976.

Żelaźniewicz, A., 1977. Rozwój spękań w skałach metamorficznych Gór Orlickich. (Development of fracturing in metamorphic rocks of the Góry Orlickie (Sudetes). Rocznik Polskiego Towarzystwa Geologicznego, 47: 163–191. 1977.

Comment by Gabor Tari – CC1

Very nice manuscript and I have only a minor comment/request. I would like to use Figure 15 reminds me another basin and therefore would like to use it as an analogue... So it would be helpful to see a scale on this figure, I assume it is drawn H=V. Also, how it is constrained exactly in the subsurface?

Thanks,

Our response to Comment by Gabor Tari – CC1

Dear Dr Tari,

We appreciate your positive impression of our manuscript, as well as the particular interest in one of our examples of Late Cretaceous-Early Palaeogene tectonic structures in the Sudetes, namely the Czerwieńczyce Reverse Graben presented in our Figure 15. The figure shows Józef Oberc's (1972) interpretation, redrawn by us from his original figure without changing the structure's geometry. We confirm that the horizontal and vertical scales are equal in this figure. The width of the graben is almost precisely 1 km at the ground surface and now we realize the necessity to add the scale bar to the figure in the revised manuscript and, perhaps, also to complete the figure with the geological information concerning the graben's both sides and basement.

The constraints on the geometry and stratigraphic content of the Czerwieńczyce Reverse Graben, as illustrated in Oberc's cross-section originally resulted from the surficial field mapping of this author (Oberc 1957), who was most probably also inspired by underground geological cross sections coming from the nearby coal mines directly adjacent to the portrayed part of the reverse graben in the NE, N and SW. The coal mines were active from the 19th to the early or late (some of them) 20th centuries. The geology of the Czerwieńczyce Reverse Graben as shown in Fig. 15, although conjectural at depth, was later partly verified and mostly confirmed by the exploration borehole Dzikowiec IG-1, drilled in 1984-1985 c. 1.5 km SSE of the cross-section presented in Fig. 15. The borehole has penetrated the Rotliegend base at a depth of 991 m and the base of Carboniferous clastics/top of Lower Devonian gabbro at 1422 m, the latter rock variety found to continue until the borehole terminal depth at 1800 m (Bossowski 1995).

References:

Bossowski, A. (ed.): Dzikowiec IG-1. *Profile głębokich otworów wiertniczych Państwowego Instytutu Geologicznego.* Zeszyt 82 (Profiles of deep boreholes of the Polish Geological Institute. *Fascicle 82*). Państwowy Instytut Geologiczny, Warszawa, 70 p. (in Polish), 1995.

Oberc, J.: Region Gór Bardzkich (Sudety). Przewodnik dla geologów (The Bardzkie Mountains region, Sudetes. Field guide for geologists). Wydawnictwa Geologiczne, Warszawa, 284 p. (In Polish), 1957.

****

As concerns all the three reviews, we are glad to have opportunity to exchange views and arguments with the referees and to implement their constructive suggestions in our revised manuscript. We, thus, thank them all for the careful reading and their contribution in improving the quality of our paper.

Apart from the changes made in the text, we have also improved many of the figures, mostly in their graphical and editorial aspects, without much changes concerning their geological content except for Fig. 10b, where an entire seismic section was replaced due to a mistake originally made – repeated section from Fig. 9b.

Yours sincerely

Andrzej Głuszyński and Paweł Aleksandrowski

January 13th, 2022