Solid Earth Discuss., author comment AC4
https://doi.org/10.5194/se-2021-99-AC4, 2021

Reply on RC1

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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-AC4, 2021

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.

(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 $\sigma1$ 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 $\sigma 1$; 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 ($\sigma 1$ 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.
Yours sincerely

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