Response to comments by Gabor Tari (marked below in blue)

We highly appreciate all the comments you provided regarding our paper on inversion tectonics in NW Poland. Here is our response to your three general comments:

1. The manuscript suggests a very important departure from the standard definition of inversion tectonics which requires basement involvement and excludes mobile substratums such as salt or shale (see many landmark papers by Mark Cooper). Regardless whether it is a good or bad idea, I suggest to have it emphasized stronger in the text. Figure 1. does a good job in this regard, but still. One argument for this broadening of the definition of positive structural inversion could be the aim to include large salt basins where basin-scale inversion (like in the Zechstein region) manifests itself very dramatically and folks use the term already for a long time (incorrectly, by the existing definition!), anyhow.

Indeed, one of the key points of our paper is description and analysis of thin-skinned inversion scenarios, without involvement of the crystalline basement. In order to better described and illustrate this, Fig. 1 was modified so it now also includes a model for master fault detached in evaporites (1C) as such scenario is reflected in our own data; additionally, relevant part of the text has been expanded.

2. Personally, I think negative inversion is a useful term and it should be kept. The word reactivation is the really general one which is being used typically not with inversion in mind, but rather refering to a fault which keeps reactivating in the same sense. Perhaps it would be good to add a few sentences to the manuscript in this regard, either pro or contra.

Discussion on negative versus positive inversion has been expanded to better clarify our opinion that the term inversion should be rather restricted to the “classic” scenario proposed by Bally (and clearly described in your own paper from this volume). This is also in agreement with remarks made by Mark Cooper in his review of our paper. Additionally, we checked several textbooks and key papers (but not all, of course, so we might have missed something …) and haven’t found definition of “fault reactivation” suggesting that reactivation implies renewed activity in the same kinematic sense; we think that “reactivation” means just renewed activity, in the same or opposite kinematic sense: normal fault could be reactivated as normal or reverse / thrust fault, and vice versa, reverse / thrust fault could be reactivated as reverse / thrust or normal fault. Actually, very good illustration of reactivation of thrusts during normal faulting (i.e. in opposite kinematic sense) is shown in Fig. 2 in your very interesting recently published paper on the Miocene extension within the Alpine – Carpathian junction area (*Tari et al. 2021, The connection between the Alps and the Carpathians beneath the Pannonian Basin: Selective reactivation of Alpine nappe contacts during Miocene extension. Global and Planetary Change, https://doi.org/10.1016/j.gloplacha.2020.103401*).

3. It is good to see that the authors claim that they only speculate about the extensional reactivation of Caledonian thrust planes during the late Paleozoic in the Study Area. If there is a convincing subsurface example for this proposition anywhere in NW Europe, it would be worth

to show it ... with a seismic line and/or wells. I have my doubts that there is a bullet-proof example out there, but I am might be wrong (hopefully!)

We indeed just speculate about extensional reactivation of Caledonian thrusts being responsible for formation of half-grabens recently imaged by 3D seismics in NW Poland beneath the Zechstein evaporitic cover as much deeper high-quality seismic imaging, comparable to the results of e.g. PolandSPAN survey, would be required in order to map master faults at greater depths and to actually see their relationship to the Caledonian FTB. In the text we cited several papers that document late Paleozoic extensional reactivation of Caledonian thrusts and formation of extensional grabens (Coward et al., 1987; Fossen, 2010; Koehl et al., 2018; Norton et al., 1987; Osmundsen and Andersen, 2001; Rowan and Jarvie, 2020; Scisciani et al., 2021; Séranne et al., 1989; Stemmerik, 2000). Additionally, Lassen et al. (2001), also cited in our paper, has an example from "our" part of Europe that is based on seismic data (although not of the highest quality). We think that these examples together with high-quality seismic image of the most frontal part of the Caledonian FTB provided by the regional high-end PolandSPAN survey that was used to construct our Fig. 9 provide enough basis for our speculations. Of course, other possibilities could not be definitely ruled out; hopefully in the future new deep seismic data would provide final answer to this problem.

Detailed comments in the annotated pdf file were mostly of technical character and have been considered while working on the revised version of our manuscript.

Some of the figures have been modified as suggested:

Fig. 1: thick-skinned and thin-skinned descriptors added in the figure caption
Fig. 3 and 5: coastline added to the legend
Fig. 6: vertical scale annotated in [km]
Fig. 7: we tried to use different color displays for seismic data but end of the day we think that grey scale version provides best results, especially for panel C, so this figure hasn't been modified.
Fig. 8: this figure was designed for one full page so removal of particular panels would not be in our opinion advisable; panel (k) was amended as suggested
Fig. 9: color of the basement was changes as suggested
Fig. 11: information about equal scales for both seismic examples was added to the text

Response to comments by Mark Cooper

All the suggested linguistic corrections have been taken into account. Regarding specific comments given in the annotated pdf file with our paper:

Line 65 and line 73: we are glad that you agree with us on our opinion regarding the term "negative inversion" etc. We expended a bit section with discussion of positive versus negative inversion in order to better acknowledge discussion on the same topic contained in Cooper &

Warren (2020), and to better clarify our point of view and to accommodate concerns raised by another reviewer (Gabor Tari).

Line 112: Laurussia i.e. the supercontinent that was formed in late Silurian as a result of a closure of Iapetus Ocean and collision of Baltica, Avalonia and Laurentia and ceased to exist due to the Variscan orogeny in late Carboniferous is correct term here (c.f. https://www.intechopen.com/chapters/37859). Laurasia was formed in Carboniferous–Permian out of Siberia, Kazakhstan and Baltica.

Line 254: unfortunately, no deep wells have been drilled into the axial parts of the sub-Permian half grabens so no maturity data is available to provide additional constrains regarding amount of erosion and burial / uplift history.

Fig. 2 and Fig. 4: 355^{O} was changed to -5^{O}

Fig. 3: indeed, this is a subcrop map of the units beneath the Variscan unconformity