Comments on the "Impact of Timanian thrust systems on the late Neoproterozoic-Phanerozoic tectonic evolution of the Barents Sea and Svalbard" by Jean-Baptiste Koehl et al (se-2021-71)

This an interesting manuscript proposing an alternative model for the accretion of western Barents Sea and Svalbard. As a non-specialist in Timanian orogeny nor in Svalbard/Artic geology, I found it very challenging in comparing existing models, but well worth being published in Solid Earth. Hence, my comments are mainly regarding the methods used and presented observations/interpretations. This study is based on the geophysical methods, that are the interpretation of seismic reflection profiles, total gravity and magnetic field anomalies.

The study area records a very complex geology with several deformation phases (four compressional and two extensional) took place that are overprinting each other as it is stated by the authors. In addition, the study area is located in the offshore with only three wells presented. Therefore, this study greatly relies on the geophysical methods. In such a case and in order to increase the accuracy of the presented interpretation I would start by carefully characterizing (as much as available data and previous studies allows) geophysical signature (here magnetic, gravity and seismic reflection) of the known Timanian structures onshore/close to the shore.

In this setting, Trollfjorden-Komagelva Fault Zone (TKFZ) as a well-known Timanian structure onshore northern Norway could be the best candidate as the geophysical onshore-offshore data in northern Norway are fairly accessible. It would be very interesting and helpful if authors quantify geophysical character of the TKFZ, its spatial relationship to the overprinting Caledonian and younger events and describe how this structure extends to the offshore then use that as an analogue for the study area.

In potential filed data one would try to used different filtering techniques and attributes in order to separate deeper (presumably older) structures form the shallower (younger) structures and then study spatial development of the interested structures. Without an attempt to separate relative depth of causative bodies observed in potential filed data it is very hard to identify structures related to different tectonic events. I am not sure if we can simply interpret all E-W striking anomalies observed on total magnetic field and gravity data as Timanian and all N-S to NNE-SSW as Caledonian. I do agree and acknowledge that the main trends can be identified in gravity and magnetic data, but author should also consider and discuss alternative interpretations for observed trends, especially considering spatial geometry of structures over several hundreds of kilometers. In the next step, observations from the potential field data can be compared with the seismic reflection data.

As it is mentioned by the authors, e.g. Barrère et al. 2009, Gernigon et al. 2014 & 2018, and the ATLAS, Geological History of the Barents Sea (Geological survey of Norway, 2009, not cited by the authors) have done such a methodology in different parts of the Barents Sea concluding that post-Timanian events (mainly Caledonian) overprinting the Timanian structures and continuation of Timanian structures is only identified in northern Norway (TKFZ). I understand that above mentioned studies might have not been aiming for mapping the westward extension of the Timanian structures, but I think it would be of interest if authors consider discussing similarities and differences between potential filed data interpretation in this study and previous ones.

Another major Timanian structure identified in Novaya Zemlya Island is the Baidaratsky Fault Zone (BaFZ, shown in Figs. 1 and 5). BaFZ is mapped onshore Novaya Zemlya as awide (ca. 30 km) fault zone (e.g. Lopatin et al., 2001; Korago et al., 2004). Korago 2004 in their Fig. 8 show a NW continuation of BaFZ (dashed line) and state that "presumably" BaFZ continues NW into the eastern Barents Sea.

However, Korago et al., 2004 did not carried any offshore studies in this regard and refer to Lopatin 2001. While Lopatin et al. 2001 also did not studied western offshore Novaya Zemlya and only show the location of BaFZ onshore. Therefore, based on Lopatin et al., 2001 and Korago et al., 2004 it is really difficult to conclude any NW extension of BaFZ into the eastern Barents Sea. I understand that accessing geophysical data in eastern Barents Sea is challenging, however, some across border studies (e.g. ATLAS, Geological History of the Barents Sea, 2009, Geological Survey of Norway) are available and could be used in gravity and magnetic analysis and interpretations. Looking at filtered magnetic and gravity and presented derivatives presented in their Chapter 2 (IMAGING DEEP STRUCTURES BENEATH THE SURFACE) I can recognize E-W to ENE-WSW oriented structures onshore and offshore south of Novaya Zemlya Island extending SE into the Russian main land (Pechora Basin?). Farther west from Novaya Zemlya and into the central Barents Sea main structures are N-S striking. Based on above, I have difficulties tracing BaFZ all the way into the western Barents Sea and link it to the E-W structures south of Olga Basin shown in Fig.1b. I do agree that in the western Barents Sea there are structures orienting E-W and ENE-WSW, but also there are N-S and NE-SW structures. It would be very helpful if authors could explain such a complexity in the western Barents Sea and westward extension of BaFZ specially across the areas with very strong N-S orienting magnetic and gravity signature.

I would assume that westward extension of identified thrust zones into the onshore Svalbard is based on the gravity and magnetic data. Looking at Fig.5, onshore Svalbard is at the edge of the dataset and it is not really possible to see any trends, while filtered magnetic and gravity maps shown in the ATLAS, Geological History of the Barents Sea, 2009 covers the entire Svalbard and its western offshore, showing N-S trends being very pronounced. I would suggest authors compare their observations with above mentioned reference and discuss potential differences and similarities observed. Also, as Fig. 1b shows there are seismic profiles available on the western offshore Svalbard, do those seismic profiles have also been studied? Do they show extension of identified thrust zones across Svalbard? Sine authors argue that Timanian structures onshore Svalbard are unnoticed because of the remoteness of the area and the strongly eroded character of the area, showing the extension of Timanian structures west of Svalbard could provide an additional proof for the presence of Timanian structures across the Svalbard.

I assume that the shown seismic reflection profiles are the best examples from many other studied and interpreted profiles. However, the quality of presented profiles really does not allow readers to attempt interpreting profiles, even higher quality version of seismic profiles made available by authors did not help. I would suggest authors to use higher quality and less noisy profiles (if available), in the shown profiles I can see some intra-basement trends, but I also can add in much more patterns. As an example, along the profile shown in Fig.3b lost of patterns are not interpreted in the center of the profile, what would those reflections represent? In addition, confirming thrust zones dip direction (since dip directions mentioned in the text are apparent dip) it would be much more convincing if author show at least one profile parallel to 3a and 3c farther east as fig.1b shows that are more profiles available east of 3a and 3c.

As profile 3b is semi-perpendicular to main Caledonian N-S trend, it would be very interesting if authors consider interpreting Caledonian structures along profile 3b and show/discuss the spatial relationship between Timanian and Caledonian structures.

Authors claim that that thicker Precambrian basement rocks shows higher Bouguer anomaly values (lines 532-535) and take this as an evidence for the thrusting causing thickening of basement rock into the footwall of thrust faults. Looking at profile 3a, the southern parts of the profile shows thickest Devonian-Permian sedimentary rocks and thinner Precambrian basement rocks. Such a configuration should be reflected as low Bouguer anomaly (thick sedimentary rocks) while shown gravity anomaly profile in the lower panel show high gravity values. On the opposite end of the same profile (Fig. 3a) where the Precambrian units are thicker gravity anomaly profile shows very low values. Same inconsistency also appears along profiles 3b (in the center) and 3c (to the north). This is confusing, please consider clarifying.

Closest well utilized for well-seismic tie in the study is the well Hopen-2 which is located 40-45 km north of profile shown in Fig. 3b. According to Harald and Kelly 1997 and Anell et al. 2014 well Hopen-2 is drilled into Late Carboniferous sedimentary rocks and the top basement is not reached. Please consider briefly explaining how boundaries between Precambrian, Cambrian-Silurian, Devonian-Mississippian and Devonian-Permian are identified and interpreted.

In the proposed model shown in Fig. 7, I am wondering when a several km thick shear/thrust zone inherited form the Timanian event exist (Fig. 7a) why such a structure is not simply reactivated as strike-slip fault/shear zone and instead it is folded and cross-cut by Caledonian structures? Could authors back up this model with natural cases or modeling studies? A discussion elaborating this would be of interest.

In general, this is a well-written article presenting geophysical evidence for and further highlighting existing models proposing westward extension of Timanian structures across the Barents Sea. The study also discusses pre-Caledonian plate tectonics implications of such a configuration that it might be of great interest for Solid Earth readers. I believe the paper is very interesting and can be published after addressing my comments. I would be happy to further discuss my comments and look forward to seeing this manuscript being published.

Best regards,

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