Review Report

Koehl et al., Timanian fold-and-thrust belt and Caledonian ovewrprint in the Selis Ridge imaged by new 3D seismic attributes and spectral decomposition, TEKTONIKA, 2023

Table of Contents

1st Round of Revisions

Decision Letter

Dear J.B. Koehl and co-authors,

First of all, we would like to thank you for your submission to Tektonika. It is fundamental our community participate to the effort of launching this new journal and we really appreciate your contribution.

Your manuscript fits the scheme of the journal, presents new data and shows interesting new observations that help strengthen our understanding of the Timanian and Caledonian orogenies – what is of great interest for the study region. Based on this, we are convinced your manuscript should be published with Tektonika. The subject, methodology, and findings are adapted to the scope of the journal. However, at this stage, we recommend a resubmission after modifications. To make sure your contribution will find its audience, it needs to be slightly reworked, shortened, and focused (see reviewers' comments) and we want you to take the time that is necessary for.

The reviews provided by Roy Gabrielsen and Laurent Gernigon are fair, clear and justified. They should help you work through your manuscript efficiently. Particularly, the manuscript tends to be slightly unfocused, what may dilute your message to readers not familiar with the study site – so please try to shorten and focus your descriptions to clarify your findings. Based on this, we also recommend reworking the illustrations to propose less figures with improved quality.

If you have any questions, please do not hesitate to contact us.

Additionally, if you have any comments or recommendations for the journal Tektonika (how the ms is handled, the website, the emails/contacts), please do not hesitate to contact us. You are part of the first manuscripts we are handling, so any feedback will be useful and greatly appreciated.

Hongdan Deng, Associate Editor,

Gwenn Peron-Pinvidic, Executive Editor

August, 11th, 2022

Comments by Reviewer 1

General evaluation and recommendations:

This paper presents new data of great interest for the description and understanding of the pre-late Palaeozoic history of the Barents Sea. The problems addressed are well presented. The descriptions, findings and discussions are generally well formulated, and the English language is generally of reasonable standards. I agree in most of the conclusions drawn from the present data and the context in which the observations are discussed.

The manuscript still has several shortcomings when the architecture, structure phrasings are concerned. The manuscript lacks a little in maturity, in that the relation between the Introduction, Description and Discussion is not entirely well-balanced.

The enclosed annotated manuscript contains detailed comments on the presentation. Generalized major items that should be improved are:

1) The structure/architecture of the manuscript should be considered carefully. See comments below and in ms.
2) Detailed descriptions are not complete on all points and a consequent use of terms in the structural geology description should be focused upon.
3) There is a need for enhanced and more specific application of the principally important seismic facies concept.
4) Information about real geometries (e.g. dips) is lacking on some essential points. Such information is needed in the interpretations of some structural elements.
5) The discussion chapter takes up elements that are not fully constrained by present data, giving a slightly whimsical impression.
6) Several statements need some explanation and clarification and some basic nomenclature used in an imprecise, ambiguous and sometimes even used in an incorrect manner.
7) I think there is a need for a simplification and a full revision of the Conclusions-section (see below).
8) Although the manuscript has an extensive reference list, some important (not only recent) references are lacking or have been misread. A thorough review of their reference list and the use of it by the authors would be required.

This paper would be an important contribution to understanding the early history of the northern part of the Baltic shield in general and the Barents Sea in particular and the paper as such, deserves publication. It is therefore recommended that the authors are invited to re-submit the paper to Tektonika after improving its shortcomings.

Specific comments

Most specific comments have been included as word-formatted comments in the enclosed annotated manuscript, and only general evaluations of each section s given here:

Introduction

The Introduction is well written and defines the scientific aims of the paper well, but the authors should consider whether the Introduction- and the Discussion-chapters are are completely I harmony. The section is still a little under-referred with respect to previous and related works from the area. Some of the missing references appear in the following section "Geological setting", but here in a slightly different context.

I think that the Introduction would benefit from restructuring to contain:

a) An introduction in the chapter that focuses on the problems focused addressed in the Discussion, and an expansion of those structural items/periods of the structural history that are of particular importance.
b) Inclusion of a couple of recent works of relevance that seems to be lacking.

Geological setting

This part is generally well written in a clear language. Still, some references to works that are indeed relevant are lacking (see comments in annotated ms). Unfortunately, this chapter seems slightly defocused when the central topics of the paper are concerned (see comments to the Introduction-chapter): It offers a summary of the entire structural geologic history of the Barents Sea, all of which I think is not equally relevant for the present paper. It makes the reader to wonder whether the "Geological setting" could be condensed and brought in more complete harmony with the main items addressed in the Discussion chapter.

I think the paper already contains the bulk of this information, so it should be easy for the authors to make this rearrangement.

Methods

This section should be expanded under the heading **Data and methods**.

This important section now lacks in information of reflection seismic data (not even names of survey(s) are given), and other technical information are also lacking. I think the idea of defining and describing seismic facies (Figure 3) is fine, but it presently not fully utilized. In the Data- and Interpretation-chapter seismic facies now blends with the tectonic segment descriptions, making the Results-and interpretation-chapter little fuzzy.

It seems to me that the descriptive part of the paper would gain in clarity if the seismic facies-concept with definitions (FPx-y) were included in this chapter.

Other technical information like evaluation of dips of shear planes (these are time sections, and no depth-conversion or 1:1-dispalys have been attempted), so all angles shown in the figures are obviously unreal. The real dips of key structures like faults should be estimated and given in the Description-chapter Did I overlook the mention of this problem in the structural analysis?

The Supplement (my copy did not contain figures) generally offers information on cores and some general information seismic characters and was of little help in the items mentioned above.

Results and interpretation

I do endorse that this section start with an ingress/introduction text as already done. I think, however, that the ingress should be expanded, giving first a general outline of the Selis Ridge (including its position relative to other neighboring structural elements). It is OK that the reflection seismic character of the ridge is included here but it could be more visible to the reader if it were transferred to the section “Data and methods”; see comment above).

I also endorse that the authors have chosen very strictly to separate between geometries in the descriptions of the seismic sections and the interpretations. This is a clean-cut strategy, but I think the authors are a little over-cautious in some cases e.g. in using terms like “U-shaped” reflections instead of “syncline”.

I also think the description chapter could be further improved by:

a) More clearly explaining the approach in an introduction to the chapter
b) include a short description the characteristics the ridge in general,
c) explain the rationale for the structural subdivision of the ridge into segments, followed by a more systematic description of each segment
d) Description of structural items like folds and faults like it is already done.

Interpretations

I agree with the authors about their general interpretations, although I do not always agree in their technical descriptions (see annotated specific comments).

Discussion

I think the main argumentation in the Discussion chapter from the study area *sensu stricto* is fair and I generally agree with the conclusions drawn.

I still find that some reference to some important works in the study area are lacking:

*For example is only one of Breivik et al’s many works on the Caledonian grain/sutures in the Barents Sea referred to, and not much discussed, although their conclusion is at cross with the present paper.

*The “Finnmarkian” problem is treated rather whimsically, introducing “magmatic folding” (whatever that is) referring Krill & Zwaan (1987). To my knowledge the concept of “magmatic folds” (which I understand as folding due to flow in magmas or igneous bodies) was not introduced by these workers (or others) in connection with the “Finnmarkian” deformation. On the contrary, K&Z argued that the relative dating of the “Finnmarkian” as suggested by Sturt et al. was not valid and that dyke intrusion occurred pre-folding, rendering it to the main Caledonian deformation and not Timanian, nor “Finnmarkian”. This was elaborated upon by Roberts et al (2006), who demonstrated that the magmatism was indeed Timanian and the subsequent (tectonic) shortening/folding was mainly of Scandian age. Perhaps some re-reading by the authors is necessary?

*Some recent papers of high relevance to the discussion are not included, but this can be due to that they were released in 2022 (after submission?)

Conclusions

I agree with the main conclusions. I am still sorry to say that I have the feeling that the Conclusion-chapter was written in a haste. First of all, I find that the first set of conclusions (conclusions #1-3) could be simplified and merged (see annotated comment in text). Some of the conclusions following the major ones (beyond #8) are based on musings in the discussion and are not based on direct observations in the study area *per se*. I therefore think the number of items in Conclusion "chapter" should be simplified and shortened.

Simultaneously, the authors could offer a sentence or two reminding the reader what is the rationale on which each conclusion was drawn.

Figures

The paper caries an extensive figure list, mainly with seismic examples, so that the descriptions almost drown in examples. Bearing in mind that the Description chapter introduces a seismic facies list and three ridge segments displayed in figures 2, 4 and 5, one can wonder whether references to these figures in combination with the seismic facies characteristics in Figure 3 would make some of the magnifications (Figures 6-16) superfluous. I am sure this would improve the readability of the paper.

By the way, I am not charmed by the brutalistic style of the interpretations of many of these figures (mainly Figures 6-16): I think the color-line interpretations should be toned down, but I guess this style the choice of the authors and should be decided upon in a discussion with the technical editor of Tektonika.

Also see my comments above on the real geometries and dip-relations.

In spite of the extensive figure material, I miss an additional dedicated summary figure that supports the discussion and conclusion-chapters.

References

The paper offers an impressive list of references. No complete control of the reference list has not been conducted by me: I have only picked arbitrary references for a full check and rather concentrated on references that I either have missed, or where I have seen references supporting statements that were a surprise to me.

In many cases I have found that the missing references were there, but sometimes referred other locations than where I had expected them to be. This is a matter of taste and reference strategy, I guess. However, I did find that some central references are at miss, and some cases where the (in my opinion) there is misunderstanding/misreading of the paper. I have noted those cases in the comments in the manuscript.

Oslo July 27, 2022

Roy H. Gabrielsen

Comments by Reviewer 2

The study of Koehl et al. documents a new 3D seismic dataset of the Selis Ridge on the Barents Sea. Koehl et al. propose a seismic and structural interpretation of the survey and then discuss the tectonic and regional implications of the various trends and structures observed at the edge of the Loppa High.

The results of the 3D survey are interesting and the description of dominant E-W basement trend at the western edge of the Loppa High is definitively intriguing. However, I will be more cautious about the regional interpretation and extrapolation of the ambiguous E-W fold and shear system imaged at top basement level along the Selis ridge.

Koehl et al. suggest that the E-W trending folds may represent the prolongation of the Timanian structure preserved and well documented further west in the Kola-Kanin-Pechora area and in large part of the eastern Barents Sea. It is true that the Timanian structures are relatively well preserved in the eastern part of the Barents Sea up to the Varanger Peninsula. They can also be documented onshore, even if the datation of the “expected Timanian” deformation remains unclear. However, west of the Middle Allochthon Front, the Caledonian deformation expected in most of the western Barents Sea and surrounding onshore area is extremely severe and is characterized by the presence of allochthonous nappes transported over hundreds of kilometers. By correlation with onshore geology, the basement imaged at the level of the Selis ridge may be likely part of the Middle to Uppermost allochthonous rocks. Back to Caledonian time, the Selis Ridge was probably located in the central part of the Caledonian orogenic system, with a suture possibly expected between Stappen and Loppa High. It is very surprising for me that such a severe tectonic and compressional regime simply develops mild NE-SW folds at the edge of the Loppa High. In such a context, why the Neoproterozoic sediments are so poorly deformed is difficult to explain. It needs to be explained.

Furthermore, before the onset of Caledonian, it won´t be surprising that the basement rocks or potential paleo-basins described at the edge of the paleo-Loppa High could have been located far-away and with a different orientation compared to their final position. I remind you that part of the rocks from the Upper Allochthon observed onshore further south have some affinities with Laurentia. Comparing trends of parautochthonous terranes with other trends observed in allochthonous terranes might be problematic and tricky. Many paleogeographic and paleomagnetic studies of Svalbard also concluded that East and West Svalbard were likely part of Laurentia with more affinities to paleo-Greenland. Therefore, a simple and direct NW-SE correlation with the Timanian trends of Baltica proposed by Koehl et al. remains controversial.

It is also important to remind that the 3D cube from Lundin is only a stamp in the regional picture of the entire Barents Sea. It cannot be excluded that the severe but local E-W trends are not Timanian but simply Caledonian in age. Local stress reorganisation, lateral ramp of the staked nappes or tear fault could also explain local structural trends perpendicular to the main thrust orientation in orogenic system. The authors should also consider such alternative hypothesis more compatible with an extreme Caledonian deformation.

A quick comparison with the magnetic data (Gernigon et al., 2014) also shows that south

of Neiden, we can clearly identify a NW-SE magnetic trend that divided the Loppa in two main blocks. This NW-SE magnetic trend is probably an old Precambrian contact/structure for us. Surprisingly, it is not fitting with any of the E-W and N-S seismic trends documented at top basement level in this study. It suggests that the basement geometry in depth is probably more complicated than expected. Old trend may also influence the geometry and the uppermost structures of the subsequent Caledonian nappes.

In the northern part of Selis Ridge, the seismic facies and the strong amplitude reflection may also reflect the presence of intrusions or mafic complex in depth. This may complicate the structural interpretation of the basement as well. The presence of old mafic material or younger intrusion in the Northern part of the Selis Ridge is not considered in this paper.

I also find the last paragraph of the discussion about the Seiland Igneous Ridge out of the topic considering the previous data description. This is not supported by any of the data presented in the paper. I suggest that the authors remove this part, which is apparently part of papers in preparation mentioned in this manuscript but not available for this review.

In conclusion, I do not reject the paper, but I suggest a major revision of the manuscript before publication. I would suggest that the authors stay focus on the new 3D data and improved their structural interpretation of the cube, especially in depth to get the most of it. The authors mentioned several attributes in the introduction but at the end, few of them are really documented in the paper. Additional time slices could be relevant to understand the full 3D geometry of the basement structure. I would also suggest that the authors improve the figures quality. Orientation of the figures are often confusing. Figure 9 also appears to be a perspective view and it is sometimes difficult to distinguish the different features described in the paper. Coordinates and wells locations are missing.

I would finally recommend being more cautious about the regional interpretation and discussion of the various basement trends in the Barents Sea. Interpretation of Timanian deformation at the edge of the Loppa High is too speculative.

Additional minor comments can be seen directly in the annotated Word document attached.

Regards,

Laurent Gernigon

Authors' Reply to Reviewer 1

Dear Prof. Gabrielsen, thank you very much for your input on the manuscript, it is highly appreciated. Here is our reply to your comments. We hope the changes we implemented improve the shortcomings of the manuscript highlighted by your comments and suggestions. Please do not hesitate to contact us shall this not be the case for some comments.

Comments from Prof. Gabrielsen

Comment 1: This paper presents new data of great interest for the description and understanding of the pre-late Palaeozoic history of the Barents Sea. The problems addressed are well presented. The descriptions, findings and discussions are generally well formulated, and the English language is generally of reasonable standards. I agree in most of the conclusions drawn from the present data and the context in which the observations are discussed.

Comment 2: The manuscript still has several shortcomings when the architecture, structure phrasings are concerned. The manuscript lacks a little in maturity, in that the relation between the Introduction, Description and Discussion is not entirely well-balanced. The enclosed annotated manuscript contains detailed comments on the presentation.

Comment 3: Generalized major items that should be improved are: (1) The structure/architecture of the manuscript should be considered carefully. See comments below and in ms.

Comment 4: (2) Detailed descriptions are not complete on all points and a consequent use of terms in the structural geology description should be focused upon.

Comment 5: (3) There is a need for enhanced and more specific application of the principally important seismic facies concept.

Comment 6: (4) Information about real geometries (e.g. dips) is lacking on some essential points. Such information is needed in the interpretations of some structural elements.

Comment 7: (5) The discussion chapter takes up elements that are not fully constrained by present data, giving a slightly whimsical impression.

Comment 8: (6) Several statements need some explanation and clarification and some basic nomenclature used in an imprecise, ambiguous and sometimes even used in an incorrect manner.

Comment 9: (7) I think there is a need for a simplification and a full revision of the Conclusionssection (see below).

Comment 10: (8) Although the manuscript has an extensive reference list, some important (not only recent) references are lacking or have been misread. A thorough review of their reference list and the use of it by the authors would be required.

Comment 11: This paper would be an important contribution to understanding the early

history of the northern part of the Baltic shield in general and the Barents Sea in particular and the paper as such, deserves publication. It is therefore recommended that the authors are invited to re-submit the paper to Tektonika after improving its shortcomings.

Comment 12: Specific comments Most specific comments have been included as word-formatted comments in the enclosed annotated manuscript, and only general evaluations of each section s given here: Introduction The Introduction is well written and defines the scientific aims of the paper well, but the authors should consider whether the Introduction- and the Discussion-chapters are are completely I harmony. The section is still a little under-referred with respect to previous and related works from the area. Some of the missing references appear in the following section “Geological setting”, but here in a slightly different context. I think that the Introduction would benefit from restructuring to contain: a) An introduction in the chapter that focuses on the problems focused addressed in the Discussion, and an expansion of those structural items/periods of the structural history that are of particular importance. b) Inclusion of a couple of recent works of relevance that seems to be lacking.

Comment 13: Geological setting This part is generally well written in a clear language. Still, some references to works that are indeed relevant are lacking (see comments in annotated ms).

Comment 14: Unfortunately, this chapter seems slightly defocused when the central topics of the paper are concerned (see comments to the Introduction-chapter): It offers a summary of the entire structural geologic history of the Barents Sea, all of which I think is not equally relevant for the present paper. It makes the reader to wonder whether the “Geological setting” could be condensed and brought in more complete harmony with the main items addressed in the Discussion chapter. I think the paper already contains the bulk of this information, so it should be easy for the authors to make this rearrangement.

Comment 15: Methods This section should be expanded under the heading Data and methods.

Comment 16: This important section now lacks in information of reflection seismic data (not even names of survey(s) are given), and other technical information are also lacking.

Comment 17: I think the idea of defining and describing seismic facies (Figure 3) is fine, but it presently not fully utilized. In the Data- and Interpretation-chapter seismic facies now blends with the tectonic segment descriptions, making the Results-and interpretation-chapter little fuzzy. It seems to me that the descriptive part of the paper would gain in clarity if the seismic faciesconcept with definitions (FPx-y) were included in this chapter.

Comment 18: Other technical information like evaluation of dips of shear planes (these are time sections, and no depth-conversion or 1:1-dispalys have been attempted), so all angles shown in the figures are obviously unreal. The real dips of key structures like faults should be estimated and given in the Description-chapter Did I overlook the

mention of this problem in the structural analysis?

Comment 19: The Supplement (my copy did not contain figures) generally offers information on cores and some general information seismic characters and was of little help in the items mentioned above.

Comment 20: Results and interpretation I do endorse that this section start with an ingress/introduction text as already done. I think, however, that the ingress should be expanded, giving first a general outline of the Selis Ridge (including its position relative to other neighboring structural elements).

Comment 21: It is OK that the reflection seismic character of the ridge is included here but it could be more visible to the reader if it were transferred to the section “Data and methods”; see comment above).

Comment 22: I also endorse that the authors have chosen very strictly to separate between geometries in the descriptions of the seismic sections and the interpretations. This is a clean-cut strategy, but I think the authors are a little over-cautious in some cases e.g. in using terms like “Ushaped” reflections instead of “syncline”.

Comment 23: I also think the description chapter could be further improved by: a) More clearly explaining the approach in an introduction to the chapter b) include a short description the characteristics the ridge in general, c) explain the rationale for the structural subdivision of the ridge into segments, followed by a more systematic description of each segment d) Description of structural items like folds and faults like it is already done.

Comment 24: Interpretations I agree with the authors about their general interpretations, although I do not always agree in their technical descriptions (see annotated specific comments).

Comment 25: Discussion I think the main argumentation in the Discussion chapter from the study area sensu stricto is fair and I generally agree with the conclusions drawn.

Comment 26: I still find that some reference to some important works in the study area are lacking: *For example is only one of Breivik et al’s many works on the Caledonian grain/sutures in the Barents Sea referred to, and not much discussed, although their conclusion is at cross with the present paper.

Comment 27: *The “Finnmarkian” problem is treated rather whimsically, introducing “magmatic folding” (whatever that is) referring Krill & Zwaan (1987). To my knowledge the concept of “magmatic folds” (which I understand as folding due to flow in magmas or igneous bodies) was not introduced by these workers (or others) in connection with the “Finnmarkian” deformation. On the contrary, K&Z argued that the relative dating of the “Finnmarkian” as suggested by Sturt et al. was not valid and that dyke intrusion occurred pre-folding, rendering it to the main Caledonian deformation and not Timanian, nor “Finnmarkian”. This was elaborated upon by Roberts et al (2006), who demonstrated that the magmatism was indeed Timanian and the subsequent (tectonic) shortening/folding was mainly of Scandian age. Perhaps some re-reading by the

authors is necessary?

Comment 28: *Some recent papers of high relevance to the discussion are not included, but this can be due to that they were released in 2022 (after submission?)

Comment 29: Conclusions I agree with the main conclusions. I am still sorry to say that I have the feeling that the Conclusionchapter was written in a haste. First of all, I find that the first set of conclusions (conclusions #1-3) could be simplified and merged (see annotated comment in text).

Comment 30: Some of the conclusions following the major ones (beyond #8) are based on musings in the discussion and are not based on direct observations in the study area per se. I therefore think the number of items in Conclusion “chapter” should be simplified and shortened. Simultaneously, the authors could offer a sentence or two reminding the reader what is the rationale on which each conclusion was drawn.

Comment 31: Figures The paper caries an extensive figure list, mainly with seismic examples, so that the descriptions almost drown in examples. Bearing in mind that the Description chapter introduces a seismic facies list and three ridge segments displayed in figures 2, 4 and 5, one can wonder whether references to these figures in combination with the seismic facies characteristics in Figure 3 would make some of the magnifications (Figures 6-16) superfluous. I am sure this would improve the readability of the paper.

Comment 32: By the way, I am not charmed by the brutalistic style of the interpretations of many of these figures (mainly Figures 6-16): I think the color-line interpretations should be toned down, but I guess this style the choice of the authors and should be decided upon in a discussion with the technical editor of Tektonika.

Comment 33: Also see my comments above on the real geometries and dip-relations.

Comment 34: In spite of the extensive figure material, I miss an additional dedicated summary figure that supports the discussion and conclusion-chapters.

Comment 35: References The paper offers an impressive list of references. No complete control of the reference list has not been conducted by me: I have only picked arbitrary references for a full check and rather concentrated on references that I either have missed, or where I have seen references supporting statements that were a surprise to me.

Comment 36: In many cases I have found that the missing references were there, but sometimes referred other locations than where I had expected them to be. This is a matter of taste and reference strategy, I guess. However, I did find that some central references are at miss, and some cases where the (in my opinion) there is misunderstanding/misreading of the paper. I have noted those cases in the comments in the manuscript.

Comment 37: line 38: This is strictly not entirely correct.INfo from adjacent areas (Svalbard, Finnmark) alone, or in combination with potential field data has been been used to evaluate this. See e.g. several works of Fichler and coauthors

Comment 38: lines 49–50: You think about the Timanian orogeny? Please state. Furthermore, this is rather well established; e.g. Roberts & Siedlecka, Gee, Pease, Herrevold. For summary see Gabrielsen et al (2022) JGSL

Comment 39: line 50: Meaning what? Early Caledonian? “Finnmarkian”?

Comment 40: line 90: This is generally correct, but should be substantiated by references to dedicated works from the area, e. g. Herrevold et al. 2009, Gabrielsen e al 2022

Comment 41: line 106: “Timamian” Misprint

Comment 42: lines 110–111: What kind of data? Potential field? Please state

Comment 43: line 141: Rather: recorded as

Comment 44: line 142: metamorphism rocks

Comment 45: line 144: aged

Comment 46: lines 146–151: Actually Breivik et al in their works 8e.g. 2002, 2005) suggested that the Caledonides defined two separate branches, one striking N-S to Spitsbergen and the other trending NE-SW into the Barents Sea

Comment 47: line 152: This picture is somewhat more complicated. See works of Roberts, Townsend, Rice and summary in Gabrielsen metal. (2022). Some of these works are actually referred to already, but the content is not always taken fully into consideration.

Comment 48: line 155: Rather describe in a “top-to-contraction” context, thus using similar nomenclature as above.

Comment 49: line 178: Please specify: Is this figure reference to the present paper or in Indrevær et al. 2017

Comment 50: line 183: At this point of reading it s unclear wether or not his part of the history of the Barents Sea is relevant. Is it? I it is, please make an ingress text/introduction in the Geological Setting chapter and explain why. If not: Take out.

Comment 51: line 202: See previous comment

Comment 52: line 218: See previous comment

Comment 53: line 229: There seems to be no other description of the data base in the paper. A statement explaining which seismic surveys ere used, specification of parameters, processing, cut-of depth, which seismic attributes were applied (interpretation tool) and availability for the reader would have been useful.

Comment 54: lines 255–260: I think this ingress/intro should be expanded. See comment in evaluation

Comment 55: lines 268–269: is characterized by a partly chaotic signature

Comment 56: line 269: Please give a general description starting with the general outline before describing the complications. See comments in evaluation letter. I am

not sure that the reflection pattern is “partly chaotic”, but that the deep pattern in parts of the ridge rather shows (systematically) extended,folded and thrusted strata.—

Comment 57: line 285: end-member of these structures geometries

Comment 58: line 287: end-members

Comment 59: lines 314–315: in harmony with modeling by Fichler & Pastore (2022), who suggested the presence of large intermediate–mafic intrusions in the southern part of the Loppa High

Comment 60: line 316: meta-igneous

Comment 61: lines 318–319: . and by petrological modelling suggesting the presence of large intermediate–mafic intrusions in the southern part of the Loppa High (Fichler and Pastore, 2022).

Comment 62: lines 321–325: This is the type of information I think it would be beneficial to have in a presentation of the segments at an earlier stage (see comment in Letter to Editor)

Comment 63: line 328: Size of folds are usually given in wavelength and amplitude…

Comment 64: lines 330–335: Perhaps a more systematic fault classification with a systematic subdivisionntinto fold type nomenclature (as used would enhance clarity, e.g.: The following fold types were identified: a) Orthogonal folds with up-right axial planes with dimensions of….. b) etc

Comment 65: line 332: duplexes

Comment 66: line 336: Non-cylindical? Double-folds? Can any of these be classified as fault-propagation folds? See comment n Discussion

Comment 67: line 342: Packages with synclinal shapes? Comment 68: line 348: Synclinal?

Comment 69: lines 351–352: I think it is fair that the descriptions are separated from the interpretations of them. It would, however , been helpful that the authors include some information on an (expanded) introduction to the Results and Interpretations-chapter.

Comment 70: line 359: Rather: synforms Comment 71: line 360: Rather: antiforms

Comment 72: line 363: Qualitative statement on the physical outline on the election (that is an image) Please take out

Comment 73: line 372: Rather: limbs or flanks?

Comment 74: lines 378–379: Perhaps not necessary with two references to the same figure in two subsequent sentences?

Comment 75: lines 381–384: This sentence does not read good. Please rephrase.

Comment 76: line 387: Agree. I think the authors ca allow themselves to use “folds” instead of the wishy-washy “U-shaped reflections already in the Description.

Comment 77: lines 387–389: Agree again. See comment above, but is it taken into consideration that some might de dykes? That could give the same reflection type and erosional characteristics. Please discuss.

Comment 78: line 404: You probably mean brittle fault rocks

Comment 79: line 405: Cataclastic mylonite does not exist. Brittle deformation: rocks of the cataclaste series. Plastic deformation: rocks of the mylonite series

Comment 80: lines 407–408: Please use similar level of description for all fold families (xx-yytrend, top-zz displacement. IT would also be natural to state fold type/geometry, amplitude and wavelength here.

Comment 81: line 407: Have you considered whether these are fault-propagation-folds+ ? Perhaps you should.

Comment 82: line 414: Rather: Ductile fault Comment 83: lines 417–418: See comment above on fault-propagation-folds.

Comment 84: lines 431–432: I think a comparison with the thickness of shear zones/thrusts in the Caledonides and Tianides onshore Varanger would be an interesting point of comparison here.

Comment 85: line 444: Folding cannot be anticlinal. Folds consists of pairs or trains of anticlines and synclines. But in systems of fault-propagation fielding, anticlines can be stacked. Please rephrase, refer to and use basic nomenclature concept for such systems like e.g. Boyer& Elliot (1982)

Comment 86: line 448: flattens so that it becomes parallel to parallels

Comment 87: line 449: of the surfaces within

Comment 88: line 450: creating a décollement above the top of the basement. detaching them from underlying

Comment 89: line 451: At deeper levels Further down, the this reflection fault

Comment 90: line 452: Do these reflections occur on the top of or inside the bassinet sequence? Please state clearly

Comment 91: line 454: I agree that it is formally correct to use “synform” and “antiform”, but I guess here is a slim chance that these structures are not real synclines and anticlines here? (See also comment #25 above)

Comment 92: line 463: Please say at which stratigraphic level, compare to the stratigraphic level described in the present paper and comment

Comment 93: line 477: You mean thick-skinned?

Comment 94: lines 480–481: Here I think you jump very quickly to a conclusion

Comment 95: lines 485–487: Exactly (see e.g. Gabrielsen et al (1997). So there is (obviously) no genetic relation between the steep and shallow fold structures? I would agree to that, but please state explicitly. I see this is addressed in the Discussion, but this still be stated here with a reference to the Discussion.-chapter

Comment 96: line 491: Please state which stratigraphic level

Comment 97: lines 497–498: In other words: Double-folding (see comment above)

Comment 98: lines 516–517: See also double-folding described from onshore Varanger (Herrevold et al (2009), Gabrielsen et al. 2022)

Comment 99: line 521: I guess the recent contribution from Koglin et al (2022) is relevant here

Comment 100: line 538: Would it note be geologically logical to treat the Timanian deformation before the Caledonian?

Comment 101: line 559: This is of course correct for the Barents Sea, but interesting geochronological onshore data exists in e.g. Roberts,R.J. Corfuet al (2006) Comment 102: lines 567–570: Again: Please see Koglin et al (2022)

Comment 103: lines 633–634: I think the geometrical properties of the folds could be given more attention in the descriptive part of the paper (see my comments there)

Comment 104: line 637: W are dealing with un-depth-converted time sections here, so real dipangles are not easily evaluated. Please treat this problem thoroughly in the Description chapter and give some estimates there.

Comment 105: lines 689–721: I think the paper lacks an illustration that summarizes the important conclusion and the discussion in general

Comment 106: lines 690–697: See my comments above concerning Breivik et al's analysis of this problem, and please refer (whether you agree with Breivik, or not)

Comment 107: lines 751–795: I agree tat this is an important discussion for the Caledonian development and setting of the Varanger Peninsula, but this chapter has the flare of a less central afterthought in this paper. Is it strictly necessary?

Comment 108: lines 798–805: Are not conclusions 1-3 more or less similar? Could this be straightened up and simplified?

Comment 109: line 799: Perhaps "zone" is better?

Comment 110: lines 810–811: Do you imply with this that the Selis Ridge rocks is a suspect terrane derived outboard Baltica? What would be the implications of this?

Comment 111: line 816–817: Are you sure you want tome make this a main conclusion based on the present investigation?

Comment 112: lines 818–819: And this?

Authors' Responses

Comment 1: agreed.

Comment 2: agreed. See response to comment 12.

Comment 3: agreed. See response to comment 12.

Comment 4: agreed. see response to comments 22, 56, 63, 64, 66, 73, 76, 80, 81, 83, 85, 93, 95, 97, and 98.

Comment 5: see response to comment 17.

Comment 6: agreed. See response to comment 104.

Comment 7: agreed.

Comment 8: agreed. see response to comments 22, 56, 63, 64, 66, 73, 76, 80, 81, 83, 85, 93, 95, 97, and 98.

Comment 9: agreed. See response to comments 7, 27, 29, 30, and 104.

Comment 10: agreed. See also response to comment 40.

Comment 11: agreed. See response to all the other comments.

Comment 12: agreed. See also response to comments 7, 29, 40, 47, 85, 99, 102, 104, 111, and 112. However, the authors of the present manuscript do not feel that there is a need to repeat reference to works already extensively made in the geological setting chapter unless some of them need to be mentioned in a different context. The reviewer is welcome to specify whether this is the case for some references and, if so, for which references and in what context.

Comment 13: agreed.

Comment 14: disagreed. The purpose of the geological setting is not to be completely in harmony with the discussion (otherwise why naming it “geological setting” instead of “pre-discussion”?), but to give readers (especially those unfamiliar with the region) an overview of the tectonic history of the area. In doing so, the authors of the present manuscript are transparent about all possible explanations that were considered to explain the observed structures. This approach therefore reaches out to a larger readership and enables the readers to decide for themselves whether the conclusions drawn by the present manuscript are sound or not. The authors of the present manuscript remain open to discuss this issue further with the reviewer and/or the editor.

Comment 15: agreed.

Comment 16: agreed. However, since Lundin Energy Norway has merged with AkerBP Norway, it is taking unusually long time to locate the required data (survey names and list of all the attributes applied). We therefore hope that adding such information at a later stage is alright with the referee and the editor (either in the methods chapter in the manuscript or in the acknowledgement section).

Comment 17: the reviewer may specify what he means by “not fully utilized”. For now, the respective seismic facies correlate roughly with the northern and southern segments (SF1 and SF3), which consist of folded metasedimentary rocks, the southernmost and central segments (SF2), which consist of folded metaigneous rocks, and the boundaries of the southernmost–southern and central–northern segments (SF4), which are separated by major shear zones. The authors of the present manuscript may include such rough description in the “Data and methods” chapter. However, it is judged unnecessary to repeat the description of each seismic facies, which is already included in Figure 3 and a simple reference to Figure 3 was added to the chapter.

Comment 18: agreed. See response to comment 104.

Comment 19: agreed.

Comment 20: agreed.

Comment 21: agreed. See also response to comment 17.

Comment 22: disagreed. The authors of the present manuscript believe that it is best to be consistent with the “clean-cut” strategy of separating geometry from interpretation drastically throughout the manuscript not to include terms such as “syncline”. Not only is the term interpretative, but also speculative because there is no control on the age of the successions and folds may very well be overturned. Inclusion of terms such as “synform” and “antiform” could be considered as descriptive, though they still imply a fold-and-thrust belt setting.

Comment 23: agreed. See also response to comments 17 and 20.

Comment 24: the specific comments mentioned refer to replacing descriptive terms, e.g., “Ushaped” and “upwards-curving” by interpretative terms such as “synforms/synclinals” and “antiforms/anticlinals”. The authors of the present manuscript believe that these terms are inappropriate to a description section. See also response to comments 22, 56, 67 and 68.

Comment 25: agreed.

Comment 26: see response to comments 46 and 106.

Comment 27: agreed. See response to comment 7.

Comment 28: agreed. See response to comments 40, 47, 85, 99, and 102.

Comment 29: agreed. See response to comment 108.

Comment 30: agreed. See response to comments 29, 104, 111, and 112.

Comment 31: the authors of the present manuscript argue that figures 6–16 are necessary to visualize structure types in basement rocks of the Selis Ridge. Nevertheless, the authors of the present manuscript may be open to moving some of these figures to the supplements if judged more appropriate by both reviewers and the editor.

Comment 32: awaiting decision from the editor as suggested by the reviewer’s comment.

Comment 33: see response to comment 18.

Comment 34: agreed. See response to comment 105.

Comment 35: no change commanded by the reviewer’s comment.

Comment 36: agreed. Also see response to comments 7, 40, 47, 85, 99, and 102.

Comment 37: agreed.

Comment 38: agreed that this is was, thus far, well established for northern Finnmark, but not at all for the Barents Sea.

Comment 39: agreed.

Comment 40: agreed.

Comment 41: agreed.

Comment 42: agreed.

Comment 43: agreed.

Comment 44: agreed.

Comment 45: agreed.

Comment 46: agreed. However, the N–S-trending Caledonian branch they delineate is the Atomfjella Antiform in Svalbard, a major Caledonian anticline or antiformal thrust stack, not a suture at all (no high-grade metamorphism). This is even more true since the Billefjorden Fault Zone was downgraded from major terrane boundary to a simple (though large at human scale) tens of km long fault (Koehl and Allaart, 2021). The only possible suture outlined in Breivik et al. (2005) is therefore the NE–SW-trending branch through the Barents Sea.

Comment 47: agreed. However, unless the reviewer has specific suggestions on what should be added (including which references), the authors of the present manuscript believe that the level of detail proposed to include is irrelevant to the present manuscript.

Comment 48: disagreed, because the vergence of Caledonian thrusts in Svalbard is composite (topeast or top-west; e.g., Manby, 1986; Ohta et al., 1989; Witt-Nilsson et al., 1998; Birkenmajer, 2004).

Comment 49: agreed.

Comment 50: the present comment is puzzling because it targets only one section of the geological setting chapter. Why not also target the previous section about “late–post-Caledonian extensional collapse” as well? Or is the comment about writing an introduction paragraph to the geological setting chapter? If so, the authors of the present manuscript feel that it would only lengthen the manuscript without improving it significantly. The targeted section is relevant to the present manuscript to give readers that are not familiar with the tectonic history of the region an overview of what events occurred in the area. It also help the reader understand what possibilities were considered to explain the structures that are presented in the manuscript. As it turns out, only the late section of the geological setting chapter (“Mid–late Cenozoic extension”) is not directly relevant to the discussion chapter, but it is still important for readers not familiar with the area and short enough to remain in the manuscript.

Comment 51: see response to comment 50.

Comment 52: see response to comment 50.

Comment 53: agreed, although the availability of the data is specified in the “Data availability” chapter at the end of the manuscript.

Comment 54: agreed. See response to comment 20.

Comment 55: agreed.

Comment 56: agreed, a general description is needed and is already given. Adding terms such as “folded and thrusted strata” as suggested by the reviewer is interpretative and inappropriate to the present description section. In addition, the segment of the Selis Ridge presently described is indeed partly chaotic with poor lateral and vertical continuity of individual seismic reflections (see Figure 2 in the present manuscript).

Comment 57: agreed. However, “endmember” is a correct form of “end-member”.

Comment 58: see response to comment 57.

Comment 59: disagreed. Although the suggested change does not affect greatly the overall meaning, mentioning modeling by Fichler and Pastore (2022) before the results of exploration wells would imply that the authors of the present manuscript regard modeling (indirect argument) as a more reliable argument than direct correlation with rocks penetrated by exploration well. This is not the case. The other of the present manuscript would therefore prefer to leave the current order, in line with the inverted pyramid concept in journalism (the most important information must be first).

Comment 60: disagreed, metaigneous is a correct alternative to “meta-igneous” and is consistent with the non-hyphenation of “endmember”.

Comment 61: see response to comment 59.

Comment 62: agreed. However, this information cannot be just stated up front at the very beginning of the description chapter or in the ingress of the chapter as suggested by the reviewer. This interpretation is based on all the observations of large- and small-scale (from the dataset perspective/dataset scale) structures and features and needs to be properly argued for. The authors of the present manuscript feel that the description of small-scale features and structures contributing to this interpretation would not fit in the ingress paragraph of the result chapter.

Comment 63: agreed.

Comment 64: this is exactly what was attempted the in the present sentence. The authors of the present manuscript concede that the sentence readability is not optimal due to the extensive description of the fold types.

Comment 65: agreed.

Comment 66: agreed, the descriptive term “non-cylindrical” is lacking in the manuscript. However, the authors of the present manuscript are not confident in adding the term “double-folding” suggested by the reviewer to the present manuscript because it is not broadly used in the literature and might be confusing event though it seems to describe in a simple way dome- and trough-shaped folds produced by the interaction of two fold trends as those described in the present manuscript. In addition, although it was considered, the term “fault-propagation fold” is too imprecise/has a too broad meaning to be adequate in the present manuscript since many of the observed fold structures may qualify as fault-propagation folds (e.g., folds in antiformal thrust stacks, ramp

anticlines, and isoclinal recumbent folds). The authors of the present manuscript believe that adding this nonsegregating term would only lengthen the manuscript without improving the readability.

Comment 67: disagreed. The term suggested is interpretative and does not belong to the present section. See also response to comment 22.

Comment 68: see response to comments 22 and 67.

Comment 69: agreed. See also response to comments 20, 54, and 62.

Comment 70: agreed.

Comment 71: agreed.

Comment 72: agreed.

Comment 73: agreed, the term “flank” may be appropriate, but “limb” would be interpretative and assume the features to be folds. Regardless, the term “edge” currently used is still most appropriate since it is dealt with seismic reflections. Nevertheless, the authors of the present manuscript are open to update the manuscript consistently with the term “flank” should it be judged more appropriate by the reviewers and the editor.

Comment 74: agreed.

Comment 75: agreed.

Comment 76: disagreed. See also response to comments 22, 56, 67 and 68. However, this could be ultimately decided discussing with the two reviewers and the editor.

Comment 77: agreed. Good point by the reviewer, although the offsetting character and relationship of fold structures adjacent to the disruptions (upwards-convex/antiformal above and downward-convex/synformal below) are strongly supporting an interpretation as thrusts. Regardless, the potential presence of dykes would only strengthen the observed high reflectivity with added density contrasts.

Comment 78: agreed. The term “cataclased” is not correct.

Comment 79: agreed.

Comment 80: the present paragraph is an interpretation supported by the three types of fold structures in the area (see also line 346–351 and response to comment 64). The authors of the present manuscript concede that the fold types present in each fold-and-thrust system could be specified. However, this would greatly lengthen the present paragraph without providing much more insights than already provided. The suggested change would include a new sentence line 428: “The former system includes hundreds of meter- to kilometer-wide, north- and southverging, close to tight folds (Error! Reference source not found.) arranged into duplexes and/or antiformal thrust stack structures (Error! Reference source not found. and Error! Reference source not found.; Boyer and Elliott, 1982), (2) several kilometers wide, isoclinal, recumbent synclines and (ramp) anticlines (Error! Reference source not found.d–e), and (3) open upright folds (Error! Reference source not found., Error! Reference source not found.e, and

Error! Reference source not found.), and the latter system hundreds of meter- to kilometer-wide, westverging, close to tight folds (Error! Reference source not found.) arranged into duplexes and/or antiformal thrust stack structures (Error! Reference source not found. Error! Reference source not found.a and d; Boyer and Elliott, 1982), (2) several kilometers wide, isoclinal, recumbent synclines and (ramp) anticlines (Error! Reference source not found.10a and d), and (3) open upright folds in the northernmost tip and central, southern and southernmost segments (Error! Reference source not found.10 , 12b and d, and 16)". The sentence is a double repetition of lines 346–351 and the only relevant information one may extract from it is that the E–W-trending fold-and-thrust system shows north- and south-verging folds, whereas the N–Strending system shows west-verging folds. Therefore, such a level of detail may not be necessary in the present paragraph.

Comment 81: see response to comment 66.

Comment 82: agreed.

Comment 83: see response to comment 66.

Comment 84: agreed, and this is already partly done by referring to the work by Koehl et al. (2018a), which discusses a discrete shear zone of Caledonian (or Timanian according to Koehl, in prep.) shear zone in Finnmark and the southwestern Barent Sea. However, additional reference to the work by Johansen et al. (1994) and Gudlaugsson et al. (1998) on the Finnmark Platform should also be added in the present sentence. The reviewer may suggest specific references detailing the thickness of Caledonian and/or Timanian thrusts and shear zones onshore northern Norway?

Comment 85: agreed. However, since the present section corresponds to the description, it is premature to refer to relevant literature. Nevertheless, reference to appropriate literature should be made in the interpretation section.

Comment 86: agreed.

Comment 87: disagreed. The authors of the present manuscript prefer the current phrasing.

Comment 88: agreed.

Comment 89: agreed.

Comment 90: agreed.

Comment 91: agreed.

Comment 92: agreed.

Comment 93: no, the fold structures referred to in the present sentence are restricted to basement rocks.

Comment 94: if the reviewer is referring to the interpretation of basement reflections as "mylonitic fabrics", this was done in previous "interpretation" section of the results. However, agreed if the reviewer is referring to the exploitative character of the faults with basement shear zones.

Comment 95: agreed.

Comment 96: agreed.

Comment 97: agreed. See also response to comment 66.

Comment 98: there is no clear definition of “double-folding” in Gabrielsen et al. (2022) and no reference to the term in Herrevold et al. (2009).

Comment 99: agreed.

Comment 100: agreed. However, it is considered more appropriate here to begin “where there is light”, i.e., with common ground (Caledonian deformation well-known in the Barents Sea; e.g., Gernigon et al., 2014 among others). Segregating what is Caledonian first further helps the reader come to the same conclusion as the authors of the present manuscript: the sub-orthogonal WNW– ESE- to E–W-striking folds and thrusts must be Timanian.

Comment 101: agreed. However, the present paragraph deals with offshore areas in the Barents Sea. In addition, the suggested reference (Roberts et al., 2006) interprets intrusions related to the Seiland Igneous Province and coeval with the Timanian Orogeny as rift-related features and is therefore not appropriate to mention in the present sentence.

Comment 102: agreed.

Comment 103: see response to comments 22, 56, 63, 64, 66, 73, 76, 80, 81, 83, 85, 93, 95, 97, and 98.

Comment 104: agreed. This is an excellent comment by the reviewer. The authors of the present manuscript calculated to the dip angle range of the main shear zone based on seismic velocities from Gernigon et al. (2018) and found out that the angle is very similar to typical thrusts (31–46°). Modifications are therefore needed.

Comment 105: agreed.

Comment 106: Breivik et al.’s inferred location of the Caledonian suture in the central Barents Sea is already discussed and rejected in Koehl et al. (2022). See also response to comment 46. However, the authors of the present manuscript concede that it could be mentioned that the present findings further reject the conclusions by Breivik et al.

Comment 107: agreed. See response to comment 7.

Comment 108: agreed.

Comment 109: the large width of the fold-and-thrust belt in the Selis Ridge (40–50 km wide from north to south) suggest that the term “system” is appropriate.

Comment 110: no, simply that basement rocks of the Selis Ridge were already accreted to Baltica in the latest Neoproterozoic, just like the rest of the Barents Sea (Koehl et al., 2022), i.e., that they were already part of the same plate. In addition, these rocks were most likely not part of an exotic terrane prior to the Timanian Orogeny because of the probable relationship with the Rodinia rift margin of Baltica (see

discussion chapter lines 757–786).

Comment 111: agreed. See response to comment 7.

Comment 112: agreed. See also response to comment 7. 3.

Changes implemented

Comment 1: none commanded by the reviewer's comment.

Comment 2: see response to comment 12.

Comment 3: see response to comment 12.

Comment 4: see response to comments 22, 56, 63, 64, 66, 73, 76, 80, 81, 83, 85, 93, 95, 97, and 98.

Comment 5: see response to comment 17.

Comment 6: see response to comment 104.

Comment 7: deleted section lines 761–805. Also deleted lines116–132, 826–827, ", and the origin of the Seiland Igneous Province" lines 26–27, ", the origin of the Seiland Igneous Province" line 66, and Andersen and Sundvoll (1995), Bergström and Gee (1985), Draut and Clift (2001), Elvevold et a. (1994), Griffin et al. (2013), Kirkland et al. (2006), Krauskopf (1954), Krill and Zwaan (1987), Larsen et al. (2018), Nasuti et al. (2015), Rice et al. (2004), Roberts (1975), Roberts et al. (2006, 2010), Robins and Gardner (1975), Speedyman (1983), Stumpfl and Sturt (1964), Sturt and Ramsay (1965), Torsvik and Trench (1991), and Vasey et al. (2021) from the reference list. Added "and" line 26.

Comment 8: see response to comments 22, 56, 63, 64, 66, 73, 76, 80, 81, 83, 85, 93, 95, 97, and 98.

Comment 9: see response to comments 7, 27, 29, 30, and 104.

Comment 10: see also response to comment 40.

Comment 11: see response to all the other comments.

Comment 12: added "These works suggest that Precambrian structural trends exist in basement rocks of the Barents Sea, and that these Precambrian trends can be segregated from other trends and mapped in detail." lines 57–59, "the reworking of Timanian folds and thrusts by subsequent tectonic events," line 68. See also response to comments 7, 29, 40, 47, 85, 99, 102, 104, 111, and 112.

Comment 13: added reference to Herrevold et al. (2009) and Gabrielsen et al. (2022). See also response to comment 40.

Comment 14: none.

Comment 15: added "Data and" line 240.

Comment 17: added "in the Selis Ridge, which consists of four segments (southernmost, southern, central, and northern; Error! Reference source not found.) characterized by seismic reflections grouped into discrete seismic facies (SF; Error!

Reference source not found.). Notably, the southern and northern segments are dominated by reflections of SF1 and SF3, the southernmost and central segments by SF2 reflections, and the boundary between the southernmost and southern and the central and northern segments by SF4 reflections (Error! Reference source not found.)" lines 258–263.

Comment 18: see response to comment 104.

Comment 19: none commanded by the reviewer's comment.

Comment 20: added "is located in the western part of the Loppa High, where it defines an elongated N–S-trending ridge of uplifted pre-Devonian basement rocks. It is bounded from the Hammerfest Basin by the Asterias Fault Complex in the south, from the Polhem Sub-Platform by the Jason Fault Complex in the west, and from the Bjørnøya Basin by the Bjørnøyrenna Fault Complex in the northwest (Error! Reference source not found.b). The Selis Ridge" lines 267–271.

Comment 21: see response to comment 17.

Comment 22: none.

Comment 23: see response to comments 17 and 20.

Comment 24: see also response to comments 22, 56, 67 and 68.

Comment 25: none commanded by the reviewer's comment.

Comment 26: see response to comments 46 and 106.

Comment 27: see response to comment 7.

Comment 28: see response to comments 40, 47, 85, 99, and 102.

Comment 29: see response to comment 108.

Comment 30: deleted lines 833–834. Also see response to comments 29, 104, 111, and 112.

Comment 31: none yet.

Comment 32: awaiting decision from the editor as suggested by the reviewer's comment.

Comment 33: see response to comment 18.

Comment 34: see response to comment 105.

Comment 35: no change commanded by the reviewer's comment.

Comment 36: see response to comments 7, 40, 47, 85, 99, and 102.

Comment 37: added "tectonic" line 40.

Comment 38: added "Timanian" line 54.

Comment 39: deleted "paleo-" line 55.

Comment 40: added reference to Herrevold et al. (2009) and Gabrielsen et al. (2022)

line 97 and to the reference list, and to Gabrielsen et al. (2022) line 90.

Comment 41: corrected into “Timanian” line 111.

Comment 42: added “in the field” line 116.

Comment 43: replaced “preserved in” by “recorded as” line 146.

Comment 44: replaced “rocks” by “metamorphism” line 147.

Comment 45: deleted “-aged” line 149.

Comment 46: none.

Comment 47: replaced “a” by “an overall” line 157.

Comment 48: none.

Comment 49: moved reference to Figure 1b from line 188 to line 186.

Comment 50: none yet. Awaiting further clarification by the reviewer.

Comment 51: see response to comment 50.

Comment 52: see response to comment 50.

Comment 53: added “and the results of exploration wells 7220/6-1 and 7220/6-2 R” line 841. Also added “The seismic volume was cut at a depth of 3.5 seconds (TWT).” Lines 248–249.

Comment 54: see response to comment 20.

Comment 55: changed “becomes partly chaotic” into “is characterized by a partly chaotic signature” lines 284–285.

Comment 56: replaced “poorly continuous character” by “poor lateral and vertical continuity” line 286.

Comment 57: replaced “geometries” by “features” line 301.

Comment 58: see response to comment 57.

Comment 59: none.

Comment 60: none.

Comment 61: see response to comment 59.

Comment 62: none.

Comment 63: replaced “size” by “wavelength” line 343, and added “with amplitudes of a few tens to hundreds of milliseconds (TWT)” lines 344–345.

Comment 64: added numbers “(1)”, “(2)”, and “(3)” to separate each fold type lines 346–349 and make the sentence clearer.

Comment 65: corrected “duplex” into “duplexes” line 347.

Comment 66: added “non-cylindrical” line 352.

Comment 67: see also response to comment 22.

Comment 68: see response to comments 22 and 67.

Comment 69: see also response to comments 20, 54, and 62.

Comment 70: corrected “synformal folds” into “synforms” line 374.

Comment 71: corrected “antiformal folds” into “antiforms” line 375.

Comment 72: deleted “poorly developed,” line 378.

Comment 73: awaiting decision by the reviewers and the editor.

Comment 74: deleted “ (Figure 4)” line 394.

Comment 75: split the sentence lines 397–400 into two and rephrased as follows “An analogous is observed at the boundary of the southern and southernmost segments of the Seils ridge. There, a kilometer-wide package of steeply north-dipping, moderate-amplitude SF4 reflections abruptly truncates less steeply dipping moderate- to high-amplitude SF1 reflections in the north, including reflections part of the U-shaped SF3 package (Error! Reference source not found.).”.

Comment 76: none yet. See also response to comments 22, 56, 67 and 68. Awaiting further discussion with the two reviewers and the editor.

Comment 77: added “and high-amplitude” lines 404–405, “with possible fault-parallel dykes” lines 405–406, “(and possible fault-parallel dykes)” line 407, “and tilted dykes” and “respectively” line 412, “and by density contrast of igneous rocks (especially if mafic) with adjacent mylonite and metasedimentary rocks (e.g., Phillips et al., 2018; Koehl et al., 2022)” lines 414–415, “and magmatic intrusions” line 416, “; Phillips et al., 2018 their figure 1c” line 419, “(and possibly related intrusions)” line 434, “with possible shear-zone-parallel dykes” lines 447–448, “(and possible intrusions)” line 449, “(and possibly shear-zone-parallel intrusions)” lines 830–831, and Phillips et al. (2018) to the reference list. Also moved “along shear zones” from line 417 to 416 and replaced “and” by “, which” line 417.

Comment 78: changed “cataclased” into “cataclastic” line 424. Alternatively, the authors of the present manuscript are open to change “cataclastic” and “foliated” into “brittle” and “ductile” respectively, although the former terms are more specific.

Comment 79: added “, cataclasite,” and deleted “cartaclastic” line 425.

Comment 80: added “(with north- and south-verging folds)” line 427.

Comment 81: see response to comment 66.

Comment 82: added “ductile” line 434 and added “a” and “ductile” line 421.

Comment 83: see response to comment 66.

Comment 84: added “Johansen et al., 1994;” line 454 and “Gudlaugsson et al., 1998; “ line 455.

Comment 85: deleted “and anticlinal folding” line 466 and added “, which are folded into an anticline” line 467. Also added “are interpreted as fault-propagation folds (Suppe and Medwedeff, 1990), which suggest inversion of the faults. These folds” lines

506–507 and reference to Supper and Medwedeff (1990) to the reference list, and replaced ". This" by ", which" line 508.

Comment 86: replaced "parallels" by "flattens so that it becomes parallel to" line 471.

Comment 87: none.

Comment 88: replaced "detaching them from" by "creating a décollement above the top of" line 473.

Comment 89: replaced "Further down" by "At deeper levels" line 474.

Comment 90: added ", below the basin" line 474.

Comment 91: added "(probable syncline)" lines 476–477.

Comment 92: added "within post-Caledonian sedimentary successions" lines 486–487.
Comment 93: none.

Comment 94: added "merge" line 503 and "and" line 506, and moved "show an exploitative relationship (Phillips et al., 2016)" lines 505–506.

Comment 95: added "There seem to be no relationship between" line 508, and "and folds in basement rocks (see discussion chapter). The folds in post-Caledonian rocks" lines 509–510.

Comment 96: added "within basement rocks" line 518.

Comment 97: see also response to comment 66.

Comment 98: none.

Comment 99: added reference to Koglin et al. (2022) in the present sentence line 550 and to the reference list.

Comment 100: none.

Comment 101: added "offshore" line 586.

Comment 102: added reference to Koglin et al. (2022) line 597.

Comment 103: see response to comments 22, 56, 63, 64, 66, 73, 76, 80, 81, 83, 85, 93, 95, 97, and 98.

Comment 104: designed a new supplement 2 including the details of the calculation of the main NNE-dipping shear zone's dip angle. Deleted "potentially reworked preexisting Neoproterozoic rift basins and highs, and", line 21, ", Neoproterozoic rifting" lines 26–27 and 69, "and thrusts" line 666, "respectively" line 667, "Although angles are not preserved in time-scaled seismic data, E–W-striking Timanian thrusts and shear zones in the Selis Ridge still appear steeper than typical thrusts." lines 668–670, "and rotated into steeper dips. However, due to their sub-perpendicular orientation to E–W Caledonian contraction, tightening (i.e., rotation into steeper dips) should have occurred mostly along folded portions of the E–W Timanian folds and thrusts (i.e., Caledonian folding into N–S-trending structures and possible tightening along the same N–S-trending axis in the limbs of the N–S-trending Caledonian folds), whereas

portions of the folds and thrusts that retained their initial E–W to WNW–ESE trend and strike (i.e., within the hinge zone of N–Strending Caledonian folds) must have remained relatively unaffected by Caledonian contraction, which is not the case (Error! Reference source not found.). Other possible explanations may be that the Selis Ridge is located closer to the Timanian collision front and suture, and/or that the main shear zone and metamorphosed sedimentary rocks in the Selis Ridge initiated as a normal-faultbounded half-graben basin in the Neoproterozoic. Both hypotheses are discussed in subsequent sections." lines 671–681, and Bergh and Torske (1988), Bøe and Gautier (1978), Cawood and Pisarevsky (2017), Cawood et al. (2010), Gautier et al. (1987), Gayer and Rice (1989), Hartz and Torsvik (2002), Koehl et al. (2018b), Li et al. (1999, 2008), Nystuen et al. (2008), Siedlecka et al. (2004), Torsvik and Rehnström (2001). Added "(see Supplement 2 for depth conversion of the structure dip angle)" liens 383–384, ", and depth conversion suggests a gentle to moderate dip (31– 46°; Supplement 2)" lines 397–398, "Depth conversion suggests a moderate dip angle (41–46°; Supplement 2)." line 403, "This is further supported by the gentle to moderate dip angle of the structure based on depth conversion (31–46°; Supplement 2)." lines 451–452. Replaced ") and steeper (" by "; " and "a" by "d" line 667. Deleted section about potential Neoproterozoic basins lines 759–788. Deleted point 8 of the conclusion.

Comment 105: added a new summary figure (Figure 17) and reference to it in the text.

Comment 106: added "thus rejecting the inferred location of the Caledonian suture by Breivik et al. (2005) and" lines 727–728 and replaced "the Caledonian suture" by "it" line 728.

Comment 107: see response to comment 7.

Comment 108: reworked points 1, 2, and 3 of the conclusion into "The Selis Ridge is transected by a 40–50 km wide, several kilometers thick, E–W-trending, latest Neoproterozoic, Timanian fold-and-thrust system, including a NNE-dipping shear zone (and possibly shear-zone-parallel intrusions), which was mildly reworked into dome-shaped structures by a less-developed 10–20 km wide, N–S-trending system of west-verging Caledonian folds and thrusts in the early–mid Paleozoic.". Also updated the numbers of the conclusion points.

Comment 109: none.

Comment 110: none.

Comment 111: see response to comment 7.

Comment 112: deleted conclusion point 10. See also response to comment 7.

Authors' Reply to Reviewer 2

Dear Dr. Gernigon, thank you very much for your input on the manuscript, it is highly appreciated. Here is our reply to your comments. We hope the changes we implemented improve the shortcomings of the manuscript highlighted by your comments and suggestions. Please do not hesitate to contact us shall this not be the case for some comments. 1.

Comments from Dr. Gernigon

Comment 1: The study of Koehl et al. documents a new 3D seismic dataset of the Selis Ridge on the Barents Sea. Koehl et al. propose a seismic and structural interpretation of the survey and then discuss the tectonic and regional implications of the various trends and structures observed at the edge of the Loppa High.

Comment 2: The results of the 3D survey are interesting and the description of dominant E-W basement trend at the western edge of the Loppa High is definitively intriguing. However, I will be more cautious about the regional interpretation and extrapolation of the ambiguous E-W fold and shear system imaged at top basement level along the Selis ridge.

Comment 3: Koehl et al. suggest that the E-W trending folds may represent the prolongation of the Timanian structure preserved and well documented further west in the Kola-Kanin-Pechora area and in large part of the eastern Barents Sea. It is true that the Timanian structures are relatively well preserved in the eastern part of the Barents Sea up to the Varanger Peninsula. They can also be documented onshore, even if the datation of the "expected Timanian" deformation remains unclear.

Comment 4: However, west of the Middle Allochthon Front, the Caledonian deformation expected in most of the western Barents Sea and surrounding onshore area is extremely severe and is characterized by the presence of allochthonous nappes transported over hundreds of kilometers.

Comment 5: By correlation with onshore geology, the basement imaged at the level of the Selis ridge may be likely part of the Middle to Uppermost allochthonous rocks.

Comment 6: Back to Caledonian time, the Selis Ridge was probably located in the central part of the Caledonian orogenic system, with a suture possibly expected between Stappen and Loppa High. It is very surprising for me that such a severe tectonic and compressional regime simply develops mild NE-SW folds at the edge of the Loppa High. In such a context, why the Neoproterozoic sediments are so poorly deformed is difficult to explain. It needs to be explained.

Comment 7: Furthermore, before the onset of Caledonian, it won´t be surprising that the basement rocks or potential paleo-basins described at the edge of the paleo-Loppa High could have been located far-away and with a different orientation compared to their final position. I remind you that part of the rocks from the Upper Allochthon observed onshore further south have some affinities with Laurentia.

Comment 8: Comparing trends of parautochthonous terranes with other trends observed in allochthonous terranes might be problematic and tricky.

Comment 9: Many paleogeographic and paleomagnetic studies of Svalbard also concluded that East and West Svalbard were likely part of Laurentia with more affinities to paleo-Greenland. Therefore, a simple and direct NW-SE correlation with the Timanian trends of Baltica proposed by Koehl et al. remains controversial.

Comment 10: It is also important to remind that the 3D cube from Lundin is only a stamp in the regional picture of the entire Barents Sea. It cannot be excluded that the severe but local E-W trends are not Timanian but simply Caledonian in age.

Comment 11: Local stress reorganisation, lateral ramp of the staked nappes or tear fault could also explain local structural trends perpendicular to the main thrust orientation in orogenic system. The authors should also consider such alternative hypothesis more compatible with an extreme Caledonian deformation.

Comment 12: A quick comparison with the magnetic data (Gernigon et al., 2014) also shows that south of Neiden, we can clearly identify a NW-SE magnetic trend that divided the Loppa in two main blocks. This NW-SE magnetic trend is probably an old Precambrian contact/structure for us. Surprisingly, it is not fitting with any of the E-W and N-S seismic trends documented at top basement level in this study. It suggests that the basement geometry in depth is probably more complicated than expected. Old trend may also influence the geometry and the uppermost structures of the subsequent Caledonian nappes.

Comment 13: In the northern part of Selis Ridge, the seismic facies and the strong amplitude reflection may also reflect the presence of intrusions or mafic complex in depth. This may complicate the structural interpretation of the basement as well. The presence of old mafic material or younger intrusion in the Northern part of the Selis Ridge is not considered in this paper.

Comment 14: I also find the last paragraph of the discussion about the Seiland Igneous Ridge out of the topic considering the previous data description. This is not supported by any of the data presented in the paper. I suggest that the authors remove this part, which is apparently part of papers in preparation mentioned in this manuscript but not available for this review.

Comment 15: In conclusion, I do not reject the paper, but I suggest a major revision of the manuscript before publication. I would suggest that the authors stay focus on the new 3D data and improved their structural interpretation of the cube, especially in depth to get the most of it. The authors mentioned several attributes in the introduction but at the end, few of them are really documented in the paper. Additional time slices could be relevant to understand the full 3D geometry of the basement structure.

Comment 16: I would also suggest that the authors improve the figures quality. Orientation of the figures are often confusing. Figure 9 also appears to be a perspective view and it is sometimes difficult to distinguish the different features described in the paper. Coordinates and wells locations are missing.

Comment 17: I would finally recommend being more cautious about the regional interpretation and discussion of the various basement trends in the Barents Sea.

Interpretation of Timanian deformation at the edge of the Loppa High is too speculative.

Comment 18: line 12: new

Comment 19: line 28: new Comment 20: line 38: And Early Paleozoic as well

Comment 21: lines 46–47: Not really part of the "basement"

Comment 22: lines 58–61: Read like a conclusion sentence Comment 23: lines 99–101: Except that the location of East and West Svalbard was very different from the present day situation. This connection is far from being clear. Check also the paleomag and Precambrian reconstruction (Torsvik (2003), Lie et al., (2008)

Comment 24: lines 119–120: What do you mean by magmatic folds ??

Comment 25: lines 149–151: Once again, ,many paleogeographic reconstruction agree that West and East Svalbard were part of Laurentia !! Showing some affinity with NE Greenland or Pearya Terrane

Comment 26: line 166: The interpretation of extensional "core complex" is Svalbard is questioned (Dallmann and Piepjohn, 2020).

Comment 27: lines 210–214: Note however, that the opening of the NE Atlantic and the Senja Fracture Zone do not initiates in Late Cretaceous.

Comment 28: line 230: May be the reference of the cube?

Comment 29: lines 237–244: Yes but where are the related figures in this paper? I only see the Figure 9 with possibly (?) a spectral deconvolution)

Comment 30: line 312: Why igneous, could be metasediments as well if not contrast?

Comment 31: lines 319–322: Instead I would expect here a tectonic units severely intruded by mafic complex and other kind of sills. This may also explain the strong amplitude and some of the geometry observed. What about seismic velocities ?? The well only constrain the top few meters of the nappe.

Comment 32: lines 327–335: Some seismic feature could be the result of intrusions old or young

Comment 33: line 523: ..and up to a large part of the Bjarmeland Platform.

Comment 34: lines 536–538: TKFZ is mostly a strike slip feature interpreted to be Timanian. You also have WNW.ESE striking Timanian folds, but the timing is not so well constrained. Alternative explanations also exist.

Comment 35: lines 540–542: Once again, check the relative position of Northern Svalbard in Neoproterozoic time

Comment 36: lines 549–550: Not so clear on figure 9. Could you guide the reader

Comment 37: lines 551–553: Well, in Gernigon et al the North Loppa High Shear Zone was originally interpreted a lateral ramp initiated at the edge of the salient system proposed during the lateral escape of the Caledonian nappes north of Finnmark. Then it behaves as a transfer system during the Mesozoic rifting and Bjørnøya basin propagation.ww

Comment 38: lines 597–599: Possibly but you are very far form the main orogenic system

Comment 39: lines 603–605: Even Before See Gernigon et al. 2014. The Pohlem Platform likely initiated in Devonian accommodated by the large crustal detachment observed at the edge of Selis Ridge

Comment 40: lines 613–615: Well in figure 7 of Indrevær, theses minor folds are drapped and onlapped by Aptian to intra Cenomanian sediments. It can not really fits with and early Cenozoic phase of compression. It is older ! Globallly the compressions suggested by Indrevaer are mostly forced folding features if you have a closer look. With a bit of transtensional deformation to accommodate the Loppa high rotation at that period. No need to look for any enigmatic orogenic phase.

Comment 41: lines 625–628: I would like to thrust you butthis paper is not yet published

Comment 42: line 691–692

Comment 43: lines 711–713: It could be Caledonian deformation !

Comment 44: lines 719–729: This need to be better explained and documented

Comment 45: lines 731: At least NGU carried out rocks datation on some of these wells (confidential results)

Comment 46: lines 747–790: May be out of topic in this paper ? The new data do not cover this area.

Comment 47: line 819: Link not working 2.

Authors' reply

Comment 1: agreed.

Comment 2: disagreed. The regional correlation of Timanian thrust systems from the Russian Barents Sea to central Spitsbergen by Koehl et al. (2022a) is unambiguous and fully supports the interpretation of a Timanian age for E–W- to WNW–ESE-striking structures in the Selis Ridge as argued for in the present manuscript. The similarities of structures in the Selis Ridge and in the northern Barents Sea and Svalbard are simply to striking (dominant NNE-dipping geometries, reworking into north- to NNE-plunging Caledonian anticlines and synclines, mylonitic character of main thrusts and shear zones with associated south- to SW-verging folds).

Comment 3: agreed. The poorly constrained ages of Timanian structures on the Varanger Peninsula arise from their relatively low metamorphic grade because located near the southern termination (front thrust) of the Timanian Orogen (e.g., Dallmeyer and Reuter, 1989; Rice et al., 1989; Gorokhov et al., 2001). This is also true for adjacent areas onshore northwesternmost Russia (e.g., Sredni and Rybachi peninsulas; Rice and Roberts, 1995; Roberts et al., 1998). By contrast, Svalbard was located within the main area of deformation of the Timanian Orogeny and rocks there recorded much higher (amphibolite facies) metamorphic grades (Manecki et al., 1998; Majka et al., 2008) and preserve traces of Timanian-aged shearing and thrusting (Faehnrich et al., 2020).

Comment 4: despite the severity of Caledonian deformation in Caledonian nappes onshore

northern Scandinavia and despite their proximity to the Caledonian paleo-margin of Baltica, Archean–Paleoproterozoic basement rocks, such as those making up the West Troms Basement Complex (Zwaan, 1995; Bergh et al., 2010), were only very mildly deformed (if at all) and they still preserve a well-developed Svecofennian foliation, which was not overprinted by Caledonian deformation. It is therefore conceivable that basement rocks in other areas along the Caledonian paleo-margin were also only mildly reworked.

Comment 5: the present manuscript does discuss probable Caledonian deformation within basement rocks in the Loppa High, but the data presented do not allow correlation of the studied basement rocks to any specific Caledonian Allochthon onshore northern Norway. In addition, Caledonian allochthons and nappes are challenging to correlate from north to south, even onshore Norway (e.g., Corfu et al., 2014). Therefore, any onshore–offshore correlations may be premature.

Comment 6: metasedimentary rocks in the Selis Ridge are indeed intensely deformed. The fact that basement rocks in the Loppa High were little reworked during the Caledonian Orogeny may be explained either by (1) the presence of a décollement of which there are no evidence, or (2) by intense deformation of the basement rocks by an older tectonic event, during which unsuitably oriented (sub-orthogonal to the inferred Caledonian paleo-margin and sub-parallel to the main E– W-oriented Caledonian stress) structures and fabrics developed, making the basement rocks in the Selis Ridge difficult to further rework and deform, even during an intense tectonic event. The main reason why Archean–Paleoproterozoic basement rocks of the West Troms Basement Complex were only mildly overprinted by Caledonian deformation is still being investigated, but the existence of NW–SE-striking (i.e., also sub-parallel to the main, NW–SE-striking Caledonian stress along this part of the margin) Svecofennian fabrics and structures in this unit likely made them unsuitable/unlikely to further deform extensively. The authors of the present manuscript concede that this should be specified in the present manuscript.

Comment 7: agreed, rocks of the Loppa High could have been located elsewhere than near Baltica in the Paleozoic. However, the data published in Koehl et al. (2022a) and the present data suggest that crustal rocks constituting the whole Barents Sea and Svalbard were already accreted to Baltica in the latest Neoproterozoic. The mistake of the reviewer is, once again, a critical one because affinities of rocks of the Upper Allochthons to Laurentia do not preclude that these rocks were also located near Baltica too. See also response to comment 25.

Comment 8: from the presented evidence, the correlation between basement rocks in the Selis Ridge and the northern Barents Sea and Svalbard is clear (see also Koehl et al., 2022a). The authors of the present manuscript present evidence suggesting that basement rocks in the Selis Ridge show very similar structural trends and tectonic history as basement rocks in the northern Barents Sea and Svalbard (Timanian top-SSW fold and thrusting, and superimposed Caledonian N–S-striking structures) and, therefore, were most likely already attached/accreted to Baltica and the northern Barents Sea in the latest Neoproterozoic, i.e., that the Selis Ridge consists of parautochthonous rocks. Nevertheless, the authors of the present manuscript concede that this should be specified in the discussion section.

Comment 9: disagreed. See response to comments 7, 23, and 25.

Comment 10: disagreed. This is highly unlikely because of the regional correlation of Timanian thrust systems established from the Russian Barents Sea to central Svalbard (i.e., all three terranes of Svalbard) by Koehl et al. (2022a) and because of the late Neoproterozoic ages obtained by Faehnrich et al. (2020) for the onshore prolongation of one of the main Timanian thrust system in Svalbard (Vimsodden–Kosibapasset Shear Zone). These imply the presence of continuous Timanian structures at depth throughout the Barents Sea and all the way to western Svalbard. Thus, Timanian structures were there first and was then overprinted and reactivated by Caledonian deformation.

Comment 11: disagreed. The WNW–ESE Timanian trend in the Selis Ridge is no local or isolated phenomenon (see also response to comments 2, 7, 8, 10, 25, and 35), but is a prominent and pervasive structural trend throughout the Barents Sea, Svalbard, and northern Norway (e.g., Klitzke et al., 2019; Koehl, 2020; Koehl et al., 2022a). Geoscientists should consider the lack of N–S-striking Caledonian faults away from the western Spitsbergen–western Barents Sea plate margin and the unequivocal dominance of WNW–ESE-striking thrust systems throughout the Barents Sea (Koehl et al., 2022a) as evidence of the involvement of the whole Barents Sea– Svalbard region/plate in Timanian deformation and reworking of these structures during the Caledonian Orogeny mildly in the plate interior and intensely in the plate margin like in the Selis Ridge where they are folded into km scale anticlines (see figure 7 in Koehl et al., 2022a).

Comment 12: disagreed. If the reviewer is referring to the narrow, E–W-trending magnetic lineament near the center of the southern half of the Loppa High along the western edge of the high on the magnetic tilt-derivative data in Gernigon et al. (2014, their figure 4), the lineament coincides exactly with the location of the main shear zone shown in the present manuscript. We agree though that this should be mentioned in the present manuscript.

Comment 13: partly agreed. See also response to comment 31.

Comment 14: agreed.

Comment 15: agreed. However, since Lundin Energy Norway has merged with AkerBP Norway, it is taking unusually long time to locate the required data (survey names and list of all the attributes applied). We therefore hope that adding such information at a later stage is alright with the referee and the editor (either in the methods chapter in the manuscript or in the acknowledgement section). In addition, additional time slices are not needed since six time slices are already provided in Supplement S3.

Comment 16: agreed. See response to comment 47 regarding the link to high-resolution versions of the figures on the DataverseNO Open Access online repository.

Comment 17: disagreed. See also response to comments 2, 7, 8, 10, 25, and 35.

Comment 18: agreed.

Comment 19: agreed.

Comment 20: agreed.

Comment 21: agreed.

Comment 22: agreed.

Comment 23: disagreed. See also response to comment 25. Paleomagnetic data for the Ediacaran–early Cambrian are completely unreliable. For further information also follow the recently started ERC-Consolidator project of Dr. Mathew Domeier at the University of Oslo focusing specifically on this outstanding question. The evidence showed in Koehl et al. (2022a) should therefore prevail over paleomagnetic data for this time period.

Comment 24: the section was deleted. See response to comment 14.

Comment 25: disagreed. The regional study of Timanian thrust systems in the northern Barents Sea and eastern, central and southern Svalbard by Koehl et al. (2022a) unambiguously demonstrate that all three terranes of the Svalbard Archipelago were already accreted together and attached to the Barents Sea and to northern Baltica. These findings invalidate previous paleogeographic and paleomagnetic studies because (1) paleomagnetic data for the Ediacaran– early Cambrian are typically globally unreliable (e.g., Abrajevitch and van der Voo, 2010), and (2) paleogeographic reconstructions of Svalbard (e.g., Fortey and Cocks, 2003; Cocks and Torsvik, 2005), though rightfully claiming a proximity of northeastern Svalbard and northern Greenland based, e.g., on trilobite assemblages (Fortey and Cocks, 2003), these studies wrongly claim a long-distance separation of southern Svalbard and northern Greenland because of the differences of fossil assemblages. Based on this terrane separation principle, in a distant future, one would be able to claim a long-distance separation of Svalbard and Norway and a proximity of Svalbard and northern Greenland based on the presence of fossils of polar bears in Svalbard and Greenland and on their absence in Norway, which is obviously erroneous (Svalbard and Norway are actually part of the same tectonic plate, whereas Svalbard and Greenland are not) and forgets to consider the huge impact of climate belts on the regional presence of fauna and flora assemblages. While a proximity of northeastern Svalbard with Laurentia is highly probable, it is in no way incompatible with a proximity of northeastern Svalbard with Baltica as well. The continents at that time formed a large landmass (Gondwana) and most blocks of continental crust were highly likely to be located close to each other.

Comment 26: agreed. However, recent and ongoing works highlight the flaws of the studies and conclusions by Dr. Dallmann and Dr. Piepjohn's research groups and further support continued extension throughout the Devonian–Carboniferous since the Svalbardian contractional event is unlikely to have occurred in Svalbard (Koehl et al., 2022b and references therein; https://doi.org/10.5194/se-13-1353-2022). The authors of the present manuscript are open to add "A Late Devonian episode of contraction, the Svalbardian Orogeny was previously inferred (Vogt, 1928, 1941; Roberts, 1983; Torsvik et al., 1986; Piepjohn, 2000), but was later firmly rejected in Norway (e.g., Hossack, 1984; Norton, 1987; Chauvet and Séranne, 1994) and is now thought to be unlikely to have occurred in Svalbard either (Koehl, 2021; Koehl et al., 2022b and references therein)." lines 170–175. The authors would also add Vogt (1928, 1941), Roberts (1983), Torsvik et al. (1986), Piepjohn (2000), Hossack (1984), Norton (1987), Chauvet and Séranne (1994), Koehl (2021) and Koehl et al. (2022b) to the reference list. However, the authors of the present manuscript are aware that these additions would lengthens the manuscript and would

prefer to have the opinion of the editor and both reviewers on this. No trace of Svalbardian deformation was found in the present study and the event itself was firmly rejected in Norway and is, at the very least, strongly challenged and highly doubtful in Svalbard. It is therefore probably not relevant anymore to mention this event.

Comment 27: agreed.

Comment 28: agreed. See response to comment 15.

Comment 29: the data are still private and this information cannot be included.

Comment 30: disagreed. There is a high probability that several kilometers thick metasedimentary units would, at least in places, show major acoustic impedence contrasts due to lithological changes and the occurrence of bedding surfaces. The present interpretation is supported by the results of three exploration wells (7120/1-1, 7220/6-1, 7220/6-2 R), which penetrate basement rocks in the area and provide excellent tools to correlate seismic facies to rock types.

Comment 31: partly agreed in that the presence of metavolcanics within the metasedimentary units is probable, especially if the metasedimentary units were deposited during Neoproterozoic rifting of Rodinia. However, if the reviewer is thinking of post-Caledonian sills and dikes, the seismic data did not show any evidence for these.

Comment 32: in theory, this is entirely possible. However, the consistency of the structures described in the present manuscript with fold-and-thrust-belt-related structures strongly suggest otherwise. However, no such features were observed in the presented data (see also response to comment 31). The authors of the present manuscript are open to include such features in the present manuscript, pending that the reviewer is more specific as to which features he refers to on the data presented.

Comment 33: agreed.

Comment 34: disagreed. First, although the Trollfjorden–Komagelva Fault Zone in nothern Norway and the Vimsodden–Kosibapasset Shear Zone in Svalbard show indications for strikeslip movements, these are Timanian in age. The former is considered to be the Timanian thrust front and the latter yielded Timanian ages (Faehnrich et al., 2020). Second, these apparent strikeslip character probably reflect younger overprinting and reactivation by tectonic stresses oriented sub-parallel to Timanian faults (e.g., Caledonian Orogeny, post-Caledonian extension; e.g., Herrevold et al., 2009; Gabrielsen et al., 2022). Third, the Vimsodden–Kosibapasset is associated to Timanian-aged metamorphism (Manecki et al., 1998; Majka et al., 2008, 2012) and associated to a Timanian-aged major unconformity (e.g., Bjørnerud, 1990; Bjørnerud et al., 1991; Birkenmajer, 1991). Fourth, the Timanian-aged Vimsodden–Kosibapasset was recently correlated to a massive, several (to tens of) km thick, thousands of km long, 60 km wide, overall NNEdipping major thrust system, the Kinnhøgda–Daudbjørnpynten fault zone, which merges with Timanian-aged folds and thrusts in northwestern Russia (Koehl et al., 2022a). Fifth, Koehl et al. (2022a) present a reliable explanation for the apparent strike-slip character of Timanian faults near the Caledonian margin (their figures 7 and 8). Sixth, the timing of formation of the Trollfjorden–Komagelva Fault Zone is not well constrained because (1) it represents the front thrust of

the Timanian Orogeny and was associated with low- to very low-grade metamorphism, which is probablematic to date via geochronological analyses (Dallmeyer and Reuter, 1989; Roberts et al., 1998; Gorokhov et al., 2001), and (2) it was reworked by Caledonian tectonism. Alternative explanations may of course exist, but they are not the focus of the present manuscript, which focuses on the scenario considered most likely based on the analyzed data.

Comment 35: the study of Koehl et al. (2022a) shows that the Svalbard Archipelago (i.e., all three basement terranes of Svalbard) and the Barents Sea where accreted together and to northern Baltica in the latest Neoproterozoic during the Timanian Orogeny. These findings urge the geoscience community to reconsider the establishment of terranes solely on the basis of surficial data. This is further illustrated by a recent seismic study north of Svalbard demonstrating that the long-speculated terrane boundary between the northeastern and northwestern terranes of Svalbard, the Billefjorden Fault Zone, does not continue in northern Wijdefjorden and north of Svalbard (Koehl and Allaart, 2021). Moreover, the seismic data show that basement rocks of the so-called northeastern basement terrane continue across the fjord (i.e., across the terrane boundary), thus suggesting that both speculated terranes consist of the same rocks. Further constraints are therefore needed to segregate two lithospheric blocks from one another.

Comment 36: agreed. The figure was updated for clarity.

Comment 37: the bit about lateral escape of Caledonian nappes in Finnmark is not specified in Gernigon et al. (2014). However, it is specified that the shear zone/discontinuity may be responsible for the abortion of the Bjørnøya Basin. At the very least, this shear zone appears to be a major discontinuity (major thinning of the crust across the shear zone; Gernigon et al., 2014) in the crust and may reflect the influence of preexisting fabrics (e.g., Timanian fabrics) at depth.

Comment 38: agreed.

Comment 39: agreed. The Polhem Subplatform most likely formed in the Devonian. However, the present study does not focus on the deep Devonian detachment, but on a shallow portion of a fault complex (Bjørnøyrenna Fault Complex), which merges with the Devonian detachment at depth (and whose formation was controlled by the preexisting Devonian detachment?).

Comment 40: the Eurekan event is no enigmatic event and is well constrained timewise and structurally both in Svalbard (Dallmann et al., 1993 among others) and in the northwestern Barents Sea (Bergh and Grogan, 2003). By contrast, the event described by the reviewer and in Indrevær et al. (2016) is rather enigmatic. Observe the discrepancies between Indrevær et al. (2016)'s interpretation in the main seismic transect in their figure 7 and in the lower right corner inset, the former of which shows that a brittle fault is traced well into the Aptian–Cenomanian strata (i.e., that it formed during formed while or after these sediments were deposited (i.e., Cenomanian or post-Cenomanian). The overall high-angle geometry of the fault reminds that of a normal fault. The question is why this normal fault would bend this abruptly into so gentle a dip within Jurassic rocks. It is no secret that Jurassic rocks in the Barents Sea include oil-rich shales of the Hekkingen Formation, which

are prolific source rocks throughout the Barents Sea. Regarding the rheological properties of shales, it is common that they tend to absorb deformation, especially during contractional episodes. The low angle geometry of the fault precisely at Jurassic level suggests that the initial normal fault was partly and mildly reworked during (postCenomanian) minor contractional movements in the area, possibly during the formation of a partial, local décollement within the Jurassic shales. A probable candidate event is the Eurekan event, which is well constrained in nearby areas of the northwestern Barents Sea and Svalbard and which is responsible for the formation of numerous décollements within shale units in postCaledonian sedimentary successions in Svalbard (e.g., Maher, 1984; Maher et al., 1986, 1989; Andresen et al., 1988; Bergh and Andresen, 1990; Haremo et al., 1990; Haremo and Andresen, 1992; Andresen et al., 1992; Dallmann et al., 1993; Bergh et al., 1997; Tessensohn et al., 2001). The Eurekan event is therefore a much more likely trigger for the formation of the folds described in Indrevær et al. (2016) than a poorly constrained and enigmatic contractional event in the Cretaceous. The authors of the present manuscript are open to mention this in the present manuscript but are aware that this would further lengthen an already long manuscript. Alternatively, a reworked version of the present answer to the reviewer's comment might be included as a new supplement entitled "A reinterpretation of intra-Mesozoic fold structures in the southwestern Barents Sea mapped by Indrevær et al. (2016)" or similar. The author await further instructions from the editor and the reviewers on this matter.

Comment 41: disagreed. This paper was published in January 2022 (https://se.copernicus.org/articles/13/85/2022/).

Comment 42: see response to comment 41.

Comment 43: see response to comment 10.

Comment 44: agreed.

Comment 45: should the referee not be able to disclose the results of geochronological analyses on the exploration wells mentioned in the present sentence, the results of these geochronological analyses can therefore not be used to discuss the present manuscript's findings.

Comment 46: agreed. Also see response to comment 14.

Comment 47: agreed. The issue was pointed out to the authors of the present manuscript by the data repository (DataverseNO) in late July and is now working again. The authors of the present manuscript apologize for this inconvenience and welcome both reviewers to have a look at the high-resolution versions of the figures. 3.

Changes implemented

Comment 1: none commanded by the reviewer's comment.

Comment 2: none. Comment 3: none.

Comment 4: none commanded by the reviewer's comment.

Comment 5: none.

Comment 6: added "The mild reworking of basement rocks in the Selis Ridge during Caledonian contraction may be explained by the existence of unsuitably oriented structures and fabrics that stroke sub-parallel to the main, E–W-oriented Caledonian stress, making basement rocks in the area unlikely to be further intensely deformed." lines 573–577.

Comment 7: none.

Comment 8: added ", and that basement rocks in the Selis Ridge are parauchthonous of Baltica" lines 700–701.

Comment 9: see response to comments 7, 23, and 25.

Comment 10: none.

Comment 11: none. See also response to comments 2, 7, 8, 10, 25, and 35.

Comment 12: added ", which coincides with an E–W- to WNW–ESE-trending magnetic lineament (Gernigon et al., 2014)." lines 372–373.

Comment 13: see response to comment 31.

Comment 14: deleted section lines 761–805. Also deleted lines116–132, 826–827, ", and the origin of the Seiland Igneous Province" lines 26–27, ", the origin of the Seiland Igneous Province" line 66, and Andersen and Sundvoll (1995), Bergström and Gee (1985), Draut and Clift (2001), Elvevold et a. (1994), Griffin et al. (2013), Kirkland et al. (2006), Krauskopf (1954), Krill and Zwaan (1987), Larsen et al. (2018), Nasuti et al. (2015), Rice et al. (2004), Roberts (1975), Roberts et al. (2006, 2010), Robins and Gardner (1975), Speedyman (1983), Stumpfl and Sturt (1964), Sturt and Ramsay (1965), Torsvik and Trench (1991), and Vasey et al. (2021) from the reference list. Added "and" line 26.

Comment 15: none yet. Awaiting to get the information from Lundin/Aker BP.

Comment 16: added the location of the exploration wells to figure 9. See response to comment 47 regarding the link to high-resolution versions of the figures on the DataverseNO Open Access online repository.

Comment 17: see response to comments 2, 7, 8, 10, 25, and 35.

Comment 18: deleted "new" line 12.

Comment 19: deleted "new" line 28.

Comment 20: added "–early Paleozoic" line 40.

Comment 21: split the sentence line 48–51 into two and rephrased the second part into "In addition, oil discoveries in fractured–karstified Pennsylvanian–Permian carbonates over deformed basement rocks in the Loppa High in the Barents Sea (Matapour et al., 2019) suggest there might be a relationship between basement-seated structures and the distribution of overlying karst systems and hydrocarbon accumulations.".

Comment 22: the sentence was deleted.

Comment 23: none. See also response to comment 25.

Comment 24: the section was deleted. See also response to comment 14. Comment 25: none.

Comment 26: awaiting further instruction from the editor and reviewers.

Comment 27: added “, possibly starting” line 220.

Comment 28: see response to comment 15.

Comment 29: none.

Comment 30: none.

Comment 31: added “(possibly with metaigneous/metavolcanic)” lines 330–331 and 742–743, and “(+/- metaigneous/metavolcanic) line 744”.

Comment 32: none yet. Awaiting further specification from the reviewer.

Comment 33: added “and the Bjarmeland Platform” lines 525–526.

Comment 34: none.

Comment 35: none.

Comment 36: updated Figure 9.

Comment 37: none.

Comment 38: added “, which is consistent with the location of the study area some distance from the main Eurekan orogenic system in western Spitsbergen (Steel et al., 1985; Dallmann et al., 1993) and the northwestern Barents Sea (Bergh and Grogan, 2003)” lines 601–603.

Comment 39: none.

Comment 40: awaiting further instructions from the editor and the reviewers.

Comment 41: none.

Comment 42: none. See response to comment 41.

Comment 43: see response to comment 10.

Comment 44: added “(which possibly brought the shear zone into steeper dips; e.g., Koehl et al., 2022a their figure 7)” lines 740–741, deleted “(i.e., opposite dip) ” line 744, and added “i.e., tilted towards the shear zone instead of folded into major anticlines and tilted away; ” lines 746– 747.

Comment 45: none commanded by the reviewer’s comment.

Comment 46: see response to comment 14.

Comment 47: the link to the high-resolution versions of the figures is now working again.

2nd Round of Revisions

Decision Letter

Dear Jean-Baptiste Koehl and co-authors,

Thanks for re-submitting your manuscript to Tektonika. We appreciate your willingness to contribute to the early volumes of this community-led, peer-reviewed free-access journal.

We found that you have given a thorough response to the reviewers' comments and have made many of the required changes. However, we only see limited changes to improve the quality of figures that both reviewers suggested. In particular, we agree with Roy Gabrielsen that your paper is somewhat overloaded with seismic examples, making the manuscript long and difficult to follow. Please have another look to determine if all the figures (including the magnifications) are really needed to make your arguments.

Also in deference to Roy's comments, could you please try to generate the interpreted sections with less bold interpreted reflector lines, particularly the yellows – perhaps just adjust the transparency a bit

Once we have reached an acceptable accommodation on these last changes, we will be ready to go to the production stages. We therefore recommend you resubmit the manuscript after modifications, and we'll take it from there.

If you have any additional questions, please do not hesitate to contact us.

Hongdan Deng, Associate Editor,

Tony Dore, Executive Editor

16th, December 2022

NOTE: It was decided between the EE and AE that, in view of the very thorough response to the referees' comments, only a quick check by one of the referees (Gabrielsen) would suffice. Gabrielsen confirmed that the ms attended to most of the points raised and suggested only minor cosmetic changes to the second draft. Because of some problems with the AE accessing the online review system, the second review was conducted by email only.

Comments by Reviewer 1

I have gone through Koehl et al.'s revised manuscript.

The manuscript has been improved on most points as suggested by me in my first review. The reference list has been adequately updated and unclear phrasings corrected. A couple of points that I found speculative and less well founded according to previous literature have been adjusted/removed. Graphics in figures have been generally improved.

I am, however, still a little concerned about two of the figures: 1) Figure 6 a,b, still has a "brutalistic" style. Perhaps the heavy (red) faults could be toned down a little. 2) The full message in Figure 8 is difficult to read. Perhaps the color contrasts (background data) could be adjusted?

Assuming that these figures are adjusted, I recommend acceptance of this paper as it is.

All the best,

Roy

Authors' Reply to Reviewer 1

Dear Hongdan,

Thank you very much for your email.

We agree with the typo corrections (further corrected in one instance).

Figure 6b, c and d were updated so that the red lines show a similar thickness on all insets. Included is the updated high-resolution version of figure 6.

However, Figure 8 cannot be updated unfortunately since it shows the data themselves.

We hope that this is alright with you and the journal.

All the best,

Jean-Baptiste

Acceptance Letter

Dear Jean-Baptiste Koehl and co-authors,

Thanks for submitting the manuscript to Tektonika, a community-led Diamond Open Access journal publishing peer-reviewed research papers.

We are pleased to announce to you that your paper entitled "Timanian fold-and-thrust belt and Caledonian overprint in the Selis Ridge imaged by new 3D seismic attributes and spectral decomposition" has been preliminarily accepted.

Before the final acceptance, you will need to make some minor corrections of the manuscript:

- Figure 6a,b still has a brutalistic style. Perhaps the heavy (red) faults could be toned down a little;
- The full message in Figure 8 is difficult to read. Perhaps the color contrasts (background data) could be adjusted?
- We have pointed out some typo errors. Please double check and correct them.

Once finishing all the corrections please send us the revised materials to me for the production stage as soon as possible. We will still need to go through the manuscript again to double check all the details to make sure that they are correct and publishable.

Hongdan Deng, Associate Editor

Tony Doré, Executive Editor

Gwenn Peron-Pinvidic, Executive Editor

10th, January 2023