tektonika

Review Report

Causer et al., Cenozoic Relative Movements of Greenland and North America by Closure of the North Atlantic-Arctic Plate Circuit: The Labrador Sea, Davis Strait, Baffin Bay, and Eurekan Orogen
TEKTONIKA, 2025.

Table of Contents

1st Round of Revisions

Decision Letter

Dear Annabel Causer,

Thank you for submitting your manuscript to Tektonika and sorry for the delay of the review process. We have now received 3 reviews of your manuscript, which are attached to this email.

Based on the reviews, and our own evaluation of the manuscript, we have determined that this article requires substantial revision. While one review is mostly positive, the two others raise a number of critical concerns about the methodology and dataset used in your manuscript, the lack of explanation and discussion about the significant differences between your results and interpretations, and previous works. They also couldn't asses the validity of your uncertainty estimates.

Given these critical concerns, we have the regret to inform you that we are declining your submission to Tektonika. I am sorry that we cannot be more positive at this point, especially after the delays of the revision of your manuscript. However, note that we are still open to receive a revised version of your manuscript, if all these fundamental issues raised in the reviews are carefully addressed.

If you decide to resubmit your manuscript to Tektonika, please include a point-by-point response to all reviewer remarks, and a "tracked-changes" (changes highlighted or otherwise noted) version of the manuscript. This revision will be subject to re-review, possibly by the same reviewers.

Thank you for your interest in Tektonika.

Sincerely,

Maëlis Arnould, Associate Editor

Tektonika

Comments by Reviewer 1

Overall evaluation: In this manuscript, Causer et al. use plate circuits to resolve the opening of the Labrador Sea, Davis Strait, Baffin Bay, and eventual Eurekan orogenesis, using constraints outside those systems (magnetic isochrons and fracture zones), namely from North American – European plate kinematics and from Greenland – European plate kinematics, to jointly invert for rotation poles for North American – Greenland plate kinematics from 84 Ma to present. The results are then compared against geological and geophysical constraints from within those systems, as well as existing plate reconstruction models.

For full disclosure, I was already aware of this work both through having reviewed an earlier version of the manuscript for another journal several years ago and through having been an external examiner for Dr. Causer's doctoral defence. As such, I know that this work has already been heavily revised over some time. I feel that the target of investigation is compelling, the methodological approach used is sound, and the insights derived from the results contribute significantly to our understanding of the tectonic evolution of this region of northeastern Canada and Greenland. The manuscript is well written, appropriately illustrated, and comprehensively referenced. I do have a fair number of editorial comments, listed later in this document, but I feel overall that this work is appropriate for publication in Tektonika with only minor revision to address my points.

Most of my comments (numbered in later sections) relate to editorial changes. I do not have any issues with the science, the interpretations, and the implications of this work. It is a valuable contribution to our understanding.

Introduction:

1) At line 75, it is stated that "The crust marginwards of C27 in the Labrador Sea, and thus its history between breakup and 62 Ma, is less well understood." A few lines before, it states that breakup and seafloor spreading began as early as 62 Ma. As such, the quoted sentence does not make sense. It should say "its history prior to breakup at 62 Ma".

2) Regarding lines 79 to 83, the pre-C27 magnetic isochrons may well be due to production of magnetite during the serpentinization process and this is not mentioned. See Sibuet et al., 2007 in JGR.

3) Start of line 131, should be "are" not "is".

4) Line 145, "estimates" is misspelled as "esimates".

Data and methods:

1) In line 195, the word "of" is missing between "isochrons" and "the".

2) At line 261, what is the justification for the assumption that GEN corresponds to C25? A citation or explanation is needed.

3) Access to the supplementary material is very much appreciated.

Results:

1) In line 342, should be “that has caused” rather than “have”.

2) At line 345, it is stated that “The age of GEN in the model is thus indistinguishable from the timescale age of C25”. That is to be expected as it was one of the fundamental assumptions of the inversion, no?

Discussion and conclusion:

1) On line 421, it should be “the Iceland mantle plume” and not “Iceland plume mantle”.

2) On line 562, should be referring to Fig. 14 rather than Fig. 13.

Figures and tables:

1) In the caption for Figure 1, the Aegir Ridge is said to be abbreviated by “AR” but is “AER” in the actual figure.

2) In the caption for Figure 2, Ellesmere is misspelled in the N.EI. abbreviation explanation. The abbreviation listing at the end is also incorrect as UFZ is not adjacent to its definition, which should be Ungava Fracture Zone, not Ungave Fracture Zone.

3) In the caption for Figure 3a, the only abbreviation provided, GES, is not actually on the figure.

4) In the captions for Figures 4 and 6, it should say “from a previous model by Gaina et al.”. The word “by” is currently missing.

5) In the caption for Figure 7, should be “ellipses from the Eurasian” and not “form”. Final sentence should be “represent two rotation poles” not “represents”.

6) In the captions for Figures 9, 12 and 13, at the end, Chalmers is misspelled.

7) In the caption for Figure 14, should be “Eastern Canadian Arctic” not just “Eastern Canadian”.

Comments by Reviewer 2

This paper presents a study of the kinematics of North Atlantic for the last ca. 84 million years. The authors use published magnetic picks and own interpretation of selected fracture zone and continental margin structures for calculating rotations at selected time intervals. The adopted inversion method has been used in several other published papers with different datasets. The only novelty of this study is using the inversion method for deriving the kinematics of North Atlantic.

The resulting new set of rotations are used to discuss possible implications for the tectonics of the Northwest Atlantic-Ellesmere Islands..

Although this exercise may have the merit of trying an established method with a published dataset, and compare with results derived by using different methods, I note several shortcomings that makes this paper difficult to be published:

1. A considerable (and relevant to the objective of this study) amount of oceanic lithosphere in the studied region was formed at a triple junction and was the result of a 3 tectonic plates (simultaneous) motion. Solving for pairs of 2 plates motion and inferring the 3rd component of the triple junction is prone to introduce artifacts.
2. The information about fracture zone picking is insufficient, and judging from the figures presented, the picks used are not respecting basic kinematic rules (e.g. using conjugate segments for calculating the rotation). Besides, some interpretation seems to be partial or total erroneous (for example the Jan Mayen Fracture zone or the features interpreted as fracture zones in the Eurasia Basin). Because of this, some newly derived rotations could be seriously skewed.
3. The so-called GEN marker is not well explained (necking offshore SDRs? Could you include one or several interpreted seismic lines from this margin and illustrate the concept?).
4. The authors presents other published studies and then compare the results obtained in the present study with the published ones, without properly and critically describing the methods used by other studies. As mentioned in the introduction of this review, the main novelty of this study could be the use of an alternative kinematic technique, and its value can be only be determined by comparing it with other established techniques.

Besides, some of the selected published models are either identical or very similar (for example Torsvik et al., 2008 is using Gaina et al., 2002 rotations and that is mentioned in Table 1 of Torsvik et al., 2008). Published uncertainty ellipses should also be displayed in the relevant figures with model comparisons.

1. Model validation is very poorly presented. The synthetic flowlines should be shown together with a dataset that illustrates the location and structure of fracture zones (like the gravity anomaly or some relevant) derivative of it. The

authors discuss their model predictions versus several independent geological and geophysical findings – this has to be better documented, maps with seismic line locations or structural interpretation are needed for strengthening their arguments.

2. A lot of figures are poorly designed, symbols and lines difficult to see and references missing.
3. The Discussion and conclusion part include many poorly or unsubstantiated claims, some of the results (for example the presence of faults in the youngest part of the Labrador Sea oceanic lithosphere) do not belong here and previous literature severely omitted.

Few comments about figures below, the rest in the annotated document

Figure 1:

- please show geographic coordinates on the map
- define ALL abbreviations used on the map
- I can see symbols which are not explained
- please mention what is "this study" interpretation and what is taken from other studies (and which ones)

Figs 3, 4:

- Fracture zones interpretation has to be presented on top of the dataset used to derive this interpretation. The same with synthetic flowlines
- For Figure 4 -where is the Jan Mayen Fracture Zone (the most important fracture zone in the NE Atlantic)
- Fig4. The confidence ellipses can be hardly identified on this figure. For completeness, the uncertainty ellipses from Gaina et al., 2002 should be also shown

Comments by Reviewer 3

This study covers reconstructions of the Greenland / North America / Eurasia system during their divergence during the Cretaceous to present. The new reconstruction has some interesting implications if correct. There are also some sections about the implications for deformation at the northern margin of Greenland/Baffin Bay which are potentially interesting but also of secondary importance and currently underdeveloped and unclear, see comments below).

My main concerns in relation to the main argument of the paper relate to some details of how the reconstruction is built, and the quantification of uncertainty:

- If the uncertainty estimates are taken literally, then there seem to be stark contrasts between the new poles of rotation and those derived in previous studies. Put another way, the previous results fall far outside the 95% uncertainty ellipses of the new work (e.g. figure 4 and 6). It is not clear why this would be - presumably the magnetic picks are the same, so what is different?
- The new reconstruction relies in the interpretation of fracture zones in the study area which would seem to me quite difficult, ambiguous based on available data. The current work doesn't really say much about this or illustrate the data used to generate the FZ picks, which made me less convinced in the result than I may otherwise have been.

Substantiating these points are important for the whole study, since they govern how much confidence we place in anything that follows.

Comments by section:

Data and Methods

Uncertainty in fracture zone picks – the text quotes a value of 10-15 km for location uncertainty. I suspect this is more than reasonable in some areas (where the fracture zones are well defined), yet hard to justify in others (where, from my own review of the gravity anomalies, I find it hard to pick fracture zones at all).

Section on necking zone. There are rather a lot of studies already for the North Atlantic regions that take a more quantitative and robust approach to reconstructing syn-rift crustal extension. Here, the authors take a different approach which is apparently a qualitative proxy based on gravity anomalies? The rationale for this, while ignoring the more robust and quantitative work already done, is mysterious. Previous studies have quantified uncertainty in the quantified restoration of extension, and this can be >100 km – thus, no proxy could be indistinguishable within ~50 km. The disclaimer that this is true 'away from large prograded sediment bodies' thus becomes key – how do we quantify what is 'large'? A sediment thickness map for the considered here reveals large expanses of 'thick' sediment along the margins – what was done for these areas to account for this?

Are then any alternative hypotheses that could be considered? For example, is it possible that there is unrecognized deformation within Greenland, or elsewhere?

Results

Figure 4, it is hard to see confidence ellipses for some of the poles, are they plotted? Or are they so small that they are almost impossible to see? (if the latter, it would be helpful to state this). Is it surprising that confidence ellipses would be so small, and so much smaller for some chrons that others?

Compared to the size of the confidence ellipses, the magnitude of the difference between the new poles and those of Gaina et al (2002) seems huge – what do we attribute this to?

Same question applies to figure 6 – each pole from Gaina et al 2002 falls far outside the confidence ellipse of the new results. The text mentions that the new reconstructions show similarities with the previous work, yet based on the uncertainty ellipses the difference seems massive. I would be helpful to plot flowlines for the Gaina et al (2002) reconstruction in comparison to the new result.

Figure 8, I cannot see where SFZ, CFZ, JHFZ and MFZ are defined (they are not in figure 1)

Line 330: “The flowlines closely match the lengths and orientations of fracture zone segments preserved on both flanks of the extinct mid-ocean ridge”

I am confused. Some fracture zones are drawn on figure 8 (SFZ, MFZ?), and the new model seem to be incongruent with them. Maybe you only meant to refer to the pre-C26 section? The other reconstruction models in figure 9 seem to be more congruent to greater or lesser extents, perhaps the reason these reconstructions differ is because they sought to fit the fracture zones?

Discussion

Section 5.1: This is an interesting idea, however in the end I’m don’t find it hugely convincing based on the evidence so far presented. The new results hare are based solely on the results of magnetic anomaly interpretations that were already available, and fracture zone picks whose uncertainty would seem to be significant (but which isn’t explained/illustrated in detail).

Line 403: Perhaps I am misunderstanding, is the intention to imply that there was no plate boundary between Australia and Antarctica during almost the whole of the Cretaceous? There would seem to be abundant evidence to the contrary, presumably this is an unfortunate phrasing.

Line 464: the sentence refers to confidence ellipses for C13-aged points. I cannot see any ellipses, is this because they are too small at this scale? If so, please state in figure caption. Since this is an important point, maybe a zoom in panel would be useful.

Line 569: Confusing, my impression from the Gion et al work was that there starting point was a model based on seafloor spreading for the major plates as the ‘framing’ for the Eurekan orogeny. As was the case for Lawver et al (1990) before them. Either the work presented here is basically doing the same thing as those previous works (but with different motions for the major plates, hence different conclusions), or else you’re doing something different which isn’t clear to me. (Further, presumably Gion et al’s model is built on a specific reconstruction for NAM, GRN, EUR motions, it would be useful to state which for context.)

Line 599: Confusing, I have no idea what it means to be using ‘formal uncertainties of our model’s independent framework’ in the context of an area undergoing complex deformation. For the Nares Strait section I can see a certain logic in the comparison, given some assumptions which were clearly stated (and which may or may not be reasonable). By constrast, the meaning of the motion path (and related uncertainty ellipses) for the seed point within ‘AHI’ are not clear, and I am not sure how you can compare this to a reconstruction where presumably different domains have different defined kinematics (or, the kinematics is not modelled as rigid motions but a continuum)? If I understand correctly, the deforming model is based on shortening estimates and so that model would attempt to be consistent with those, but could be problematic if these estimates were incomplete or inaccurate, at least the general approach is clear. I am not sure what you are doing differently here, and where any differences arise from? Are you arguing that your model can resolve the independent motion of AHI? And other elements of the orogen? Independently? It is not clear how you can compare your new result (based on rigid plate motions) to a deforming model in a meaningful way.

Since the earlier parts of the paper argue for a spreading history different from previous models, it is essential to explain what spreading history underpins the Gion et al model – in the end, I think your argument is that if Gion et al had used a better reconstruction for the major plates framing the area then they would have found it easier to reconcile the shortening estimates within the zone of orogeny.

Figures

Something missing in the current figures is that lack of illustration of source data that underlie any of the results – primarily, gravity data to define fracture zones and magnetic data to define seafloor isochrons. The figures are heavily weighted towards interpretation products (mag picks, interpreted fracture zone paths) which do not allow for critical evaluation of how confident we should be in these interpretations in different areas. Having reviewed the gravity data across the study area myself, it seems that fracture zone interpretations are clear in some places but much more ambiguous in others – how this uncertainty has been identified and quantified needs to be addressed.

Several figures are indicated to contain confidence ellipses, but these are hard to see – too small to be visible at this scale?

Some abbreviations used in figures are not defined in captions, please check.

Authors' Reply to Reviewer 1

1) At line 75, it is stated that "The crust marginwards of C27 in the Labrador Sea, and thus its history between breakup and 62 Ma, is less well understood." A few lines before, it states that breakup and seafloor spreading began as early as 62 Ma. As such, the quoted sentence does not make sense. It should say "its history prior to breakup at 62 Ma".

Change adopted.

2) Regarding lines 79 to 83, the pre-C27 magnetic isochrons may well be due to production of magnetite during the serpentinization process and this is not mentioned. See Sibuet et al., 2007 in JGR.

Change adopted.

3) Start of line 131, should be "are" not "is".

Corrected.

4) Line 145, "estimates" is misspelled as "esimates".

Corrected.

5) In line 195, the word "of" is missing between "isochrons" and "the".

Corrected.

6) At line 261, what is the justification for the assumption that GEN corresponds to C25? A citation or explanation is needed.

We have added an explanation that notes the young end of C24 provides the GRN-EUR pair's oldest consensus picks, but that deep water and basins lie marginwards of those picks, and so are likely to be products of pre-C24 divergence for which the simplest dating assumption is to use next-oldest constrained reversal isochron in the timescale (C25).

7) Access to the supplementary material is very much appreciated.

No action necessary.

8) In line 342, should be “that has caused” rather than “have”.

Corrected.

9) At line 345, it is stated that “The age of GEN in the model is thus indistinguishable from the timescale age of C25”. That is to be expected as it was one of the fundamental assumptions of the inversion, no?

Within the GRN-EUR inversion, “GEN” is merely a label. We could assume that GEN dates from any other time than C25 without it affecting the shapes of the GRN-EUR flowlines in any way. The assumption first becomes important when we add the GRN-EUR rotations to the EUR-NAM ones in order to generate GRN-NAM rotations by summation.

10) On line 421, it should be “the Iceland mantle plume” and not “Iceland plume mantle”.

Corrected.

11) On line 562, should be referring to Fig. 14 rather than Fig. 13.

Corrected.

12) In the caption for Figure 1, the Aegir Ridge is said to be abbreviated by “AR” but is “AER” in the actual figure.

Corrected.

13) In the caption for Figure 2, Ellesmere is misspelled in the N.El. abbreviation

explanation. The abbreviation listing at the end is also incorrect as UFZ is not adjacent to its definition, which should be Ungava Fracture Zone, not Ungave Fracture Zone.

Corrected.

14) In the caption for Figure 3a, the only abbreviation provided, GES, is not actually on the figure.

Abbreviation removed from caption.

15) In the captions for Figures 4 and 6, it should say “from a previous model by Gaina et al.”. The word “by” is currently missing.

Captions rewritten.

16) In the caption for Figure 7, should be “ellipses from the Eurasian” and not “form”. Final sentence should be “represent two rotation poles” not “represents”.

Caption rewritten.

17) In the captions for Figures 9, 12 and 13, at the end, Chalmers is misspelled.

Captions rewritten.

18) In the caption for Figure 14, should be “Eastern Canadian Arctic” not just “Eastern Canadian”.

Figure replaced and caption rewritten.

Authors’ Reply to Reviewer 2

1. A considerable (and relevant to the objective of this study) amount of oceanic lithosphere in the studied region was formed at a triple junction and was the result of a 3 tectonic plates (simultaneous) motion. Solving for pairs of 2 plates motion and inferring the 3rd component of the triple junction is prone to introduce artifacts.

We agree that it is necessary to be aware of the sources of possible artefacts in any model’s results. We can reasonably expect reviewers to assume that we have taken reasonable steps to minimize the occurrence of such artefacts in designing our experiments, and have also added text and figures to demonstrate what these steps are, and where and why we think artefacts may remain.

2. The information about fracture zone picking is insufficient, and judging from the figures presented, the picks used are not respecting basic kinematic rules (e.g. using conjugate segments for calculating the rotation). Besides, some interpretation seems to be partial or total erroneous (for example the Jan Mayen Fracture zone or the features interpreted as fracture zones in the Eurasia Basin). Because of this, some newly derived rotations could be seriously skewed.

We have reviewed and revised our fracture zone data pick sets and included new supplementary figures to show and contextualize our interpretations more explicitly. It is important to us here to also note that <u>fracture zone segments are not conjugate features</u> and – despite a long list of studies that do so – should not be treated as such. To understand this, consider the zero-age points of two fracture zones that formed on a shared transform fault (i.e. at their ridge-transform intersections). If pairs of fracture zones really were to be considerable as conjugates, then these two points should coincide in space. This is clearly not the case. The observation that fracture zones do not have conjugates has been made multiple times in the literature, including in several of the papers we cite, and we have repeated it again in our revised text.

3. The so-called GEN marker is not well explained (necking offshore SDRs? Could you include one or several interpreted seismic lines from this margin and illustrate the concept?).

The original description clearly stated that the necking zones were picked from data landward of the SDRs (not offshore as in the reviewer comment). We have added new text to explain and justify our picks of GEN, also in light of reviewer 3’s comments about it. For now, we have decided not to show seismic data in view of the fact that these were not involved in any stage of picking our GEN markers.

4. The authors presents other published studies and then compare the results obtained in the present study with the published ones, without properly and critically describing the methods used by other studies. As mentioned in the introduction of this review, the main novelty of this study could be the use of an alternative kinematic technique, and its value can be only be determined by comparing it with other established techniques.

We have slightly expanded the background description of our method and its comparison to other methods so that they adequately serve the manuscript. A full treatment is given elsewhere, and we provide citations for it.

Besides, some of the selected published models are either identical or very similar (for example Torsvik et al., 2008 is using Gaina et al., 2002 rotations and that is mentioned in Table 1 of Torsvik et al., 2008).

Rather than for its EUR-NAM rotations, which indeed came from Gaina et al (2002), we cited the Torsvik et al (2008) model for its GRNNAM rotations, which attributes them to Gaina et al 2008 (in preparation). The “in preparation” paper appears never to have been published, which is why we retained the citation to Torsvik et al.

Published uncertainty ellipses should also be displayed in the relevant figures with model comparisons.

Published ellipses have been added to all relevant figures.

1. Model validation is very poorly presented. The synthetic flowlines should be shown together with a dataset that illustrates the location and structure of fracture zones (like the gravity anomaly or some relevant) derivative of it. The authors discuss their model predictions versus several independent geological and geophysical findings – this has to be better documented, maps with seismic line locations or structural interpretation are needed for strengthening their arguments.

We have altered the validation figures, changing the sizes and layering of the various picks, so that they are easier to see. In most cases, we have chosen not to show our flowlines over the gridded gravity data, because the models are run to fit picks generated from those data, not the data themselves. We agree, however, that the pathway from gridded data, to flowline picks, to model synthetic flowlines should have been more easily navigable in the original manuscript, and have worked to make this the case in the updated version by the addition of supplementary figures showing the gridded data in closer detail.

2. A lot of figures are poorly designed, symbols and lines difficult to see and references missing.

We have re-drafted all of the figures, replacing many of them completely, bearing this comment in mind whilst doing so.

3. The Discussion and conclusion part include many poorly or unsubstantiated claims, some of the results (for example the presence of faults in the youngest part of the Labrador Sea oceanic lithosphere) do not belong here and previous literature severely omitted.

Apart from the idea of faults in the Labrador Sea, there is no list of the “many poorly or unsubstantiated claims” or “severely omitted” literature for us to respond to. The burden of making such a list is not ours. For the youngest Labrador Sea, we have altered the text to make clearer that we do not claim to observe the presence of large oblique slip faults, but instead speculate on their presence in a setting without an active analogue. In our view, this approach is applicable for a paper’s discussion section.

Few comments about figures below, the rest in the annotated document

Figure 1:

- please show geographic coordinates on the map Coordinates added/made more visible.

- define ALL abbreviations used on the map All abbreviations now defined in caption.

- I can see symbols which are not explained Explanation added to caption

- please mention what is “this study” interpretation and what is taken from other studies (and which ones) Sources added.

Figs 3, 4:

- Fracture zones interpretation has to be presented on top of the dataset used to derive this interpretation. The same with synthetic flowlines. Done for fracture zones – see new supplementary figures. Not done for synthetics, see response above.

- For Figure 4 -where is the Jan Mayen Fracture Zone (the most important fracture zone in the NE Atlantic). The Jan Mayen Fracture Zone is shown in a different colour in the updated figure and is clearly identifiable by name, via the figure caption.

- Fig4. The confidence ellipses can be hardly identified on this figure. For completeness, the uncertainty ellipses from Gaina et al., 2002 should be also shown. More detailed comments are included in the attached annotated manuscript. In following up this comment, we discovered that the ellipses in figure 4 were plotted without their second dimension having been passed correctly. This made the ellipses appear very skinny. We have fixed this problem in the revised version. We have added the comparable ellipses from Gaina at al 2002.

(Annot on Fig 3c): I do not think there are fracture zones in Eurasia Basin. Gaina et al (2011) has definitely not interpreted this! Please add reference to the picks taken from other studies.

We revisited the Arctic gravity data grid and agree that many of the short fracture zones in our original model might be seen as having been too aggressively interpreted. We retained tectonic flowline features that emanate from first order segment boundaries along the Gakkel Ridge, near 5°E, 85°N and 64°E, 87°N. These linear features do not resemble classical fracture zone valleys all along their lengths, probably owing to a combination of the irregular distribution of shipboard gravity data the grid is built from and their formation in thin and discontinuous oceanic crust of the Eurasian basin, but their co-polarity with the fracture zones to the south in Fram Strait leaves little doubt that they formed at long lived stable ridge crest offsets. Hence, these features are less clearly imaged than their counterparts further south, but still suitable as constraints for modelling with our technique. In view of all this, we have resampled them at lower density and retain their use. A new supplementary figure highlights these features.

(Annot on Fig 8): What is the background image - free air gravity? Please give more information.

Yes, the image is free-air gravity, as described in the updated caption.

(Annot on Fig 8): Green ridge crest flowlines are hardly visible.

The green line is no longer necessary with our updated models.

(Annot on Fig 11): Could you refer to a Table? This is not clear!

We hope that the updated version of this figure and new table make this section clearer.

Authors' Reply to Reviewer 3

My main concerns in relation to the main argument of the paper relate to some details of how the reconstruction is built, and the quantification of uncertainty:

• If the uncertainty estimates are taken literally, then there seem to be stark contrasts between the new poles of rotation and those derived in previous studies. Put another way, the previous results fall far outside the 95% uncertainty ellipses of the new work (e.g. figure 4 and 6). It is not clear why this would be - presumably the magnetic picks are the same, so what is different?

The reviewer was right to question the confidence regions, as we have detailed in responses to previous comments. Despite this, the confidence ellipses in the corrected and updated version of the manuscript remain smaller than in previous studies. Although we indeed used largely the same magnetic isochron pick data as those studies, the reason for this is our technique's approach to using data from fracture zones, allowing them to deliver a 2 orders of magnitude increase in useful flowline constraints when compared to the Hellinger technique of, for example, Gaina et al's (2002; 2009) studies. The addition of these flowline picks increases the numbers of constraints of all kinds by over 20% in the EURNAM model and over 45% in the GRNEUR model. Given the independent and complementary nature of the added constraints and the more appropriate way they are treated in the new model, it should be clear that the new and previous inversions are very differently constrained, and that there should be no formal reason to expect their confidence regions to overlap.

• The new reconstruction relies in the interpretation of fracture zones in the study area which would seem to me quite difficult, ambiguous based on available data. The current work doesn't really say much about this or illustrate the data used to generate the FZ picks, which made me less convinced in the result than I may otherwise have been.

We have added several lines of new text and new supplementary figures to describe and show gravity anomalies and how we went about generating our flowline interpretations from them.

Substantiating these points are important for the whole study, since they govern how much confidence we place in anything that follows.

Comments by section:

Data and Methods

Uncertainty in fracture zone picks – the text quotes a value of 10-15 km for location uncertainty. I suspect this is more than reasonable in some areas (where the fracture zones are well defined), yet hard to justify in others (where, from my own review of the gravity

anomalies, I find it hard to pick fracture zones at all).

No action necessary regarding the uncertainty. We have completely revised our flowline pick sets so that they are derived from more conservative identifications that we suspect are likely to be closer to those of the reviewer's own assessment. We have added new text and figures describing how we went about doing so.

Section on necking zone. There are rather a lot of studies already for the North Atlantic regions that take a more quantitative and robust approach to reconstructing syn-rift crustal extension. Here, the authors take a different approach which is apparently a qualitative proxy based on gravity anomalies? The rationale for this, while ignoring the more robust and

quantitative work already done, is mysterious. Previous studies have quantified uncertainty in the quantified restoration of extension, and this can be >100 km – thus, no proxy could be

indistinguishable within ~50 km. The disclaimer that this is true 'away from large prograded sediment bodies' thus becomes key – how do we quantify what is 'large'? A sediment thickness map for the considered here reveals large expanses of 'thick' sediment along the

margins – what was done for these areas to account for this?

We are surprised that this comment seems to build from a statement that the range of results derived by retro-deformation of geophysically and interpretationally extraordinarily uncertain features – COBs – should be expected to define the picking uncertainty of signals of the more precisely defined and often clearly imaged necking zones. Regardless, we can agree with the reviewer that the locational uncertainty of either product is large, and we show in the revised manuscript that the spacing of our conjugate necking zone edge picks does not lie outside the uncertainty in the spacing implied by a 100 km uncertainty in the results of a recent retro-deformation model. Hence, our proxy picks can be regarded as statistically indistinguishable from the results of the reviewer's "quantitative and robust approach". Beyond this relatively neutral point, however, picking the necking zone edges in gravity data has numerous advantages, which have been listed elsewhere (Eagles et al, 2015; citation in the manuscript). Here, the chief among these advantages is that picking a proxy is independent of any *a priori* estimate of the earliest divergence azimuth, whereas a retro-deformation model is not. This retains the ability for us to generate a model of plate divergence from its observable products (i.e. gravity anomalies over necking zones) without explicit mathematical 'filtering' through any previous model.

Are then any alternative hypotheses that could be considered? For example, is it possible that there is unrecognized deformation within Greenland, or elsewhere?

We are keen to remain focussed on the hypotheses already identified.

Results

Figure 4, it is hard to see confidence ellipses for some of the poles, are they plotted? Or are they so small that they are almost impossible to see? (if the latter, it would be helpful to state this). Is it surprising that confidence ellipses would be so small, and so much smaller for some chrons that others?

The ellipses are indeed very small, although we have identified and fixed a scripting error that made them appear unduly thin in figure 4, which did not help. The revised text describes the ellipses more fully, and how the first order variation in their sizes is attributable to the geographical distribution of data used for the models.

Compared to the size of the confidence ellipses, the magnitude of the difference between the new poles and those of Gaina et al (2002) seems huge – what do we attribute this to?

As noted above and in the revised text, the difference is attributable to the better use and greater amenability of flowline constraints in and to our models.

Same question applies to figure 6 – each pole from Gaina et al 2002 falls far outside the confidence ellipse of the new results. The text mentions that the new reconstructions show similarities with the previous work, yet based on the uncertainty ellipses the difference seems massive. I would be helpful to plot flowlines for the Gaina et al (2002) reconstruction in comparison to the new result.

We have already described why we need not expect the new rotation poles to plot within the confidence ellipses of the older ones. We agree that flowline comparisons are helpful, and note that some had been present as insets in the first versions figures. For this version, we have increased their prominence by plotting them in new supplementary Figure S3.

Figure 8, I cannot see where SFZ, CFZ, JHFZ and MFZ are defined (they are not in figure 1)

CFZ and JHFZ are now more easily identifiable in the figure. We have removed references to SFZ and MFZ, which were not named in the text anyway.

Line 330: “The flowlines closely match the lengths and orientations of fracture zone segments preserved on both flanks of the extinct mid-ocean ridge”. I am confused. Some fracture zones are drawn on figure 8 (SFZ, MFZ?), and the new model seem to be incongruent with them. Maybe you only meant to refer to the pre-C26 section? The other reconstruction models in figure 9 seem to be more congruent to greater or lesser extents, perhaps the reason these reconstructions differ is because they sought to fit the fracture zones?

We indeed meant only to refer to the older seafloor, and have removed the figure’s younger FZ interpretations (which seem to have been based largely on Oakey and Chalmers’ magnetic isochron offsets and are difficult to reproduce in gridded free-air gravity). In the revised text, we note similarities between trends in the updated GRNNAM synthetic flowlines and short troughs, which may or may not be FZ-related, in the post-C25 seafloor of the Labrador Sea.

Discussion

Section 5.1: This is an interesting idea, however in the end I’m don’t find it hugely convincing based on the evidence so far presented. The new results hare are based solely on the results of magnetic anomaly interpretations that were already available, and fracture zone picks whose uncertainty would seem to be significant (but which isn’t explained/illustrated in detail).

We have remodelled this section extensively with the upshot that this comment no longer requires a specific response.

Line 403: Perhaps I am misunderstanding, is the intention to imply that there was no plate boundary between Australia and Antarctica during almost the whole of the Cretaceous? There would seem to be abundant evidence to the contrary, presumably this is an unfortunate phrasing.

See the comment above.

Line 464: the sentence refers to confidence ellipses for C13-aged points. I cannot see any ellipses, is this because they are too small at this scale? If so, please state in figure caption. Since this is an important point, maybe a zoom in panel would be useful.

The revised manuscript shows no C13-aged points. After refining the EURNAM and GRNEUR models it is now possible to show that rotations derived from the distributions of their C13 and younger data sets sum to produce GRNNAM rotations that are indistinguishable from null rotations at 95% confidence level.

Line 569: Confusing, my impression from the Gion et al work was that there starting point was a model based on seafloor spreading for the major plates as the 'framing' for the Eurekan orogeny. As was the case for Lawver et al (1990) before them. Either the work presented here is basically doing the same thing as those previous works (but with different motions for the major plates, hence different conclusions), or else you're doing something different which isn't clear to me. (Further, presumably Gion et al's model is built on a specific reconstruction for NAM, GRN, EUR motions, it would be useful to state which for context.)

We agree with the reviewer's impression of Gion et al's study. According to the paper's text, its framing used rotations that were ultimately derived from Oakey & Stevenson (2008) and Oakey & Chalmers (2013). The GPlates project control files, however, show that this was not the case, with rotations from "Gaina et al 2008" (i.e. the same apparently never-published paper cited by Torsvik et al 2008) and Gaina et al, (2002) heavily relied on instead. Within its Eurekan zone, the Gion et al model applied rotations that aimed to recapitulate published strain estimates of relative motions between orogenic blocks. They contended that the strain estimates sum, within their errors, to a relative plate motion history that closely approximates the major plate framing. Finally, in their GPlates hierarchical rotation 'tree', the residual misfit (i.e. of "rotations describing the sums of strain estimates" minus "framing rotations") is applied tacitly across Nares Strait, but in the text is attributed to tectonic shortening of northernmost Greenland for which no strain estimates exist.

Our approach is, in detail, a different one. Instead of using field based strain estimates, which are likely to be both individually and collectively incomplete as records of relative plate motions, our blocks are all constrained to move according to finite proportions of the framing rotations, which we take from our GRNNAM and EURNAM models and assume to be more complete based on their derivation from much larger data sets describing divergent plate motions. The set of these block rotations explicitly sums to the total framing; that is, no non-zero rotations are left undefined in the rotation tree. In the text, we then go on to compare local displacements calculated from the partial rotations to field based strain estimates, including but not limited to the set chosen by Gion et al for their relative block motions. We hope to have made all this clearer in the revised text.

Line 599: Confusing, I have no idea what it means to be using 'formal uncertainties of our model's independent framework' in the context of an area undergoing complex deformation. For the Nares Strait section I can see a certain logic in the comparison, given some assumptions which were clearly stated (and which may or may not be reasonable). By

constrast, the meaning of the motion path (and related uncertainty ellipses) for the seed point within 'AHI' are not clear, and I am not sure how you can compare this to a reconstruction where presumably different domains have different defined kinematics (or, the kinematics is not modelled as rigid motions but a continuum)? If I understand correctly, the deforming model is based on shortening estimates and so that model would attempt to be consistent with those, but could be problematic if these estimates were incomplete or inaccurate, at least the general approach is clear. I am not sure what you are doing differently here, and where any differences arise from? Are you arguing that your model can resolve the independent motion of AHI? And other elements of the orogen? Independently? It is not clear how you can compare your new result (based on rigid plate motions) to a deforming model in a meaningful way.

We have rewritten this part of the manuscript to attempt making our approach, and what might be expected to glean from it, clearer.

Since the earlier parts of the paper argue for a spreading history different from previous models, it is essential to explain what spreading history underpins the Gion et al model – in the end, I think your argument is that if Gion et al had used a better reconstruction for the major plates framing the area then they would have found it easier to reconcile the shortening estimates within the zone of orogeny.

See responses above.

Figures

Something missing in the current figures is that lack of illustration of source data that underlie any of the results – primarily, gravity data to define fracture zones and magnetic data to define seafloor isochrons. The figures are heavily weighted towards interpretation products (mag picks, interpreted fracture zone paths) which do not allow for critical evaluation of how confident we should be in these interpretations in different areas. Having reviewed the gravity data across the study area myself, it seems that fracture zone interpretations are clear in some places but much more ambiguous in others – how this uncertainty has been identified and quantified needs to be addressed.

New supplementary figures now show original data in the forms we gathered and used them (gravity anomaly grid maps, magnetic isochron picks).

Several figures are indicated to contain confidence ellipses, but these are hard to see – too small to be visible at this scale?

See previous responses.

Some abbreviations used in figures are not defined in captions, please check.

See previous responses.

2nd Round of Revisions

Decision Letter

Dear Annabel Causer, Graeme Eagles, Lucía perez-diaz, Jürgen Adam:

We have now received one review, made some ediorial commments and reached a decision regarding your submission to τεκτονικα, "Cenozoic relative movements of Greenland and North America by closure of the North Atlantic-Arctic plate circuit: The Labrador Sea, Davis Strait, Baffin Bay, and Eurekan Orogen".

Our decision is: Revisions Required.

You will find the review as well as some editorial comments on the manuscript attached with this email. We thank you for your patience during this reviewing stage and hope to receive an updated version of your manuscript as well as a point-by-point response to the reviewer's comments soon. Both should notably better address the uncertainty estimates and better acknowledge/answer criticisms of the basic methodology.

Best,

Maëlis Arnould,
Associate Editor, Tektonika DOAJ

Tony Doré,
Executive Editor, Tektonika DOAJ

Comments by Reviewer 3

This is a review of a previously considered submission

The authors have made substantial changes to the text and figures. The revised mansucript is improved and addresses most of the concerns to some extent. There are still some points that remain unclear, which I outline briefly below.

Regarding model evaluation - for example figures S3, it would help to show the fracture zone picks that are used. The flowlines do not exactly match the trend that gravity defines, at least maybe we expect closer, but I am not sure the picks that the algorithm see are matching the image? If you could plot these also, or perhaps illustrate the misfits, this would help. The revised text has a more clear explanation of the fitting method. However, despite it was already clear the potential upsides of fitting flowlines as well as magnetic picks, a reader may still confuse why uncertainty for a specific chron would be much smaller than for a Hellinger, the magnetic picks not changing. Is the misfit significantly different between these cases? Still remains the possibility that the uncertainty ellipses calculation is based on flawed assumptions regarding the characteristics of the input data, and the reason I focus on this point because it is quite fundamental to everything that follows. But the authors are at least describe what they have done to some level of detail.

Further on model evaluation - in the previous version I had some doubts for the flowlines and fracture zones for the Labrador Sea, and how well they fit. Now the new version illustrates these less than before (only figure 9, compared to figures 8 and 9 before). I found it useful to see the alternative models and interpreted fracture zone traces plotted on the maps for Labrador, I suggest to add them back. Also, the Labrador Sea flowlines from the current version appear visibly quite different from the previous version in the younger section, is this reflecting the difference in the fitting, treatment of fracture zone uncertainty?

Regarding figure 3 - I am confused on this point about the points labelled as 'GEN'. And the related text "Figure 3 also confirms, as cited above, that the spacing of the GEN picks is indistinguishable from that of the quantitatively restored pre-rift plate edge, within its estimated 100 km uncertainty (Barnett-Moore et al., 2016)." So my expectation is to see that the points labelled GEN would lie within the extent of the gold-coloured dots, yet most of them do not. How to explain this? (I don't think this is a major point, however I was already doubt the method to quantify the pre-rift configuration, the new figure seems to evidence these doubts are correct yet the text seems to contradict the figure).

Please check the plotting of the error ellipses, figure 11 - green ellipses are sometimes not elliptical in shape, is this correct?

Regarding the reconstruction of the deformation, the revision helps me to see what was done more clearly, I can understand now that the approach used is perhaps not so different from other studies compared to the original sense. The key lines are in the lines from 1530-1542, it is more precise now that the new model is also using structural data, the major difference is that the magnitudes of deformation is not used, only the kinematic sense? My impression from the original version was that the method was fundamentally different (but what exactly was not sure). Now it is mostly clear, and more similar to the traditional approach. The one sentence, "we examined the individual stages of the motion histories implied by our rotations in a search for similarities to published strain sense and azimuth estimates, and assigned them across the pertinent block boundaries where we found plausible matches" - this still sounds quite subjective, and sounds like someone else given the same data may or may not follow the same trajectory - can you explain the steps in a way that makes them sound more reproducible and scientific?

Editorial comments

- I am not sure why Figures 1 and 2 are called "supplementary."
- Figure S1 - Legend showing the Chron colours should be larger
- Figure 4 - legend needs to be larger. I cannot see the yellow great circle line and, at the resolution of my quite large screen, I cannot make out "small black triangles". Needs to be made clearer for publication.
- Figure 5 - make legend larger. There are more colours of confidence ellipses shown on the figure than indicated on the legend. Make colours consistent, and choose colours that show up (unlike the very faint brown/grey.)
- Figure 6 - again, increase size of legend. I wish the grey symbols for the new isochron picks were a bit more prominent, but I guess we can let this pass.
- Figure 7 - make the side geographical coordinates bigger, and label a couple of landmarks, so we know where we actually are in the world. Again, increase size of legend.
- Figure 8 -make legend larger, add a couple of landmarks (after all, we're in the southern hemisphere now).
- Table 4 - table entries in black, not grey, please.
- Figure 9 - Good, this is what a legend should look like! But the confidence ellipses should be bolder, because they are lost in the grey background.
- Figure 11 - good on both legend and visibility of ellipses. This is what the others (e.g. Fig. 9) should look like.
- Figure 12 - make ellipses bolder, like in Fig. 11.
- Table 5 - table entries in black, please.
- "Conclusion" should read "Conclusions."

See the comments directly within the version of the manuscript attached to this email.

Authors’ Reply to Reviewer 3

Regarding model evaluation - for example figures S3, it would help to show the fracture zone picks that are used. The flowlines do not exactly match the trend that gravity defines, at least maybe we expect closer, but I am not sure the picks that the algorithm see are matching the image? If you could plot these also, or perhaps illustrate the misfits, this would help.

The FZ picks are shown in figs 4 & 6 (“small triangles“). We note that one of the editor comments was that these picks are difficult to discern. The sizes of the symbols have been increased, and we have worked on figure 4 to produce a supplementary version, at A3 page size, in which the picks are more easily seen. The misfits can also be assessed visually in the figures 4, 6, and S4 by comparing the picks to the underlying blue synthetic flowlines.

The revised text has a more clear explanation of the fitting method. However, despite it was already clear the potential upsides of fitting flowlines as well as magnetic picks, a reader may still confuse why uncertainty for a specific chron would be much smaller than for a Hellinger, the magnetic picks not changing.

Further to our previous additions, we now add a citation to Shaw and Cande (1990) that allows readers to follow up the geometrical basis for understanding why it is that the reduction in uncertainty is a widely accepted and entirely foreseeable consequence of the order of magnitude greater number of fracture zone constraints that our approach starts with.

Is the misfit significantly different between these cases?

The published results of the earlier Hellinger inversions are not sufficient to support a detailed comparison of isochron misfit populations. This would require the full inversion architectures. We approached Carmen Gaina to request these materials for her published work, but she was unable to make them available to us. Instead, we note here that our inversions’ *a posteriori* uncertainty estimates for isochron misfits, which are based on the standard deviations of the misfit populations, lie well within the corresponding estimates from the older studies. This correspondence is consistent with the idea that the two techniques do similarly good jobs of fitting isochrons together. Where possible, visual comparison (e.g. compare our figure 4 to fig. 2 of Gaina et al 2002) backs this up.

Still remains the possibility that the uncertainty ellipses calculation is based on flawed assumptions regarding the characteristics of the input data, and the reason I focus on this point because it is quite fundamental to everything that follows. But the authors are at least describe what they have done to some level of detail.

The reviewer remains sceptical of the presented model uncertainties. We note that the backbone of our uncertainty calculation technique, as introduced in the cited 1990 paper by Chang and others, is the same as that used by the Hellinger-type predecessor models. Hence, if the assumptions are flawed, then the same flaws will also affect the estimated uncertainties in all previous models. This would not only be quite fundamental to everything that follows, as the reviewer notes, but also to everything that went before, and indeed to much of what has been built on it. In contrast, we are confident that the manuscript covers enough background material, with original text, figures, and multiple citations to papers dealing with both theoretical fundamentals and practical examples of the calculation of uncertainties in plate motion parameters from seafloor spreading data, for its readers to confidently rule out that its results are invalidated by the effects of flawed assumptions.

Further on model evaluation - in the previous version I had some doubts for the flowlines and fracture zones for the Labrador Sea, and how well they fit. Now the new version illustrates these less than before (only figure 9, compared to figures 8 and 9 before). I found it useful to see the alternative models and interpreted fracture zone traces plotted on the maps for Labrador, I suggest to add them back.

We disagree. It is enough that previous Labrador Sea interpretations appear in figure 1, but in later figures and the text of §4.3 it is clear from the satellite-derived gravity field that most of the magnetic anomaly offsets in the Labrador Sea are not related to the presence of fracture zones.

Also, the Labrador Sea flowlines from the current version appear visibly quite different from the previous version in the younger section, is this reflecting the difference in the fitting, treatment of fracture zone uncertainty?

The treatment of fracture zone uncertainty has not changed between the first and current versions of the manuscript. The differences in the Labrador Sea flowlines appear by virtue of the governing models in the updated manuscript having been rebuilt from scratch, including by the addition of multiple new isochron pick sets that were not used in the previous models.

Regarding figure 3 - I am confused on this point about the points labelled as ‘GEN’. And the related text “Figure 3 also confirms, as cited above, that the spacing of the GEN picks is indistinguishable from that of the quantitatively restored pre-rift plate edge, within its estimated 100 km uncertainty (Barnett-Moore et al., 2016).” So my expectation is to see that the points labelled GEN would lie within the extent of the gold-coloured dots, yet most of them do not. How to explain this? (I don’t think this is a major point, however I was already doubt the method to quantify the pre-rift configuration, the new figure seems to evidence these doubts are correct yet the text seems to contradict the figure).

It should be clear from the quoted sentence that it is not the locations of picks that are being compared, but their spacings. A visual inspection of the figure clearly makes it possible to conclude that the spacings of the GEN picks lies comfortably within the range of spacings implied by the green disks around the Barnett Moore et al products.

Please check the plotting of the error ellipses, figure 11 - green ellipses are sometimes not elliptical in shape, is this correct?

The odd shapes are an effect of the rotation pole's uncertainty region approaching and, in case of some of the green ellipses for C27, straddling the region at 90° distance from the test point. Crossing this 'equator' whilst at the same time sampling only the uncertainty region's perimeter causes the test points' ellipses to form a figure-8 shape. We are grateful to the reviewer for pointing this out, as we had not encountered this condition before. Evidently, our sampling of the uncertainty region's perimeter was insufficient. The problem is now fixed by sampling the interior as well.

Regarding the reconstruction of the deformation, the revision helps me to see what was done more clearly, I can understand now that the approach used is perhaps not so different from other studies compared to the original sense. The key lines are in the lines from 1530-1542, it is more precise now that the new model is also using structural data, the major difference is that the magnitudes of deformation is not used, only the kinematic sense?

This is correct.

My impression from the original version was that the method was fundamentally different (but what exactly was not sure). Now it is mostly clear, and more similar to the traditional approach. The one sentence, "we examined the individual stages of the motion histories implied by our rotations in a search for similarities to published strain sense and azimuth estimates, and assigned them across the pertinent block boundaries where we found plausible matches" - this still sounds quite subjective, and sounds like someone else given the same data may or may not follow the same trajectory - can you explain the steps in a way that makes them sound more reproducible and scientific?

We have altered the text so that our treatment cannot be understood as quite so subjective. To do this, we note that our model solution defines three distinct consecutive stages of plate kinematics, and that examples of strain histories consistent with each of the three are known from major faults and fault systems north of Greenland. We clearly state that the remaining subjectivity is primarily to be understood in regard to the choices/numbers of individual block-bounding faults, and the partitioning of model-derived motion onto them.

Authors' Reply to Editorial comments

- I am not sure why Figures 1 and 2 are called "supplementary."

We have moved supplementary figures to a separate document, making it easier to refer to them more specifically.

- Figure S1 - Legend showing the Chron colours should be larger

Done.

- Figure 4 - legend needs to be larger. I cannot see the yellow great circle line and, at the resolution of my quite large screen, I cannot make out "small black triangles". Needs to be made clearer for publication.

We increased the legend size and used brighter yellow for great circles. We felt it was important to show the summary of this very large model on a single figure in order to give a clear idea of the numbers of data involved. This, however, did make it hard to discern individual features. For readers interested in seeing those features more clearly, we have reproduced this figure in more detailed parts that we present as a new supplementary figure at A3 size.

- Figure 5 - make legend larger. There are more colours of confidence ellipses shown on the figure than indicated on the legend. Make colours consistent, and choose colours that show up (unlike the very faint brown/grey.)

We have altered the caption to clearly note that the colours used for our ellipses are the same as those used for the corresponding picks throughout the manuscript. We changed the figure to use stronger grey and brown colours for the ellipses of older studies.

- Figure 6 - again, increase size of legend. I wish the grey symbols for the new isochron picks were a bit more prominent, but I guess we can let this pass.

Legend enlarged.

- Figure 7 - make the side geographical coordinates bigger, and label a couple of landmarks, so we know where we actually are in the world. Again, increase size of legend.

Done

- Figure 8 -make legend larger, add a couple of landmarks (after all, we're in the southern hemisphere now).

Done

- Table 4 - table entries in black, not grey, please.

Done

- Figure 9 - Good, this is what a legend should look like! But the confidence ellipses should be bolder, because they are lost in the grey background.

Done

- Figure 11 - good on both legend and visibility of ellipses. This is what the others (e.g. Fig. 9) should look like.

Done

- Figure 12 - make ellipses bolder, like in Fig. 11.

Done

- Table 5 - table entries in black, please.

Done

- "Conclusion" should read "Conclusions."

Done

See the comments directly within the version of the manuscript attached to this email.

We have responded affirmatively to most of these comments. For details, see the replies to the comments in the “Track changes” version of the updated manuscript.

Acceptance Letter

Dear Annabel Causer, Graeme Eagles, Lucía perez-diaz, Jürgen Adam:

We thank you for addressing the comments raised on the previous version of your manuscript submitted to тектопіка, "Cenozoic relative movements of Greenland and North America by closure of the North Atlantic-Arctic plate circuit: The Labrador Sea, Davis Strait, Baffin Bay, and Eurekan Orogen". After the careful read of your updated manuscript and your response to the reviewers comments, we have reached the following decision: Accept Submission

We thank you for your patience during this process. You will shortly be contacted by the production team, regarding the publication of your manuscript.

Best regards,

Maëlis Arnould,

Associate Editor, Tektonika DOAJ