Review Report

Larson et al., Practical Assessment of Quartz Crystallographic Preferred Orientation Strength, TEKTONIKA, 2023.

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1st Round of Revisions

Decision Letter

Sent 15th March 2023

Dear Dr. Larson and co-authors,

We have now received three reviews on your manuscript, “Practical Assessment of Quartz Crystallographic Preferred Orientation Strength” submitted to Tektonika. The three reviews are all positive and we agree with them that your manuscript will make an important contribution to the discipline. However, the reviewers have raised some minor points which we agree should be addressed, and so we are recommending that your paper undergoes minor revisions.

Our decision is to: Request Revisions

We do not anticipate the need to send your paper for further review. We look forward to receiving a revised manuscript from you within 3 weeks, including a response to the points raised, changes to the revised manuscript highlighted and a clean copy of the manuscript. Reviewer 3 has used our review form to provide their comments, and we ask that when submitting revisions you use the review form to answer these comments. If this suggested timeline is difficult for you please let us know.

For your guidance, we append the reviewers' comments below.

Thank you for giving us the opportunity to consider your work.

Yours sincerely,

Dripta Dutta, PhD, Associate Editor - Tektonika

Janine Kavanagh, PhD, Executive Editor - Tektonika

Comments by Reviewer 1

Thank you for the opportunity to review this manuscript by Larson et al. This manuscript is an important contribution to the calculation of quartz c-axis strength, which is being increasingly used as a strain gauge in naturally deformed rocks. The accuracy of these methods, and the communication thereof, is of significant importance. Indeed, there are a number of limitations in calculating quartz c-axis strength, which have been poorly communicated hitherto. For these reasons, this manuscript is an important step in improving the 'quartz CPO strength' method and saving the geological community years of misinterpretations that were informed by flawed measurements. I also think that this manuscript serves as a very informative review of CPO calculation methods, and the references to previous work in this field of study is very comprehensive.

The modelling depicts the versatility of Jpf as a CPO strength measurement - this is demonstrated quite clearly across Figures 2, 4, 5, and 7. I found that these figures, and the outcomes, were clear and easy to follow.

The only criticism I have is that there could be a bit more discussion regarding the important of a quartz-rich natural samples, and the effects of pinning/interphase boundaries on quartz CPO. I think it could be beneficial to more clearly state in the manuscript that non-quartz phases (e.g. mica) can invariably affect the CPO strength and therefore quartz-rich rocks, ideally quartzite, should be prioritised for measurement. I do note the final paragraph in the manuscript does nod to these issues, but I think something a bit more firm may help the manuscript become even more informative and helpful. Possible refs to consider: Lister and Price, Tectonophysics (1978); Price, GSAB (1978); Evans et al, JSG (1980); Song and Ree, JSG (2007); Hunter et al, JSG (2019); and much of Marco Herwegh's work on second phases.

Following the suggestion above, I would recommend the authors consider switching the sections in the discussion so that the study limitations are presented last. This would allow for the final paragraph in the 'real world' section, regarding the effects of lithology and non-quartz phases, to be accommodated as part of the limitations.

Another small editing consideration: check the consistency of 'eigenvector' and 'eigen-vector' through the manuscript

Aside from these small criticisms, I think the work would make a great contribution for the journal. I recommend the Editor accept the publication following the minor revisions (or author responses) related to my suggestions.

Comments by Reviewer 2

The paper is well written, concise and scientifically sound. I have a few major comments on the purpouses of the paper and conclusions.

Please, explain in the introduction what would be the characteristics of a perfect quantification method (related to the characteristics of geological CPOs and deformation concepts), and explain why the previous quantification methods failed in the process. Then explain in the discussion/conclusion how your method fits the expected characteristics.

I suggest the authors to clearly specify in the introduciton that they are proposing a new quantification method for the analyses of CPos strength. I would avoid any discussion or reference to geological implications on the opening angle.

Comments from Associate Editor

Tektonika peer-review form

Section A: Overview of manuscript

A1) Overall evaluation, general comments & summary

A1.1) Reviewer's comments

A1.1.1) General evaluation and publication suggestion – Required:

Please use this space to describe, in your own words, the core subject of the submission and your overall assessment of its suitability for publication.

The manuscript titled 'Practical Assessment of Quartz Crystallographic Preferred Orientation Strength' by Larson et al. points out the possibility of erroneous interpretations that may arise depending on the parameter one chooses to quantify the ordering strength of the quartz c-axis plots. The authors have used computer/software generated artificial datasets as well as those obtained from naturally deformed samples to prove that, unlike the eigenvector-based CPO strength indices such as the cylindricity index (B), the Jpf magnitude is independent of the variations in the quartz c-axis opening angles. The manuscript is well-written and structured, with clear and relevant illustrations. Despite all the merits manuscript, there is some scope for improvement still. Two things that struck me were - why 'Introduction' and 'Background' are separate? And why 'Interpretations' were not merged with 'Discussions'? I have outlined my other concerns and suggestions in the subsequent sections of this review form. And therefore, I suggest **MINOR REVISIONS** for the manuscript before it can be accepted for final publication.

A1.1.2) What does the submission need to be publishable? (select as needed; comment for all cases)

- ☐ No changes required
- ☐ Rewriting
- ☐ Reorganising
- ☐ More data/figures
- ☐ Condensing
- ☐ Reinterpretation
- ☒ Other

Comments: The manuscript can be improved by elaborating on some of the statements I have pointed out below in this review form.

A1.1.3) Can the submission be improved by reducing/adding any of the following? (select as needed; comment for all cases)

- ☐ Text
- ☐ Table
- ☒ Figures
- ☐ Supplementary material

Comments: Please check my comments in Sec. B6.1 below.

A1.1.4) Please complete the following section if you recommend that the submission is NOT appropriate for publication (select as needed; comment if a box is selected)

- ☐ Quality is poor
- ☐ Research is not reproducible
- ☐ Other

Comments:

A1.2) Author(s) Responses:

A2) Summary of main merits and main points of improvement

A2.1) Reviewer's comments

Please describe below in a few sentences (100 to 300 words) the main merits of the submission and suggestions for improvements.

The main merits I have found are...

Please check my comments in Sec. A1.1.1 of this review form.

The main points of improvement I have found are...

Please check the section-wise comments below.

A2.2) Author's responses:

Section B: Detailed evaluation of manuscript

B1) Title and abstract

B1.1) Reviewer's comments

*These statements are a **guide** to what good* Titles *and* Abstracts include*. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Title* describes the main topic of the manuscript **accurately** — [YES]

The *Title* describes the main topic of the manuscript **succinctly** — [YES]

The *Title* includes **appropriate key terms** — [YES]

The *Abstract* includes a **clear aim and rationale** — [YES]

The *Abstract* supports the rationale with **sufficient background information** — [YES]

The *Abstract* includes a **well-balanced description of the methods** — can be improved

The *Abstract* describes the **main results sufficiently and adequately** — [NO]

The *Abstract* clearly describes the **importance/impact of the study** — [YES]

The *Abstract* clearly states the **conclusions of the study** — [YES]

The *Abstract* is **clear** and **well structured** — [YES]

Comments: Although the authors state that the study used artificially generated datasets, adding a line or two about the method used would help the readers. The

abstract lacks the potential limitations of the study the authors encountered while analysing CPO datasets from naturally deformed samples. They can elaborate on how the complications in natural deformation 'obfuscate the relationship' between CPO opening angle and Jpf values.

B1.2) Author's responses

B2) Introduction

B2.1) Reviewer's comments

*These statements are a **guide** to what good* Introductions *include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Introduction* provides **sufficient background and context** for the study — [YES]

The *Introduction* describes the **aim/hypothesis/rationale** clearly, providing **sufficient context** — [YES]

The *objective/hypothesis/rationale* **flows logically from the background** information — [YES]

The *Introduction* describes the study's **objective and approach** (last paragraph) — [YES]

The *Introduction* contains **relevant, suitable citations** — [YES]

The *Introduction* is **organized effectively** — [YES]

Comments: Line 32: 'require fewer assumptions'. Please state a few of them.

Lines 85-87: stating the 'limitations', maybe a few of them, would be a good idea

Line 100: It would also be better to state that higher B values signify less random CPO distributions.

The authors are also requested to add a line about the limitations of their study in the

last paragraph of the Introduction.

B2.2) Author's responses

B3) Data and methods

B3.1) Reviewer's comments

*These statements are a **guide** to what good* Method *sections include and good practices for* Dataset *accessibility. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Methods* are described **concisely and with enough detail** for reproducibility — [YES]

Necessary information about **data sources/acquisition/processing** is included — [YES]

Data used are accessible via either supplementary files or links in the data availability statement — [YES]

The *Dataset and/or Methods* are **organized effectively** — [YES]

Comments:

B3.2) Author's responses

B4) Results

B4.1) Reviewer's comments

*These statements are a **guide** to what good* Result sections *include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Results* findings are **supported by data** — [YES]

The *Results* findings are presented **clearly and succinctly** — [YES]

The text in the *Result* section **cites tables and figures appropriately** — [YES]

The *Results* directly **relate to the study objectives** — [YES]

The *Results* present **data for all the approaches** described in the *Methods* section — [YES]

The *Results* **text belongs to the Results section**, not to *Introduction*, *Methods*, or *Discussion.* — [YES]

The *Results* section is **organised effectively** — [YES]

Comments:

B4.2) Author's responses

B5) Discussion and conclusions

B5.1) Reviewer's comments

*These statements are a **guide** to what good* Discussions *and* Conclusions *include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Discussion* is **focused on the objectives** of the study — [YES]

The *Discussion* **addresses all major results** of this study, which are shown in *Results* — [YES]

The *Discussion* section makes **comparisons with other studies** that are relevant and informative — [YES]

The *Discussion* section properly identifies all **speculative statements** — [YES]

The *Discussion* section presents the **implications of the study** persuasively — can be improved

The *Discussion* section **highlights novel contributions** appropriately — [YES]

The *Discussion* section **addresses the limitations** of the study appropriately — can be improved

The *Discussion* section is **organised effectively** — [NO]

The *Conclusions* are **consistent** with and **summarise** the rest of the manuscript — can be improved

The *Conclusions* are **supported by the data** in *Results* and **follow logically** from the *Discussion* — [YES]

The *Conclusions* are **clear and concise** — [YES]

Comments: I could not understand why 'Interpretations' are separated from the 'Discussions'. Perhaps, it would be better to merge them. Moreover, it is unclear why the relationship between the opening angles and deformation thermometry is discussed (Lines 164-170). Do the authors intend to emphasize that Jpf values are particularly useful for quartz grains deformed at higher temperatures? If so, then I request them to state the same explicitly.

Lines 233-237: Please explain the unexpected behaviour observed for the Khimti Khola valley dataset. The influence of multi-stage deformation, particularly, the late-stage lower CPO opening angle on the Jpf value, which was unaffected by the opening angles as illustrated by the artificial data. Please elaborate this in detail.

Lines 238-244: If I correctly understood, this portion of the manuscript highlights that perhaps the importance of Jpf over other CPO strength indices is limited to monomineralic quartzites or quartz-rich samples only. If that is the case, then I request the authors to comment on the possible effects the various factors listed in lines 239 and 240, could have on the relevance of Jpf over others.

The limitations of the study must be moved to the end of the discussions. The authors may include the response to the comment above alongside the already-stated limitations.

B5.2) Author's responses

B6) Figures, tables and citations

B6.1) Reviewer's comments

*These statements are a **guide** to what good* Figures *and* Tables *include and how they are presented. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

Tables and *Figures* are **ordered logically** and **numbered sequentially** — [YES]

Tables and *Figures* have **captions that explain** all their major features — [YES]

Tables and *Figures* have **captions that complement** the information in the main text — [YES]

Tables and *Figures* present data that **relate** to the study objective — [YES]

Tables and *Figures* present data that are **consistent** with and support the description of results — [YES]

Tables and *Figures* have **succinct and informative titles** — [YES]

Figures are **accessible** (elements are clearly labelled, accessible colour palettes, colour contrasts, font size legible, etc.…) — [NO]
Please, check our [Figure guidelines]

Figures with **maps or cross-sections** contain all **elements to be understood** (north arrow orientation, scale, visible coordinates, sufficient coordinate grid intercepts) — NA

Figures with **maps** have **sufficient location information** (in the map or caption) — NA

Cross-sections have clear labels for **scale and coordinates** at ends and within-section kinks — NA

All georeferenced elements are provided in common format (.shp, .geotiff, .kml) [in an open-access repository] — NA

Citations throughout are relevant, suitable, and comprehensive — [YES]

Comments: Under the methods section, the authors have described the process of generating the artificial data in Lines 118-125. In my opinion, a step-by-step illustration, if possible, would be great for the readers.

The text inside Figures 1, 4, 5, 8, should be increased. The font size in Figure 3 is more legible. Also, the boxes (a,b,c,d) inside the plots in Figure 2 should be placed so that they do not overlay the curves.

Neither the text nor the symbols are legible in Figure 8.

B6.2) Author's responses

Section C: Additional comments

C1) Minor/line-numbered comments

C1.1) Reviewer's comments

C1.2) Author's responses

C2) Other remarks

C2.1) Reviewer's comments

C2.2) Author's responses

Authors' Reply to Reviewer 1

Thank you for the opportunity to review this manuscript by Larson et al. This manuscript is an important contribution to the calculation of quartz c-axis strength, which is being increasingly used as a strain gauge in naturally deformed rocks. The accuracy of these methods, and the communication thereof, is of significant importance. Indeed, there are a number of limitations in calculating quartz c-axis strength, which have been poorly communicated hitherto. For these reasons, this manuscript is an important step in improving the 'quartz CPO strength' method and saving the geological community years of misinterpretations that were informed by flawed measurements. I also think that this manuscript serves as a very informative review of CPO calculation methods, and the references to previous work in this field of study is very comprehensive.

We appreciate the supportive comments. Thank you.

The modelling depicts the versatility of Jpf as a CPO strength measurement - this is demonstrated quite clearly across Figures 2, 4, 5, and 7. I found that these figures, and the outcomes, were clear and easy to follow.

This is great to hear. We worked hard on the clarity of these diagrams.

The only criticism I have is that there could be a bit more discussion regarding the important of a quartz-rich natural samples, and the effects of pinning/interphase boundaries on quartz CPO. I think it could be beneficial to more clearly state in the manuscript that non-quartz phases (e.g. mica) can invariably affect the CPO strength and therefore quartz-rich rocks, ideally quartzite, should be prioritised for measurement. I do note the final paragraph in the manuscript does nod to these issues, but I think something a bit more firm may help the manuscript become even more informative and helpful. Possible refs to consider: Lister and Price, Tectonophysics (1978); Price, GSAB (1978); Evans et al, JSG (1980); Song and Ree, JSG (2007); Hunter et al, JSG (2019); and much of Marco Herwegh's work on second phases.

This suggestion is appreciated. We have updated that section of the text with citations as suggested:

Further complications in real-world datasets may be introduced by inter-sample variability. Changes in overall lithology, volume % quartz and other phases, the interconnected nature of quartz, finite strain, flow type and differential stress may all impact the development and strength of the CPO (Graziani et al., 2020; Herwegh et al., 2011; Kilian et al., 2011; Lister & Price, 1978; Lister & Williams, 1979).

Following the suggestion above, I would recommend the authors consider switching the sections in the discussion so that the study limitations are presented last. This would allow for the final paragraph in the 'real world' section, regarding the effects of lithology and non-quartz phases, to be accommodated as part of the limitations.

We investigated this suggestion but prefer to retain the original order of the manuscript. The reason for this is that the discussion about the limitations of the models is specific to the artificial distributions generated (i.e. the increase in J_{PF} for distributions where the girdles essentially overlapped). We view it as critical to discuss the limitations of the models before investigating real-world data. We think this helps avoid potential confounding factors that could be attributed to natural data, but are actually model limitations.

Another small editing consideration: check the consistency of 'eigenvector' and 'eigen-vector' through the manuscript

Thank you for catching this we have checked/modified all for consistency.

Aside from these small criticisms, I think the work would make a great contribution for the journal. I recommend the Editor accept the publication following the minor revisions (or author responses) related to my suggestions.

Authors' Reply to Reviewer 2

The paper is well written, concise and scientifically sound. I have a few major comments on the purpouses of the paper and conclusions.

Please, explain in the introduction what would be the characteristics of a perfect quantification method (related to the characteristics of geological CPOs and deformation concepts), and explain why the previous quantification methods failed in the process. Then explain in the discussion/conclusion how your method fits the expected characteristics.

We are not proposing a new quantification system in this paper. We are presenting a systematic review of existing quantification methods. In lines 50 through 53 of the original work, we specifically cite previous work that has use the I2 norm to assess CPO density and cite the work that originally referred to the measure as Jpf:

"An alternative method to quantify the strength of a sample of directions, such as in a pole figure, is to calculate the l^2- norm (Euclidean norm) of an estimated probability density function calculated for the spherical distribution of the orientation data (Botev et al., 2010; Kilian & Heilbronner, 2017; Mainprice et al., 2015). Following Mainprice et al. (2015), we refer to this measure as J_{PF}."

I suggest the authors to clearly specify in the introduciton that they are proposing a new quantification method for the analyses of CPos strength. I would avoid any discussion or reference to geological implications on the opening angle.

Please see comment above. We are not proposing a new quantification system. In the introduction (as cited in the previous response) we cite past work that has used the same metric and past work that has named the metric we employ.

Authors' Reply to Associate Editor

Comment: Although the authors state that the study used artificially generated datasets, adding a line or two about the method used would help the readers. The abstract lacks the potential limitations of the study the authors encountered while analysing CPO datasets from naturally deformed samples. They can elaborate on how the complications in natural deformation 'obfuscate the relationship' between CPO opening angle and Jpf values.

Response: We understand what the reviewer is suggesting, but feel it is critical that the reader reads the methodology section to understand the method employed to create the artificial dataset.

We have modifed the end of the abstract to address the reviewer's comments on the complications related to natural deformation. The text is quoted below with new text underlined:

"The direct correlations between pole figure *c*-axis opening angles and ordering strength noted in the artificial distributions are also demonstrated in the evaluation of real-world data, though significant complications, perhaps related to the occurrence of non-quartz phases, variation in flow type and/or critically resolved stress, in the natural specimens can partially obfuscate the relationship."

Comment:

Line 32: 'require fewer assumptions'. Please state a few of them.

Lines 85-87: stating the 'limitations', maybe a few of them, would be a good idea

Line 100: It would also be better to state that higher B values signify less random CPO distributions.

The authors are also requested to add a line about the limitations of their study in the last paragraph of the Introduction.

Response:

Line 32: Added '(such as undeformed geometry)' after "…fewer assumptions"

Lines 85-87: The limitations are discussed in 51 through 57 of the original manuscript – before this statement is made. The reader, therefore, will already have been introduced to the limitations. As such, we have added '(as discussed above)' after '…the limitations of eigenvalue-derived plots…'

Line 100: The reviewer's suggestions is synonymous with the existing text (underlined for emphasis):

"Vollmer further defined a fourth index, cylindricity (B), the sum of P and G (equivalent to 1-R), which informs the ordering of the distribution independent of geometry (Vollmer, 1990)"

As such, we have not changed the text.

We appreciate the reviewer's suggestion to add a line about the limitations of the study at the end of the Introduction, however, it would be without context. We need to explain the methodology of the test and the results to discuss the potential limitations, which is why we do this in a dedicated section of the discussion.

Comment:

I could not understand why 'Interpretations' are separated from the 'Discussions'. Perhaps, it would be better to merge them. Moreover, it is unclear why the relationship between the opening angles and deformation thermometry is discussed (Lines 164-170). Do the authors intend to emphasize that Jpf values are particularly useful for quartz grains deformed at higher temperatures? If so, then I request them to state the same explicitly.

Lines 233-237: Please explain the unexpected behaviour observed for the Khimti Khola valley dataset. The influence of multi-stage deformation, particularly, the late-stage lower CPO opening angle on the Jpf value, which was unaffected by the opening angles as illustrated by the artificial data. Please elaborate this in detail.

Lines 238-244: If I correctly understood, this portion of the manuscript highlights that perhaps the importance of Jpf over other CPO strength indices is limited to monomineralic quartzites or quartz-rich samples only. If that is the case, then I request the authors to comment on the possible effects the various factors listed in lines 239 and 240, could have on the relevance of Jpf over others.

The limitations of the study must be moved to the end of the discussions. The authors may include the response to the comment above alongside the already-stated limitations.

Response:

The Interpretations section summarizes our thoughts on the results of the modelling, whereas the Discussion section expands on the implications of those interpretations.

To combine them could confound what some readers may already find a potentially complex topic. We prefer to keep the manuscript in its current form; we were very careful to avoid unnecessary repetition between the sections.

In Lines 164-170, we mention temperature in the context of opening angles only to demonstrate that the models constructed cover the range of opening angles commonly encountered in natural rocks. We make no specific determinations or recommendations around the usage of Jpf as it relates to temperature.

Lines 233-237: We attempted to address this in the original manuscript, but have now new text for further clarity (underlined below):

"The apparent departure from an expected 'flat' relationship between J_{PF} and opening angle can be attributed to the multi-stage deformation history of the area. The specimens at the top of the structural section (>0.8 relative structural height; Figure 7B) are interpreted to record a late, high-strain deformation episode characterised by smaller quartz CPO opening angles and greater organization (Larson, 2018) resulting anomalously higher J_{PF} that distort the relationship."

Lines 238-240: Relationships observed in the model data may not always be obvious in real-world data. In these lines, we point out some of the many potential complications introduced by heterogenous natural specimens. We did not synthetise these mechanisms in an artificial model and as such we cannot speak to their potential effect on Jpf over the eigenvalue-based methods. The only impact we can point to is that heterogeneity hampers inter-specimen comparisons. It would be an interesting line of research to investigate some of the potential complications with visco-plastic modelling but is well beyond the scope of the paper.

As noted in our response to Reviewer 2, we disagree with the suggestion to move the 'Limitations' section to the end of the Discussion. The limitations discussed in that portion of the manuscript are specifically related to the models constructed. They are not limitations related to the assessment of real-world data – those are cited at the end of the Discussion already. We feel the separation is necessary to avoid confusing one set of limitations with the other as they do not share a common basis.

Comment:

Under the methods section, the authors have described the process of generating the artificial data in Lines 118-125. In my opinion, a step-by-step illustration, if possible, would be great for the readers.

The text inside Figures 1, 4, 5, 8, should be increased. The font size in Figure 3 is more legible. Also, the boxes (a,b,c,d) inside the plots in Figure 2 should be placed so that they do not overlay the curves.

Neither the text nor the symbols are legible in Figure 8.

Response:

As the reviewer noted, we describe, step-by-step in the text how the artificial distributions were generated. Moreover, we provide the annotated R code used to generate the distributions in the open data repository. We feel this provide all the necessary information required for the reader to both assess how we created the distributions and how to create their own.

We have updated all figures as suggested by the reviewer. With regard to Figure 2 and the overlapping boxes, space is limited in those diagrams and overlap is difficult to avoid without resulting in a heterogeneous look. The labels are positioned such that they do not detract from any single data point and as such we have left them as originally drafted.

2nd Round of Revisions

Decision Letter

Sent: 7th April 2023

Dear Dr. Larson and co-authors,

Thank you for your resubmission of your article “Practical Assessment of Quartz Crystallographic Preferred Orientation Strength” submitted to Tektonika. We have now reviewed your resubmitted manuscript and your response to the reviewers’ comments and have reached a decision regarding your article.

Our decision is: Revisions Required

Thank you for making many of the suggested changes to your manuscript and providing a detailed response to the comments made. We are mostly satisfied that the changes made have addressed the comments raised and believe your manuscript has improved during the review process. However, there are a couple of minor points we would like you to address before we can accept your article:

- Abstract – as asked by one of the reviewers, please add a statement to the abstract regarding the method you have used in the study. At the moment it is not clear in the abstract that you generated the artificial data and then conducted an analysis on it; it reads more as background/context at the moment, and so undersells your contribution.
- Figure 3 – we ask that you modify the colour palette or change the symbol type between the difference variance categories to improve accessibility
- Figures 4, 5 and 6 – similarly to Figure 3, please modify the colour palette to improve accessibility. The colours are too similar for those with monochromacy (see https://www.color-blindness.com/coblis-color-blindness-simulator/ to check after modification). The text size on this figure should also be increased please.
- Figure 8 – please modify colours of the dashed lines or vary line type of strength to make them more easily distinguishable for those who are colourblind
- Some minor additional corrections to the text are provided in the attached file

We do not anticipate sending your manuscript out for a new round of reviews and look forward to receiving a revised manuscript from you within 2 weeks, including a response to the points raised, changes to the revised manuscript highlighted, a clean

copy of the manuscript for publication and the updated accessible figure.

Thank you for giving us the opportunity to consider your work.

Yours sincerely,

Dripta Dutta, PhD, Associate Editor - Tektonika
Janine Kavanagh, PhD, Executive Editor - Tektonika

Authors' Reply

Abstract – as asked by one of the reviewers, please add a statement to the abstract regarding the method you have used in the study. At the moment it is not clear in the abstract that you generated the artificial data and then conducted an analysis on it; it reads more as background/context at the moment, and so undersells your contribution.

Completed – we have modified the abstract as follows (new/modified text underlined):

In this study, we generate a series of artificial c-axis distributions using the R software environment and examine the correlations between different distribution geometries (i.e., the opening angle in a cross-girdled quartz c-axis pole figure) and the strengths of those distributions calculated using eigenvalue-based evaluation methods. This comparison demonstrates that larger pole figure opening angles correlate with decreasing distribution ordering strength.

Figure 3 – we ask that you modify the colour palette or change the symbol type between the difference variance categories to improve accessibility

Completed – different symbols are now used for the variables

Figures 4, 5 and 6 – similarly to Figure 3, please modify the colour palette to improve accessibility. The colours are too similar for those with monochromacy (see https://www.color-blindness.com/coblis-color-blindness-simulator/ to check after modification). The text size on this figure should also be increased please.

Completed – All figures have been modified to insure readability with different symbols for each variable. We have also modified the transparency to help promote readability when symbols partially overlap.

In addition, all figures have been resized/scaled to ensure compliance with figure guidelines of Tektonika (i.e. 85mm wide max for 1 column & 200 mm max length). We have further worked to ensure consistent scaling across the figures such that

12pt text in each is the same size (a problem that resulted in inconsistent and sometimes small text size previously)

Figure 8 – please modify colours of the dashed lines or vary line type of strength to make them more easily distinguishable for those who are colourblind

Completed. Some of us are colourblind ourselves – we appreciate this attention to detail.

Some minor additional corrections to the text are provided in the attached file

All completed in the revised text (see tracked changes document)

Acceptance Letter

Sent: 11th April 2023

Dear Dr. Larson and co-authors,

Thank you for your resubmission of your article “Practical Assessment of Quartz Crystallographic Preferred Orientation Strength” submitted to Tektonika. We have now reviewed your resubmitted manuscript and have reached a decision regarding your article.

Our decision is: Accept submission

Thank you for making many of the suggested changes to your manuscript. We have noticed some discrepancies in the recently uploaded figures; Figure 3 is the same as the previously uploaded Figure 5, and Figures 4 and 6 are also swapped in the recent version, but without any changes in the corresponding figure captions. However, these issues can be addressed during the production phase, so we ask that you please pay particular attention to this when asked to review proofs of your paper.

Our production team will be in touch with you soon.

Congratulations and thank you again for submitting your work to Tektonika.

Yours sincerely,

Dripta Dutta, PhD, Associate Editor - Tektonika

Janine Kavanagh, PhD, Executive Editor - Tektonika