TEKTONIKA

Review Report

Peters et al., The Pınarbaşı Metamorphic Sole (Turkey) as a Geological Archive of Subduction Initiation in the Cretaceous Neotethys Ocean, TEKTONIKA, 2026.

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1st Round of Revisions

Decision Letter

Thank you for submitting your work to Tektonika. We apologize for the unexpectedly long review process. Having now obtained the recommendations from two reviewers, we have reached a decision regarding your submission to ТЕКТОNIKA, "The Pınarbaşı metamorphic sole (Turkey) as a geological archive of subduction initiation in the Cretaceous Neotethys Ocean".

Our decision is: Revisions Required

Both reviewers have carefully considered the contribution and have provided extensive recommendations for improving the manuscript. Please refer to the comments below and to both of the attached documents when preparing your revision.

You now have THREE months in which to submit your revision, but we hope that you will be able to make the necessary revisions well within that time. Your revision is due by 25-Sept-2026.

When submitting your revised manusript, please provide a reponse to each of the reviewer comments, in whatever format is most convenient for you. Changes to the manuscript should be highlighted in the revised manuscript (e.g. in bold or colour). Please also upload clean files (remove bold font or track changes). We look forward to receiving your revised manuscript.

Comments by Reviewer 1

The manuscript entitled “The Pınarbaşı metamorphic sole (Turkey) as a geological archive of subduction initiation in the Cretaceous Neotethys Ocean” provides the significance of the metamorphic sole rocks, together with ^{40}Ar-^{39}Ar radiometric dating and pressure–temperature (P–T) conditions of the metamorphic sole rocks from the Pınarbaşı region of Central Türkiye. The authors discuss the geodynamic implications and the subduction initiation processes based on these data. Thus, the authors are emphasizing the Pınarbaşı metamorphic sole represent one of the important examples from the Tethyan oceanic realm. The manuscript makes some important points and the data set is valuable and is of potential broad interest to researchers working on oceanic crust generation and subduction zone metamorphism. The results are valuable and effectively compared the other metamorphic sole rocks from Eastern Mediterranean region. Overall, the manuscript is well written and well organized. I have some minor suggestions and corrections to the paper. My minor comments and suggestions are provided in the related part of the review form. I would suggest acceptance after the correction the these points.

Recommendation: Revisions Required

Comments by Reviewer 2

This manuscript by Peters et al. provides a field, structural, petrological, and geochronological documentation of the Cretaceous Pınarbaşı metamorphic sole. While the Oman sole serves as the classic global benchmark, well-preserved lateral equivalents are rare. This study presents a well-detailed sole section. An interesting finding is the identification of a higher-temperature structural slice interleaved within the lower-temperature base, which the authors attribute to sole imbrication via subduction erosion during plate interface growth. Supported by new hornblende dates, the authors link the Pınarbaşı sole to the broader Western Neotethyan framework. Overall, I found this to be an interesting metamorphic study integrating a diverse dataset. The paper will be a useful addition to Tektonika however requires major revision in its current format.

That data appears of good quality and generally supports the interpretations that are developed. I've made several recommendations below, which range in scale. I would rank my requested revisions as major. Please see the annotated pdf for additional comments.

Thanks for the opportunity to learn something new about subduction dynamics.

Major issues

1. Framing and Contribution: The regional and global importance of this study is not yet clearly articulated. Too much text is spent placing this work within the framework of what is known about Oman. While benchmarking against Oman is necessary given its significance to our understanding of subduction initiation, the current framing undermines the independent contribution of the Pınarbaşı sole.I would suggest the authors focus first on the key contributions and local datasets established by this study. Once the Pınarbaşı data is clearly framed, expand out to a balanced comparison with Oman and explore the wider variations across broader Neotethyan ophiolitic sole datasets. If the data is introduced primarily through the lens of Oman, it inadvertantly leaves the reader questioning the use of studying this specific sole.
2. The discussion of imbrication is one of the most notable and exciting features of the paper. However, there is currently no exploration of whether other metamorphic soles globally preserve similar evidence of imbrication. Please provide a comparative discussion on the presence or absence of imbrication features in other global soles (which may be genuine or a function of exposure quality). This context is critical to strengthening your broader conclusions.

Moderate issues

1. Introduction: It would be helpful to more explicitly outline for the reader the key questions you are trying to explore or answer in this study and link these to the datasets/techniques you will use to answer these questions. This has been achieved to some degree below but a clearer outline will allows the reader to more clearly go

into the main text understanding what the paper is going to achieve other than collecting lots of data on a well preserved sequence.

2. The manuscript discusses a significant number of legacy geochronological studies, but not all age determinations can be treated equally. Furthermore, there is an ongoing debate regarding the timing of sole formation in Oman (e.g., the Garber vs. Guilmette datasets). This preference is currently given, while opposing arguments are brushed aside without sufficient exploration of why such age discrepancies exist.
    1. Please apply consistent reporting standards for legacy data: either use "ca." for all compiled ages or provide explicit uncertainties so the reader can assess data confidence (e.g., treating Rb/Sr white mica ages with equal weight to U/Pb or Lu/Hf accessory phase analyses is problematic).
    2. Provide explore other perspectives on the Oman timing debate. Address whether the discrepancy is a function of isotopic record or if earlier stages of prograde garnet growth were not preserved. This is notable especially given that the same sections were analyzed using the same isotopic system. Alternatively consider whether discussion of these datasets are sufficiently relevant to the aims of the study.
    3. Eliminate the repetition of discussion of Oman Lu/Hf geochronology data between the introduction and discussion sections.
3. The development of the argument in section 2 needs both shortening and a clearer logical progression. You could also consider whether this section should be condensed and integrated into the introduction. The logic of the introduction could then be to explain the concepts and different arguments around metamorphic soles and subduction initiation and then frame the Anatolian ophiolite as a suitable locality in which to understand this problem.
4. Several section of the paper read like they are written more for a thesis than for an academic paper. Sections can be excessively long or information reported in a list on a single study basis. I suggest going through the paper to condense these arguments and to keep sentences concise and to the point.
5. Several broad statements lack appropriate referencing or contain significant jumps in tectonic logic. For example, lines 230–231 state: “In all cases, the metamorphic soles have nearly identical composition, structure, and metamorphism,” which is then used to suggest “that their formation is genetically related to the subduction initiation process, irrespective of their tectonic setting.” This is a provocative point that requires referencing and clarity. If tectonic settings vary, they would presumably be accompanied by varying subduction rates and distinct bounding thermal/rheological properties. The authors need to explain how and why the subduction initiation process remains so uniform across these variable states. Address these logical gaps here and at line 642 (noted in the annotated pdf).
6. The field and petrographic descriptions of units need expanding and restructuring. Currently, petrographic data is mixed haphazardly with field observations. Please separate or clearly distinguish field observations from thin-section petrography (consider renaming or splitting sections as appropriate). For each unit, systematically present data in a strict, consistent order (e.g., unit color, grain size, fabric/layering,

mineral assemblage, unit thickness, cross-cutting relationships, and mineral growth orientations relative to foliation/lineation). Ensure these sections remain entirely observational; all interpretations must be moved to the discussion. Review the entire section to condense or remove unnecessary local variations or redundant individual outcrop descriptions.

7. The descriptions of both the electron microprobe (EPMA) and thermodynamic modeling methods are insufficient.
    1. EPMA: Detail the analytical parameters, standards used, and the exact method utilized for mineral formula recalculations (including how XFe3 was determined for amphibole).
    2. How were modal proportions determined?
    3. P-T Pseudosections: Specify the exact activity-solution models used, the fluid activity (aH2O) and fluid abundance, State what bulk XFe3 was chosen for modelling.
    4. Isopleths: Discuss the rationale behind choosing specific mineral composition variables for isopleth intersection calculations, how sensitive your P-T results are to these choices. Where were no composition variables were used for plagioclase?
8. It would be worth exploring any evidence for or against alternative explanations that may account for the higher temperature slice within the lower-temperature portion of the sole. For example could the difference in metamorphic record be a function of the preserved conditions of the rock. I.e., the assumption of preserving peak conditions can depend on a variety of factors, have the units actually experienced the same tectonic record but instead there has been variation in local fluid abundance or fluid composition that may have impacted what segment of the P-T path has been preserved.

Minor issues

1. Why is the unit called dark amphibolite? Why are you mixing a clearly defined rock term with a colour. I suggest adjusting to amphibolite A/B or clarify with appropriate indicator mineral.
2. Field photos should include relevant marked structures of deformation fabrics or minerals (e.g., Figure 4)
3. When giving the assemblage please make a note of what order the assemblage is presented in (by decreasing abundance or by indicator minerals first etc.)
4. Terms Unit IB and IA are variably used in some places but not in others. Apply these terms consistently and structure the unit descriptions within this or remove.
5. Throughout the field description section assess whether it is necessary to include all local variation or individual outcrop observations. These sections could be rewritten to present this information in a more condensed and concise manner.
6. When describing a foliation please also discuss what minerals define and form the foliation.

7. There is discussion of thin section observations but no clear distinction of a section relating to this. Petrographic observations appear jointly given with field observations. Please provide clearer distinction and rename the section or split it as appropriate.
8. Caution is warranted with the use of Fe-Ti-rich phases to determine peak assemblage which contribute little to the gibbs free energy of the assemblage (e.g., Weller et al., 2014). It is also worth considering whether titanite determines a peak phase or whether it is a decompression feature (where rimming rutile, e.g., line 511).
9. How were the Fe2+ and Fe3+ ratios determined for amphibole? By the results section of the paper the mineral chemistry could have been given for each amphibole and the appropriate terminology could then be used.
10. Figure 7 is not a readable figure. This figure needs to be remade or split into multiple figures, some of which can potentially go into the supplementary.
11. Please detail evidence for amphibolite-facies shearing (e.g., line 294).
12. Figure 8 is a useful and important figure but contains too much data and it is too difficult to extract the clear trends. See notes on annotated pdf for suggestions to improve this figure.

Recommendation: Revisions Required

Authors' Reply to Reviewer 1

In the final part of the abstract, the main conclusion of the study should be stated more clearly. For example, briefly summarizing in one sentence what is gained from the comparison with the metamorphic sole rocks from Oman which would improve the scope of the abstract and help readers better understand the overall contribution of the manuscript.

We thank the reviewer for their constructive and supportive evaluation. We have modified the final sentences of the abstract to make the implications more explicit:

The Pınarbaşı metamorphic sole may serve as a lateral equivalent as the Oman sole and likely formed at the initial stages of formation of the same plate boundary. The few Myr younger cooling and ophiolite spreading ages in Pinarbasi may thus reflect lateral geodynamic variations in the maturation of the subduction zone and the generation of upper plate extension. Their comparison offers new opportunities to study the regional geodynamic drivers and plate kinematic context of subduction initiation.

The Introduction is generally well written and detailed explains the significance of the concept of metamorphic sole rocks as well as the importance of the Pınarbaşı metamorphic sole rocks. However, the section is somewhat too long for an introduction section. Condensing certain parts and presenting them in a more concise manner would improve readability and make this section more effective for the reader.

We agree, and we have reduced the amount of review in the first three paragraphs to come to the point quicker. This reduced the length by ~20%.

In the statement regarding the geochemical data (Line 335), the protolith geochemistry of the metamorphic sole rocks appears to be indicated solely as an OIB source. However, there are also results indicating MOR-type and SSZ protolith affinities for the Pınarbaşı metamorphic sole rocks. It is recommended that knowledges regarding these geodynamic environments be included into the manuscript.

Corrected.

Previous geochemical analyses suggested that the basaltic protoliths display a range of compositions varying from MORB to OIB and IAT affinities (Çörtük et al., 2023)

The generation concept of the metamorphic sole rocks are provided in the section 2, but, the details regarding the protolith geochemistry of these rocks have not been discussed. This is important, for example the presence of SSZ-type geochemical protoliths in metamorphic sole rocks may indicate subduction erosion of a formed SSZ oceanic crust. Incorporating such geochemical perspectives into the relevant section would enhance the scope and depth of this section.

We agree that this information adds interesting insights, but we refrained from adding this to the summary of section 2. The reason is that the geochemical data by itself are still ambiguous: IAT geochemical signature of a sole protolith may be interpreted as subduction erosion but may also reflect that the ocean floor that was subducted formed in an arc setting, long before subduction initiation. For the Turkish soles, that appears to be the case: the Neotethys likely opened in an upper plate position above a subduction zone. The former may not be likely: the sole rocks of Turkey likely formed before the SSZ ophiolite formed, so it's hard to recycle the SSZ ophiolite in the sole. This ambiguity would take too much space to discuss, and because we do not offer a new perspective or data on this subject, we prefer to keep the focus on metamorphic and geochronological constraints.

The Discussion section addresses the topics from two main perspectives: (i) the internal structure of the Pınarbaşı metamorphic sole rocks, and (ii) the relationship between the metamorphism and cooling ages of the metamorphic sole rocks and timing of the SSZ ophiolite generation. Overall, it is well written and evaluates the subject from different approaches. However, my suggestion provided for Section 2 is also applicable in this section.

See our previous response: because we consider the interpretation of the geochemistry as a source of discussion rather than a constraint on the evolution, we prefer to keep the focus as we had it. The geochemical perspective is interesting, but may be discussed in other papers.

Since the geological map of the study area (Figure 3) specifically represents the Pınarbaşı metamorphic sole rocks. Therefore, it would be more appropriate to place it under the section of “Field Observations” (Section 4).

Agreed. We moved it.

References should be added into the related sentences for Figure 1 under the Geological Setting section.

We clarified that both maps are based on van Hinsbergen et al. (2020; 2021)

Mineralogy and Petrography: In the section describing the petrographic characteristics of the metamorphic sole rocks, figure references related to specific features should be explicitly cited within the relevant parts of the manuscript.

Done where suggested in the annotated manuscript.

Line 231: This in an interesting point. So how is the subduction initiation process consistent if the tectonic setting varies, presumably variation in tectonic setting provide varying rates of subduction as well as thermal and rheological properties. And if these are true, then why are the compositional and geological record of these soles then consistent?

These are good questions, but beyond the scope of this review section. As we point out: the similarities of soles despite the different tectonic settings of subduction initiation suggests that their characteristics are linked to the processes

Line 237; “orogen” statement were used two times in one sentence. One of them should be excluded and the sentence modified in this direction.

Corrected.

Authors' Reply to Reviewer 2

Major issues

1. Framing and Contribution: The regional and global importance of this study is not yet clearly articulated. Too much text is spent placing this work within the framework of what is known about Oman. While benchmarking against Oman is necessary given its significance to our understanding of subduction initiation, the current framing undermines the independent contribution of the Pınarbaşı sole.I would suggest the authors focus first on the key contributions and local datasets established by this study. Once the Pınarbaşı data is clearly framed, expand out to a balanced comparison with Oman and explore the wider variations across broader Neotethyan ophiolitic sole datasets. If the data is introduced primarily through the lens of Oman, it inadvertantly leaves the reader questioning the use of studying this specific sole.

Following the reviewer's suggestions in the annotated manuscript, we have shortened the introduction by ~20% and focused more on Pinarbasi. In the discussion, we have removed overly long discussion of opportunities for the future, or comparisons with other soles, but discussed in more detail the impact of our new findings.

2. The discussion of imbrication is one of the most notable and exciting features of the paper. However, there is currently no exploration of whether other metamorphic soles globally preserve similar evidence of imbrication. Please provide a comparative discussion on the presence or absence of imbrication features in other global soles (which may be genuine or a function of exposure quality). This context is critical to strengthening your broader conclusions.

We are not aware of any other examples of this imbrication. We have explained this now in detail in the manuscript - this is indeed an interesting finding. We have also added a remark in response to the annotated manuscript, explaining that we have no evidence that the higher-grade lense reflects retrograde overprinting of the structurally higher sole levels.

Moderate issues

1. Introduction: It would be helpful to more explicitly outline for the reader the key questions you are trying to explore or answer in this study and link these to the datasets/techniques you will use to answer these questions. This has been achieved to some degree below but a clearer outline will allows the reader to more clearly go into the main text understanding what the paper is going to achieve other than collecting lots of data on a well preserved sequence.
We have shortened the introduction and added more explicit questions that we set out to solve, following the guidlines and questions of the reviewer in the annotated manuscript.

2. The manuscript discusses a significant number of legacy geochronological studies, but not all age determinations can be treated equally. Furthermore, there is an ongoing

debate regarding the timing of sole formation in Oman (e.g., the Garber vs. Guilmette datasets). This preference is currently given, while opposing arguments are brushed aside without sufficient exploration of why such age discrepancies exist.
We thank the reviewer for this comment. We have added a more balanced explanation of the dataset and removed our bias towards it. We have offered the options in such a way that a reader can make up their own mind.

1. Please apply consistent reporting standards for legacy data: either use "ca." for all compiled ages or provide explicit uncertainties so the reader can assess data confidence (e.g., treating Rb/Sr white mica ages with equal weight to U/Pb or Lu/Hf accessory phase analyses is problematic).
Done

2. Provide explore other perspectives on the Oman timing debate. Address whether the discrepancy is a function of isotopic record or if earlier stages of prograde garnet growth were not preserved. This is notable especially given that the same sections were analyzed using the same isotopic system. Alternatively consider whether discussion of these datasets are sufficiently relevant to the aims of the study.
We have explained in more detail what the possibilities are provided by the original authors, and how the differences may be reconciled. We do not address this issue in this paper any further because we only show Ar/Ar data here, but it indicates why the finding of garnet amphibolites, and leucosomes, in the Pinarbasi ophiolite is important: future study may test whether Pinarbasi also contains garnets that long predate the zircon and Ar ages. This is why we included this section.

3. Eliminate the repetition of discussion of Oman Lu/Hf geochronology data between the introduction and discussion sections.|
Done, we have removed this paragraph in the discussion, because we have nothing to add to it in this paper.

3. The development of the argument in section 2 needs both shortening and a clearer logical progression. You could also consider whether this section should be condensed and integrated into the introduction. The logic of the introduction could then be to explain the concepts and different arguments around metamorphic soles and subduction initiation and then frame the Anatolian ophiolite as a suitable locality in which to understand this problem.
We prefer to keep the introduction short, in four paragraphs outlining the general problem (and we made it shorter than it was). We have shortened section 2, but we think it is useful to provide a short review of the history of thought on soles and their role in the debate on subduction initiation, and how this shifted significantly in the last decade. We made this more explicit in the last paragraph of section 2, in response to a comment in the annotated manuscript:

In all cases, the metamorphic soles have nearly identical appearance, suggesting that their formation is genetically related to the processes that connect the initial thrusting to the

formation of a mature subduction zone, and not to the specifics of the tectonic setting in which initial thrusting occurred (Guilmette et al., 2023). This makes the study of metamorphic soles even more important to understand the subduction initiation process than previously already appreciated.

4. Several section of the paper read like they are written more for a thesis than for an academic paper. Sections can be excessively long or information reported in a list on a single study basis. I suggest going through the paper to condense these arguments and to keep sentences concise and to the point.
The reviewer kindly indicated these sections in the annotated manuscript: we have everywhere shortened and sharpened those sections.

5. Several broad statements lack appropriate referencing or contain significant jumps in tectonic logic. For example, lines 230–231 state: “In all cases, the metamorphic soles have nearly identical composition, structure, and metamorphism,” which is then used to suggest “that their formation is genetically related to the subduction initiation process, irrespective of their tectonic setting.” This is a provocative point that requires referencing and clarity. If tectonic settings vary, they would presumably be accompanied by varying subduction rates and distinct bounding thermal/rheological properties. The authors need to explain how and why the subduction initiation process remains so uniform across these variable states. Address these logical gaps here and at line 642 (noted in the annotated pdf).
See above: we rephrased this sentence. The logic that we follow here comes from the observation that soles are remarkably uniform despite that they formed after subduction initiation in very different tectonic settings. Rather than going at length to explain this, we draw the obvious conclusion, which was previously noted by Guilmette et al. (2023) that we now properly credit, that the specifics of sole development are likely related to the processes between initial thrusting and mature subduction, and not to the setting in which that initial thrusting occurred.

6. The field and petrographic descriptions of units need expanding and restructuring. Currently, petrographic data is mixed haphazardly with field observations. Please separate or clearly distinguish field observations from thin-section petrography (consider renaming or splitting sections as appropriate). For each unit, systematically present data in a strict, consistent order (e.g., unit color, grain size, fabric/layering, mineral assemblage, unit thickness, cross-cutting relationships, and mineral growth orientations relative to foliation/lineation). Ensure these sections remain entirely observational; all interpretations must be moved to the discussion. Review the entire section to condense or remove unnecessary local variations or redundant individual outcrop descriptions.
We renamed this section, removed unnecessary detail, and followed the same logic for each unit.

7. The descriptions of both the electron microprobe (EPMA) and thermodynamic modeling methods are insufficient.

1. EPMA: Detail the analytical parameters, standards used, and the exact method utilized for mineral formula recalculations (including how XFe3 was determined for amphibole).
2. How were modal proportions determined?
3. P-T Pseudosections: Specify the exact activity-solution models used, the fluid activity (aH2O) and fluid abundance, State what bulk XFe3 was chosen for modelling.
4. Isopleths: Discuss the rationale behind choosing specific mineral composition variables for isopleth intersection calculations, how sensitive your P-T results are to these choices. Where were no composition variables were used for plagioclase?

These details were added to the manuscript, along with other more detailed requests indicated in the annotated manuscript.

8. It would be worth exploring any evidence for or against alternative explanations that may account for the higher temperature slice within the lower-temperature portion of the sole. For example could the difference in metamorphic record be a function of the preserved conditions of the rock. I.e., the assumption of preserving peak conditions can depend on a variety of factors, have the units actually experienced the same tectonic record but instead there has been variation in local fluid abundance or fluid composition that may have impacted what segment of the P-T path has been preserved.

We added a remark to the discussion that acknowledges these options. We found no evidence of retrograde resetting in the overlying epidote amphibolites that would suggest differential preservation of peak conditions.

Minor issues

1. Why is the unit called dark amphibolite? Why are you mixing a clearly defined rock term with a colour. I suggest adjusting to amphibolite A/B or clarify with appropriate indicator mineral.

We had this in the text to indicate the difference with the amphibolites with abundant felspar-rich pockets. But we made the distinction based on mineralogy now, rather than color.

2. Field photos should include relevant marked structures of deformation fabrics or minerals (e.g., Figure 4)

Added.

3. When giving the assemblage please make a note of what order the assemblage is presented in (by decreasing abundance or by indicator minerals first etc.)

Done.

4. Terms Unit IB and IA are variably used in some places but not in others. Apply these terms consistently and structure the unit descriptions within this or remove.

Removed.

5. Throughout the field description section assess whether it is necessary to include all local variation or individual outcrop observations. These sections could be rewritten to present this information in a more condensed and concise manner.
We went through it again and removed unnecessary detail and wordiness.

6. When describing a foliation please also discuss what minerals define and form the foliation.
Added

7. There is discussion of thin section observations but no clear distinction of a section relating to this. Petrographic observations appear jointly given with field observations. Please provide clearer distinction and rename the section or split it as appropriate.
We renamed the section

8. Caution is warranted with the use of Fe-Ti-rich phases to determine peak assemblage which contribute little to the gibbs free energy of the assemblage (e.g., Weller et al., 2014). It is also worth considering whether titanite determines a peak phase or whether it is a decompression feature (where rimming rutile, e.g., line 511).
Titanite is interpreted to represent a peak PT phase. It occurs as inclusion in garnet and cpx. It is also showing some incipient transformation and coexistence with rutile.

9. How were the Fe2+ and Fe3+ ratios determined for amphibole? By the results section of the paper the mineral chemistry could have been given for each amphibole and the appropriate terminology could then be used.
The Fe2/3 ration of amphibole is calculated following the nomenclature proposed by Hawthorne et al. (2012) and Leake et al., (1997). We added this to the text.

10. Figure 7 is not a readable figure. This figure needs to be remade or split into multiple figures, some of which can potentially go into the supplementary.
We split the figures in two parts on two pages.

11. Please detail evidence for amphibolite-facies shearing (e.g., line 294).
We indicated that the foliation is defined by hornblende.

12. Figure 8 is a useful and important figure but contains too much data and it is too difficult to extract the clear trends. See notes on annotated pdf for suggestions to improve this figure.
We changed the lines according to the reviewer's suggestion.

2nd Round of Revisions

Decision Letter

Thank you for submitting your work to Tektonika. We have reached a decision regarding your revised submission to τεκτονικα, "The Pınarbaşı metamorphic sole (Turkey) as a geological archive of subduction initiation in the Cretaceous Neotethys Ocean".

Our decision is: Only Minor Revisions Required (these will not be subject to a new round of review)

You now have ONE month in which to submit your final revision, but we hope that you will be able to make the necessary revisions well within that time. Your revision is due by 5-Oct-2026.

When submitting your revised manuscript, please highlight the changes to the manuscript (e.g. in bold or colour) and please also upload clean files (remove bold font or track changes) so that they can be used for typesetting. We look forward to receiving your final revised manuscript.

Comments by Reviewer 2

The authors have made a series of good changes to the manuscript, particularly those to help bring out the original contributions made by the work. I believe these have strengthened the takeaways from this study. I recommend this article to be accepted with a few minor revisions. Minor points noted in the annotated pdf.

One point to highlight is that the reasons for undertaking the study at the end of the introduction could do with strengthening. It is not entirely clear from the current reasoning the logic for collecting the P-T and geochronological datasets.

It is still not explicit what XFe3 has been used. If the logic is that XFe3 in amphibole is “low”, to just ignore it does not seem justifiable. An XFe3 between 0-0.1 is still noticeable and varying it between these values can have a prominent impact on pseudosection topology. Furthermore when considering compositional variables are used to constrain P-T conditions, the chosen variable set will still be directly or indirectly be impacted by the chosen bulk rock XFe3.

Recommendation: Revisions Required

Authors' Reply to Reviewer 2

Thank you for this second round of minor revisions. We return the manuscript to you with the requested changes. We followed all suggestions in the annotated manuscript and respond to the two points raised by the reviewer below. We thank you for the editorial handling, and look forward to seeing our paper in print.

The authors have made a series of good changes to the manuscript, particularly those to help bring out the original contributions made by the work. I believe these have strengthened the takeaways from this study. I recommend this article to be accepted with a few minor revisions. Minor points noted in the annotated pdf.
All suggestions in the annotated pdf were followed.

One point to highlight is that the reasons for undertaking the study at the end of the introduction could do with strengthening. It is not entirely clear from the current reasoning the logic for collecting the P-T and geochronological datasets.
We clarified by rewriting the final part of the introduction:
We provide new $^{40}Ar/^{39}Ar$ geochronology to test whether its cooling history coincides with supra-subduction zone oceanic spreading recorded in the ophiolites, a trend seen in these systems worldwide (e.g., van Hinsbergen et al., 2015). We compare our results to the sole structure and PT conditions, and the geochronological relationships between sole cooling and ophiolite spreading to establish whether the Pınarbaşı Ophiolite may serve as a representative natural laboratory for subduction initiation processes. Additionally, we provide plate tectonic context for sole formation using recent detailed kinematic reconstructions of eastern Mediterranean orogenesis. We will critically evaluate similarities and differences between the Pınarbaşı metamorphic sole and other well-studied metamorphic sole sections elsewhere, and identify opportunities for future study on the Pınarbaşı sole in the light of current models and outstanding questions on sole formation, its role in the subduction initiation process in general, and in the establishment of the 1000s of km long subduction plate boundary in the Cretaceous Neotethys in particular.

It is still not explicit what XFe3 has been used. If the logic is that XFe3 in amphibole is "low", to just ignore it does not seem justifiable. An XFe3 between 0-0.1 is still noticeable and varying it between these values can have a prominent impact on pseudosection topology. Furthermore when considering compositional variables are used to constrain P-T conditions, the chosen variable set will still be directly or indirectly be impacted by the chosen bulk rock XFe3.
We set XFe3+ to 0 for the pseudosection calculation. It relies on different indication.
Fe3+ calculated in cpx is null. In garnet the andradite pole accounts for less that 2% (i.e. below microprobe precision). For amphibole, the average XFe3+ is 0.14, the median is 0.10. Amphibole inclusions in garnet have the lowest Fe3+ content.
With respect to the bulk composition, the 50-60 % of amphibole would account for a global XFe3+ of 7-15%. Most amphibole correspond to matrix amphibole that are certainly after the peak PT. Then the assumption of XFe3+ = 0 is certainly conservative, but also very safe. In

addition, calculation with Fe3+ makes ilmenite stable in the pseudosection but this phase is never observed in this sample. The oxidation state as probably changed during the retrograde path of the rock (and increased), but the rock is not even significantly reactive because this is not seen but in amphibole (almost certainly due to recrystallization).

Acceptance Letter

We have reached a decision regarding your submission to ᴛᴇᴋᴛᴏɴɪᴋᴀ, "The Pınarbaşı metamorphic sole (Turkey) as a geological archive of subduction initiation in the Cretaceous Neotethys Ocean".

Our decision is to: Accept Submission