

Geothermal gradient change during subduction recorded by ultrahigh-pressure eclogite

Corresponding Author: Dr Jan Schönig

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

This manuscript reconstructs the thermal evolution recorded by the youngest ultrahigh-pressure (UHP) eclogite from the D'Entrecasteaux Islands, eastern Papua New Guinea. The authors combine existing 2D numerical models with several complementary datasets, including U–Pb zircon LG-SIMS dating, phase equilibrium modelling, Raman spectroscopy, and melt inclusion re-homogenization experiments. The dataset appears robust and internally consistent, and the study addresses an important gap in our understanding of how the thermal structure of nascent subduction systems evolves and the conditions under which UHP rocks form and are preserved.

The interpretations presented by the authors are well supported by the data, and the core conclusions are of broad interest and suitable for publication in Nature Geoscience. The manuscript is also conceptually engaging, particularly in the way multiple analytical approaches are integrated to address the thermal evolution of subduction systems.

However, several aspects of the presentation and discussion could be clarified or slightly expanded to improve robustness of the interpretations within the constraints of the short format.

1. While the manuscript focuses primarily on constraining the early thermal state of the subducting slab, it would be helpful to briefly clarify why the garnet rim was not included in the phase equilibrium modelling. A short explanation would help readers assess whether the rim was excluded because it records later stages of the P–T evolution that are not central to the study, or because of methodological challenges related to phase equilibrium modelling.

2. The authors use the zircon age range of 5.1–4.4 Ma to infer burial from ~1.5 GPa to >2.9 GPa, linking the oldest zircon growth to garnet core formation and the younger ages to later stages of garnet growth. While this interpretation is consistent with the zircon trace-element compositions, it relies on zircon separates that lack direct petrographic relationships with specific garnet growth zones. It would therefore strengthen the manuscript if the authors briefly acknowledged the uncertainty in linking zircon growth directly to specific stages of garnet growth. This is relevant because the inferred timing and duration of the prograde P–T–t path, as well as the calculated burial rates, depend on this temporal linkage. It may also be worth considering whether alternative scenarios—for example zircon growth during a short residence interval at high or ultrahigh pressure—could contribute to the observed age dispersion, and how such possibilities, if plausible, might affect the interpretation.

3. Lines 204–205: The manuscript states that the garnet core formed under “warmer conditions” than the garnet mantle. Because the absolute temperatures inferred for the mantle are higher than those recorded by the core, replacing the term “warmer conditions” with “higher geothermal gradients” would improve clarity.

Ludovico G. Scorsolini

Reviewer #2

(Remarks to the Author)

Review by Sarah Penniston-Dorland, University of Maryland

Overall, I found this manuscript to be well-written and well-thought out.

The authors state that they are challenging a paradigm of UHP rocks being unequivocal evidence for a mature, self-sustained, steady-state, cold, modern-style subduction (lines 76–78). The authors need to more clearly establish that this is a

well-accepted paradigm. As it stands, currently they explain the reasoning behind this paradigm in lines 50-53, but do not include any citations indicating that this is what the community follows as its paradigm. This could be explained better and should include references cited in which others have stated this paradigm/assumption.

I think a clearer distinction of what information are model predictions/estimations for modern subduction systems and evidence from exhumed HP & UHP rocks would be helpful. For example, line 43 needs to more clearly state the basis for this assertion. e.g., "Model predictions of the thermal structure of mature, modern-style subduction zones indicate that they are characterized by..." Another example is in line 206 – instead of "...conditions agree with the thermal structure of mature..." say "...conditions agree with the model predictions (or estimations) of the thermal structure of mature..."

I think that the authors could do a better job of explaining the methods used and its uncertainties. I am not a U-Pb specialist nor am I a melt inclusion specialist. I do have experience with Zr-in-rutile measurements, and I found the representation/documentation of how those data were handled was not complete. It makes me wonder about whether the details of the other methods need more explanation as well. In the end, I think my criticisms (below) will not impact the overall outcome and interpretation of the study, but I think that the authors could more clearly represent the data (how many analyses) and its uncertainty (what is the variability and the analytical uncertainty).

I think it is important to represent that the rutile Zr contents from the garnet core are based on just 2 grains (n=2) while n=13 rutile inclusions in the garnet mantle were analyzed in the main body of the text. In the methods, please also describe whether rutile grains were homogeneous or chemically zoned with respect to Zr content. Please describe the approach to choosing the location of analytical spots within the rutile grains (e.g., were they always in the center of grains). Lines 129-130 – what are the +/- values based on? I did a quick calculation of the standard deviation of the population of mantle rutile Zr contents, and it is 45 ppm, not 13. Is the uncertainty described here the analytical uncertainty? Why was this used instead of the variability of the population itself? I don't see anywhere in the methods where the analytical uncertainty (and how it was assessed) is reported.

A lot of significance is attached to the finding of coesite in the garnet mantle zones. This is justified because it is an indicator of an ultra-high pressure history experienced by a rock. There is some literature that attributes the presence of high-pressure minerals to overpressure effects rather than a record of lithostatic pressure (e.g., Schmalholz and Podlachikov, 2013; Tajcmanova et al., 2015). It is a question that I think has not been fully resolved and deserves some discussion in this manuscript as to why the authors think that it does not explain the presence of coesite in these rocks. The garnet crystals also contain cristobalite, which is stable at very high temperatures (above any temperatures inferred to have been reached by these samples) and relatively low pressures. I think it is worth briefly describing what the meaning of these cristobalite inclusions is and why their presence does not tell us about a high-temperature history.

Line 205 – the actual temperature is cooler not warmer in the garnet core compared to its mantle – maybe say "warmer geothermal gradient" or something else to indicate this.

Lines 243-253 – I am wondering about the comparison illustrated in Figure 5b between the P-T conditions recorded by rocks and the model from Holt and Condit (2021). The time difference predicted by the model for the progression from the P-T conditions of the garnet core to the garnet mantle is from 6.2 Myr (core) to somewhere around 8.8-11.9 Myr (mantle), which is a time range of 2.6 to 5.6 Myr. This span of time is longer than the range of zircon ages within the garnet permits (5.1 to 3.1 Ma is a total of 2 Myr). I think some discussion of this is warranted – this could take the form of acknowledging the uncertainty of such a comparison and recognition that the numbers should not be directly compared. Another solution might be to remove the actual ages of the model P-T paths and just indicate early to late with an arrow.

The literature citation focuses on a few select studies and leaves out others that are relevant, and in some cases the papers where an idea was originated. Below are a couple of examples of literature that should be included (and yes, they are references to my papers, because they are ones I am most aware of that have been excluded!)

Line 214 – Penniston-Dorland et al. (2025, EPSL) is another citation that should be added here.

Line 260 – Add in reference to Penniston-Dorland et al (2015, EPSL) here.

Reviewer #3

(Remarks to the Author)

Review of Schönig et al for Nature Geoscience

Schönig and coauthors present an interesting and provocative investigation of a remarkably young eclogite sample from Papua New Guinea. They make the case that the sample attained ultrahigh pressure (UHP) conditions (coesite stability) around 4-5 Ma and claim it represents the youngest example of UHP eclogite on Earth. The meat of the paper is in the derivation of a segment of the sample's prograde PTt trajectory that transits a region of PT space not typically associated with subduction (1.5 GPa, 700 °C). This observation is interpreted with published numerical models of subduction in which time-dependence is included. The authors conclude that the Papua New Guinea UHP eclogite formed during young subduction before a colder thermal architecture could be established. They speculate that examples of such young subduction may be overlooked in the rock record with implications for plate tectonic evolution.

I enjoyed reading the paper and found the topic, data and tectonic setting of the sample to be especially compelling and of potential interest to the broad readership Nature Geoscience commands.

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Main comments

Thermal evolution of the subduction interface: the paper is a little one-dimensional in its assessment of possible explanations for the PT conditions of garnet core growth (~1.5 GPa, 700 C central to the take-home message). Alternative mechanisms to ageing of the subduction zone should be considered (downgoing plate age, dissipation, 3D effects of the subduction geometry etc). I raise this not to say that I disagree with the authors, but more as a suggestion to improve confidence in the conclusions. Related to this, the comparison between the PT path and the thermal model on which the conclusions rest is underdeveloped; for instance, Figure 5 shows a temporal evolution of calculated PT paths spanning a much longer timescale than that proposed to separate the timing of garnet core and rim growth. Are other parameters used in the model suitable to enable such a comparison?

Interpretation of the PTt path: it is argued that because the oldest zircon dates lack an Eu-anomaly, they record the timing of garnet core growth at ~1.5 GPa and ~700 C. However, ED Fig. 2 indicates that plagioclase has a restricted stability field and is limited to $P < 1$ GPa (or $T > 750$ C). This implies that the absence of an Eu-anomaly isn't a particularly diagnostic indicator of eclogite-facies conditions for this rock. The oldest zircon could have equilibrated at lower-grade PT conditions, further shortening the time available for the PT loop. There are a few more issues here: i) the PT conditions of garnet core growth don't seem to agree with the 'garnet-in' line on ED Fig. 2; ii) the calculated mineral assemblage at PT conditions of garnet core equilibration contains amphibole and biotite—this implies that the rock would more likely be a garnet amphibolite, not an eclogite. I raise this not to be pedantic, but to offer the suggestion that under such PT conditions there are plausibly a wider range of rock types associated with subduction than eclogites and blueschists.

Zircon HREE(t) trend: the decrease in HREE concentration with zircon age could also be accounted for by melt removal (as opposed to garnet crystallization). Two observations seem to support this: i) the reported absence of zircon in the matrix, and ii) the lack of progressive MREE/HREE fractionation with zircon age suggests bulk depletion of HREE from the system. If this interpretation is accurate, then garnet grew during heating, not necessarily compression, which has implications for the reported rates of subduction.

Line-by-line comments

Line 20: 'mature' is ambiguous in this context; I would have thought that subduction of continental margins such as PNG is not expected to adhere to the same rules that apply to long-lived oceanic plates

Lines 35-43: no need to start with climate and early Earth to justify interest in subduction; suggest a clear sentence stating that subduction zones are the primary sites of chemical differentiation and host to large earthquakes and volcanoes

Line 77: I'm not convinced that UHP rocks alone are diagnostic indicators of these traits because thermal gradients vary with depth during subduction (typically following the progression from high to low dT/dP from the plate-plate to the slab-wedge interface); i.e. the presence of coesite in an eclogite doesn't place constraints on the prograde PT path at shallower depths.

Line 106: few people will be familiar with dmsteinbergite and kumdykolite; helpful to include exemplar formulae to guide the reader

Line 124: same comment as above for kokchetavite

Line 169: "In the case of temperature conditions,..." doesn't make sense; please rewrite

Figure 3: are the error ellipses calculated (i.e. as the interception of multiple equilibria)? If so, this should be stated in the figure caption. Also, what constrains the magnitude of the garnet mantle ellipse?

Line 205: "warmer" is confusing in this context as the mantle is at higher-T than the core. Presumably the authors mean that the apparent thermal gradient derived from the core PT condition is steeper (higher dT/dP) than that of the mantle.

Lines 214-217: this is counter intuitive: warmer slabs would lead to densification of mafic eclogites (I believe Matt Kohn raised this point in his PNAS paper on the subject). Temperature isn't the sole control on exhumation potential (see England & Smye (2023), Smye & England (2023) for consideration of the force balance required to exhume along the slab-wedge interface, relevant to this study)

Line 228: I'm not sure this is true as ED Fig. 2 indicates plagioclase isn't a stable phase across a large range of 'non-eclogitic' PT space

Line 230: melt depletion could also explain a bulk decrease in HREE without fractionation

Line 261: this is one interpretation, but it is worth acknowledging that there are many others such as the densification of quartz-bearing rocks (qtz->coe) at these P's and the dehydration of antigorite in serpentinite (often proposed to supply body force for deep exhumation).

Line 268: this statement should be supported by references

Figure 5: as the authors are well aware, there has been significant debate about the validity of thermal models that produce (hottest) temperatures of 450 C at ~2 GPa given that most exhumed HP-LT rocks preserve higher T's – see Penniston-Dorland, Kohn, England+Smye etc. The purple envelope implies a minimal role for dissipation – fine if the authors intend for this to be the case, but some justification of the model PT paths is required here

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

Dear Authors,

I enjoyed reading the revised version of your manuscript, "Earth's youngest ultrahigh-pressure eclogite formed in an immature subduction zone." I believe the revisions have substantially strengthened the contribution.

In particular, I appreciated the more explicit discussion of key limitations and uncertainties, and the overall stronger link between the analytical results and the tectonic interpretations. These revisions significantly improve the clarity and robustness of the manuscript.

Overall, I believe the manuscript now represents an important contribution to our understanding of subduction initiation and ultrahigh-pressure metamorphism, and I look forward to seeing it published in Nature Geoscience.

Best regards,
Ludovico G. Scorsolini

Reviewer #2

(Remarks to the Author)

The authors addressed the comments I raised quite nicely. They do a better job of describing the methods and associated uncertainties in this revised version of the manuscript. My main outstanding question has to do with the PT point at 1.5 GPa and 700 C (Figure 3). The main constraint on the pressure of that estimation is based on thermodynamic modeling/equilibrium phase diagrams shown in Extended Data Figure 3. The garnet isopleths are the main constraint on the P of this intersection. There is a lot riding on this – the whole interpretation that these rocks were relatively hot rides on this low-pressure estimate! In particular, the phase equilibrium diagram predicts the presence of amphibole under those conditions. Mafic rocks at those PT conditions would probably more likely be amphibolites, not eclogites. In and of itself, this is not a problem, however the authors only report inclusions of omphacite and no amphibole in the core of the garnets. It makes me wonder if the garnet originally grew at higher pressure, beyond the stability of amphibole, at the temperatures recorded by the rutile. Perhaps the modeling is not accurate or there was a resetting of the major-element composition of the core of the garnet at the high temperatures reported here (which I think are well-recorded by the rutile and the presence of melt inclusions). I think this would be an important point to address in the text - could the intersection have been at a higher-pressure? Why are there no amphibole inclusions, only omphacite, in the garnet cores?

Reviewer #3

(Remarks to the Author)

I have read the revised version of the manuscript and find that the authors have satisfactorily addressed my concerns regarding derivation of the PT path. In this regard, the paper is ready for publication.

However, the authors have not, in my opinion, shown that the 'dog-legged' prograde PT path of the UHP eclogite is unequivocal evidence for immature subduction. They have added a generic statement that other heat sources may be important, but there is no demonstration showing that their proposed mechanism is THE explanation:

"Alternatively, it has been suggested that thermal models of mature subduction zones may neglect important heat sources such as shear heating, based on the observation that subduction-related rocks often record warmer conditions than models predict³³⁻³⁵, but the magnitude at HP–UHP depths remains debated³."

Further, this statement is inaccurate. The depth range they need to assess the possibility of additional heating is <45 km (or P ~1.5 GPa), shallower than those relevant to HP-UHP metamorphism. This is important as the thermal evolution of the plate interface is controlled by very different processes over this depth range (i.e. the plate convergence rate is predominantly accommodated by earthquakes) compared to those operating on the interface at greater depths (i.e. slab-wedge interface where convergence is accommodated predominantly by flow in the mantle wedge).

My hope was that the authors would include some independent constraint on the age of onset of subduction or a calculation showing the plausible range of prograde PT paths with dissipation included and how an immature subduction zone fits their data best.

I would like to see this paper published; the PT path alone is of very high quality and will be of wide interest, but I am concerned that the strength of the paper's main claim (that UHP metamorphic rocks preferentially form and are exhumed from immature subduction zones), stated in the title and conclusions, is not currently supported by the data.

I think the paper would be strengthened by toning down or removing the linkage to immature subduction (unless additional data can be included to unequivocally make the case for this hypothesis) and focusing on what the data DO show: that a UHP rock (one of the youngest on the planet) attained such conditions along a PT path with a shallow segment along which dT/dP exceeded that derived from dT_{max}/dP_{max}. This shows that use of dT_{max}/dP_{max} for UHP rocks (as is done in several, heavily-cited compilations of metamorphic PT data to infer onset and evolution of plate tectonic regime) may lead to spurious conclusions. This is an important finding worthy of publication in Nature Geoscience.

Another, more minor issue: on lines 217-219 it is written: "Rocks preferentially exhumed during the incipient or terminal stages of subduction⁴⁰, whereby slab temperatures are higher due to, respectively, mantle wedge flow extending to shallower slab depth or slab-stalling-related warming⁴¹". Please note that the dynamical reason for this statement is lacking. Hotter eclogites are likely to be denser than colder equivalents (Kohn et al.); the main dynamical reason behind exhumation is the removal of shear tractions (during typical subduction these far exceed available buoyancy force of rocks under HP/UHP conditions). However, this hypothesis demands a cessation in subduction. This should be clarified.

Version 2:

Reviewer comments:

Reviewer #2

(Remarks to the Author)

I have no further comments for the authors.

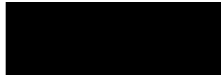
Best wishes - Sarah Penniston-Dorland

Reviewer #3

(Remarks to the Author)

The authors have improved the paper by addressing my prior concerns over the geodynamic interpretation of the PT evolution. I think it's pretty much ready for publication. One suggestion: the PT path is interpreted/presented as recording 'non-steady-state' subduction, which I don't think needs to be the case. The reason is that thermal gradients are expected to be high on the plate-plate interface (where the relative motion is accommodated by earthquakes) and low on the slab-mantle wedge (where the motion is accommodated by flow in the mantle wedge); the garnet core and mantle PT constraints are consistent with this -- no need to invoke 'non-steady-state subduction' to explain the shape of the PT path.

Hope these comments are useful -- I enjoyed reviewing the paper.

 Reviewer name redacted.

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Point-by-Point Response

Reviewer #1 (L.G. Scorsolini)

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The interpretations presented by the authors are well supported by the data, and the core conclusions are of broad interest and suitable for publication in Nature Geoscience. The manuscript is also conceptually engaging, particularly in the way multiple analytical approaches are integrated to address the thermal evolution of subduction systems.

However, several aspects of the presentation and discussion could be clarified or slightly expanded to improve robustness of the interpretations within the constraints of the short format.

[Response: Many thanks. Please find our response to individual points below.](#)

R1.1: While the manuscript focuses primarily on constraining the early thermal state of the subducting slab, it would be helpful to briefly clarify why the garnet rim was not included in the phase equilibrium modelling. A short explanation would help readers assess whether the rim was excluded because it records later stages of the P–T evolution that are not central to the study, or because of methodological challenges related to phase equilibrium modelling.

[Response: It is actually a combination of both. The garnet isopleths for garnet mantle formation are already very insensitive to P-T changes and thus, modelling for the garnet rim would become even more complex, and very speculative, considering the many assumptions to be made \(effective bulk-rock composition, exact formation conditions of the mantle, melt volume, melt extraction, etc.\). Thus, and because the rim formation conditions are also not central to the study, we decided not to try to model rim formation conditions and solely report in the original submission that “Following garnet mantle formation, pressure conditions decreased considering the increasing grossular and constant pyrope contents in the garnet rim \(Extended Data Fig. 3\)”. In the revised version, we added the explanation “Specific conditions for garnet rim formation, however, have not been determined due to modelling limitations already observed for the mantle and also because this later stage of garnet formation is beyond the scope of this study” \(lines 191-194 in tracked changes version\).](#)

R1.2: The authors use the zircon age range of 5.1–4.4 Ma to infer burial from ~1.5 GPa to >2.9 GPa, linking the oldest zircon growth to garnet core formation and the younger ages to later stages of garnet growth. While this interpretation is consistent with the zircon trace-element compositions, it relies on zircon separates that lack direct petrographic relationships with specific garnet growth zones. It would therefore strengthen the manuscript if the authors briefly acknowledged the uncertainty in linking zircon growth directly to specific stages of garnet

growth. This is relevant because the inferred timing and duration of the prograde P–T–t path, as well as the calculated burial rates, depend on this temporal linkage. It may also be worth considering whether alternative scenarios—for example zircon growth during a short residence interval at high or ultrahigh pressure—could contribute to the observed age dispersion, and how such possibilities, if plausible, might affect the interpretation.

Response: The correlation of zircon HREE and U–Pb dates and the continuous trend from 5.1 to 4.4 Ma (Fig. 4b) call against zircon crystallization at a short interval at specific PT conditions, but is in line with our interpretation of continuous growth, which also makes sense at suprasolidus conditions. It is important to note here that the burial rates from ~1.5 GPa to >2.9 GPa are a minimum estimate, because i) we only know that the first zircon formed at plagioclase-absent conditions which might be during garnet core formation or slightly before (lines 253–256 in tracked changes versions), and ii) the 2.9 GPa for garnet mantle formation is a minimum estimate. Thus, in any case, the interpretation of burial rates >60 mm/yr are robust. In the revised version, we briefly acknowledged the uncertainty by adding the sentence “*Although zircon was analyzed from separates, the zircon trace-element composition can be used to link zircon dates to garnet growth zones with reasonable certainty*” (lines 251–253 in tracked changes version).

R1.3: Lines 204–205: The manuscript states that the garnet core formed under “warmer conditions” than the garnet mantle. Because the absolute temperatures inferred for the mantle are higher than those recorded by the core, replacing the term “warmer conditions” with “higher geothermal gradients” would improve clarity.

Response: Done (line 221 in tracked changes version). To avoid confusion, we also decided to change the title from “*A cold rock born hot: Earth’s youngest ultrahigh-pressure eclogite*” to “*Earth’s youngest ultrahigh-pressure eclogite formed in an immature subduction zone*”.

Reviewer #2 (S. Penniston-Dorland)

Overall, I found this manuscript to be well-written and well-thought out.

Response: Many thanks. Please find our response to individual points below.

R2.1: The authors state that they are challenging a paradigm of UHP rocks being unequivocal evidence for a mature, self-sustained, steady-state, cold, modern-style subduction (lines 76–78). The authors need to more clearly establish that this is a well-accepted paradigm. As it stands, currently they explain the reasoning behind this paradigm in lines 50–53, but do not include any citations indicating that this is what the community follows as its paradigm. This could be explained better and should include references cited in which others have stated this paradigm/assumption.

Response: This paradigm occurs in several publications, sometimes rather vague, sometimes more clearly stated. One good example is the review paper of Liou et al. (2009), who state in the abstract that “*UHP metamorphic assemblages require relatively cold lithospheric subduction to mantle depths*”. As described in the manuscript, this is because peak P–T

conditions imply cold geothermal gradients but we actually don't know if the peak P-T conditions were reached via the blueschist and low-T eclogite facies, because reports of the prograde path of UHP rocks are very scarce. We think this is well explained in lines 49-56 (tracked changes version) but we add the citation to the Liou et al. (2009) paper in line 56. In addition, we toned down the last sentence of the introduction by not claiming that this is a well-established paradigm but stating that our study shows that peak T/P conditions of UHP rocks are not indicative for the prograde path and thus, the geodynamic setting of formation (lines 77-80 in tracked changes version).

R2.2: I think a clearer distinction of what information are model predictions/estimations for modern subduction systems and evidence from exhumed HP & UHP rocks would be helpful. For example, line 43 needs to more clearly state the basis for this assertion. e.g., "Model predictions of the thermal structure of mature, modern-style subduction zones indicate that they are characterized by...." Another example is in line 206 – instead of "...conditions agree with the thermal structure of mature..." say "...conditions agree with the model predictions (or estimations) of the thermal structure of mature..."

Response: Done (lines 45 & 222 in tracked changes version).

R2.3: I think that the authors could do a better job of explaining the methods used and its uncertainties. I am not a U-Pb specialist nor am I a melt inclusion specialist. I do have experience with Zr-in-rutile measurements, and I found the representation/documentation of how those data were handled was not complete. It makes me wonder about whether the details of the other methods need more explanation as well. In the end, I think my criticisms (below) will not impact the overall outcome and interpretation of the study, but I think that the authors could more clearly represent the data (how many analyses) and its uncertainty (what is the variability and the analytical uncertainty).

I think it is important to represent that the rutile Zr contents from the garnet core are based on just 2 grains (n=2) while n=13 rutile inclusions in the garnet mantle were analyzed in the main body of the text. In the methods, please also describe whether rutile grains were homogeneous or chemically zoned with respect to Zr content. Please describe the approach to choosing the location of analytical spots within the rutile grains (e.g., were they always in the center of grains).

Lines 129-130 – what are the +/- values based on? I did a quick calculation of the standard deviation of the population of mantle rutile Zr contents, and it is 45 ppm, not 13. Is the uncertainty described here the analytical uncertainty? Why was this used instead of the variability of the population itself? I don't see anywhere in the methods where the analytical uncertainty (and how it was assessed) is reported.

Response: The number of rutile analyses have been added in the main text in lines 135 & 136. Only two inclusions have been analyzed in the core because the core is rather small and very rarely a random cut exposes a rutile inclusion in the core that is large enough to be analyzed. Due to the small size of the rutile inclusions analyzed (~7–15 μm), the inclusions were only analyzed at the center with a focused beam (this has been added to the methods section in line

473 of the tracked changes version). The uncertainty is based on the standard error of the mean, i.e. standard deviation divided by the square root of the number of observations, which has also been added for clarity in lines 475-476 of the tracked changes version.

In addition, we added the uncertainty of the oldest and youngest zircon date in line 203 of the tracked changes version, and note in the method section that this uncertainty corresponds to two times the standard error (lines 571-572 in tracked changes version).

R2.4: A lot of significance is attached to the finding of coesite in the garnet mantle zones. This is justified because it is an indicator of an ultra-high pressure history experienced by a rock. There is some literature that attributes the presence of high-pressure minerals to overpressure effects rather than a record of lithostatic pressure (e.g., Schmalholz and Podlachikov, 2013; Tajcmanova et al., 2015). It is a question that I think has not been fully resolved and deserves some discussion in this manuscript as to why the authors think that it does not explain the presence of coesite in these rocks. The garnet crystals also contain cristobalite, which is stable at very high temperatures (above any temperatures inferred to have been reached by these samples) and relatively low pressures. I think it is worth briefly describing what the meaning of these cristobalite inclusions is and why their presence does not tell us about a high-temperature history.

Response: The role of tectonic overpressure is indeed an unresolved topic. Although we know that pressure can deviate from lithostatic pressure, even after two decades of research we still don't know the potential magnitude of this effect (see Tajcmanova et al. 2021, *Elements*), which is an uncertainty affecting every study that relates pressure to burial depth. We think that discussing this topic is beyond the scope of our study. The only point where it becomes relevant is our estimation of subduction rates, whereby we clearly state that this estimation only holds under the assumption of lithostatic pressure (lines 262-263 in tracked changes version), which has now also been added to the abstract (line 31 in tracked changes version).

Regarding cristobalite, note that it never occurs as monomineralic inclusion, but it is always associated with at least one feldspar polymorph and phyllosilicate in a nanogranitoid (a droplet of anatectic melt now crystallized as a multiphase inclusion, as supported by the fact they can be re-homogenized and as detailed in the text at lines 141-143). Under these conditions, cristobalite and other feldspar polymorphs crystallize as metastable phases well outside their stability fields. This is extensively documented in many case studies where cristobalite forms at variable pressure (from Low-P to UHP) and temperatures well below the cristobalite stability field, even at $T < 750\text{ }^{\circ}\text{C}$ (see references in the text and also Carvalho et al., 2025, *J. Pet.*). In a nutshell, the fact that the melt is encased in garnet creates peculiar conditions on cooling which lead to supersaturation and fast crystallization of a melt droplet, a condition favoring the formation of simpler mineral phases with respect to their more common counterparts (see references in the text and also Ferrero & Angel, 2018, *J. Pet.*) as also supported by the Goldsmith's principle of simplicity (Goldsmith, 1953, *J. Geol.*). To make this clearer for the reader, we added a sentence in lines 144-146 of the tracked changes version.

R2.5: Line 205 – the actual temperature is cooler not warmer in the garnet core compared to its mantle – maybe say “warmer geothermal gradient” or something else to indicate this.

Response: Done (line 221 in tracked changes version). To avoid confusion, we also decided to change the title from “*A cold rock born hot: Earth’s youngest ultrahigh-pressure eclogite*” to “*Earth’s youngest ultrahigh-pressure eclogite formed in an immature subduction zone*”.

R2.6: Lines 243-253 – I am wondering about the comparison illustrated in Figure 5b between the P-T conditions recorded by rocks and the model from Holt and Condit (2021). The time difference predicted by the model for the progression from the P-T conditions of the garnet core to the garnet mantle is from 6.2 Myr (core) to somewhere around 8.8-11.9 Myr (mantle), which is a time range of 2.6 to 5.6 Myr. This span of time is longer than the range of zircon ages within the garnet permits (5.1 to 3.1 Ma is a total of 2 Myr). I think some discussion of this is warranted – this could take the form of acknowledging the uncertainty of such a comparison and recognition that the numbers should not be directly compared. Another solution might be to remove the actual ages of the model P-T paths and just indicate early to late with an arrow.

Response: Yes, the numerically modelled and observed paths are not matching exactly, which is expected given the idealized, non-site-specific nature of the model. We agree that the timescales implied by the model are not expected to match the zircon age range exactly, and that these values should not be interpreted as a direct one-to-one comparison with the natural data. The important point is that the overall shape and position of the P-T path largely agree and that the model captures a plausible first-order rate of thermal evolution in a young, immature subduction zone. This gives us confidence in the applicability of the model to such a young subduction zone. We now elaborate on this and the limitations in lines 280-287 of the tracked changes version.

R2.7: The literature citation focuses on a few select studies and leaves out others that are relevant, and in some cases the papers where an idea was originated. Below are a couple of examples of literature that should be included (and yes, they are references to my papers, because they are ones I am most aware of that have been excluded!)

Line 214–Penniston-Dorland et al. (2025, EPSL) is another citation that should be added here.

Line 260 –Add in reference to Penniston-Dorland et al (2015, EPSL) here.

Response: Agree. Both references have been added.

Reviewer #3 ([REDACTED] Reviewer name redacted.

Schönig and coauthors present an interesting and provocative investigation of a remarkably young eclogite sample from Papua New Guinea. They make the case that the sample attained ultrahigh pressure (UHP) conditions (coesite stability) around 4-5 Ma and claim it represents the youngest example of UHP eclogite on Earth. The meat of the paper is in the derivation of a segment of the sample’s prograde PTt trajectory that transits a region of PT space not typically associated with subduction (1.5 GPa, 700 C). This observation is interpreted with published numerical models of subduction in which time-dependence is included. The authors conclude that the Papua New Guinea UHP eclogite formed during young subduction before a colder

thermal architecture could be established. They speculate that examples of such young subduction may be overlooked in the rock record with implications for plate tectonic evolution.

I enjoyed reading the paper and found the topic, data and tectonic setting of the sample to be especially compelling and of potential interest to the broad readership Nature Geoscience commands.

[Response: Many thanks. Please find our response to individual points below.](#)

R3.1: Thermal evolution of the subduction interface: the paper is a little one-dimensional in its assessment of possible explanations for the PT conditions of garnet core growth (~1.5 GPa, 700 °C central to the take-home message). Alternative mechanisms to ageing of the subduction zone should be considered (downgoing plate age, dissipation, 3D effects of the subduction geometry etc). I raise this not to say that I disagree with the authors, but more as a suggestion to improve confidence in the conclusions. Related to this, the comparison between the PT path and the thermal model on which the conclusions rest is underdeveloped; for instance, Figure 5 shows a temporal evolution of calculated PT paths spanning a much longer timescale than that proposed to separate the timing of garnet core and rim growth. Are other parameters used in the model suitable to enable such a comparison?

[Response: We agree and included a short discussion about other potential scenarios that could explain the offset between garnet core formation conditions and numerical models of mature subduction zones. First, different ages of the downgoing slab are already considered in the thermal models of mature subduction zones and so we find this factor insufficient to explain the offset. This is now more clearly described in lines 224-226 of the tracked changes version. Second, we now also discuss shear heating \(dissipation\), and how it alone is insufficient to explain the offset \(lines 230-235 of the tracked changes version\). We therefore interpret a young, immature subduction zone as the best explanation for the observed prograde P-T path.](#)

[For the model comparison, please see our response to R2.6, where we note that the modelled timescales are not intended to match the zircon age range directly, but rather to provide first-order context for the inferred P-T path.](#)

R3.2: Interpretation of the PTt path: it is argued that because the oldest zircon dates lack an Eu-anomaly, they record the timing of garnet core growth at ~1.5 GPa and ~700 C. However, ED Fig. 2 indicates that plagioclase has a restricted stability field and is limited to $P < 1$ GPa (or $T > 750$ C). This implies that the absence of an Eu-anomaly isn't a particularly diagnostic indicator of eclogite-facies conditions for this rock. The oldest zircon could have equilibrated at lower-grade PT conditions, further shortening the time available for the PT loop. There are a few more issues here: i) the PT conditions of garnet core growth don't seem to agree with the 'garnet-in' line on ED Fig. 2; ii) the calculated mineral assemblage at PT conditions of garnet core equilibration contains amphibole and biotite—this implies that the rock would more likely be a garnet amphibolite, not an eclogite. I raise this not to be pedantic, but to offer the suggestion that under such PT conditions there are plausibly a wider range of rock types associated with subduction than eclogites and blueschists.

Response: Thanks for raising this point. We agree that zircon growth in the absence of plagioclase might slightly pre-date garnet core formation at lower P. As you noted, this would further shorten the time for the prograde path and thus, does not affect our burial estimate of > 60 mm/yr. In the revised version, we now only state that the absence of a negative Eu-Anomaly indicates the absence of plagioclase during zircon crystallization (e.g. line 254 in tracked changes versions) and that we interpret the 5.1 Ma date as a maximum age of garnet core formation (line 256 in tracked changes versions).

Yes, the garnet core formation conditions are above the thermodynamically predicted “garnet-in” isograd. Thus, the reaction was either overstepped or earlier formed garnet was resorbed. We added a sentence describing this issue in lines 173-176 of the tracked changes versions.

R3.3: Zircon HREE(t) trend: the decrease in HREE concentration with zircon age could also be accounted for by melt removal (as opposed to garnet crystallization). Two observations seem to support this: i) the reported absence of zircon in the matrix, and ii) the lack of progressive MREE/HREE fractionation with zircon age suggests bulk depletion of HREE from the system. If this interpretation is accurate, then garnet grew during heating, not necessarily compression, which has implications for the reported rates of subduction.

Response: We agree that melt extraction could have contributed to the depletion in HREE in zircon from 5.1 to 4.4 Ma. Nevertheless, this effect is expected subordinate mainly because i) garnet was present and growing in the residuum during melting as evidenced by the melt inclusions and thus, HREE would have been partitioned into garnet rather than the melt (note also that HREE in zircon increase again after 4.4 Ma, which we interpret as garnet resorption); and additionally because ii) the amount of melt is predicted to be at 3-9 vol% at the core formation conditions (depending on H₂O and the temperature uncertainty of ± 20 °C) and this amount does not increase at conditions of garnet mantle formation. To generate > 9 vol% melt at 3 GPa, temperatures > 950°C would be required and we don't have any indications for such high temperatures. This is now briefly discussed in lines 256-261 of the tracked changes versions

R3.4: Line-by-line comments

Line 20: ‘mature’ is ambiguous in this context; I would have thought that subduction of continental margins such as PNG is not expected to adhere to the same rules that apply to long-lived oceanic plates

Response: Removed (line 21 in tracked changes versions).

Lines 35-43: no need to start with climate and early Earth to justify interest in subduction; suggest a clear sentence stating that subduction zones are the primary sites of chemical differentiation and host to large earthquakes and volcanoes

Response: We would like to keep this section as it is. The importance of subduction zones comprises only the first sentence. The following two sentences (lines 39-45 in tracked changes versions, a half-sentence added for clarity) are to make clear that P-T conditions of subduction-

related rocks have been used to interpret the geodynamic evolution of planet Earth, a point we challenge in our final section (lines 309-313 in tracked changes versions).

Line 77: I'm not convinced that UHP rocks alone are diagnostic indicators of these traits because thermal gradients vary with depth during subduction (typically following the progression from high to low dT/dP from the plate-plate to the slab-wedge interface); i.e. the presence of coesite in an eclogite doesn't place constraints on the prograde PT path at shallower depths.

Response: Yes, we do fully agree and the results of this study show that this is indeed not necessarily the case. We replaced the final sentence of the introduction in lines 77-80 in tracked changes versions (please see also our response to R2.1).

Line 106: few people will be familiar with dmisteinbergite and kumdykolite; helpful to include exemplar formulae to guide the reader

Response: Formulas were added to the main text (lines 130-131 in tracked changes versions).

Line 124: same comment as above for kokchetavite

Response: Formula was added to the main text (line 130 in tracked changes version).

Line 169: "In the case of temperature conditions,..." doesn't make sense; please rewrite

Response: Replaced by "With regard to temperature, ..." (line 182 in tracked changes version).

Figure 3: are the error ellipses calculated (i.e. as the interception of multiple equilibria)? If so, this should be stated in the figure caption. Also, what constrains the magnitude of the garnet mantle ellipse?

Response: The ellipses are not calculated. For the core, the ellipse simply depicts the P-T region where we have the best agreement between the different methods applied. For the mantle, we decided to assume a large uncertainty for P ranging from the quartz-to-coesite transition to maximum P in common UHP rocks (excluding some reports of extreme P conditions). The magnitude for T is where the Zr-in-Rt thermometer crosses this P range.

Line 205: "warmer" is confusing in this context as the mantle is at higher-T than the core. Presumably the authors mean that the apparent thermal gradient derived from the core PT condition is steeper (higher dT/dP) than that of the mantle.

Response: Agree. "Warmer conditions" has been replaced by "higher geothermal gradients". To avoid confusion, we also decided to change the title from "*A cold rock born hot: Earth's youngest ultrahigh-pressure eclogite*" to "*Earth's youngest ultrahigh-pressure eclogite formed in an immature subduction zone*".

Lines 214-217: this is counter intuitive: warmer slabs would lead to densification of mafic eclogites (I believe Matt Kohn raised this point in his PNAS paper on the subject). Temperature

isn't the sole control on exhumation potential (see England & Smye (2023), Smye & England (2023) for consideration of the force balance required to exhume along the slab-wedge interface, relevant to this study)

Response: Apologies, we were not clear at this point - our intention was not to say that higher temperatures promote exhumation. That rocks preferentially exhume during the early and late stage of subduction is an observation, while the higher temperature is a consequence of the timing. This is now clearer by referencing the observation and replacing "where" by "whereby" (lines 239-242 in tracked changes version). Note that the Kohn et al. (2018) and England & Smye (2023) references have been added to the main text when discussing shear heating (see response to R3.1 and lines 230-233 in tracked changes version).

Line 228: I'm not sure this is true as ED Fig. 2 indicates plagioclase isn't a stable phase across a large range of 'non-eclogitic' PT space

Response: See response to point R3.2.

Line 230: melt depletion could also explain a bulk decrease in HREE without fractionation

Response: See response to point R3.3.

Line 261: this is one interpretation, but it is worth acknowledging that there are many others such as the densification of quartz-bearing rocks (qtz->coe) at these P's and the dehydration of antigorite in serpentinite (often proposed to supply body force for deep exhumation).

Response: We agree that there are multiple such factors that likely influence exhumation from great depths. In quartz-bearing rocks, the quartz-to-coesite transition increases the density but they are still buoyant and as you note, even serpentinites can become buoyant at great depths. Thus, the question is why they are so rarely being exhumed from >80 km depth and the distinct decoupling-to-coupling transition provides a reasonable, possible explanation. Nevertheless, we agree that this is a complex topic and we don't want to provide here a review of all the factors proposed to contribute to exhumation. Thus, we decided to remove this paragraph lines (292-302 in tracked changes version), which also makes the final statement more concise and direct.

Line 268: this statement should be supported by references

Response: We deleted this sentence.

Figure 5: as the authors are well aware, there has been significant debate about the validity of thermal models that produce (hottest) temperatures of 450 C at ~2 GPa given that most exhumed HP-LT rocks preserve higher T's – see Penniston-Dorland, Kohn, England+Smye etc. The purple envelope implies a minimal role for dissipation – fine if the authors intend for this to be the case, but some justification of the model PT paths is required here.

Response: Please see our response to R2.6 and R3.1. Note that the Kohn et al. (2018) and England & Smye (2023) references have been added to the main text when discussing shear heating (lines 230-233 in tracked changes version).

Point-by-Point Response

Reviewer #1 (L.G. Scorsolini)

Dear Authors,

I enjoyed reading the revised version of your manuscript, “Earth’s youngest ultrahigh-pressure eclogite formed in an immature subduction zone.” I believe the revisions have substantially strengthened the contribution.

In particular, I appreciated the more explicit discussion of key limitations and uncertainties, and the overall stronger link between the analytical results and the tectonic interpretations. These revisions significantly improve the clarity and robustness of the manuscript.

Overall, I believe the manuscript now represents an important contribution to our understanding of subduction initiation and ultrahigh-pressure metamorphism, and I look forward to seeing it published in Nature Geoscience.

Response: Many thanks for your time efforts and valuable comments on the previous version of the manuscript, which were very helpful to improve the manuscript. We are glad that you find the manuscript acceptable for publication now.

Reviewer #2 (S. Penniston-Dorland)

The authors addressed the comments I raised quite nicely. They do a better job of describing the methods and associated uncertainties in this revised version of the manuscript.

Response: Many thanks. We are glad that you are fine with our revisions.

My main outstanding question has to do with the PT point at 1.5 GPa and 700 °C (Figure 3). The main constraint on the pressure of that estimation is based on thermodynamic modeling/equilibrium phase diagrams shown in Extended Data Figure 3. The garnet isopleths are the main constraint on the P of this intersection. There is a lot riding on this – the whole interpretation that these rocks were relatively hot rides on this low-pressure estimate! In particular, the phase equilibrium diagram predicts the presence of amphibole under those conditions. Mafic rocks at those PT conditions would probably more likely be amphibolites, not eclogites. In and of itself, this is not a problem, however the authors only report inclusions of omphacite and no amphibole in the core of the garnets. It makes me wonder if the garnet originally grew at higher pressure, beyond the stability of amphibole, at the temperatures recorded by the rutile. Perhaps the modeling is not accurate or there was a resetting of the major-element composition of the core of the garnet at the high temperatures reported here (which I think are well-recorded by the rutile and the presence of melt inclusions). I think this would be an important point to address in the text - could the intersection have been at a higher-pressure? Why are there no amphibole inclusions, only omphacite, in the garnet cores?

Response: Indeed, the thermodynamic modelling predicts that amphibole is still part of the assemblage at our best estimate for garnet core growth at 1.5 ± 0.2 GPa and 700 ± 20 °C, but we did not observe amphibole inclusions in the garnet core. However, it should be considered that, at the given conditions, garnet and clinopyroxene are growing at the expense of amphibole, i.e. amphibole is breaking down and garnet + clinopyroxene are newly forming. For instance,

at 1.6 GPa compared to 1.5 GPa, the amount of amphibole reduces by > 50 vol%, while clinopyroxene increases by > 30 vol% and garnet by ~ 10 %. At slightly higher pressure (1.7 GPa) amphibole is not part of the predicted assemblage, and this is covered by the given uncertainty of 1.5 ± 0.2 GPa. Thus, the absence of amphibole inclusions at pressure conditions of 1.5 GPa with an uncertainty of ± 0.2 GPa seems reasonable and we did not apply any changes to the manuscript.

Reviewer #3 () Reviewer name redacted.

I have read the revised version of the manuscript and find that the authors have satisfactorily addressed my concerns regarding derivation of the PT path. In this regard, the paper is ready for publication.

Response: Many thanks. We are glad that you are fine with our revisions regarding the derivation of the P-T path.

However, the authors have not, in my opinion, shown that the ‘dog-legged’ prograde PT path of the UHP eclogite is unequivocal evidence for immature subduction. They have added a generic statement that other heat sources may be important, but there is no demonstration showing that their proposed mechanism is THE explanation:

“Alternatively, it has been suggested that thermal models of mature subduction zones may neglect important heat sources such as shear heating, based on the observation that subduction-related rocks often record warmer conditions than models predict³³⁻³⁵, but the magnitude at HP–UHP depths remains debated.”

Further, this statement is inaccurate. The depth range they need to assess the possibility of additional heating is <45 km (or $P \sim 1.5$ GPa), shallower than those relevant to HP-UHP metamorphism. This is important as the thermal evolution of the plate interface is controlled by very different processes over this depth range (i.e. the plate convergence rate is predominantly accommodated by earthquakes) compared to those operating on the interface at greater depths (i.e. slab-wedge interface where convergence is accommodated predominantly by flow in the mantle wedge).

My hope was that the authors would include some independent constraint on the age of onset of subduction or a calculation showing the plausible range of prograde PT paths with dissipation included and how an immature subduction zone fits their data best.

I would like to see this paper published; the PT path alone is of very high quality and will be of wide interest, but I am concerned that the strength of the paper’s main claim (that UHP metamorphic rocks preferentially form and are exhumed from immature subduction zones), stated in the title and conclusions, is not currently supported by the data.

I think the paper would be strengthened by toning down or removing the linkage to immature subduction (unless additional data can be included to unequivocally make the case for this hypothesis) and focusing on what the data DO show: that a UHP rock (one of the youngest on the planet) attained such conditions along a PT path with a shallow segment along which dT/dP exceeded that derived from $dT_{\text{max}}/dP_{\text{max}}$. This shows that use of $dT_{\text{max}}/dP_{\text{max}}$ for UHP

rocks (as is done in several, heavily-cited compilations of metamorphic PT data to infer onset and evolution of plate tectonic regime) may lead to spurious conclusions. This is an important finding worthy of publication in Nature Geoscience.

Response: We agree, and we now included a more detailed discussion about the potential contribution of shear heating, coming to the conclusions that

“Heating due to energy dissipation during shearing and formation in a young subduction zone are both reasonable and non-mutually exclusive mechanisms to explain the high-T/P gradient observed for depths ≤ 45 km. While quantifying the relative contributions of the two mechanisms is beyond the scope of this study, our data clearly show that although the identification of UHP metamorphism indicates subduction to, and exhumation from, depths > 90 km, it is not unequivocal evidence that subduction was exclusively cold and evolved at a thermal steady-state” (lines 271-277 in the clean version of the revised manuscript)

To implement the changes:

1. The discussion about the interpretation of the zircon dates and trace-element composition has been moved to the section “U–Pb Dating and Composition of Zircon Inclusions” (lines 199-218 in the clean version of the revised manuscript) to make the discussion in the following section clearer.
2. The section “Non-Steady-State Regime during Incipient Subduction” has been renamed to “Non-Steady-State Thermal Regime during Subduction” to not focus on a just one mechanism.
3. Figure 5 have been revised, now showing individually the comparison of the observed P-T path to (a) thermal models of mature subduction zones without considering shear heating, (b) with shear heating considered, and (c) with age of the subduction zone considered. Accordingly, we have added a discussion about the effect of shear heating (lines 236-247 in the clean version of the revised manuscript)
4. As a result, we toned down the “immature subduction” part and focus on the observed change in T/P during subduction (either be explained by shear heating or immature subduction or a combination), concluding that peak conditions alone are insufficient to reconstruct the thermal structure of a subduction zone
5. Accordingly, we changed the title of the manuscript to “Earth’s youngest coesite eclogite reveals geothermal gradient changes during subduction”, revised the last part of the abstract (lines 30-34 in the clean version of the revised manuscript), and revised the last part of the introduction (lines 72-75 in the clean version of the revised manuscript)

Another, more minor issue: on lines 217-219 it is written: “Rocks preferentially exhume during the incipient or terminal stages of subduction⁴⁰, whereby slab temperatures are higher due to, respectively, mantle wedge flow extending to shallower slab depth or slab-stalling-related warming⁴¹”. Please note that the dynamical reason for this statement is lacking. Hotter eclogites are likely to be denser than colder equivalents (Kohn et al); the main dynamical reason behind exhumation is the removal of shear tractions (during typical subduction these far exceed

available buoyancy force of rocks under HP/UHP conditions). However, this hypothesis demands a cessation in subduction. This should be clarified.

Response: This part has been removed.

Point-by-Point Response

Reviewer #2 (S. Penniston-Dorland)

I have no further comments for the authors.

Response: Many thanks for reviewing our manuscript. We are glad that you are fine with our revisions.

Reviewer #3 ([REDACTED] Reviewer name redacted.

The authors have improved the paper by addressing my prior concerns over the geodynamic interpretation of the PTt evolution. I think it's pretty much ready for publication.

Response: Many thanks for your time. We strongly appreciate your comments on previous versions of the manuscript, which have been very helpful to improve this work. We are glad that you are fine with our revisions regarding the geodynamic interpretation.

One suggestion: the PTt path is interpreted/presented as recording 'non-steady-state' subduction, which I don't think needs to be the case. The reason is that thermal gradients are expected to be high on the plate-plate interface (where the relative motion is accommodated by earthquakes) and low on the slab-mantle wedge (where the motion is accommodated by flow in the mantle wedge); the garnet core and mantle PT constrains are consistent with this -- no need to invoke 'non-steady-state subduction' to explain the shape of the PT path.

Response: We agree that the 'non-steady-state' term does not correspond to the potential mechanism of shear heating. In the manuscript, we clearly differentiate between the two potential mechanisms to explain the P–T–t path, which is shear heating and formation in a young subduction zone. In the latter, T/P gradients are obviously time dependent, which means that a quasi-steady-state is not yet reached. Thus, we stay with the 'non-steady-state' term in the descriptions of the potential formation in a young subduction zone in the main text of the manuscript. The only point where we did not separate it clearly from the shear heating mechanism is the heading of our discussion section “Non-Steady-State Thermal Regime during Subduction”. We changed this heading to “Geothermal Gradient Change during Subduction”, which is now more descriptive and does not link to a specific mechanism to explain the P–T–t path.