TEKTONIKA

Review Report

Mulder et al., Zircon Inheritance Refines the Cambrian Orogenic Architecture of Southeast Australia, TEKTONIKA, 2025.

Table of Contents

1st Round of Revisions

Decision letter

Dear Jacob Mulder, Jacqueline Halpin, Laura Morrissey, Yousef Zoleikhaei, John Everard, Sebastien Meffre, Mike Hall, Oliver Nebel, Peter Cawood:

Many apologies for the delay in our review process. We have now reached a decision regarding your submission to τεκτονικa, "Inherited zircon mining refines the Cambrian orogenic architecture of southeast Australia".

Our decision is: Revisions Required

The two reviews received praise highly the manuscript and from my own perusal I agree that the paper is well-written, -argued, and -illustrated. Both reviewers highlight some minor revisions that will help improve the paper, and I would particularly draw your attention to the comments that the manuscript could be made more accessible to the general reader who may be unfamiliar with the region.

We thank you for supporting Tektonika and chosing to submit to us, and look forward to receiving your revised manuscript. Please do not hesitate to ask if you have any questions.

Kind regards,

Craig Magee

Comments by Reviewer A (Dylan Vasey) and author response

I have completed a review of the manuscript "Inherited zircon mining refines the Cambrian orogenic architecture of southeast Australia" by Mulder et al. In this study, the authors present zircon U-Pb and Hf isotopic data from Devonian granitoids in southeast Australia interpreted to have inherited older zircons from the Proterozoic-Cambrian rocks they intrude. These data are supplemented with pseudosection modeling designed to assess the melt fertility of the source rocks. The authors argue that their results indicate that the Selwyn Block, a section of largely unexposed crust inferred from seismic data, does not have the same composition and provenance as the Western Tasmania Terrane, to which it is often compared. They contend that the Selwyn Block is likely a Cambrian intra-oceanic arc that can be incorporated into a coherent history of early Paleozoic accretionary tectonics in southeast Australia.

The data appear to be high quality and suitable for publication in *Tektonika*, though I am not a specialist in the underlying analytical methods. My expertise is in convergent margin tectonics and the synthesis of datasets such as these to interpret tectonic histories, though I work primarily in the Alpine-Himalayan belt. I enjoyed the opportunity to learn more about the Proterozoic-Paleozoic geology of southeast Australia, and I think the data collected reveal new insights into a complex tectonic history. The manuscript is clearly motivated, well written, and organized, with figures that effectively support the main argument. As such, I am recommending relatively minor revisions for this manuscript prior to publication. I outline two main concerns below and then include some specific comments in reference to line numbers in the text and to the figures.

I hope the authors find my comments constructive and useful, and I would be happy to answer any additional questions they might have.

All the best,

Dylan Vasey

Main Concerns

1) The background and discussion contain a lot of detail and region-specific jargon, and at times it is difficult to clearly see how information presented in the background will become relevant later in the study. The manuscript would benefit from shortening and streamlining the background to indicate why information is being presented as it is introduced.

We have streamlined the introduction and background of the manuscript to remove unnecessary details of the local geology and to make the study more accessible to an

international audience. Specific edits are detailed below.

2) I am a little confused about the motivating assertion that accreted terranes are rare in the Tasmanides, given that the entire system is interpreted as forming due to Paleozoic accretion of material onto Gondwana. I think the authors may be saying that since exposure is poor, the overall makeup of the orogenic belt is not well understood? One problem in general with the "terrane" concept is that it creates an expectation that accretion is a discrete processes in which distinct "blocks" are collaged together along sutures, but there is no requirement that every accretionary system have a large number of distinct, recognizable domains. I would suggest tweaking this motivating idea to emphasize the exposure problem (which the inherited zircon approach nicely addresses and could address in other systems), rather than an arbitrary need to find more terranes in the Tasmanides.

We clarify that accreted terranes are rare in the Tasmanides due to generally poor exposure and also highlight the likelihood that much of the exposed geology of the Tasmanides was deposited in a back arc setting. The importance of identifying accreted terrane is retained in the text because most tectonic models for the Tasmanides (e.g., Collins et al,. 2002; Cawood, 2005; Cayley et al., 2011; cited) hypothesise that terrane accretion triggered periods of plate convergence resulting in regional orogenesis, magmatism, and outward growth of the continental margin.

We remove the comparison to other accretionary orogens to prevent the impression that that the paper is arbitrarily trying to find more of these terranes or that one should expect a comparable number of accreted terranes in the Tasmanides relative to the examples given (Altaids, Cordillera).

Specific Comments

Line 1: The word "mining" (especially when followed by the word "refines") has an industrial connotation in a geology manuscript that may miscue the reader (especially if not a native English speaker) to expect direct economic applications. I would suggest removing this word from the title.

Removed 'mining' from text and title.

Line 22: Why not give the exact n rather than saying that it is ">800"?

Exact number is now given.

Line 30-31: This sentence does not make it clear whether Hf and Sr isotopic data are new to this study and/or compiled from previous work.

Clarified that Sr isotope data is previously published and zircon Hf data is new.

Line 39: I will try not to belabor this point, which reflects my own stylistic preferences, but I find "continental and oceanic terranes" to be unnecessarily vague. If the authors mean tectonic features such as microcontinents, island arcs, seamounts, etc., it is more informative to just state those features rather than describe them as "terranes," which is often applied haphazardly to just about any fault-bounded block of crust.

We have revised the text to more specifically define 'accreted terrane' as used in our study. This includes highlighting the different types of accreted terrane (seamounts, arcs, microcontinents, etc.), that accreted terranes are transferred from the subducting plate to the overriding plate, and that this accreted material can be delineated as mappable, fault-bound 'terranes'.

Lines 52-53: What makes the Tasmanides archetypal, as opposed to active retreating systems in the Mediterranean, for example?

Removed archetypal.

Line 54: Is it the Pacific in the Paleozoic?

Clarified.

Line 57: How long is "long-term?"

Clarified to emphasise that rollback is the dominant tectonic mode of this orogen, rather than operating for a specific time span.

Lines 58-59: Is it "outward growth of continental crust" or "subduction zone advance" that is "triggered by terrane accretion?"

Clarified.

Lines 59-68: I am a little confused about the assertion that there are few accreted terranes in the Tasmanides if the entire orogenic belt is interpreted as accretionary. What makes up the Tasmanides if not accreted terranes? This reflects in part I think my general misgivings about different meanings ascribed to the word "terrane."

We have clarified this point by providing a more specific definition of accreted terrane as used in this study at the start of the introduction. We also highlight that the majority of the exposed geology of the Tasmanides is inferred to have been deposited in a back-arc setting. This geology does not fit our definition of accreted terrane, hence highlighting the importance of studying true accreted terranes, such as the Selwyn Block.

Line 74: What about the seismic data indicates the Selwyn Block is a "terrane?" I would instead emphasize the geophysical observation rather than this interpretation.

Removed 'terrane' and text now focuses on the seismic character of the Selwyn Block.

Lines 75-80: It would be helpful to have some indication here of how it specifically impacts Paleozoic Australian tectonics?

Clarified.

Line 86: Accreted when?

Clarified that accretion is inferred to have occurred during the early Paleozoic. Existing models advocate for accretion sometime between the Cambrian and Silurian but the specific timing is debated and is not essential for the new tectonic model presented in this paper. In the interest of space, we avoid presenting a detailed discussion of the timing of accretion of the Selwyn Block.

Line 104: As with the title, I would suggest using a word other than "mining," given the industrial connotation.

Removed ‘mining’.

Lines 115-130: Similar to “terranes,” the terms “orogen” and “orogeny” start to become confusing rather than helpful when they are used to mean different things. In this case, the Lachlan “Orogen” (i.e., mountain belt) is described as experiencing multiple “orogenies” (i.e., deformational episodes). I know this terminology is hard to avoid because it is often deeply embedded in the historical literature, but I would suggest using more descriptive terms where possible. Capitalization of “orogen” when it follows “Lachlan” also seems to be inconsistent in this section.

We maintain the use of ‘orogeny’ and ‘orogen’ to delineate the tectonic event and the tract of crust it affects, respectively. The orogenies in the Tasmandies are not simple deformational events but are accompanied by metamorphism, magmatism, and sedimentation, thus ‘orogeny’ is appropriate. Accordingly, the tracts of crust that experienced these spatially and temporally distinct orogenies define separate orogens, so we maintain the latter.

We have updated the capitalisation of orogen to be consistent throughout.

Lines 153-178: The details of the Tyennan Orogeny are a bit challenging to follow here without a figure – perhaps a simplified version of Figure 6 showing the broad strokes of this collision and where the features mentioned are located early in the manuscript would be useful.

We have reduced the text on the Tyennan Orogeny in this section as it is outlined in more detail with reference to Figure 6 in the discussion.

Lines 160-162: Cite Figures 1 and/or 2 for ophiolite locations.

Western Tasmania Terrane ophiolites and their ages are now highlighted on figure 2.

Lines 162-164: It is a little hard to tell from Figures 1 and/or 2 where these high-strain (and high P/T) metamorphic complexes are.

The location of the metamorphic complexes is no longer relevant after streamlining the introduction and background text. The significance of the high-P complexes is outlined in the discussion section with reference to Figure 6.

Lines 180-182: The introduction of a second Paleozoic orogeny here increases the need for an early figure that shows these tectonic events in broad strokes, for the reader who is not already familiar with Australian tectonics in the Paleozoic.

The second orogeny (Tabberabberan orogeny) is only briefly discussed in the text to provide context for the generation and emplacement of the Devonian granitoids. The spatial context provided in Figure 1 and the tectonic context outlined in the introduction is sufficient to convey the point that this orogeny is one of several that are recorded in the Tasmanides. The specific details of the tectonic framework and evolution of the Tabberabberan orogeny is not essential to this paper as our focus is on the preceding Cambrian orogenic history and architecture.

Lines 186-187: Does the Carboniferous King Island granitoid need to be mentioned or explained away at all? I'm not sure it's relevant to the present study.

The absolute age of this granitoid is in the Carboniferous period but it was emplaced in the same event as the remaining Devonian (*sensu stricto)* granitoids in the Western Tasmania Terrane. We have removed reference to its Carboniferous age to streamline the text.

Lines 191-193: Is the peraluminous chemistry and mineralogy of these 3 specific granites going to become relevant later? If not, I think this could be cut. See main point about streamlining the background.

Cut as suggested.

Lines 197-199: I would add labels for the Heathcote/Governor faults to Figure 1, as well as a label for the Tabberabbera Zone to Figure 1A.

Done

Lines 201-203: I'm a little unsure of which seismic layer is which on Figure 1B. Is Togari upper, Rocky Cape middle, and Pre-Mesoproterozoic? lower? I'd suggest labeling these on the cross-section directly.

Figure and legend updated to include letters designating the inferred Togari, Rocky

Cape, and basement units within Selwyn Block

Lines 209-212: Despite the annotation, I am not clearly seeing in Figure 1C a lineament connecting King Island with Phillip Island. Perhaps the authors could add a dashed line to show its interpreted location.

Added magnetic lineament as inferred by Moore et al. (2016).

Lines 271-279: I am not sure there is additional essential information in this paragraph not already covered in the previous discussion of the Tabberaberan Orogeny, and it could potentially be cut to shorten the background.

We have shortened this text but retain the paragraph to reinforce that granitoids in the Melbourne Zone and Western Tasmania Terrane were emplaced during the same orogenic event.

Line 281: Typo – “Stratergy"

Fixed.

Lines 297-299: I would add an explanation of the abbreviations for samples/granitoids in Figure 2 to the blank space below Figure 2D, so the reader doesn’t need to go to the supplement to follow along.

Abbreviations are added below panel D as suggested.

Lines 303-305: Are the previously published data also shown on Figures 2B and 2C? If so, perhaps use different colored circles or some other means to distinguish them from new data. If not, they should be added (with distinctive symbology).

The number of inherited zircons compiled from the literature is now cited below panel D.

Lines 373-390: The white melt % curves in Figure 3 are difficult to read on the light blue

background.

Line colour changed.

Lines 397-398: Labeling these and similar peaks (or spans of ages) on Figure 4 would be useful – they can be a little challenging to eyeball at this scale.

Age peaks are now labelled.

Lines 399-409: The assertions about Meso- vs. Neoproterozoic strata are hard to evaluate in Figure 4, since they are already combined. Do they need to be discussed? What would be more useful would be to include in the figure a curve for Adelaidean strata to demonstrate that the Western Tasmania signature is distinctive.

Detrital zircon ages from the Adelaidean superbasin are now included in figure 4 as suggested.

Line 424-426: I would be inclined to make Supplementary Files 4 and 5 main text figures – the manuscript only has 6 figures and it is useful to be able to see the data from individual samples. The color scheme would need adjusting so that Western Tasmania vs. Melbourne isn't using the same dichotomy as S-type vs. I-type.

The results from individual plutons are summarised in the cumulative distribution plot in Figure 4C, which is a more compact summary of the age distribution than plotting KDEs of individual plutons. We have added the I- vs S-type designation to granitoids on Figure 1C to capture the information conveyed in supplementary files 4 and 5. We prefer to keep the individual KDE plots as supplementary files in the interest of space and brevity.

Lines 500-501: Are there data that indicate that local strata have 1300-1000 Ma or 500 Ma grains? If so, that should be stated explicitly.

We have cited Habib et al, 2023, which presents detrital zircon data from Paleozoic strata in the Western Tasmania Terrane and demonstrates that they contain 1300-1000 and 500 Ma age populations. Given these are both small inherited populations in the granitoids, we suggest it will be distracting to add their age distributions as separate curves to Figure 4.

Lines 536-539: I don't think the method for converting from whole-rock EpsNd to EpsHf was outlined in the methods, nor is it clear if this conversion appears on a figure. This either needs to be added to Figure 5 (or a separate figure) or more clearly indicated. A supplementary file with the conversion would also be appropriate.

We provide further details of the Nd-Hf conversion in the text and include the equation of Vervoort et al. (2011) on which it is based. This conversion is widely used by isotope geochemists and is not a new approach developed in this study.

The converted data is plotted on Figure 5C and detailed in the figure caption.

Lines 542-545: It would be useful for the authors to comment on why Sr isotopes would be a better discriminator than Nd/Hf isotopes. Are there potentially different histories of secondary alteration in the source rocks?

We have revised the text to acknowledge that post-crystallisation alteration could account for the differences in the radiogenic Sr and Nd isotope compositions between granitoids of the Melbourne Zone and Western Tasmania Terrane. However, we clarify that this is an unlikely scenario because both areas share similar post-Devonian geological histories. We conclude that differences in Sr-Nd isotope systematics are more readily explained by differences in melt source composition, which is supported by the new inherited zircon data.

Lines 568-570: Which populations? Are these "subtle differences" captured in the authors' new dataset?

We have clarified that the age offset of these populations (<50 Ma) and further highlight that such offsets are within the typical analytical uncertainty of zircon U-Pb analyses and are therefore not statistically significant.

Line 573: Capitalize "figure."

Fixed.

Line 573: I think the authors need to be explicit about how this point is "demonstrated in Figure 4B." I am not sure what I am being asked to see in the figure.

We now reference Section 4.2, which details the zircon age variability of the Pacific Gondwanan signature that is relevant to this discussion.

Line 700: The location of the ophiolites should also be added to the map in Figure 6A. I know they are within the Western Tasmania Terrane (2), but that should be made explicit.

Added additional text to label in Figure 6A to clarify that the Cambrian arc remnants in western Tasmania (labelled '2') are the ophiolite complexes

Line 715: More coherent compared to what? How were the previous models incoherent?

We have removed this sentence to avoid ambiguity and a detailed interrogation of the shortcomings of previous models. The preceding sentence highlights how our study advances existing Cambrian tectonic models for SE Australia.

Line 727: What are these kinematic indicators in the metamorphic sole? Are the authors sure that they have not been rotated since the Cambrian?

These indicators are detailed in the literature cited. The kinematic indicators are dated as Cambrian and there is no evidence in the structural history of western Tasmania for significant post-Cambrian vertical axis rotation.

Lines 744-746: How is south and then west typical of modern collisions? One or more specific examples would be useful.

Clarified that it is the significant degree of structural rotation (~90°) that is typical of modern arc-continent collisions, not the specific direction of rotation (west to south). Specific examples of analogue arc-continent collisions that have experienced comparable degrees of rotation are now provided from the study of Wallace et al. (2005) (Papua New Guinea, New Caledonia).

Line 776: Typo - "in Macquarie Arc"

Fixed.

Comments by Reviewer F (Adrian Castro) and author response

In "Inherited zircon mining refines the Cambrian orogenic architecture of southeast Australia" Mulder et al. synthesize zircon trace element geochemistry, wholerock geochemistry, and phase equilibria modeling to characterize the tectonic history of an historically enigmatic terrane in southeast Austriala. Though I am not an expert in detritial zircon geochronology, or Australian tectonics, I am a metamorphic petrologist and tectonicst with experience in the Appalachians – another Paleozoic accretionary orogen. I find that the author's present a large and thoroughly interrogated dataset. Their proposed model, suggesting the Selwyn Block is a Cambrian intra-oceanic arc, is consistent with their data and appears to tie-in well with the existing geochemical, geophysical and structural literature of the region.

Overall, the paper is well-organized and the logic is, for the most part, easy to follow. I believe this paper is suitable for publication in Tektonika, and I suggest only minor revisions. I present my overarching comments below along with a handful of specific comments following.

Best,

Adrian

Overall Comments:

1. I think the authors could improve the reach of their paper by revising the introduction, geologic history, and discussion to be more accessible to a broader audience. This could be accomplished by simplifying/minimizing the regional specific jargon and making more explict connections to general accretionary orogenic proccess, or at least other Paleozoic accretionary orogens.

We have streamlined the introduction and background sections of the manuscript to remove unnecessary details of the local geology and to make the study more accessible to an international audience. Specific edits are detailed below and in the response to Review A, above.

2. Overall, the results of the phase equlibria modeling are well presented. I do think, however, that while their explanation of zircon stabiity during prograde

metamorphism and anatexis is generally true, their arugements would be more explicitly supported if they included zircon (along with xenotime, apatite, YAG, and monazite) in the modeling of their specific bulk composition.

Phase equilibria modelling of accessory minerals is still largely developmental. Although we have a reasonable understanding of the behaviour of these minerals in simple experimental systems, complications arise in natural anatectic systems that limit our ability to model quantitatively their growth and breakdown (e.g. see Johnson et al 2021 Earth-Science Reviews).

The pseudosection modelling in this study uses an average composition for the Rocky Cape Group pelite composition (from n=136 legacy analyses). The goal of the modelling is to show that: (1) the depth that the Devonian granites formed places them well within the P-T conditions for partial melting, and (2) to give a first order estimate of the amount of melt, in order to suggest that the zircon cargo in these metasedimentary rocks could be transported within this melt fraction to form inherited populations in granitoids intruding the Selwyn block. We acknowledge that a detailed exploration of the crystallisation of accessory minerals in these rocks would be an interesting further step, but we suggest it is beyond the scope of this contribution given the significant assumptions and caveats currently inherent to this kind of modelling and the further constraints on the bulk rock composition required (e.g. the trace element and REE content of the Rocky Cape Group pelites are not well quantified).

Finally, we note that our results are comparable to melt modelling of typical pelite compositions that does include accessory phases (Kelsey et al., 2008; cited) and hence it is unlikely that applying these more advanced modelling techniques to the bulk pelite composition used in our study will significantly change our findings.

3. What exactly does mining mean in this context? I think the use of this term in the title (and elswhere) may lead to some confusion in readers.

We have removed ‘mining’ from the manuscript as suggested

Specific Comments:

Line 52: Can you elaborate on why this system is archetypal? A more international audience may not know.

In our simplification of the introduction section, we have removed the reference to the Tasmanides being an ‘archetypal’ accretionary orogen. Instead, the revised text highlights

the Tasmanides as an important system to study because it constructed a significant part of the Australian continent.

Lines 173-193: This history is dense and difficult to follow. Can you simplfy this and/or provide a figure? More generally, I think the authors should reconsider the level of detail the reader needs to know to understand the motivation and implications of their work. As written, this overall section bogs down the manuscript.

We have removed the discussion of post-Tyennan sedimentation from the text as it is not essential to the focus of the study.

Line 540: You should provide a brief explanation of this method.

As per response to Reviewer A, we provide further details of the approach and include the equation of Vervoort in the text.

Line 546: Is the Sr isotopic system alway better suited for this kind of discrimination, or is this true only in this case?

We have included a short discussion on causes for decoupling of Sr and Nd isotopes in granitoids including the mechanism of melting, alteration, or the composition of the melt source. We conclude that the contrasting inherited zircon signature between Melbourne Zone and Western Tasmania Terrane granitoids favours the latter interpretation.

Line 573-575: Can you provide a specific value or range of values reflective of the "subtle difference" you describe? Otherwise the reader just has to take your word for it.

Clarified similar query by reviewer A, see above.

Line 717: More coherent compared to what?

Clarified similar query by reviewer A, see above.

Line 748: You provide an explicit comparison below, but you should also add one here.

We now provide two specific examples of the Papua New Guinea and New Caledonian arc-continent collisions as potential analogues for collisional rotation during the Tyennan orogeny.

Figure 1: Is the white line in B the terrane boundary? If so, it should be labled as such.

The white line marks the upper limit of the Selwyn Block inferred by Moore et al. (2016). We now define the line in the figure caption.

Figure 2: I assume the black outlines in B and C are the boundary of the Melbourne Zone, but that should be explicitly stated.

Clarified in figure caption.

Figure 3: The white melt mode% curves are difficult to read. Can you change the color? Additionally, all mineral abbreviations should either be defined in the caption or the reader should be provided a reference (e.g. Kretz or Whitney et al.)

Line colour changed. Mineral abbreviations included in figure.

Figure 4: Labels for age peaks in A-D would be helpful. The panels are otherwise a bit too small to visually determine the ages.

Age peaks are now labelled.

Acceptance letter

Jacob Mulder, Jacqueline Halpin, Laura Morrissey, Yousef Zoleikhaei, John Everard, Sebastien Meffre, Mike Hall, Oliver Nebel, Peter Cawood:

Thank you for your contribution to Tektonika and for actioning the reviewers comments. We have reached a decision regarding your submission to ᴛᴇᴋᴛᴏɴɪᴋᴀ, "Inherited zircon mining refines the Cambrian orogenic architecture of southeast Australia".

Our decision is to: Accept Submission

I look forward to seeing the manuscript published!

Kind regards,

Craig Magee