Response to Reviewer 1's comments on manuscript egusphere-2023-1293

"An expanded workflow for detrital rutile provenance studies: An application from the Neotethys Orogen in Anatolia"

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November 15, 2023

We appreciate the thoughtful reviews of the 3 referees. First, we summarize the broad themes from the three reviews before responding in detail to Reviewer #1 below. The reviews critiqued (1) the novelty of the study, (2) the number of U-Pb analyses discarded during data reduction, (3) the potential bias of discarding data and the validity of interpreting discordant U-Pb analyses, and (4) the apparent lack of novelty and complexity in geochemical data interpretations. Regarding these points, the goal of the manuscript was to provide a transparent workflow for detrital rutile geochronology using new data from sedimentary basins in Anatolia. We acknowledge that it was confusing or misleading to present this work as a revised workflow. We intended to emphasize that we found a paucity of methods papers that provide a straightforward approach. Although we did not claim to present new workflows with the geochemical data, this was perhaps unintentionally implied with the title. We have presented as much information as we could squeeze out of our particular geochemical dataset. The revised manuscript will scale back statements on new workflows and instead refocus the title and introduction on suture zone settings and Anatolian geology while maintaining the broad overview of detrital rutile provenance and the details of our methods.

We also acknowledge that it is surprising to see the number of data discarded during data reduction. However, we contend that this is a common practice with detrital U-Pb geochronology in common Pb-bearing minerals. We are not the first to discard a significant number of analyses; we have found this practice in many published detrital rutile datasets, although it is not discussed much in the literature. We further expand on this point in our detailed reply and will add this context to the revised manuscript. This manuscript gives precedence for papers to be transparent in data reporting—including the number of grains analyzed and criteria for rejection—as well as examination of the full dataset in Tera-Wasserburg diagrams. Our manuscript provides an opportunity to show the consequence and potential value of the full dataset, which is relevant to others working with this type of data. While the use of common Pb-bearing minerals is common in some labs and geographic settings, the application of these tools is still far behind detrital zircon geochronology, for which there is a well-established global framework. We have encountered many detrital geochronology users who want to add detrital rutile to their toolkits but are still uncertain in how to collect and interpret these data. Here, we present a complicated detrital rutile U-Pb dataset that can serve as an example for how to treat and interpret complex, yet potentially meaningful, discordant data.

Reviewer #1 provided a thoughtful review of our manuscript that highlighted several ways to improve the manuscript that include scaling back statements on 'new workflows' and clarifying the new aspects of the data workflow, clarifying common Pb correction calculations, and expanding the analysis of the detrital rutile trace element geochemistry. The review includes several main suggestions for improvement that we will follow in the revised manuscript. We thank Reviewer #1 for their helpful suggestions, which we plan to incorporate into the revised manuscript.

Reviewer #1's comments are included below in black text, grouped by theme. Below we state how we will implement Reviewer #1's suggestions in a future revision, with our response in purple text and the specific changes highlighted in ***bold, italic purple text***.

1. **Comments regarding the data workflow and low-U threshold filtering**

- Suggestions for improvements including: (a) scaling back on the strong statements about new workflows etc. – because there are papers out there already which identify all the rutiles on a mount (SEM-EDS or Raman), analyse all grains for U-Pb and trace elements, including Cr vs Nb discrimination and Zr-in-rutile temperatures. (b) Keep the U threshold aspect in, but shorten significantly and do not make it a key aspect of the paper as I do not think it is all that common an approach nowadays
- The use of the phrases "expanded workflow" (title) or "new workflow" (section 7.2 heading). This revised workflow appears to be mainly not applying U concentration thresholding in an initial trace element session. The majority of labs nowadays (as far as I know) are not doing this, so that is not new. It is shown that it is inappropriate, but if it is only undertaken by a small subset of labs then is it all that important ? it certainly doesn't warrant inclusion in the title. e.g. in L26 "We present a new workflow that accounts for low- U rutile..." - I can show you lots of published papers that date all the rutile in the rock and do not undertake U thresholding, five from my lab alone extending back to 2019.
- L124 (Challenge 1). I dispute the sentence "many detrital rutile methods first analyse trace elements then only collect U-Pb data on rutile above a given U concentration threshold (4-5 ppm).". I have reviewed quite a few studies in the last few years with detrital rutile U-Pb and trace element data in them, and I have never (as far as I remember) encountered this approach. I can see why it may have been applied historically (maybe over a decade ago), where quadrupole-ICP-MS or a slow-scanning sector field MS was used to give the trace elements and U-Pb was subsequently analysed by sector field ICP-MS. But let us talk about the last few years (i.e. what is currently happening). The amount of labs doing this now I feel is very small. A modern quadrupole such as an iCAP or Agilent 7900 can easily produce all the necessary TEs and good U-Pb data simultaneously in the same spot ablation. It may appear that I am making a big deal of this - but then L125-130 then make a big deal of this. I strongly agree it would introduce a bias and this is shown later on. But I feel that such an approach is hardly ever used nowadays and so the authors are arguing against a false premise as a rationale for this paper. I feel challenge #1 needs rewriting and the screening part removed, or convincing demonstration it is still a common approach (e.g. look at all detrital rutile studies published in the last five years and find the % that did U thresholding). I feel this is entirely restricted to sector field labs (a subset of all data produced) and only a subset of those studies would in turn screen by U thresholding.
- L306-308. Exactly how common is this approach nowadays? To the best of my knowledge I have never reviewed a detrital rutile U-Pb paper that does this. I think nowadays it is a fairly (or even very) uncommon approach. For this reason alone, I am not sure section 5.2 it is worth including in the manuscript, certainly not in so much detail.
- What exactly is the new workflow - not doing a U-threshold and analysing all grains including those identified by SEM-EDS? There are lots of studies already doing that – I cannot see the justification for "New workflow" in the abstract text or in the heading for section 7.2. For example, Caracciolo et al. (2022) present a large U-Pb detrital rutile (n =712) dataset (along with zircon and apatite), where all rutile grains in the heavy mineral fraction determined by Raman were analysed for U-Pb and trace elements (including Cr/Nb discrimination and Zr-in-rutile temperatures). I do not think the phrase "new workflow" is justified.

The workflow that we present includes two elements: evaluating the importance of including low-U rutile grains in provenance analysis and considering the effects of U-Pb discordance on provenance interpretations. We agree with Reviewer #1 that most labs that analyze detrital rutile do not apply a U-threshold filter. While not a global problem, this is a regional problem. There are 4 published detrital rutile U-Pb datasets from Türkiye (including this study), and 2 of them (Okay et al., 2011; Şengün et al., 2020) only analyze U-Pb on grains with uranium concentrations above ca. 4-5 ppm. This is a regional problem and imparts a bias, which we wanted to address in this manuscript as detrital rutile analysis is still an uncommon tool for Anatolia. The 2 studies that do not use a U-threshold filter but instead analyze all detrital rutile grains (Shaanan et al., 2020; this study) have to discard data due to very low uranium signals and must implement a protocol for evaluating discordance because of common Pb incorporation. For example, Shaanan et al. (2020) discard 60% of their detrital rutile U-Pb data due to discordance. Similarly, Reviewer #1 points to the study by Caracciolo and co-authors (2021) that analyzes 712 detrital rutile grains without a U-filter, yet, after discordance filtering, only 347 grains remained (48%) (however we have not been able to examine the data as it is not available online or from the journal or lead author). We agree that automated Raman or automated mineralogy are better suited for identifying polymorphs than handpicking and/or SEM-EDS. Importantly, in the study of Caracciolo and others, there were not enough rutile ages per sample to discuss sample-by-sample provenance interpretations, which we also experienced with our dataset. This points to a larger problem in trying to scale up detrital rutile to large-*n* provenance applications. For this reason, we wanted to confidently include as many U-Pb analyses as possible in our interpretations, which led to the exploration of U-Pb discordance.

To address this concern, ***we will change the text to reduce the discussion of U-threshold filtering. The revised manuscript will clarify that U-threshold filtering is currently not a common practice but is used regionally in Anatolia.***

Following the comments of Reviewers #1 and #3, we will move away from phrases like 'new workflow.' The revised manuscript will have an updated title and introduction that is oriented toward Türkiye and suture zone settings.

2. Comments regarding common Pb corrections

- Suggestions for improvements including: (c) Clarify the choice of initial age estimate to stick into the 207Pb correction – if an iterative approach has not been used (with at least five iterations) then I am not sure why it is included. But keep the bit on discordance filtering.
- L184 I am confused here. "We explore using an initial date estimate from the uncorrected date (ti) and from the 208Pb-corrected date (t208)." How many iterations are you using after this initial age estimate? It doesn't really matter what the age estimate is if it eventually converges on a solution? That is what is important. Unlike for the 208Pb correction you do not specify the amount of iterations after this initial age estimate?
- L200 How many iterations are used following this initial age estimate. Five was quoted for the 208Pb correction, but the number of iterations is not quoted for the 207Pb correction, and it urgently needs to be. I found in Chew et al. (2011) that it was generally insensitive to the choice of the initial age estimate input into Stacey & Kramers after a few iterations.
- the section on the choice of initial age estimate to stick into the 207Pb correction (uncorrected age [t_initial] versus the 208Pb corrected age [t_208]) is really confusing. I am really puzzled by the large difference between the two approaches for discordant data (Fig. 6). In a 2011 Chemical Geology paper I showed that the final 207Pb-corrected age differs by < 0.05% if an initial age estimate of 1 Ma is used instead of 1 Ga, demonstrating it is not dependent on the choice of initial age after five iterations. As far as I can see after five iterations in a 207Pb correction, you have converged on the answer, regardless of the starting age estimate. So I cannot explain Fig. 6 unless only one iteration of the correction has been undertaken? If that is indeed the case (only one iteration of the correction has been done), then that entire section should be removed as the process has not yet converged on a solution.

- Section 5.1 I found this section really hard to assess when it came to the 207Pb correction using a starting estimate of t_initial or t_208, as the amount of iterations in the 207Pb correction calculation (as far as I could see) was not explicitly specified earlier. I would be somewhat surprised to see any significant variation after a few iterations (say five). It doesn't matter if there is a difference after one iteration – what matters is the variation after the iterative process has been completed. For example, there is a surprising large age difference in Fig 6 for the low concordance grains between an initial age estimate using t_Initial vs an initial age estimate of t_208. If this is after five iterations, then that is a noteworthy result. If it is after one iteration, then it is in my opinion of no significance as you have yet to converge on the solution. Hence I am not sure if the starting age estimate issue is all that important and could be removed (e.g. if Fig.6 is based on one iteration), but it is hard to assess without more information. I found Fig. 6 pretty confusing to be honest and I think the figure cpation needs more information as I am not entirely sure what was being plotted.

The manuscript did not include an iterative approach for the ^{207}Pb correction. We understand that an iterative approach is recommended (Chew et al., 2011; Thomson et al., 2012; Smye and Stockli, 2014). Below, we display preliminary results from a ^{207}Pb correction with 5 iterations as compared with the ^{207}Pb- and ^{208}Pb-corrected dates from the original manuscript (Figure R1). The preliminary iteration 5 ^{207}Pb-corrected date spectrum is similar to the ^{208}Pb corrected spectrum. ***The revised manuscript will calculate ^{207}Pb-corrected ages using an iterative approach with at least 5 iterations. The text and equations explaining the iterative process will be updated (Section 2.3.2). All figures and tables will be updated. Furthermore, interpretations will be updated if there are significant changes in the resulting age spectra (Section 7.1).***

Thank you for the feedback that Section 5.1 is unclear. It will be important to clarify this section as it is the basis for the choice of discordance cutoff. Figure 6 shows the differences between $^{207}Pb_{ti}$- and $^{207}Pb_{t208}$-corrected ages versus the percent concordance. ***The revised manuscript will remove the $^{207}Pb_{t208}$ correction as this approach is irrelevant with an iterative ^{207}Pb correction. After updating the ^{207}Pb corrected ages, we will explore the difference in ^{207}Pb- and ^{208}Pb-corrected age in the revised manuscript. The figure captions and relevant text will be updated to better explain this calculation.*** In the original manuscript, the ages differed significantly (> 5%) for analyses below 40% concordant, the justification for our 40% concordance filter. The differences in age cannot be explained by grain age or Th concentration. ***As needed, the revised manuscript will revisit the concordance cutoff based on the updated ^{207}Pb-corrected ages, and all text, figures, and tables will be revised. If the significant difference in age remains, we will investigate the potential controls.***

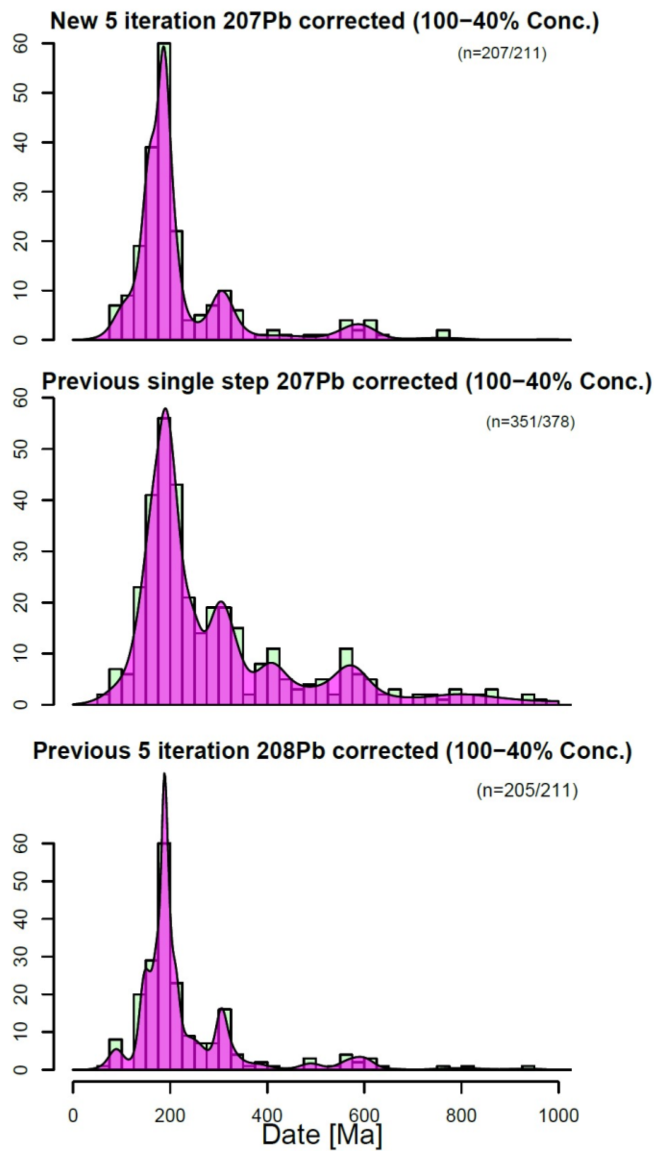

Figure R1: A preliminary comparison of ^{207}Pb-corrected dates with 5 iterations (top), ^{207}Pb-corrected dates in the original manuscript (middle), and ^{208}Pb-corrected dates with 5 iterations in the original manuscript (bottom). All ages 0 to 1000 Ma that are 100-40% concordant are displayed.

- The choice of common Pb composition – it is interesting to explore the difference between the 207Pb and 208Pb methods, but they ultimately do not show much of a difference. That is new, but maybe not that significant a result. But I like the general approach to discordance filtering.
- "the various Pb correction methods produce similar age spectra and do not change the final provenance interpretations" – so maybe that section should be scaled a bit as ultimately it does not appear to be that important.
- L190 I would like to see more about the choice of the Pb initial and whether it is appropriate to use the Stacey and Kramers (1975) model. It is well known that the 207Pb/206Pb initial ratio of metamorphic titanite is often significantly lower (i.e. more radiogenic) than the Stacey and Kramers (1975) crustal evolution model, reflecting incorporation of radiogenic Pb from rutile, a

common titanite precursor (see Essex and Gromet, 2000). But rutile replacing titanite is also seen in bedrock samples – have a look at Gumsley et al. (2023, Lithos). In their Figs 11a and 11b you have metamorphic rutile with a 207Pb/206Pb initial with 0.10 -0.12, which can be convincingly linked to breakdown of late Variscan titianite

We chose an initial $^{207/206}$Pb value based on the ^{206}Pb/^{238}U age calculated from the uncorrected ^{206}Pb/^{238}U and $^{207/206}$Pb ratios. However, after 5 iterations, the resulting ^{208}Pb age is invariant to the choice of initial age and common Pb. To address the review, we checked this by varying the initial age estimate, and therefore the initial common Pb composition, from 1 Ma to 1000 Ma and the resulting ^{208}Pb-corrected age differs by less than 0.05% for 98% of our unknowns (578 of 592 analyses). As stated by Reviewer #1, Chew et al. (2011) demonstrated a similar result for ^{207}Pb-corrected ages: the choice of initial age results in a < 0.05% difference in the final ^{207}Pb-corrected age after 5 iterations. ***The revised manuscript will include an iterative approach to the ^{207}Pb correction. We expect that the final iteration of the ^{207}Pb age will be insensitive to the choice of initial common Pb composition. Further, we do not expect significant changes in the resulting age spectra (Figure R1) and do not anticipate major changes to the provenance interpretations. The revised manuscript will clarify the choice of the Pb initial and explain that the resulting Pb-corrected ages are insensitive to this choice, including citation of prior work. The supplementary data files will be updated. Additionally, we will assess the similarities between the resulting ^{207}Pb- and ^{208}Pb-corrected ages. Preliminarily, the 208Pb correction appears to differentiate more peaks (Figure R1), which will be explored further in the revised manuscript. We will follow the reviewer's suggestion to scale back our emphasis on Section 5.1.***

This is a good point about whether the Stacey and Kramers (1975) values are appropriate to use as estimates of Pb initial. We agree that it is possible that the ^{207}Pb/^{206}Pb initial for the detrital rutile grains may differ from the Stacey and Kramers (1975) model. As noted in other studies, there are no constraints on the initial Pb composition in detrital samples (e.g., Chew et al., 2011). We attempted to address this in the manuscript:

> *"Most rutile U-Pb dates are expected to be discordant. In-situ studies mitigate this by: (1) regressing discordia lines through co-genetic analyses in Tera-Wasserburg space, where the lower intercept of the discordia with the concordia defines the U-Pb age of Pb diffusion closure (Faure, 1986; Chew et al., 2011; Vermeesch, 2020); or (2) applying a non-radiogenic Pb correction using either an ad hoc Pb model such as that of Stacey and Kramers (1975), or measuring the composition of non-radiogenic Pb in a co-existing phase. However, by nature, the co-genetic grains in detrital samples are unknown." (Lines 142-147)*

We emphasized that, for detrital grains, there are no constraints on the composition of non-radiogenic Pb. This is unlike *in-situ* work where the common Pb composition can be determined by analyzing co-genetic grains or co-genetic, U-free phases (i.e., K-feldspar). In a detrital sample, it is uncertain whether two detrital rutile grains are co-genetic, or if a detrital rutile and a detrital K-feldspar are co-genetic, for example. Therefore, common Pb corrections based in the Stacey and Kramers (1975) Pb evolution model are dominant in the detrital rutile literature (e.g., Thomson et al., 2012; Caracciolo et al., 2021; Odlum et al., 2019; Chew et al., 2020; Najman et al., 2019; Clift et al., 2022; Mark et al., 2016).

As a thought exercise, we take a closer look at one sample for which the grains might be considered co-genetic. Sample 18DMN01 had only 4 rutile grains and the U-Pb results seem to plot in a linear array, suggesting that they may represent one age population from the same source (Figure R2). After 5 iterations, the $^{207/206}Pb_c$ initial values used range from 0.8416 to 0.8418, and the ^{207}Pb-corrected ages range from 92 Ma to 96 Ma. On the other hand, if we assume that the 4 grains from sample 18DMN01 represent one age population (i.e., are co-genetic), we can regress a discordia line through the analyses to assess the initial common Pb value. In this case, the 4 grains from 18DMN01 give a discordia age of 91.5 ± 3.8 Ma and $^{207/206}Pb_c$ of 0.802 ± 0.068 (Figure R3). This could suggest that the Stacey and Kramers (1975) values are not the best estimate of initial Pb composition. In this example, the resulting difference between an age of 91.5 ± 3.8 Ma or 92-96 Ma is within the uncertainty and does not alter the final provenance interpretation.

This approach is difficult to put into practice in provenance studies because it is inherently unclear which analyses should be grouped together (i.e., treated as co-genetic). This problem is well documented in the literature. Should a range of U-Pb ratios be treated as (1) a single age population from one source, (2) a range of ages from one source, or (3) a range of ages from multiple sources? If (1), then analyses should be grouped together and the Pb correction can be performed without an estimate of initial $^{207/206}$Pb (Figure R3). Yet, if it is (2) or (3), it is unclear which analyses should be grouped together and treated as co-genetic. Trace element discrimination may be needed to aid these decisions and provide a way forward.

All of this is to say, we acknowledge that the Stacey and Kramers (1975) model may not be the most accurate initial $^{207/206}Pb_c$ value. Yet, we are unaware of a better method for addressing this problem in detrital studies and follow in the footsteps of many studies that have applied ^{208}Pb and ^{207}Pb corrections in detrital minerals. There may be other geographic settings where the Pbc composition of sources is well characterized, such that a more appropriate $^{207/206}Pb_c$ value may be used for each age population. ***In the revised manuscript, we will add a few lines that explain that the Stacey and Kramers (1975) values may not be the correct common Pb composition but are still an appropriate initial estimate for performing iterative common Pb corrections in detrital grains.***

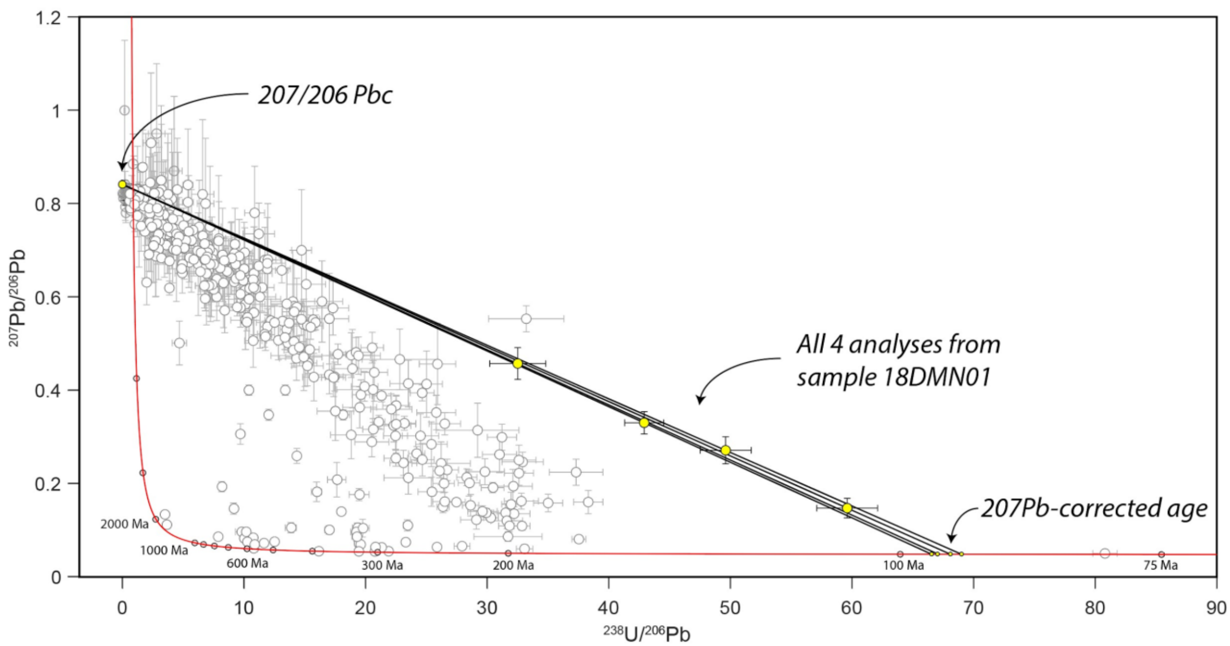

Figure R2: Tera-Wasserburg diagram of all detrital rutile U-Pb results. Analyses highlighted in yellow are from sample 18DMN01 and yield ^{207}Pb-corrected ages from 92 Ma to 96 Ma (after 5 iterations; denoted by yellow circles at intersection with concordia). The common Pb composition ranges from 0.8416 to 0.8418 for these 4 analyses. The black lines show the discordia lines fit between initial Pb_c and each analysis, and their lower intersection with the concordia is the ^{207}Pb-corrected age.

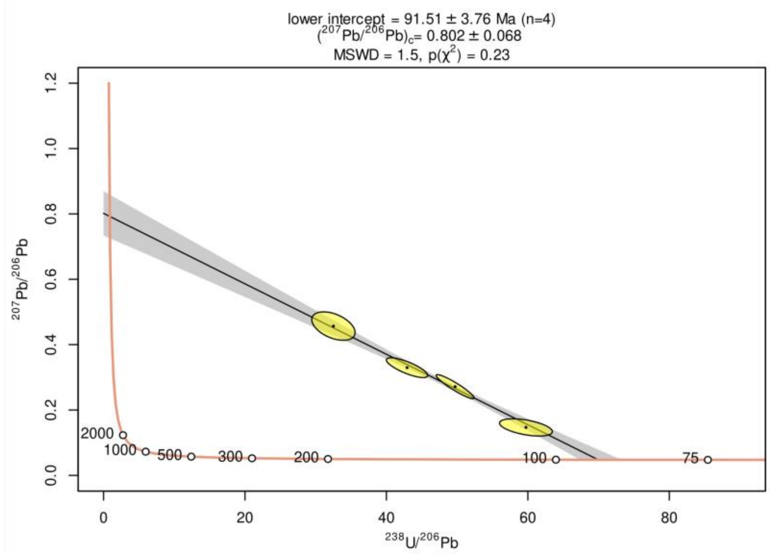

Figure R3: Tera-Wasserburg diagram of analyses from sample 18DMN01. The unanchored discordia age is 91.5 ± 3.8 Ma and the common Pb composition is 0.802 ± 0.068. Figure from IsoplotR (Vermeesch, 2018).

- L188 "Note that because the correction forces intersection with the concordia, the two dates are identical". I wouldn't mention this at all – you have only one age when doing a 207Pb correction – you report a 207Pb-corrected date.

  Thank you. ***We will exclude this in the revised manuscript.***

- L197 presumably this principle about minimum ages also applies to 208Pb corrected data?

  Yes, ***we will add this sentence to the 208Pb corrected age section*** (Section 2.3.1).

- Section 2.3 Why are 204Pb corrections not discussed?

Reviewers #1 and #3 both inquired why the ^{204}Pb correction was not discussed. This was not initially included because (a) ^{204}(Pb+Hg) and ^{202}Hg were not measured, so we did not perform a ^{204}Pb correction (Because of the high Hg background at KU IGL, a ^{204}Pb correction would be too imprecise. All commercially available UHP He gas options in the midcontinent US have high Hg), and (b) it is reviewed in other publications. Although we will not be able to explore the ^{204}Pb correction, which is a goal of future work, ***the revised manuscript will include a short overview of this correction and its application in rutile.***

- L281. Significant error here – S&K at 1000 Ma is about 0.909?

We will fix the typo regarding the ^{207}Pb/^{206}Pb$_{common}$ ratio at 1000 Ma which is 0.909 (line 281). This is a typo and we've confirmed that the correct ratio values were used in all calculations.

- L299-L300 "However, the similarity in the 207Pb with t208 cumulative date distribution for the 100–40% and 40–0% groups is notable,". I really didn't understand this clause – sorry.

The revised manuscript will exclude the t_208 correction as it becomes irrelevant with an iterative ^{207}Pb correction. This should help simplify this section. This will remove the confusion with Lines 299-300, which were intending to indicate that the cumulative distribution plot (Fig. 5) shows that the date distributions are similar for the 40-0% concordant and 100-40% concordant groups with ^{207}Pb$_{t208}$-corrected ages. This could warrant the inclusion of more discordant grains (<40%) in the final interpretations. This does not hold for the ^{207}Pb$_{ti}$-corrected ages, so ***this discussion point will be removed***.

3. **Comments regarding trace element data**

- suggestions for improvements including: (c) Make more of the PCA plot, and also of your own data.
- the PCA approach to exploring the data is interesting, but currently not much is made of it.
- Section 6.3 – PCA – Not quite sure what the main point this paragraph is trying to tell us – it could be expanded on. It does show the Cr vs Nb plot is useful in that Cr (+ V which has similar behaviour) pulls in an opposite direction to Nb (+ Ta which has similar geochemical behaviour). So the Cr vs Nb plot does a good job of separating the fields. If you were to crudely put on mafic vs pelitic fields on the PCA plot (boundary between yellow vs green), then the Hf + Zr vectors would be roughly parallel, showing the mafic vs pelitic distinction is somewhat independent of temperature. But the plot is introduced without significant additional interpretation.

We used the detrital rutile trace element dataset in several ways. We confirmed that analyzed grains were rutile and not other polymorphs using Cr, V and Zr (Appendix). The Cr and Nb concentrations are used to discriminate mafic and pelitic protoliths and Zr concentration is used to determine Zr-in-rutile temperatures. These applications only use the results of 1 or 2 elements. To evaluate the whole suite of trace elements analyzed, we used principal component analysis. Dimensionality reduction methods like PCA and tSNE are useful for evaluating clustering in large, multivariate datasets (we did not discuss tSNE in the manuscript as the results were similar to PCA). We hoped that these methods would help distinguish detrital populations. However, the PCA results show that *"the variance between [grains] can largely be explained by Hf, Zr, Sn, Cr, V, Nb and Ta. Because Cr, Nb and Ta are protolith dependent (PC 2) and Hf and Zr are temperature dependent (PC 1), the variance in detrital rutile trace element chemistry is best explained by both protolith and metamorphic grade, allowing us to track these two properties of source rocks." (Lines 376-379)*

This means that the protolith and Zr-in-rutile sections are already exploring the most interesting aspects of the trace element dataset. We agree that the PCA plot shows that protolith and temperature are independent. ***The revised manuscript will clarify the most salient points of the PCA results. We will emphasize that the protolith and temperature sections capture the most important components of the trace element results. Additionally, we will revisit the trace element data to evaluate trends in samples and/or age populations and consider adding spider plots, bar plots, and/or other clustering algorithms as relevant. The revised manuscript will include updated text, figures and supporting information to support any new findings.***

- Figure 8 – not clear which lines defining fields belong to which paper (Triebold vs Meinhold) on the Cr/Nb plot without reading the text – label them.

Thank you. ***We will update the figure labels in the revised manuscript.***

4. **Additional comments**

- First 8 lines of abstract. I really think there needs to be a caveat here. Very broadly speaking (and there are exceptions), rutile is not particularly common in igneous rocks in the crust and requires reasonably high pressures to crystalize – it is mainly a metamorphic mineral (where again it requires reasonably high pressures to crystallize). It is better than zircon in recording metamorphic events in provenance studies but it too has a relatively restricted paragenesis.

We included the mention of igneous rutile to be complete, but we agree that it is uncommon. ***The revised manuscript will be updated to include this caveat.***

- L46 – I am not sure why this sentence starts with "In convergent margin settings….", as I feel it is applicable to other tectonic settings as well.

Yes, we agree with this point. ***We will keep the focus on convergent margin settings following the recommendations of the reviewers. The revised manuscript will have an updated focus that is oriented toward Türkiye and suture zone settings, as discussed in Section 1 above and in our reply to Reviewer #3.***

- L64 – and detrital apatite. In terms of recent publications (i.e. last 5 years) I feel it is more commonly used nowadays than either detrital monazite and detrital muscovite. Please also list the geochronological system applying to the mineral – U-Pb, 40Ar/39Ar, Luf-Hf etc

The revised manuscript will be updated to include detrital apatite and isotope systems. The list in the manuscript was meant to only include popular U-Pb minerals.

- Section 2.1 This section needs something on the role of pressure and composition on the stability field of igneous and metamorphic rutile, and also the well documented instability instability in the sub-greenschist to lower greenschist facies (Zack et al., 2004; Yakymchuk et al., 2017).
    - L216. Please also provide pressure estimates. The pressure dependence on the stability of rutile is not getting much attention in this manuscript.

The original manuscript did not include much background on the pressure conditions of rutile formation. Pressure was briefly addressed with the Zr-in-rutile thermometer (Section 2.1). To calculate Zr-in-rutile temperatures, we use an average pressure of 13 kbar with an uncertainty of 5 kbar, which is a reasonable pressure range for rutile in metamorphic rocks (e.g., Zack & Kooijman, 2017). The stability field of rutile has been discussed in some review papers (e.g., Chew et al., 2020, Tropper & Manning 2008), but is not always given as much attention. ***The revised manuscript will address the stability of rutile in the background section. Additionally, this will strengthen the interpretations. The dominant 190 Ma age mode is interpreted to be sourced from the Karakaya Complex, which was metamorphosed to blueschist and epidote-amphibolite with minor eclogite facies. In the blueschist facies, rutile is expected to be stable above ~14 kbar and at temperatures ~400-500 ºC in metagranitoid compositions (Angiboust and Harlov, 2017), whereas it can coexist with ilmenite from 5-8 kbar and be solely stable above 8 kbar in metapelitic compositions (e.g., Zack & Kooijman, 2017). The 190 Ma rutiles largely have temperatures from 450 to 500 ºC. The revised manuscript will include a discussion of rutile stability in the background section and will include further justification for the interpretation of a Karakaya Complex source based on expected rutile stability.***

- Sections 7.2 – Some points below link back to substantive points made at the start of the review:
    - "the 190 Ma population is poorly represented in the detrital zircon record" – but the counterpoint needs to be made that the 90 Ma population is very important in the detrital zircon record and not in the rutile record.

This is a good point. We emphasized that the detrital rutile record contains age populations not present in the detrital zircon record. ***The revised manuscript will also emphasize that the zircon record contains prominent age modes at ca. 395 Ma, 110-66 Ma, and 50 Ma that are not well represented in the rutile record and are more likely to represent timespans dominated by magmatism/volcanism. We postulated that the absence of 395 Ma rutile could be a signal of sediment recycling (lines 405-415). We will add to this discussion that there are Late Cretaceous and Eocene magmatic flare ups associated with subduction and syn-collisional magmatism, respectively. Collision-related metamorphism is recorded in the lower plate (the samples are from upper plate basins), so the absence of Late Cretaceous-Eocene detrital rutile could support the absence of sediment transport across the suture zone (which is also shown by Okay & Kylander-Clark (2023) using the detrital zircon record).***

Although we note there are a few Late Cretaceous detrital rutile grains in our record. We will address this in detail in the revised manuscript.

References Cited

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