Reviewer 1- Anonymous

Review for Beranoaguirre, A., Millonig, L. J., Albert, R., Marschall, H. R., and Gerdes, A.: U-Pb dating of sub-ng/g U garnet by LA-MC-ICPMS, EGUsphere [preprint], https://doi.org/10.5194/egusphere-20263931, 2026.

General Comments:

The manuscript introduces a method for dating ultra-low U garnet, and illustrates its application across different garnet samples. In my opinion, this work represents a useful geochronological tool that pushes current analytical capabilities to their limits. While some of the figures could benefit from polishing to make the data more accessible, this research is a valuable addition to the literature on garnet geochronology.

We would like to thank the reviewer for taking the time to carefully evaluate our manuscript and for providing constructive comments and valuable suggestions, which have helped us to improve the quality and clarity of the paper. Below, we provide a point-by-point response to each of the comments raised.

Specific Comments:

I noticed that the abbreviation for 'inductively coupled plasma mass spectrometry' is used interchangeably as 'ICP-MS' and 'ICPMS'. Please choose one and use it consistently throughout the manuscript. Additionally, element names written out in full are occasionally capitalized. Please check for this and use lowercase (e.g., 'uranium' instead of 'Uranium').

Thanks for the point. We have now carefully checked the consistency throughout the manuscript.

The data from the Kaapvaal craton are from a previous publication, as you mentioned. There, I saw that the sample ST70 also contains sillimanite measurements in the age calculations. From what I understood, the sillimanite measurements plot at a very low $^{238}U/^{206}Pb$, with comparatively high Pb concentrations. With these data points plotting at the more 'extreme' ratios, I suppose they influence the isochron regression to some degree. Could you add a few sentences to elaborate on this, and whether it helps to also measure co-crystallized silliminates or not, and if they would not automatically be rejected with their 'high' Pb concentrations.

We thank the reviewer for this insightful comment. Indeed, in the study of Shu et al. (2024), the sillimanite analyses helped to better constrain the regression line by anchoring its upper end in the Tera–Wasserburg diagram. However, this approach is only appropriate when there is clear petrographic and textural evidence that garnet and sillimanite crystallized in equilibrium and

therefore shared the same U–Pb isotopic system. This condition cannot be assumed in every sample. In many cases, the natural variability in the U/Pb ratios within the garnet itself is sufficient to define a well-constrained regression line, making the inclusion of a coexisting mineral unnecessary. Consequently, the usefulness of incorporating sillimanite (or any other coexisting mineral) depends on the specific geological context and cannot be generalised. In any case, this is now stated in the revised manuscript.

The manuscript's presentation and readability would benefit from a few visual improvements to the figures. (1) Figures 1, 3, 5, and 6 show concentrations or cps. Could you add the uncertainty of these values to those plots to allow for a clearer visual assessment of precision. If the uncertainties are smaller than the symbol size, please just state this in the figure captions. (2) The fontsizes of the figures are not consistent throughout the paper, and particularly for figure 2 and 4, the fontsize of the ticklabels is on the limit of being too small. (3) Please make sure, that you use appropriate and ideally consistent marker sizes and styles (e.g. in Figure 6 the individual points can not be differentiated. You could add for instance an edge color to the points for better vizualization.)

We have now standardised the fonts across all figures and slightly increased the font size to improve readability. In addition, an outline has been added to the symbols in Figure 6, making the individual data points easier to distinguish.

The figures already displayed concentrations in ng/g. We intentionally followed the recommendations of the International Association of Geoanalysts (IAG), which discourage the use of terms such as "ppm" and "ppb" because of the ambiguity associated with these units. We note that the analytical uncertainties are smaller than the symbols and therefore are not visible in the figure.

The figures were already displayed concentrations in ng/g or cps. We intentionally followed the recommendations of the International Association of Geoanalysts (IAG), which discourage the use of terms such as "ppm" ("ppb" in our case) because of the ambiguity associated with these units.

The analytical uncertainties are smaller than the symbols and therefore are not visible in the figure. This is now stated in the figure captions.

Sample terminology and labeling across the figures, captions, tables, and text could be improved. For instance, the sample G99-2 is labeled only by its sample number in the Tera-Wasserburg plot, described as 'sample G99-2 from Bohemian Massif' in the caption, and associated with 'Orlica-Sniez˙nik' in Figures´ 1 and

3. To improve clarity and readability, I suggest establishing a unified naming structure throughout the entire paper to use in all figures and in the table, such as combining the sample code with a clear location identifier (e.g., as done in Figure 3). Using a consistent label across all figures, tables, and narrative text will make it much easier for readers to track individual samples throughout the study.

The text and figure captions have been modified according to the reviewer's suggestion.

Technical Corrections:

Line 20-21: The placement of ”during the 1980s” feels not ideal. You could place it either at the start of the sentence or at the end.

This has been modified according to the reviewer's suggestion.

Line 50: MC is not defined yet.

This has been modified according to the reviewer's suggestion.

Line 83: Could you please add the number in brackets of the ’sufficently low-U concentrations.’

This has been modified according to the reviewer's suggestion.

Line 129: ’Demantoid’ I guess should not be capitalized.

This has been modified according to the reviewer's suggestion.

Line 132: There is one lonely bracket.

This has been modified according to the reviewer's suggestion.

Line 183: Also add the reference of the data to the caption.

This has been modified according to the reviewer's suggestion.

Line 199: Am I mistaken, or is the average U concentration of ST66 0.7 ng/g instead of 1.1 ng/g (as it was listed in the original paper)? From figure 3, it also looks as if 1.1 ng/g is too high of an average. If so, please also adjust the number in table 1.

Sorry, the 1.1 ng/g is the mean for another sample not shown in this study. It is now corrected.

Line 224-226: Could you please elaborate on this. From an outsider perspective, who is not very familiar with the details of this approach, these are just a lot of numbers. Here it looks as if for 40 times higher background, the limit of detection increases only by four times. Is this what you wanted to get across? From the previous sentence, it sounds as if the limit of detection is generally low, due to a stable background (which I understand as meaning a low standard deviation in the background). How did this go into your numeric example?

We thank the reviewer for this comment. Following the formulation of Pettke et al. (2012), the limit of detection depends on several parameters, and in our example the only parameter that change between the two calculations is the "mean background count rate". Because this term enters the equation under the square root, the limit of detection increases much more slowly than the background intensity itself. Consequently, although the background count rate differs by a factor of 40, the calculated limit of detection increases by only about a factor of five. In any case, we have added a short explanation to the manuscript.

Line 226-227: Before (Line 223) you mentioned that regardless of the background intensity, the background is generally stable (I interpret this as generally having similar standard deviations). So I do not follow the reasoning, why the limit of quantification changes a lot depending on the background intensity if it is mainly dependent on the standard deviation rather than the absolute value. Maybe you want to rephrase or elaborate on this. It would help to show the standard deviation as uncertainties in figure 5 for this argument.

We thank the reviewer for pointing out this ambiguity. The reviewer is correct that the statement indicating that "the background is generally stable" was too vague and could be interpreted as implying similar standard deviations between analytical sessions. This is not the case. While the background signal generally remains stable throughout an individual analytical session, the standard deviations associated with the individual analyses (spots) are substantially higher during sessions with elevated background levels, which directly increases the limit of quantification. To avoid this confusion, we have removed the original sentence and revised the text accordingly.

Line 242: 'Geochronology' should not be capitalized.

This has been modified according to the reviewer's suggestion.

Line 314: '... minerals that contain up to 3500 times less U than ...' This could be misleading, as I would understand it as the U concentration being 3500 times less in the measured garnets compared to zircon. The truth is much more extrem as you showed in table 1, of the concentration being >250'000 times less in the ultra-low U garnets compared to the GJ-1 zircon. Make sure to either refer to the concentration or the total U per spot analysis.

We thank the reviewer for this excellent suggestion, which has improved the clarity of the manuscript. The text has been revised accordingly.

Figure 1: Here you use bold capitalized letters for your subplots, whereas in the other plots lower case letters with a bracket. I would suggest being consistent between all plots.

This has been modified according to the reviewer's suggestion.

Figure 3: The cut in the axis is not ideal in my opinion. I would either suggest having two distinct subfigures or a single figure showing the entire range, and if needed with a zoom into the low U corner.

We have now modified the figure, showing a small jump in the x-axis. This is also mentioned in the figure caption.

Figure 4: I saw in the original paper, in which you published the Kaapvaal data, that there are also silimanite measurements used for the age calculation in sample ST70. Is it possible to annotate these measurements in the diagram? I would also add the reference to the original paper in the caption.

This has been modified according to the reviewer's suggestion.

Figure 5: You have two dashed lines at 50 and 100 cps background. In the text you mention that quantification of the ^{207}Pb signal for ultra-low-U garnets is only feasible for a ^{207}Pb background signal below 50 cps. Can you please annotate this line in the figure, or at least mention it in the caption. What does the 100 cps line stand for?

The 100 cps line is now deleted.

Figure 6: I have to admit that this plot is not very intuitiv to understand, and I am not sure I understand its purpose. Do you want to show, that for higher ^{207}Pb cps, the 2SE of the ratio will be lower? And if ^{207}Pb cps is getting too low, it approches the limit of quantification? Annotations would be useful to get the purpose of the figure better across, like: What is the black line, and what does the kink mean? In the text you mention

a signal of 2500 cps, and a lower limit of the technique of about 100 cps, which could be highlighted. It would also help to have a legend within the plot, rather than just mentioning the colors in the caption. An additional suggestion would be, to instead of plotting the orange points individually (since there should not be a direct relationship between 2SE of the $^{207}Pb/^{206}Pb$ ratio in the sample and the limit of ^{207}Pb quantification?), giving an overall ^{207}Pb-quantification-limit range for the sessions during which the background was low enough (<50 cps) to measure ultra-low U garnets. And finally, it could be beneficial, to merge figure 5 and 6 into two subfigures to make a better/direct link between the ^{207}Pb backgrounds in the sessions (color code the sessions, in which ultra-low U garnets were measured), and the respective limit of the technique and its precision.

We thank the reviewer for these helpful suggestions. We agree that the original version of Fig. 6 could be improved to better explain its purpose, and we have therefore revised it to improve its readability. The figure caption has also been expanded accordingly to provide a more detailed explanation of the different elements of the figure, including the meaning of the black line, and highlighting the approximate signal levels discussed in the text (100 and 2500 cps). We reckon that these modifications clarify the figure without introducing further labels within the plot itself, which would have considerably reduced its readability due to the limited available space.

Regarding the suggestion to merge Figs. 5 and 6 or to replace the individual data points by session-specific ranges, we respectfully chose to keep the original layout. Figure 5 illustrates the day-to-day variability of the background signal, whereas Fig. 6 focuses on the analytical precision as a function of the measured ^{207}Pb signal intensity. We feel that combining both figures would worsen their clarity. Likewise, we prefer to retain the individual analyses in Fig. 6, as they demonstrate the continuous relationship between signal intensity and analytical precision over the full range of measured values.

Table 1: You compare total U per spot between zircon and garnet, for which you assume different ablation spot sizes. I would suggest adding this information to the table, to make the comparison of the numbers more accessible. To make sure, it does not come across as if you are saying that the GJ-1 zircon has a U concentration of 3500 times more than the ultra-low garnets, I would suggest rephrasing the last sentence of the caption to '... less uranium than a typical GJ-1 zircon analysis.' Or something similar.

Following the reviewer's suggestion, this information has been added to the figure caption.

Reviewer 2 – Dr. Martin Hugo Senger

The paper written by Beranoaguirre et al. is about a conventional dating approach in a mineral phase that has been challenging to date due to sensitivity limitations in the collectors being used. The authors successfully dated various low-U garnets using the U-Pb isotopic system with an optimized LA-MC-ICP-MS set up, that has 2 SEM and 5 CDDs. Although it is not particularly novel in a way that U-Pb ages in garnet have already been published before, this new approach will undoubtedly be useful to the scientific community, as now it is possible to expand the applicability of this low uranium chronometer. This could greatly help in the understanding of rocks that lack other U-rich mineral phases (such as zircon) or to simply better understand history of garnet.

We would like to thank the reviewer for the careful evaluation of our manuscript and for the constructive comments and suggestions. We appreciate the reviewer's recognition of the potential of this analytical approach to substantially broaden the applicability of in-situ U–Pb garnet geochronology, particularly for metamorphic rocks containing extremely low-U garnet or lacking other suitable U-bearing geochronometers. In the following, we address each of the reviewer's comments individually.

Specific comments:

- It is well explained in the text but confusingly described the array of the colletctors used in this method between lines 59-66. I would reorganize the phrasing of the sentences so that it is clear the masses that were acquired. I would add a comparative figure/table with the enabled detectors and corresponding isotopes for each configuration.

We thank the reviewer for this helpful suggestion. To make the detector configuration easier to follow, we have added a new schematic figure (new Fig. 1) illustrating the detector array of the Neptune Plus MC-ICP-MS, the positions of the measured isotopes, and the approximate signal ranges covered by the different detector types. We believe this figure provides a much clearer overview of the analytical setup and avoids overcomplicating the description in the main text.

- I don't agree with using "Balochistan" as a secondary standard if it has not been published (it currently says "personal communication" to cite a specific age).

We agree that the term "secondary standard" may be misleading in this context. The Balochistan garnet is not intended as a community reference material, but rather as an in-house quality-control material that has been routinely used in our laboratory for several years to monitor the long-term

reproducibility of the analytical method. More recently, this material has also been independently dated by ID-TIMS, providing additional confidence in its age. We have therefore revised the manuscript to refer to it consistently as an "in-house quality-control material" rather than a secondary standard, and we clarify that its purpose is solely to assess the reproducibility of the analytical procedure.

- The primary standard was NIST614. While it is discouraged to have a matrix mismatch between the standard and the unknowns, in this case it is the only possible way to proceed, since the natural materials contain too much of the analyzed isotopes that would inevitably saturate the ion counters. On that note, I don't understand why the tuning of the instrument was done under different conditions (repetition rate, spot size, and laser energy) than those of the session. If it's a matter of saturation of the detector, then this should be mentioned, and instead of changing all the parameters, only the spot size would help decrease the signal.

We thank the reviewer for this comment. We agree that the description of the tuning procedure could have led to some confusion. However, the tuning of the instrument and the analytical conditions used during the U–Pb analyses are two independent steps serving different purposes.

The tuning line on NIST SRM 614 is used solely to optimise the ICP-MS operating conditions (e.g. torch position, gas flows and ion transmission) by producing a stable signal over an extended period while monitoring sensitivity, oxide production and elemental fractionation. The specific laser parameters employed for tuning (spot size, repetition rate, fluence and line speed) are therefore chosen to provide a robust and reproducible tuning protocol and have been kept unchanged in our laboratory for more than ten years, allowing direct comparison of instrument performance between analytical sessions.

Once the instrument has been optimised, the analytical parameters used for garnet analyses are selected independently according to the objectives of the method. In this study, they were chosen to maximise the amount of ablated material while avoiding saturation of the ion counters. We have clarified this distinction in the revised manuscript.

- Add the frequency of standardization and the scanlists (if possible). Would it be possible to include the raw TRA files at the time of publishing it?

We have now included the frequency of standardisation in the Methodology section. Primary and matrix-matched reference materials were analysed every 50 unknown analyses to monitor and correct for instrumental drift throughout each analytical session.

Regarding the TRA files, we thank the reviewer for this suggestion. The processed analytical data required to calculate the age are provided in the Supplementary Material. The TRA files are relatively large and are not routinely archived as supplementary material for LA-ICP-MS studies. However, they are stored at FIERCE and can be made available upon request. It should be noted that the published isotopic ratios were obtained after manual inspection of each TRA file, including the exclusion of signal spikes or the selection of appropriate integration intervals. Consequently, the raw TRA files alone do not directly reproduce the processed dataset.

- Please, reorganize the spreadsheets the data is being shared to allow users to easily access the data. Avoid merging cells and having empty headers. Images shoved into the datasets is also discouraged. The dataset should be organized in one unique spreadsheet and using categorical columns (for example date, session, material type, sample name, etc.) to easily select specific rows.

We thank the reviewer for this helpful suggestion. We agree that the supplementary dataset could be organised in a more user-friendly format. However, at present there is no widely adopted standard for reporting LA-ICP-MS U–Pb datasets, partly because different laboratories use different data reduction workflows and software packages. In preparing the Supplementary Material, we followed the reporting recommendations of Horstwood et al. (2016). Because the data were acquired during five independent analytical sessions, each producing a large amount of analytical information, we have retained the original session-based organisation of the spreadsheets. Nevertheless, following the reviewer's suggestion, we have improved the presentation of the supplementary data by removing merged cells, eliminating empty headers, and removing embedded images, making the datasets easier to access and process.

- Correct Line 77 and 78 informing the reader that the material that was used as a secondary standard is not Mali Grandite *per se* (Seman et al., 2017), but instead another material found within the same locality (inform the reader that there is more details below).

We agree that the original wording could be interpreted as implying that we used the published Mali Grandite reference material of Seman et al. (2017). This is not the case. The matrix-matched reference material used in this study is a different garnet crystal from the same locality, selected because of its much lower U concentration, which makes it suitable for ion-counter measurements. We have revised the text to clarify this distinction and explicitly refer the reader to the detailed description of this material provided later in the manuscript.

- Explain that the datasets have asymmetric uncertainties for the calculated intercepts (for example in line 136).

The reviewer is right that, from a mathematical point of view, the uncertainty associated with a lower-intercept age in a Tera–Wasserburg diagram is inherently asymmetric, as it is defined by the intersection of a regression line with the Concordia curve. However, in LA-ICP-MS U–Pb geochronology it is common practice to report these ages using a single (symmetric) uncertainty, as the asymmetry is generally small and does not significantly affect the interpretation. For consistency with the standard presentation of Tera–Wasserburg results, we have retained this convention throughout the manuscript. The two uncertainties reported after each age correspond to the systematic and expanded uncertainties, as described in the Methodology section following Horstwood et al. (2016), and should not be interpreted as the asymmetric uncertainties in the lower intercept.

- The discussion starts with calculating the amount of U per shot, which relies on the depth of the spots. However, the is not any paper cited or data where it is clearly demonstrated the depth of the abladed pits. We already know it is a linear correlation; however, different materials may react differently based on the laser parameters. Please, conduct these short tests and add them to the paper if you find this to be so relevant as a dedicated figure. Please, take pictures with the SEM on the ablated pits and measure the depth of the pits using a reflected light microscope to support this part of the discussion.

We thank the reviewer for this valuable suggestion. The reviewer is correct that the estimated amount of U ablated per analysis depends on the ablation pit volume and, therefore, on the pit depth. This is indeed how the calculation was performed. In practice, the calculation also depends on the NIST SRM 614 analyses (i.e. the ablated pit volume), as the U concentration in garnet is determined relative to NIST SRM 614, for which the U concentration is known. Nevertheless, following the reviewer's recommendation, we have added a new figure documenting the geometry and depth of representative ablation pits using images acquired with a Keyence digital microscope. These measurements support the estimated concentration values.

- Figures:
    1. Figure 1: Remove the arrows and rectangles to highlight the data. Instead, add a legend to the side or at the bottom of the plots to differentiate both markers used for “Accepted” or “Rejected” spots. Used different markers too. If it is truly important to show all the spots, consider using a logarithmic scale for the Pb (ng/g) axis because most of the “Accepted” data is squeezed in as of now. Also, include “error bars” to all the features or at least plot the 2 SD, to give an idea of the scatter of each spot.

Following the recommendations of the reviewer, we have removed the arrows and rectangles, introduced different symbols for accepted and rejected analyses, and added a legend to improve the readability of the figure.

We have, however, retained the linear scale for the Pb axis. The purpose of this figure is to illustrate the distinction between accepted and rejected analyses, rather than to emphasize the distribution of the accepted data. A logarithmic scale would not improve this interpretation and would reduce the visual contrast between the rejected outliers and the main population. We have also reformulate the figure caption, adding the following sentence: Analytical uncertainties are smaller than the plotting symbols and are therefore not visible. Nevertheless, their inclusion would not alter the interpretation of the figure.

2. Figure 2: Increase all the font size present in this figure. It is very small and hard to read. The figure is also pixelated, so what I would do is to export the figure as a vectorized file and edit it in whatever software you prefer.

This has been modified according to the reviewer's suggestion.

3. Figure 5: I would add the dates when each of the sessions were conducted

We thank the reviewer for this suggestion. We considered adding the dates of each analytical session to Figure 5. However, the sessions were conducted over numerous consecutive days interspersed with periods during which the instrument was not used. Including all individual dates on the x-axis would substantially reduce the readability of the figure due to the limited available space and the high density of labels. For this reason, we have retained the original presentation, which we believe more clearly illustrates the day-to-day variability in the background signal without overloading the figure.

Technical Corrections

1. Line 187/188: avoid using words such as “good” or “tiny”. Use “high” and “relatively/comparatively small” instead.

This has been modified according to the reviewer's suggestion.

2. Line 295: … “dated to” … (instead of at)

This has been modified according to the reviewer's suggestion.

3. Line 315: “even for minerals that contain up to 3,500 times less U than commonly analysed accessory minerals such as zircon”. Avoid writing in this way. Nobody cares about how much less U there’s in these garnets than in zircon. Just say the threshold at which the crystals may be successfully dated.

We respectfully prefer to retain this statement. A major objective of the present study is to demonstrate the analytical capability of obtaining meaningful U–Pb ages from femtogram quantities

of U. Expressing this improvement relative to a typical zircon analysis provides an intuitive and widely recognised reference point, making the analytical advance more accessible to a broad geoscience readership. We therefore believe that this comparison adds useful context and have retained it in the revised manuscript.

Reviewer 3 – Dr. Ariela Mazoz

General comments:

This manuscript presents an interesting and timely contribution to the field of geochronology, particularly with the development of an analytical approach for obtaining U–Pb ages from garnet with extremely low U concentrations. The results show the potential of this approach to extend in-situ garnet U–Pb geochronology to geological settings where conventional accessory-mineral geochronometers may be absent, scarce, or unsuitable. I believe this work will be of great interest to the geochronology community and could have a significant impact on the field.

The manuscript is generally well written, and the analytical approach and its applications are clearly presented. I particularly appreciated the discussion of the analytical challenges associated with very low U concentrations and the factors controlling the precision of the ages obtained.

I have also tried to avoid repeating points already raised by the previous reviewers, so my comments mainly focus on aspects that I think could further improve the clarity and discussion of the manuscript.

I have listed a few comments below that I believe would help clarify some of the methodological choices and geological interpretations. Overall, I find this to be a very interesting study and believe it will be a valuable contribution to the geochronology community.

We sincerely thank the reviewer for taking the time to carefully evaluate our manuscript and for the constructive and thoughtful comments. We particularly appreciate the reviewer's positive assessment of the study and the recognition of its potential contribution to in-situ garnet U–Pb geochronology. We also thank the reviewer for focusing on aspects that complement those raised by the previous reviewers, and for providing suggestions that have helped us further clarify the methodological approach and geological interpretations. We have carefully considered all comments and have addressed them point by point below.

Specific comments:

Line 10: I suggest defining "ultra-low U concentrations" quantitatively, for example by providing an approximate concentration range or threshold.

We thank the reviewer for this suggestion. We agree that a quantitative definition of "ultra-low U concentrations" improves clarity. We have therefore defined ultra-low U garnet as garnet containing <10 ng/g U in the revised manuscript.

Line 23: Since you state that LA-ICP-MS U–Pb geochronology has become a routine analytical tool, please consider adding some more recent references to support this statement.

We have considered adding further recent references, but prefer to retain the current selection, which includes the references reporting the first applications of LA-ICP-MS U–Pb geochronology to the different mineral phases discussed. Given the extensive and continuously growing literature on LA-ICP-MS U–Pb geochronology, adding further references would result in a very long and potentially overwhelming list without substantially strengthening the statement. We therefore consider the existing references sufficient to support the general statement.

Lines 124–126: Here, you describe the geological context for the magmatic and subsequent metamorphic events during the Variscan at ca. 400–390 Ma. However, the ages reported for the two sessions analysing the demantoid are ~255 Ma. This is well explained later in the discussion (Lines 292–294), but I think a brief context for the ~255 Ma age would also be helpful here. I believe this specific part of the geological context is missing from this paragraph. At first, I thought it might be a typo, so adding a brief explanation here would help make the chronology clearer and avoid any confusion for the reader.

We thank the reviewer for this helpful suggestion. In the new version of the manuscript, we have stated that these veins cut across the foliation and postdate even the youngest regional granitoids (ca. 280 Ma).

Lines 137–138: I understand that the two sessions give ages that agree within uncertainty when treated as unanchored regressions, and that you then used the average $^{207}Pb/^{206}Pb$ value from the two unanchored regressions to anchor the final age. Is this correct? If so, I suggest briefly explaining why you chose this approach rather than using a common-Pb evolution model, such as Stacey–Kramers. Since the data appear to have a good spread and can define the regression, I think it would be useful to explain why this approach was preferred and why using a modelled common-Pb composition was not appropriate in this case.

As the reviewer correctly deduced, we calculated the regressions using the average $^{207}Pb/^{206}Pb$ ratio from the unanchored regressions to anchor both datasets. We preferred this approach for two reasons:

1- **As the reviewer noted, our data exhibit sufficient spread to define a robust unanchored regression that reflects the true initial common-Pb composition of this specific local system. Stacey–Kramers models reflect global averages and can deviate from local hydrothermal fluid compositions.**
2- **Anchoring both analytical sessions to the same $^{207}Pb/^{206}Pb$ ratio ensures that both datasets are evaluated against the exact same baseline, enabling a direct and robust comparison between the two sessions.**

We have added a brief sentence in the new manuscript to explain this better.

Line 172: Please check the name "Namaqua-Natal-Natal Belt", I believe there is a typo.

We thank the reviewer for pointing out this typo. The name has been corrected to "Namaqua–Natal Fold Belt" in the revised manuscript.

Lines 199, 204, and 240: Please replace "Uranium" with "U" here and throughout the text for consistency.

This has been modified according to the reviewer's suggestion.

Line 205: Did you mean "Table 1" rather than "figure"?

We thank the reviewer for pointing out this mistake. It has now been corrected in the revised manuscript.

Lines 224-226: This sentence is somewhat confusing. You state that the limits of detection for average background signals of 10 and 400 cps are approximately 6 and 30 pg/g, respectively. Could you please clarify what the "2 to 8 cps" values refer to? If these are the corresponding signal intensities for the stated concentration limits of detection, please clarify this in the text.

We thank the reviewer for bringing this to our attention. The 2 cps and 8 cps values refer to the net signal intensities at the limit of detection. We have rephrased the sentence to explicitly clarify this.

Lines 247–249: I would also consider mentioning that the amount of common Pb is an important factor controlling the precision of the regression. In samples with very low U concentrations and relatively high common-Pb contributions, analyses may cluster close to the upper intercept in a Tera–Wasserburg diagram, resulting in insufficient spread in the data to define the regression line and obtain a precise age.

We agree with the reviewer that high common-Pb contents and low U concentrations restrict isotopic spread and cluster the analyses near the upper intercept. While this concept was implicitly assumed when we mentioned the $^{238}U/^{206}Pb$ variability, we concur that this may also be stated. We have revised the sentence accordingly.

Line 279: Please provide an approximate value for the U–Pb closure temperature of garnet here.

Following the suggestion of the reviewer, the value of 1100 °C (Shu et al., 2024) has now been added to the text.

Lines 280–283: I suggest expanding this part and explaining in more detail what the limitations of these accessory minerals can be, with a few specific examples. For example, apatite has a relatively low U–Pb closure temperature (~350–550 °C), making its U–Pb system potentially susceptible to later thermal events and Pb loss. Apatite can also recrystallise or undergo dissolution–reprecipitation in the presence of fluids, and similar processes can affect monazite. Zircon, on the other hand, is generally very resilient and can preserve older or inherited domains; moreover, zircon grains enclosed within garnet may be shielded from subsequent processes, potentially preserving an older age rather than recording the timing of garnet

growth. A few examples such as these would help illustrate the advantages and limitations of using these accessory minerals compared with garnets.

We thank the reviewer for this excellent recommendation. We have expanded this paragraph to explicitly detail the limitations of traditional accessory minerals (rutile, apatite, monazite, and zircon) in this geological context, including closure temperatures, fluid-driven dissolution–reprecipitation, and zircon inheritance/shielding.

Lines 291–292: Please add references to previous studies that have used K–Ar and/or Rb–Sr dating of biotite.

We thank the reviewer for the comment, but we would like to clarify that our samples do not contain biotite, and we are not using K–Ar or Rb–Sr dating. Instead, our text explains that the chlorite in these veins lacks K and Rb, which makes K–Ar and Rb–Sr dating impossible. We have slightly rephrased the sentence to make this clearer.

Lines 298-299: Please add references here.

Following the reviewer's suggestion, we have now added key methodological references for traditional acid-digestion bulk-garnet geochronological techniques (Sm–Nd and Lu–Hf; Anczkiewicz and Thirlwall, 2003; Münker et al., 2001) to support this comparison.

Figure 6: Please check the calculation/description of the limit of quantification. In the text (Lines 226–229), the limit of quantification is described as 10 times the standard deviation of the measured background, whereas the figure caption refers to 10 times the standard error. Please clarify which one was used. It would also be helpful to clarify what the black curve represents.

We thank the reviewer for pointing out this discrepancy. We confirm that the limit of quantification was calculated as 10 times the standard deviation of the background signal. We have corrected the Figure 6 caption to state "standard deviation" instead of "standard error".