Dear Dr. Rubatto,

Thank you for your comments on the first submission of the manuscript "Calcite U-Pb dating of altered ancient oceanic crust in the North Pamir, Central Asia". We very much appreciate the effort the three reviewers and the associate editor put into reviewing the text. The constructive suggestions helped us a great deal to improve the manuscript. We implemented all suggested improvements. In detail these are (numbers follow the five major point of your letter from January 11th):

1. We carefully recalculated the isotope ratios, accounting for the suggestions of the anonymous reviewer. We improved our manuscript as follows:
    - Analytical errors are enhanced by propagation of systematic uncertainties, as described in lines 125ff. This includes long term variability of the $^{238}U/^{206}Pb$ ratios, $^{238}U$ decay constant error, and the analytical error of the monitor standard. This strictly follows the suggestion by Horstwood *et al.* (2016). We expanded the methodological section to clarify this. We have checked the reporting of our data against several recent publications of calcite dating including several in Geochronology. We make sure that our data reporting could be reviewed acceptable by a broad range of experts. We emphasize that we do not feel that this manuscript should be about methodology and that there have been a large number of papers focusing on LA-ICP-MS dating of calcite, to which we refer readers in the text.
    - Ablation area ages are not grouped to obtain a sample age. Instead, we present the results of each ablation area in Figures 5 and 6 and in the text (results in lines 186–212). We carefully discuss all ages to obtain an age range for ocean floor alteration (lines 261–2282). Thereby we regarded ages with MSWD<2 and propagated 2σ-errors as reliable.
2. The isotope data table was reshaped according to the exemplary table presented by Horstwood *et al.* (2016)(Supplementary table S2). The table contains the data of all unknowns and the three reference materials AHX-1D, WC-1 and PTKD. All ages and associated data were calculated using IsoplotR program (Vermeesch, 2018) version 3.5 running in R version 3.6.0 (line 182f.).
3. To ensure a better link of the age data with textures and composition we did the following:
    - We conducted cathodoluminescence imaging. This led to the extension of the methodological part (lines 84ff.), and the results part (lines 164ff.) and supported our interpretation of the most reliable ages. To illustrate, we added Figure 4.

- We clarified the evolution of calcite precipitation in the complex basalt samples by enhancing Figure 3 and associated passages in the text (e.g. 158ff.). This underlines our understanding and awareness of the complexity of the samples.
- We discussed the age data in the light of possible calcite genesis and alteration in lines 261–282.

4. We enhanced the discussion and emphasized the regional implications of our results (lines 347–354). This accounts for the major suggestion by Dr. Konopelko.
5. To facilitate geographic orientation, we added an overview figure of the larger region and marked the position of major tectonic entities and lineaments (Figure 1), considering the suggestion by Dr. Robinson. We also added GPS coordinates of the sampling locations in the text (lines 75–80).

We applied several general changes, as supposed by all three reviewers. Thereby we changed all grammatic and orthographic errors in the main text, the figure captions and the figures. We cleared all ambiguities.

Regarding your additional comments:

- We are aware that the whole rock analysis does not represent the geochemistry of fresh basalt. We clarified in the reviewed version of the manuscript that this is altered basalt (e.g. line 289). We think that REE content of the basalt gives an important hint to identify one possible source for REE content in the calcite.
- We moved text with interpretations from the result part to the discussion. Specifically, microstructures of the calcite are now discussed in lines 246ff.
- We updated former appendix D, now appendix C according to your suggestion.
- We carefully considered the Gd-anomalies and found that they seem to correlate with REE content. We cannot completely exclude the possibility of interference (We thank you very much for pointing this out). We note that the polyatomic interference table of May & Wiedmeyer (1998) states $^{141}Pr^{16}O^{+}$ as possible interfering species to the measured $^{157}Gd$ (stated in text line 310). We note that the oxide production rates of our analytical session were low as monitored by ThO/Th during tuning. Therefore, we kept interpretations regarding the Gd anomaly, but we tuned down the language and make it clear that we were not able to completely rule out the possibility of interference on Gd measurements.

Attached is a document with a detailed record of how we addressed the suggestions made by all three reviewers. We tried to be as precise as possible and included line numbers to highlight reworked passages. We hope that this enhanced the quality of the manuscript.

Best regards,

Johannes Rembe and Renjie Zhou on behalf of all co-authors

## References

Horstwood, M.S.A., Košler, J., Gehrels, G., Jackson, S.E., McLean, N.M., Paton, C., Pearson, N.J., Sircombe, K., Sylvester, P., and Vermeesch, P., 2016, Community-derived standards for LA-ICP-MS U-(Th-) Pb geochronology–Uncertainty propagation, age interpretation and data reporting: Geostandards and Geoanalytical Research, v. 40, no. 3, p. 311–332.

May, T.W., and Wiedmeyer, R.H., 1998, A table of polyatomic interferences in ICP-MS: ATOMIC SPECTROSCOPY-NORWALK CONNECTICUT-, v. 19, p. 150–155.

Detailed reply to the reviewers suggestions. Review letter of Konopelko is colored in green, review letter of Robinson is colored in orange, review letter of the anonymous reviewer is colored in red. Our reply is inserted in the relevant reviewer's text passages and in bold black text.

This is a nice contribution providing an important case study of dating of Carboniferous calcite from poorly studied volcanics of the N Pamirs arc. To my knowledge, this is, perhaps, the first case where Carboniferous calcite has been dated convincingly with LA, at least in Central Asia. The paper is certainly suitable for Gchron. The data are of apparent good quality, the figures are well shaped and the structure of the manuscript is fine. In my opinion the manuscript can be accepted for publication after Minor revisions.

My general and specific comments are listed below, and I think that the authors might underline it more clearly that, on a regional scale, the early-mid-Carboniferous arc rocks are missing entirely in the South Tien-Shan and North Tarim, indicating that the southern margin of the Turkestan Ocean developed passively during the Carboniferous, while the known occurrences of the early-mid-Carboniferous arc rocks in N Pamirs and Gissar show that these rocks formed in the Paleo-Tethys Ocean, which was located to the south. Thus, the discussion on the regional implications of the new data should be, perhaps, extended with taking into consideration this new evidence that the studied north Pamirs Carboniferous sea floor volcanics are a part of the NW Pamirs arc and its western continuation in the Tajik South Tien Shan (Gissar), and the following papers describing the N Pamirs arc and its western continuation should be, perhaps, additionally cited:

Ruzhetsev SV., Pospelov II., Sukhanov AN., Tectonics of Kalaihumb-Sauksau zone of the Norther Pamir. Geotectonics 1977, №4, 68-80

Konopelko, D., Biske, Yu.S. Kullerud, K., Ganiev, I., Seltmann, R., Brownscombe, W., Mirkamalov, R., Wang, B., Safonova, I., Kotler, P., Shatov, V., Sun, M., Wong, J., 2019. Early Carboniferous metamorphism of the Neoproterozoic South Tien Shan-Karakum basement: New geochronological results from Baisun and Kyzylkum, Uzbekistan. Journal of

Asian Earth Sciences 177, 275–286. https://doi.org/10.1016/j.jseaes.2019.03.025

**Reply: We added a paragraph discussing the relation of the North Pamir arc volcanic rocks and the South Tian Shan Kyzylkum and Gissar segments (lines 347–355). We cite the suggested literature (e.g. line 337, line 350) which supports our findings.**

Specific comments:

Line 63

Replace "splitic" with "spilitic"

**Reply: Done.**

Line 94

Replace "introduce" with "introduced"

**Reply: Done.**

Line 117

"In the Chinese Qimgan valley" – probably "In the Chinese part of the Qimgan valley"?

**Reply: The Qimgan valley is entirely situated in the Uyghur Autonomous Region of China. Therefor we kept our phrasing.**

Line 147-148

Please add here again information on how many samples from how many localities have been analysed

**Reply: Done that in lines 141–146 and more precisely in lines 72–80.**

Please add linear scale in Fig 5 a.

**Reply: We added a linear scale in old figure 5a, which is now new figure 8a.**

Please explain in fig caption what are the shaded areas in Figure D1

**Reply: Shaded areas in Appendix Figure C (former Figure D) are envelopes of all REE measurements done for this study. They visualize the range of values. We now explain that in figure captions of Appendix C.**

I hope my comments will be helpful for authors

Sincerely

Reviewer

The manuscript "Calcite U-Pb dating of altered ancient oceanic crust in the North Pamir, Central Asia" by Rembe et al presents a new application of U-Pb dating of calcite to constrain the age of oceanic crust, applied to addressing questions of the age and lateral continuity of ophiolite belts in the Pamir and Western Kunlun. The manuscript is well written with high quality, well presented data that both introduces a new technique to address the age of oceanic crust (a persistent problem in many orogens) and yields important new information on the tectonic architecture of the Pamir-West Kunlun orogenic belts. In regards to the tectonic portion of the manuscript, I feel that the results of this manuscript will have a significant impact, as the authors have shown that ophiolite belts long considered to be part of a continuous belt are unrelated and of drastically different ages. In regards to the development and application of the calcite dating as applied to oceanic crust, this topic lies a little outside of my expertise (I mostly deal with U-Pb in zircon), the results and analytical techniques appear to be of high quality, and their interpretations of the geologic significance of the ages are well justified.

My only significant comment is that it would be useful to have a regional geologic map (similar to 5a) as the first figure to help the geologic and geographic setting of the samples and study area (maybe move figure 5 to figure 1 and include a slightly large map area).

**Reply: We added an overview figure as figure 1 that helps the reader to contextualize our findings. We added geographic names and marked large scale geologic entities. The location of our sampling area is marked as well.**

Finally, in the conclusion I would suggest simply reordering the statements to match the order presented in the discussion, starting with the calcite dating technique (lines 252-254) and geochemical signatures (lines 247-251), then finish with the ages obtained and the tectonic significance (lines 243-246).

**Reply: We structured the conclusion as suggested: lines 356–368 age data, lines 369–373 geochemistry data, 374–377 tectonic significance.**

I have also made numerous minor suggestions below, but those do not impact the scientific merits of the study and its conclusions. I recommend publishing after minor technical corrections.

Sincerely,

Dr. Alexander Robinson

Minor comments/suggestions:

Abstract:

Line 15: I would suggest rephrasing "poorly investigated" – "poorly understood" perhaps?

**Reply: Done that.**

Line 17: As one age lies right on the Mississippian-Pensylvanian boundary, the authors may want to use Carboniferous (or early Carboniferous) - The use of Carboniferous is more commonly used throughout the rest of the manuscript.

**Reply: We changed the phrasing to Visean-Serpukhovian (line 18, line 271, line 317), as this seems to be the most precise age determination of the calcite. This was also changed in the light of the reevaluation of the isotope data and error recalculation.**

Line 21: Do you mean late Paleozoic (i.e. the ages obtained)? Or are you referring to the Proto-Tethys subduction?

**Reply: Primarily we date ocean floor alteration which gives an age range constraining the age of the ocean floor itself. Literature data demonstrates that this ocean floor was likely part of an intra-oceanic subduction zone along which parts of the Paleo-Tethys were consumed. We discuss this in paragraph 5.3. We divided the last sentence of the abstract to underline the two outcomes of this study: "Our results demonstrate the potential of calcite dating to constrain the age of ancient ocean floors." And "We challenge the hypothesis that a continuous early Paleozoic Kunlun Terrane extended from northern Tibet into the North Pamir."**

Introduction:

Line 30: delete comma after "process"

**Reply: Done that.**

Geologic Background

Line 53: suggest "oceanic lithosphere" after Paleo-Tethys

**Reply: Done that.**

Line 56-58: The authors set up the dissimilarity in ages between the Oytag and Kudi sutures, but then only mention the existing geochronology of the Kudi suture – how is the Oytag suture dissimilar based on existing ages (or are there any)?

**Reply: In fact, evidence for a "Oytag suture" is very sparse. To our knowledge, there is one publication (Jiang, 1992 Geol. Memoirs MGMR) and a small number of field guides presenting the supposed outcrop of ultramafic rocks in the Gez valley. There is no age data for those rocks. We emphasize this in our revisited manuscript in line 66f.**

Line 62: The sentence "An internal stratigraphy of the volcanic sequence is missing" seems out of place. Is this important for understanding the age, and if so how?

**Reply: We agree, it is irrelevant at this point. The sentence was deleted.**

Line 63: I suggest linking these sentences: "…a product of OFA, and that calcite ages can serve…"

**Reply: Done that.**

Line 68: reference the map in figure 5

**Reply: We referred to overview map figure 1.**

Results:

Line 133: “are overgrown” rather than “is overgrown”

**Reply: Changed.**

Line 134: I would suggest referencing the labeled (2) and (3) sequence calcite from figure 2 in this sentence.

**Reply: This sentence was moved to the discussion. A reference to figure 3 (ex fig. 2) is now given.**

Line 144: I would suggest “concentration” rather than content; also insert “concentration” (or content) before “…in fissures filling…”

**Reply: Done that.**

Discussion:

Line 178: insert “ages” after “reproduce”. I also suggest “produce” rather than “reproduce”

**Reply: Changed: line 242**

Line 191: I suggest linking the first two sentences and get rid of “evidently”, e.g.: “…have been studied experimentally, which have shown variable physiochemical…”

**Reply: Changed: line 293f.**

Line 229: remove comma after “corresponding”

**Reply: Done.**

Line 231: remove commas on either side of “subduction related”

**Reply: Done.**

Conclusion:

Line 253: remove comma after “show”

**Reply: The phrasing was changed here.**

Figures:

Comment on gchron-2021-27

Although the manuscript is nicely written and I like the idea of dating Palaeo oceanic curst by U-Pb in calcite, I can't recommend a publication in its present form. I see an overinterpretation of the analytical data and therefore recommend a cautious re-evaluation of the ages and their significance. I refer mainly to the analytics used, the assessment of the precision and accuracy of the data, the presentation of the results. The data must be presented more completely in the tables (online), the standardization and error consideration must be explained more clearly and adapted to the standard procedures currently in use.

**Reply: Thank you very much for these comments. We have studied them in detail and agree with these suggestions. We have realized that our original Figure 4 (whole-sample TW age plots) could be misleading. The use of large numbers of spots (up to >400 per sample) somehow is disadvantageous as it erroneously conveys a message that these ages are precise. We thank the review for pointing it out. In the revision, we provide TW plots and age calculations (including propagated errors) based on individual areas. We refer to figures 5 and 6, sections 4.2.1 through 4.4 in the results and lines 261–282 in the discussion.**

I do not believe that the calcite ages match those of the ocean crust, but possibly within an age error/scatter range of 3-6% (which I believe is realistic for the data).

**Reply: Thank you for noting this. This comment made us to realize that we should have done a better job in making our core conclusion more explicit, which was in fact consistent with the reviewer's comment here. In our original manuscript (e.g., lines 182–184, 214ff.), we interpreted the ages as minimal formation ages of these basalts because these calcites formed during alteration processes that affected the basalts after emplacement under sea-water coverage (stated at e.g., lines 42–43). We agree (here and in our original submission) that these ages do not represent the timing of rock formation. We sincerely thank the Reviewer for this comment as it highlights the unclear expression in our original submission. In the revised version, we have explicitly stated the difference between radiometric ages of volcanic rocks and the age of associated ocean floor alteration. Please see lines 243–245 and lines 366–368.**

Be aware that it is an interpretation of the data, it could be also the result of the mixing of a complex domain consisting of multi-generation veins. I would suggest using an age range (e.g. 310-330 Ma) for each sample as the results do not suggest that they formed during one short-lived event. Alternatively use weighted average (carefully evaluate the uncertainty, see below), although it is statistically incorrect.

**Reply: We recalculated and reevaluated our age data carefully. We reevaluated also the systematic uncertainties and propagated them into the age error. The result was presented for each ablation area separately (paragraphs 4.2.1 through 4.2.4) and only data with MSWD<2 and 2SE<10% was considered as derived from homogenous domains. This is emphasized in the discussion, specifically in lines 257ff.**

- Method

Referring to Su et al 2020 and Yang et al 2021 for a description of the method is insufficient, both papers are also application papers. You need to explain how the raw data were treated;

corrections and outlier rejection, standardization, and uncertainty propagation have been done. This has to follow the recommendation of Horstwood et al. (2016, Geostandards and Analytical Research)

**Reply: We updated the "Methods" section accordingly. There were no outliers rejected. We emphasize that we recalculated the isotope data and propagated systematic uncertainties of a total of 4.6% 2SE into the analytical error. This includes 238U decay constant error, long-term variance of the reference materials 238U/206Pb ratios in the lab, and the analytical error of the monitor standard. This strictly follows the suggestion by Horstwood et al. (2016). Please see lines 125–131.**

It is not clear how the authors correct for matrix-related Pb/U fractionation (using one or both of the mentioned carbonates). The material they used is no international reference material and its age and uncertainty have not been evaluated by an independent method (e.g. ID-TIMS). Please explain in more detail the applied approach to re-evaluate your uncertainties and uncertainty propagation. Report data in table S2 of your reference material measured during the sessions, report data for long-term variance (reproducibility) of your method and that of access of variance of your NIST614. Data in high-rank journals should include comparable data sets, in which uncertainties were correctly propagated (see Horstwood et al. 2016): random uncertainties, from background correction, counting statistics and access of variance (drift correction using NIST614), and systematic uncertainties, ratio uncertainty of RM (>1%), long-term variance (>1% !), decay constants.

**Reply: Thank you very much for this comment. We have included more technical details in the revised text and also discuss our results (for example, in lines 261–281). In our analysis, we used NIST614 as the primary reference material to correct for 207Pb/206Pb fractionation and for instrument drift in the 238U/206Pb ratio. 238U/206Pb ratios were calibrated with a matrix-matched reference material (our in-house reference material) and validated by using WC-1 as the monitoring standard. We will explain it better in our revised version in line with other published studies using this approach. We have conducted uncertainty propagation following the recommendations of Horstwood et al. (2016). We do believe this will significantly improve the quality of the data. We would like to note that our manuscript is intended to be an application paper as well. It is not our intention here to provide any methodology development because pitfalls and advantages of laser ablation calcite U-Pb dating have been thoroughly discussed in the community (Rasbury & Cole, 2009; Roberts & Walker, 2016; Roberts *et al.*, 2017; Roberts *et al.*, 2019; Kylander-Clark, 2020; Nuriel *et al.*, 2021; Rasbury *et al.*, 2021; Roberts *et al.*, 2021)). The message we would like to highlight here is that calcite phases yielded useful U-Pb ages that constrain (not directly date) the formation of ocean-floor basalt. We have made this point clearer in the revised text.**

The authors use WC-1 (incorrectly labeled WC01 in their text) as secondary RM to evaluate the accuracy of their data.

**Reply: Thank you for pointing it out. We have corrected this in the revised manuscript and have done a thorough check to make sure no such mistake is presented in the revised version.**

However, based on this the data is up to 5% inaccurate. The data of Roberts et al. reflects some heterogeneity of this material and is explained by sampling bright and darker domains to achieve some spread in the Pb/U (see Rasbury et al. 2021). They report a 2.5% uncertainty. Rembel et al. report a 1% younger age with a 1.5% uncertainty (no systematic uncertainties propagated), which means the data can be up 5% inaccurate! This needs to be mentioned in the paper and considered in the discussion of the data!

**Reply: We fully agree with this comment. We have propagated systematic errors from the results of the monitoring standard (WC-1) into the final reported ages. We are aware that the errors given in the original manuscript represent solely analytical errors that underestimate the geological spread and the uncertainty arising from calibration to heterogenous reference material. As a result, we propagated 4.6% 2SE of systematic uncertainties into our analytical error. See line 131.**

Based on the data it is not possible to evaluate the precision and accuracy of the ages. However, the authors report ages in the text of the manuscript with a precision of better than 1%. This is misleading and gives the impression they can date their sample and events with precision and accuracy of better than 1%. For the majority of carbonate ages so far published, uncertainties range from 3-5% or worse (Roberst et al. 2021, Geochronology) and only very few labs demonstrated a long-term variance of 2% (e.g., Guillong et al. 2021, Geochronology). The authors need to access their precision/long-term variance/accuracy and should not quote any uncertainties in the text with the figure behind the dot (300 +/- 4, instead of 300.5 +/- 3.6 Ma) and with no or maximal one figure (!!) after the dot in the figures.

**Reply: We thank the reviewer for the comments and have removed figures behind the dot for our age results. We respectfully disagree with the reviewer's comment that our data "it is not possible to evaluate the precision and accuracy of the ages". Our analytical session included the measurement of WC-1, one of the widely used reference material of for calcite U-Pb dating, as the validation of accuracy. Following reviewer's comments, we have done a better job in presenting the data, including propagating errors, openly stating the size of errors of calculated ages, and avoiding quoting too many digits after the decimal point (lines 186–217, figures 5 and 6, supplementary table S2 according to the exemplary table of Horstwood *et al.* (2016).**

U-Pb ages: Fig.4: First of all, I can't reproduce exactly the ages using the data presented in table S2.

**Reply: We added the specifics of the program used for age calculation in line 182f.: "For age calculation we used the IsoplotR program (Vermeesch, 2018) version 3.5 running in R version 3.6.0."**

Secondly, an MSWD of around 2 or higher means the data forms, not a uniform population/event or uncertainties were not probably propagated. In the first case, it would mean the different areas are not formed at the same time. In figure 2 it is shown that data was acquired from different domains, so you can't group them together and calculate a common age as you have done. This is a mixed-age with misleading incorrect precision and accuracy. You should report the ages of individual areas and compare these ages with each other as you have done in figure 3. So change Fig 4, only use some representative examples for the Tera-Wasserburg diagram. If you want to use a weighted average, do not forget that they likely represent mean ages, representing more an age range. But still, you have to add systematic/expanded uncertainties to the final age on the weighted average ages when comparing it with other data. So for Fig3 weighted average use 316 ± 8 / 12 Ma (internal/expanded uncertainty).

**Reply: We did as suggested and expanded our age data presentation in the "Results" of the manuscript (lines 186ff.) and thoroughly discussed the significance of ages (lines 257ff.). In the original submission, we grouped all ablation area ages out of the same sample together and produced the following MSWDs: 2.0 (Sample 17NP436a), 1.4 (Sample 17NP436b), 3.0 (15NP233), 2.1 (15NP236). We noted that single ablation area ages overlap within 2 sigma errors and they are statistically indistinguishable. We did not provide clear visual presentations (e.g., TW plots) and therefore made it difficult for readers to access this information. In the revision, we have followed the reviewer's suggestion and present TW plots/calculation out of each area. We refer to figures 5 and 6 and to the above-mentioned paragraphs.**

As it is visible from scans B3, B4 the investigated domains do not form by a single process of calcite vein-forming but show a more complex pattern with different generations of cross-cutting fractures. And still, the authors what to interpret the U-Pb ages as representing a single calcite precipitation event at the time of basalt-crystallization? Please be more cautious in the interpretation of your data. The obtained ages of the different areas of one sample scatter and reducing the uncertainties by statistical tricks using all data in one single Tera-Wasserburg, is not the way to go.

**Reply: We fully agree with the reviewer. He underlines what we stated in the first version of the manuscript. We now did a great effort to emphasize the complexity of the breccia calcite. We updated figure 3 (ex fig. 2), with sketches showing our interpretation of consecutive calcite formation brecciation and late-stage vein formation. We also conducted cathodoluminescence imaging and added figure 4. There we show the appearance of an exemplary vesicle filling (fig.s 4a, b) with one generation of botryoidal calcite and emphasize the variability of luminescence. We also show the CL images of ablation areas with the most precise age results (fig.s 4c, d, e) and an example of an ablation area with a less precise age result. We updated the "Results" section accordingly (lines 164–174) and used the findings to discuss the age data more carefully (section 5.1).**

I also have to comment that the geochemical data is not presented very convincing, I would like to see plots showing that the spots of the different domains show similar trace element composition e.g. using elemental ratio plots, incompatible/compatible.

**Reply: As emphasized in the text (e.g., lines 293–296) and documented by cited literature, trace element uptake during calcite precipitation is dependent on multiple factors. We use element ratios presented by Debruyne et al. (2016) that may help to trace back the origin of solutions from which the calcite was precipitated. The geological context in a volcanic environment (basalt breccia, vesicular basalt, pillow basalt), together with geochemical characteristics from calcite studied in the cited literature, convinced us that we present a valid interpretation. We present two convincing geochemical parameters in fig.7 (ex fig. 4e, f) discussed by Debruyne et al. (2016) as suitable proxies for oxic sea water.**

Figure caption of D1 does not explain the greyish field (whole rock data??).

**Reply: We clearified this in the figure captions of Appendix C. The grey area is an envelope of all REE/C1 values derived from our investigation.**

Having insitu Sr isotope data would support the interpretation.

**Reply: We thank the reviewer for pointing this out and will keep this in mind for future studies. In this case, having Sr isotope data was not part of the scope of this project and does not change the nature of these ages.**

Line 181-184: A rough overlap of U-Pb carbonate ages with metamorphic (meta-andesites) and felsic magmatism in the range of 350-314 Ma, does not prove that the obtained ages reflect calcite precipitation related to oceanic crust formation.

**Reply: All presented ages are interpreted as magmatic ages in the cited literature. We clarified this in lines 283–287. They underline the high probability of a Carboniferous age of the correlative units.**

The U-Pb data tables and metatables have to be prepared accordingly to Horstwood et al., to enable readers to understand the data quality and the method. Data tables should include U and Pb content calculated, the signal strength of 206 and/or 238 in cps, the Th/U ratio, and the rho value (even it is close to 0!).

**Reply: We agree with the reviewer and updated the supplementary data table S2 according to the exemplary table presented in Horstwood *et al.* (2016). The table contains the data of all unknowns and all standard analysis.**

Samples should be clearly separated in Table S2. Sample coordinates should be included.

**Reply: Samples are separated in S2 by the Identifier. A table with coordinates was added to S2.**

From the existing data table S2, I get similar ages and similar uncertainties but not the same numbers as reported!

**Reply: Since different calculation programs may result in slightly differing results, we now named the program that we used to handle the isotope data in line 182f. ("For age calculation we used the IsoplotR program (Vermeesch, 2018) version 3.5 running in R version 3.6.0."). We made sure that also the online version of this program (http://isoplotr.es.ucl.ac.uk/) returns the presented results.**

Figure C1, please include MSWD of each age, add the information that you used 2sigma uncertainties

**Reply: We added MSWD and information that errors are given as 2SE to fig. 5 and fig. 6 (ex appendix Figure C1).**

Line 149: 'they overlap within 2s-error for each sample'. Please explain this better

**Reply: We reworked our interpretation, as stated above.**

Line 169: … 'overlap mostly within 2s-error per sample' – I don't agree with using this as evidence they formed during one event. No, they differ in age, and looking at the scans in B4-B4, it is not compatible with the idea of one single event of vein formation.

**Reply: We agree with the reviewer about the complexity of basalt breccia samples 17NP436a and 17NP436b. We reevaluated our age data thoroughly. We focus more on individual ablation area ages (e.g. result presentation paragraphs 4.2.1 through 4.2.4) and discuss them in the light of microstructural data derived by petrologic light microscopy, cathodoluminescence imaging and XFM data. Indeed, the chemically deviating calcite found in the late-stage fissures also shows a bright yellow luminescence color. The late-stage fissure filling calcite is fine-sparitic. A distinction from the phase 2 and 3 coarse sparitic calcite was attempted during laser ablation. Age data with MSWD>2 and 2SE>10% was interpreted as mixed ages (lines 257–260) and discussed in the light of microstructural findings.**

Fig. 3: Quote the MSWD for each sample, they do not form a homogenous population, be careful to interpret this as one age. Use in addition the expanded uncertainty (see below/above). The intial Pb of the different domains scatter also quite a bit…

**Reply: See above. We agree on this and changed relevant passages in the text and related figures accordingly.**

Fig.4. figure caption, explain better, .e.g all data obtained from the different areas of the 4 samples. However, I do not agree that you can plot (U-Pb) them in this way together (see above).

**Reply: We agree on this and discarded this figure in the new version. We instead present individual ablation area ages in fig. 5b and fig. 6.**

These REE plots are for me not very convincing to support that the veins form during a single event…

Fig.5. Report all the ages with reasonable accuracy (including Ar-Ar ages, LA zircon ages, and the LA carb ages), so without figure behind the dot (only ID-TIMS data should be presented with sub-% accuracy!). It also makes ages not better accessible if more figures are reported.

**Reply: We changed the presented ages accordingly.**

## References


Horstwood, M.S.A., Košler, J., Gehrels, G., Jackson, S.E., McLean, N.M., Paton, C., Pearson, N.J., Sircombe, K., Sylvester, P., and Vermeesch, P., 2016, Community-derived standards for LA-ICP-MS U-(Th-) Pb geochronology–Uncertainty propagation, age interpretation and data reporting: Geostandards and Geoanalytical Research, v. 40, no. 3, p. 311–332.

Kylander-Clark, A.R.C., 2020, Expanding the limits of laser-ablation U–Pb calcite geochronology: Geochronology, v. 2, no. 2, p. 343–354, doi: 10.5194/gchron-2-343-2020.

Nuriel, P., Wotzlaw, J.-F., Ovtcharova, M., Vaks, A., Stremtan, C., Šala, M., Roberts, N.M.W., and Kylander-Clark, A.R.C., 2021, The use of ASH-15 flowstone as a matrix-matched reference material for laser-ablation U − Pb geochronology of calcite: Geochronology, v. 3, no. 1, p. 35–47, doi: 10.5194/gchron-3-35-2021.

Rasbury, E.T., and Cole, J.M., 2009, Directly dating geologic events: U-Pb dating of carbonates: Reviews of Geophysics, v. 47, no. 3.

Rasbury, E.T., Present, T.M., Northrup, P., Tappero, R.V., Lanzirotti, A., Cole, J.M., Wooton, K.M., and Hatton, K., 2021, Tools for uranium characterization in carbonate samples: case studies of natural U–Pb geochronology reference materials: Geochronology, v. 3, no. 1, p. 103–122, doi: 10.5194/gchron-3-103-2021.

Roberts, N.M.W., Drost, K., Horstwood, M.S.A., Condon, D.J., Chew, D., Drake, H., Milodowski, A.E., McLean, N.M., Smye, A.J., Walker, R.J., Haslam, R., Hodson, K., Imber, J., and Beaudoin, N., 2019, LA-ICP-MS U-Pb carbonate geochronology: strategies, progress, and application to fracture-fill calcite: Geochronology Discussion, doi: 10.5194/gchron-2019-15.

Roberts, N.M.W., Rasbury, E.T., Parrish, R.R., Smith, C.J., Horstwood, M.S.A., and Condon, D.J., 2017, A calcite reference material for LA-ICP-MS U-Pb geochronology: Geochemistry, Geophysics, Geosystems, v. 18, no. 7, p. 2807–2814, doi: 10.1002/2016GC006784.

Roberts, N.M.W., and Walker, R.J., 2016, U-Pb geochronology of calcite-mineralized faults: Absolute timing of rift-related fault events on the northeast Atlantic margin: Geology, v. 44, no. 7, p. 531–534.

Roberts, N.M.W., Žák, J., Vacek, F., and Sláma, J., 2021, No more blind dates with calcite: Fluid-flow vs. fault-slip along the Očkov thrust, Prague Basin: Geoscience Frontiers, v. 12, no. 4, p. 101143.