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Interactive comment on "U-Th-Pb discordia regression" *by* Pieter Vermeesch

Brenhin Keller (Editor)

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Thank you for the thorough responses to reviewers. I find that the revised manuscript addresses the reviewer comments well to the extent of my expertise. Due to the technical nature of the manuscript and of the revisions requested by Dr. Ickert, I have asked Dr. Ickert to comment further on the revised manuscript. I trust that the final proof will reflect your consideration of these comments, enclosed below.

Brenhin Keller

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Interactive comment

I would like to thank Dr. Vermeesch for responding positively to my long review, and also for the additions and that he has made to the manuscript – in my opinion this has made a good manuscript even stronger. At the request of the AE I was asked to look at the new case studies, and I have three brief comments.

1) These are excellent case studies and include both high and low $^{208}Pb^*/^{208}Pbc$. Mixed U-Th-Pb studies are rare, and Dr. Vermeesch is to be commended for finding good exemplars. 2) Unfortunately, there is a problem with the case study on allanite that may require that the Pb/U comparison results (not the results from the new algorithm) be adjusted. In ion microprobe analyses of REE-rich minerals like allanite and monazite, there is an unresolvable molecular interference near mass station 204 that complicates the measurement of ^{204}Pb. This is mentioned briefly in Janots and Rubatto (2014) near the top of page 158 and explains why the original authors did not utilize ^{204}Pb in any of their data analysis or interpretations. This isobar is described elsewhere in the literature by e.g. Gregory et al. (2007; 10.1016/j.chemgeo.2007.07.029) and Stern and Berman (2000; 10.1016/S0009-2541(00)00239-4) but is probably not well known outside of the ion microprobe community. My understanding is that it is likely to be a complex REE (possibly Nd?) molecule, but I am not aware of any published literature that can attest to this. It is easy to miss the short reference to this in the original manuscript. I am not intimately familiar with the original dataset, but I would think that if the original authors deem the ^{204}Pb data too suspect to be utilized, it is probably not suitable for an example dataset here. Adjusting the comparison to include only U, Th, and the radiogenic Pb isotopes (similar to the Parrish et al. dataset) should be straightforward and will not detract from the quality of the manuscript. 3) I would strongly suggest that the ages interpreted by the original authors be included for comparison. I understand and agree that an “apples-to-apples” comparison is useful and necessary in the context of this manuscript (in this case, a comparison of the new

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algorithm with a semi-total Pb/U isochron), but out of fairness to the original authors, their own interpretation should be compared as well. While this is *clearly not the intention of the author*, it is easy for a reader to misunderstand that the Pb/U ages in the manuscript (which are imprecise and may be inaccurate) are the interpretations presented in Parrish et al. and Janots and Rubatto. In reality, both original papers recognized that the best course of action is to combine all of the (^{204}Pb-free) U-Th-Pb data – in a broadly similar, but less rigorous, way than the present manuscript. The Parrish paper in particular is predicated on the utility of leveraging Th-Pb data along with U-Pb, when ^{204}Pb cannot be reliably measured, in order to improve the accuracy and precision of U-Th-Pb dates. Both Parrish et al. and Janots and Rubatto arrived at dates that are similar to those determined using the new algorithm. In both cases, the new dates are superior, both in rigour and in precision, so this takes nothing away from the utility of the new algorithm.

(signed)

Ryan Ickert

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