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

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The manuscript entitled "U-Th-Pb discordia regression" by P. Vermeesch describes a novel algorithm (implemented in the R statistical programming environment) that integrates the U-238 and U-235 U-Pb geochronometers with the relatively underutilized U-Th decay system. The algorithm is incorporated into the author's previously published IsoplotR package, permitting the calculation of isochrons and the generation of publication-quality graphics with ease. The paper is succinctly written and technically sound, and represents a valuable contribution to the U-Th-Pb literature. My comments are relatively minor in nature and, upon being addressed, I recommend publication of the manuscript in Geochronology.

Comments:

Page 1, Lines 3-4: "The 206Pb/238U and 207Pb/235U decay systems are routinely

combined to improve accuracy". May be more appropriate to have something along the lines of "...are routinely combined to improve the assessment of accuracy"?

Page 1, Lines 28-31: "Nevertheless, it manages to fit the data very well. The method should work even better for low-Th phases such as carbonates." These sentences are far too subjective and informal, please rewrite.

Page 1, Lines 62-62: "...variable proportions as a function of the Th/U-ratio and age." Technically, the proportions are a function of the Th/U-ratio, age, AND the 238U/235U-ratio. Here and later in the manuscript the author assumes the mean terrestrial zircon 238U/235U value (137.818, without uncertainty) of Hiess et al. (2012). While for many (most?) applications of the algorithm this assumption is possibly acceptable, insofar as broadening the general applicability of this approach I think it is worth stating this point explicitly. For example, the algorithm may find use in early Solar System/cosmochemical studies, in which the applicability of the Hiess et al. mean terrestrial zircon U isotopic composition is suspect, and U isotopic variability often observed and thereby requiring direct 238U/235U measurement.

Page 1, Lines 64-65 thru Page 2, Lines 1-4 (equations 1-4) As presented, equations 1-4 consist of only measured (m) and non-radiogenic, initial crystallization (c) Pb components, and assume only initial secular equilibrium. However, technically the equations also assume a trivial blank Pb component during analysis/sample loading. For example, equation 1 equates the measured Pb-204 with the non-radiogenic, initial Pb-204. This is only true in practice is the Pb-204 contribution from blank is trivial, which may not necessarily be the case (especially in the case of Pb!). Here and throughout the manuscript a quantitative blank correction is not addressed, which is fine, but if so a statement should be made here that the equations as currently formulated assume a trivial Pb blank component.

Page 2, Lines 71-74: "The fact that the new algorithm works very well for monazite implies that it is generally applicable low Th phases such as carbonates." First, there is

a "to" missing between "low" and "Th". More importantly, however, I suggest you show as oppose to tell on this point. You mention in the abstract, here, and in the concluding discussion that this approach is especially useful for carbonates. Couldn't you pull a representative carbonates dataset to demonstrate this point explicitly? I'd be interested to see this.

Page 3, Line 25 (fixed 238U/235U-ratio): See my comment above re. the U isotopic composition.

Page 5, Lines 16-17 (MSWD discussion): You mention in passing that data are "overdispersed if...MSWD »1". However, I think it worth stating a more general point about the acceptable MSWD range as a function of the number of degrees of freedom (i.e., data points), a la Wendt and Carl (1991). I think it worth citing Wendt and Carl (1991) here, as well as presenting a general formula for the range/uncertainty on the MSWD itself, beyond stating the oversimplification that data are overdispersed when MSWD»1.

Page 6, Line 77 (discussion of accessing algorithm in R): You should cite R for those not in the know, the most recent suggested citation I give below.

Relevant references: 1. Wendt, I. and C. Carl, 1991. The statistical distribution of the mean squared weighted deviation. Chemical Geology (Isotope Geoscience Section) 86(4):275–285, doi: 10.1016/0168-9622(91)90010-T. 2. R Core Team, 2017. R: A Language and Environment for Statistical Computing. R Foundation for Statistical Computing, Vienna, Austria. URL https://www.R-project.org/

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