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Comment on gchron-2021-7

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Community comment on "Short communication: Inverse isochron regression for Re–Os, K–Ca and other chronometers" by Yang Li and Pieter Vermeesch, Geochronology Discuss., https://doi.org/10.5194/gchron-2021-7-CC1, 2021

Community Comment by Ryan Ickert

This is a short manuscript describing how a simple change of variables can be used in isochron plots to reduce the magnitude of the uncertainty correlation. In cases where the uncertainty correlation is very high, the authors argue that these correlations obscure the data structure on conventional plots, making it difficult to use the plots to make decisions about how to use the data, and that the change of variables can be used to display the data in a better manner. The authors provide a set of three equations that can be used to transform the uncertainties and correlations from x/z vs y/z to x/y vs z/y and, apply these to two previously published datasets.

I agree with reviewer Davis that this approach is not new to isotope geochemistry. For example, the highly cited work by Williams (1998; Reviews in Economic Geology v7 p1-35) explains that one of "(t)he benefits of the Tera-Wasserburg plot (is)…the errors in measuring $^{207}Pb/^{206}Pb$ and $^{238}U/^{206}Pb$ are much more weakly correlated than those in measuring $^{207}Pb/^{235}U$ and $^{206}Pb/^{238}U$…". And in the popular textbook by McDougall and Harrison (1999; p 113) they state that "…a potential drawback of the conventional isochron plot is that, in general, the isotope measured with the poorest precision, ^{36}Ar, is common to both axes. A result is that the errors associated with both axes are highly correlated and may give rise to misleading linear correlations if the errors or the correlation coefficient are incorrectly estimated." They later go on "These problems are largely circumvented by an alternate form of isochron analysis in which $^{36}Ar/^{40}Ar$ is plotted against $^{39}Ar/^{40}Ar$…".

Although the authors of this manuscript appear to be aware that their work is not new (line 66) they don't make clear what differentiates this contribution from others. The manuscript would be improved if it were better able to highlight a novel contribution.

I agree that plotting highly correlated ellipses is rather annoying, and in rare cases can produce plots that obscure relationships between data – as do many other geochronologists, which is why, as discussed above, many use Tera-Wasserburg diagrams and Ar isotope inverse isochrons.

The argument that it more easily allows outlier identification is not particularly compelling: The Re-Os example in Fig 1 C is unconvincing – the outliers they "identify" on the plot are not clear, at least to me, and anyways that result is muddled somewhat by the fact that they have mixed samples of likely different ages on the same diagram (as described in the original paper). A better way to identify data that have undue weight on the MSWD is to simply inspect the variance normalized residuals and look for the largest values.

On the other hand, it would be nice to have the equation for the change of variables published somewhere clear and convenient, with a brief discussion of the (real) source(s) of correlation in geochronological data, and dispelling some of the myths about the change of variables. For the latter, there is a persistent belief in some workers that ages determined by one regression are better or more precise than using an inverse or vice versa (e.g., Connelly et al., 2017). This would be trivial for the authors to include, by producing regression analysis on both the isochron and it's inverse and demonstrating substantive equivalence. This is complicated somewhat by the fact that the two regressions become significantly distinct with extremely large uncertainties (as they state on line 100) and also with highly overdispersed data, but it is easy to carve out that as an exception.

As written, the manuscript gives a misleading impression about the origin of correlations in isotopic and geochronological data. While poor counting statistics on denominator isotopes may be important in some Ar isotope datasets, most uncertainty correlations in real, published datasets are due to other factors, such as fractionation corrections, interelement calibration, and blank corrections. See for example, Connelly et al. (2017 GCA V201 p345-363), Ludwig (1980 EPSL v46) and the isotope geochemistry textbook by Dickin (2005). This manuscript would be improved if this variety were better covered, however briefly.

I agree with Reviewer Davis that if the manuscript is edited to reflect changes along these lines, it would be a much better candidate for publication.

Minor elements:

Line 15-16: It is not stated that the other variables are the present day abundances (or relative abundances, as in the next equation in the text) of 187Os and 187Re and can be directly measured. It's probably worth making this explicit rather than implying it – for the non-specialist this might be unnecessarily confusing because everything else is defined.

Line 15: It's probably a good idea to list the sigma level/coverage factor/confidence limit/credibility interval for the uncertainty.

Line 19 (equation 2): The statement preceding the equation implies that normalizing to 188Os contributes to the equation no longer being underdetermined. This is not correct, of course, and I don't think that's what the authors intend to mean, so it should be clarified. There's really no reason to start at equation 1 – this isn't a lesson in the basics of isochrons – and for the sake of brevity it would probably be more useful to start with equation 2 and eliminate reference to equation 1 altogether.

Line 27: (Here and elsewhere). The word spurious is defined as "not being what it purports to be; false or fake" or "(of a line of reasoning) apparently but not actually valid". This adjective does not apply to these correlations, which, as the authors themselves demonstrate on line 29, are real, valid correlations. Perhaps the authors mean to suggest that they are annoying, distracting, or otherwise unwanted, but they are not at all spurious. A spurious correlation is something more akin to the classic "pirates are causing global warming" example (and many others, cf. https://www.tylervigen.com/spurious-correlations). This word should not be used in the manuscript to describe any of the correlations, which are all real.

Line 25-34: While this section is technically correct, it is misleading because many (probably most?) correlations in isochron-type plots that use more than one element (this is particularly true for measurements by isotope dilution like Rb-Sr, Sm-Nd, K-Ca, Re-Os etc.) are not due to the common denominator effect, but a common correction factor. This is explained in textbooks, such as Dickin (2005, 2nd edition page 36).

Line 35-43: This section is misleading, being predicated on a naïve reading of the isochron and the data, and ignores the original interpretation by Morelli et al. No one "at first glance" would read this plot as having an isochron age of 287 Ma – this is something that would be calculated. Such a calculation would almost inevitably be accompanied by an uncertainty and an MSWD, and the overdispersion would then be detected by inspection of the MSWD and p-value, regardless of whether the data are plotted. Consulting the original paper regarding the source of the overdispersion, reveals that these 16 analyses are an aggregate of 3 different samples. Individually, each of the three samples yield isochrons with little to no overdispersion. The original paper interprets the overdispersion evident upon aggregation as meaning that sample 20A has a different age than the other two. This information should be included in the manuscript and the "outlier identification" and original interpretation of the data should be discussed together.

Unlike points 1 and 3, point 2 is significant and should be the focus – highly correlated analyses are difficult to plot well and present a barrier to effective data communication.

Line 70-73: The authors seem to be unaware that low abundance denominators are not the only reason for correlations. Blank corrections can be a significant source of correlation (Connelly et al. 2017 GCA V201 p345-363), as can fractionation corrections (Ludwig, 1980 EPSL v46). It might be worth pointing this out here.

Equation 8: I've checked the equation and it appears to be correct.

Section 4: This whole section seems superfluous. A statement at the end of the manuscript stating that "these calculations are implemented in Isoplot R" is sufficient. The paragraph and screen grab are unnecessary.

Line 99: What does "mathematically equivalent" mean? For example, using the York et al. (2004) MLE algorithm on the Morelli dataset, the results from the conventional and inverse isochrons are 287.26+/- 1.70 Ma and 287.16 +/- 1.73 Ma. These agree within uncertainty but I don't think that 287.26 and 287.16 are "mathematically equivalent". They are not even consistent to within numerical precision (I assume, but do not know, that the difference is due to the Taylor series approximation)

Line 100: Like the previous reviewer, I think it would be fantastic and useful if the authors could expand on this, though I appreciate it may be out of scope. I would suggest that they also include mention that the regression and inverse will be also distinct if the data are highly overdispersed.

Conflict of interest: I am married to Associate Editor Marissa Tremblay