Author response to community comment CC5 for preprint gchron-2021-22

Issler, D. R., McDannell, K. T., O'Sullivan, P. B., and Lane, L. S.: Simulating sedimentary burial cycles – Part 2: Elemental-based multikinetic apatite fission-track interpretation and modelling techniques illustrated using examples from northern Yukon, Geochronology Discuss. [preprint], https://doi.org/10.5194/gchron-2021-22, in review, 2021.

Duddy and Green have taken a scattershot approach that attacks all aspects of our study. They conclude that both the elemental data and AFT data are unreliable but they do not seriously comment on why there is a strong correlation between independently determined AFT age populations and elemental composition for two different samples. Instead, they dismiss the correlation as something to be ignored. They interpret our reply (comment CC4) to their comment as a personal attack although it was not intended to be. Their early contributions to the field of AFT thermochronology are important and have had a strong influence on the research that we are doing today. However, their comment CC3 presents an unbalanced and biased view of our work that is based on inferring trends in our data that do not exist and therefore a firm response that questions their methods and logic is warranted (see our second reply AC4 to their comment CC3). Duddy and Green claim, "*Our initial comment was not written in defense of our methods*" and "*what we do is not at issue here*" yet they conveniently take the opportunity to promote and discuss their poorly documented model in two separate comments. Why is this even relevant if their focus is on "errors" in our study? All of the elements required for an objective scientific debate on competing methods (full disclosure of data including poor analyses and interpretation methods on both sides) are absent in this public discussion. The points we make are very relevant because the association between our "bad" data and their apparently better constrained model is an integral part of their argument.

The statement of Duddy and Green, "*That the EMPA data is too poor in quality to justify any conclusions regarding the utility, or otherwise, of r_{mr0} as a superior kinetic measure to either chlorine alone, or Dpar, is incontrovertible on the basis of our comment (CC3)*" is a provocative claim, given that they invoke nonexistent trends in our data to discredit our study (comment AC4). Some significantly low elemental totals are included in the analysis, but they are not critical to the overall interpretation. They provide results that are consistent with the better quality EPMA data, AFT population ages, and the high Dpar values beyond the range of kinetic population overlap. The suboptimal EPMA data were retained as qualitative information that are consistent with kinetic population assignments. We use a multi-parameter approach that provides some cross-checks on data interpretations. This is not possible to do when relying on a single parameter such as Cl and ignoring independent information from age mixture modelling. Importantly, our method does not rely on the absolute accuracy of eCl/r_{mr0} values for discriminating different kinetic populations. This is in recognition of the fact that natural samples will contain apatite with compositions that have not been encountered in the lab. All we require is the ability to identify different kinetic populations so we can apply the concept of relative annealing to model all the data based on principles that are already being used (i.e., the same annealing mechanism applies to all apatites but kinetic parameters vary with apatite composition).