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Interactive comment

Interactive comment on "ESR-thermochronometry of the Hida range of the Japanese Alps: Validation and future potential" *by* Georgina E. King et al.

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This study is a thorough and compelling application of ESR to low-temperature thermochronology to the Japanese Alps. The authors have put forward a tremendous effort to present a detailed comparison of multi-elevated-temperature IRSL results from feldspar against Al and Ti ESR centers within quartz. The results are encouraging and provide the reader with a broad overview of why such an advance is worthwhile (similar stability to OSL signals but applicable for slower cooling rates), while still carefully outlining methodology limitations (e.g., lack of automated measurement capabilities and uncertainties associated with reaction kinetics). Below, I offer several questions I had while reading along with a few suggestions or concerns.

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Discussion paper

Main text:

p.2,l.20: The averaging time(s) would be helpful for these rates.

p.2,l.30: Slightly unclear what 'paired' means in this context.

p.3,l.4: Here you could also mention that signal intensity is a persistent limitation for quartz OSL thermochronometry (unlike for ESR, apparently).

p.3,Eq.10ff.: The negative sign before the activation energy is difficult to see with the current typesetting.

p.3,l.30: "...a model that assumes a Gaussian distribution of activation energies, E_a around the mean trap depth, mu(E_t)(eV)." It would be good to also mention the meaning of sigma(E_t) here.

Besides the Lambert study in review, is there any precedent in ESR literature for treating the activation energy in this way? The reason for adopting this approach probably deserves either an available citation or further justification in the main text, even if only a sentence or two. The full explanation within the Supplementary Materials is excellent, but a quick note here would be good.

p.4,ll.8-9: Is this owing to the long irradiation times with common beta sources?

p.5,l.23: It seems misleading to label alpha as a 'constant' when it has a known functional form (e.g., Chen and McKeever, 1997, pp. 60-66) that depends upon the trap depth (which is often allowed to vary in studies such as this).

p.5,l.27: I believe that 'charge' encompasses electrons and electron holes.

p.6,ll.20-22: It seems important to qualify here that this result hinges on the assumption of a correct kinetic expression; this statement should not be misunderstood to mean that the authors have (at this stage in the manuscript) successfully recovered age information from slowly cooling samples, but that, to the degree that the kinetic expressions are accurate, slow cooling histories should be within resolution.

p.9,l.22: Strictly, you have quantified the room temperature detrapping. Presumably there is little thermal detrapping involved, but might be worth mentioning briefly to avoid confusion with LNT fading measurements.

p.11,l.19: That IRSL_50 signals are saturated and higher temperature signals are unsaturated in the same sample seems to me an inexplicable result. Can you comment on why this might be observed?

p.11,l.24: I could not find the King et al. (2018) citation within the references. 'Athermal field saturation values' seems to be an inappropriate concept. Even for traps which are considered stable over burial timescales (e.g., qz fast component), we still discuss 'trap lifetimes.' The same practice should apply for thermal detrapping that happens within feldspars at Earth's surface. Field saturation should therefore be understood to reflect athermal and thermal loss processes, even if athermal loss is expected to be dominant at lower temperatures.

p.11,l.32: Given that samples were taken from a transect that spans 1.2 km of elevation gain, shouldn't we expect more (and systematic) temperature variation with elevation? Most adiabatic lapse rates result in temperature loss of just under 10C per km of gained elevation.

p.12,ll.27-30: Is such variation in thermal stability between ESR and IRSL populations expected from previous work? Also, is there a reason to use OSL and IRSL interchangeably? I find it a little confusing and would prefer simply referring to 'luminescence' signals or IRSL results.

p.13,l.6: Please also mention that Grun et al. (1999) extracted quartz from granite.

Supplementary Materials:

From a physical standpoint, I'm a little dubious about the prediction that GOK decay predicts dose-dependent decay in a non-saturating system. The formulation of second-order kinetics (Garlick and Gibson, 1948) was developed for a phosphor where retrapping was predicted to increase dramatically as traps filled, therefore slowing recombination in a way that increases with dose. If, however, there is no upper limit to available trapping sites, this limitation should disappear. Therefore, I am skeptical that the transition from Eq.S3 to Eq.S7, while mathematically sound, is physically sensible.

p.5,ll.4-8: Wow! This difference in stability between OSL and ESR centers between samples is a really intriguing result!

p.5,l.17: "For all samples, the BTS model predicts..." Are there not many kinetic assumptions built into this prediction, including the nature and shape of the band-tail? In other words, could a higher stability be predicted if the tail were assumed to be quadratic or if the tailing factor were higher? Or, are all of these values sufficiently quantified for these samples? Perhaps this is what you reference in the final sentences of this paragraph?

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