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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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We thank Nathan Brown for his constructive review and respond to each of his comments in turn below.

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

Unfortunately this information is not given in the original publication, so it is not possible to add it to the text. As the saturation limits of the sample are also not described in the original paper, it is not possible for us to calculate the validity of these cooling rates for a particular time period.

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

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Removed paired.

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

As we have only explored this sample set at this point in time, we are reluctant to make a general statement about the behaviour of quartz ESR-thermochron signals. For this reason we respectfully choose not to make this addition.

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

We have added an additional space which makes the negative sign clearer.

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.

We believe that it is the first time that ESR signals have been modelled in this way. We added the following to the main text:

An alternative approach could be to use a first or second order kinetic model as has been done previously (Toyoda and Ikeya, 1991; Ikeya, 1993; Grün et al., 1999) and we discuss our model selection more completely in the supplementary material.

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

Because of the comparatively large volume of sample measured in ESR dating, it is not possible to use a beta source for irradiation. Multiple aliquot methods relate to the physical distance between gamma sources and measurement facilities, which mean

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that it is more convenient to dose multiple aliquots prior to measurement which must be done elsewhere. We have amended the sentence to make this clearer:

The former approach has only recently been made practical, following the introduction of X-ray irradiation for regenerative dosing (Oppermann and Tsukamoto, 2015), as opposed to gamma irradiation which is often done at a laboratory separate to the measurement laboratory.

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).

Amended to “alpha is a constant related to the Bohr radius of the electron trap”.

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

Agreed and amended.

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.

We think that this is implicit in the exercise performed here as it also holds for the OSL data described in the preceding lines, and prefer not to make any further qualification.

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.

We have inserted “at room temperature” to be explicit that these measurements were not made at LNT.

p.11,l.19: That IRSL_50 signals are saturated and higher temperature signals are un-

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saturated in the same sample seems to me an inexplicable result. Can you comment on why this might be observed?

This is not an uncommon observation for thermochronometry data (although much of these data remain unpublished at this time). It is simply because the IRSL50 signals exhibit much greater rates of fading than the higher temperature signals, meaning that they reach athermal steady state more rapidly. We have added this sentence for clarification:

“Saturation of the IRSL50 signals relative to the higher temperature signals is a consequence of their relatively high rate of anomalous fading”

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.

We follow the nomenclature of Kars et al. (2008); Valla et al. (2016) – QG and King et al. (2016) – QG. It is not necessary to invoke any thermal loss to explain the trapped charge concentrations of these samples, hence we refer to the athermal field saturation values. These values were calculated assuming no thermal losses using equation 8 in King et al. (2016) which is equivalent to equation 15 of Li and Li (2008). If we recalculated the trapped-charge concentrations using the full differential equation, i.e. including both thermal and athermal losses, the values would not change significantly, reflecting low rates of thermal detrapping at the surface temperatures experienced in this region. We prefer not to amend the text which clearly states that these values are calculated on an athermal basis. The citation to King et al. (2018) has now been included in the references.

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.

This is an interesting point that we have considered carefully. We were unable to find an average adiabatic lapse rate for the Japanese Alps in the literature, however as the climate here is humid, it is likely that the adiabatic lapse rate is closer to 5°C per km, rather than 10°C per km. As such, we think that our allowed uncertainty of ±5°C on our final temperature for the inversion is appropriate.

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.

We are unaware of previous work that has compared the ESR and luminescence signal stability of the same samples. This is something that we plan to pursue in future research. We found it encouraging that using the relative age differences between the luminescence and ESR signals, we could then predict what the relative difference in thermal stability should be, and that our experimental data was consistent with these predictions.

We follow Guralnik et al. (2015) – EPSL in using OSL to refer to generic optically stimulated luminescence signals, rather than being more specific regarding the type of stimulation (i.e. infrared stimulation).

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

Amended.

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.

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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.

This is a good point but for completeness we feel that is important to include this equation and consideration in the supplementary materials.

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?

Here when we refer to BTS or GAU a certain distribution of band-tails is implicit. I.e. for the BTS we assume an exponential distribution of band-tails below the conduction band, whereas for GAU we instead assume a distribution of activation energies around the trap depth. The absolute value of the distribution (i.e. band-tail width, or width of the Gauss distribution around the trap depth) is determined from fitting the isothermal holding data for that particular sample and system. The GAU model predicts higher thermal stability than the BTS model, because it assumes a different energy distribution.

We have added “sample-specific” to make it clearer that these values are calculated for the particular sample under investigation. The distributions assumed for the GAU and BTS models are described in equations 4 and 7 respectively.

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