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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 Reviewer 2 for their constructive comments and respond to them in line below:

(1) Last part of Chapter 3 and latter half of Fig.1: The authors once calculated the change with time of each signal intensity (Figs. 1b and c), then, using these results, they inverted to obtain the predicted cooling histories. Therefore, ideally, the red lines in latter half of Fig. 1 matches the white dashed lines, if I understand correctly. However, some of them are not. The authors should explain and discuss this point more clearly. It would partly because of the assumed initial condition, but there are cases that cannot be accepted, especially, slowest cooling rates for OSL centers. Probably,

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the discussion should be as such, in a case that the predicted cooling history obtained from OSL centers does not match that of ESR centers, the latter should be adopted. Then, this shall be applied to actual cases, i.e., Fig. 4.

Thank you for raising this important point. The reviewer is correct that the exercise shown in Fig. 1 tests whether the prescribed white line cooling histories can be recovered by inverting the forward modelled data shown in Figs 1a-c. The mis-match between the red and white lines provides some indication of signal performance, however the red-line is the median model of the accepted cooling histories used to generate the probability density function shown and rather the comparison should be made relative to the white line and brightly shaded parts of the PDFs. We have amended the latter part of section 3.2 to make this clearer by adding two sentences:

The results of the forward modelling and the synthetic inversions for the ESR and OSL data are shown in Fig. 1. The OSL signals for all cooling histories reach saturation (Fig. 1c), and this is reflected in the failure of the OSL to recover any of the cooling histories when inverted. This is apparent because the 1ïĄş confidence intervals show a broad range, with the highest density of cooling histories concentrated at temperatures < 20 °C over the past 500 ka indicating that the luminescence signals are saturated (as shown in Fig. 1c). The minimum cooling rate that can be resolved using OSL for sample KRG16-06 is ∼160 °C/Myr, calculated from 86% of the luminescence signal saturation level. Signal saturation is the key limitation that restricts the application of luminescence thermochronometry to regions undergoing rapid exhumation. In contrast, it is clear that the ESR data are able to resolve the 100 °C/Myr, 75 °C/Myr and 50 °C/Myr synthetic cooling histories, and cooling rates of 25 °C/Myr are distinct from isothermal holding at 0 °C over timescales of ∼2 Ma. This is apparent because of the coincidence between the prescribed cooling histories (white lines) and the highest density of accepted cooling histories shown by the brightest colours in the probability density functions. These results are significant as they show that ESR-thermochronometry is applicable in a range of geological settings beyond the rapidly exhuming locations that

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luminescence-thermochronometry is currently restricted to.

(2) Discussions for Fig. 4: Probably, for samples KRG16-101 and 104, the results for all signals seems consistent, however, for the other two samples, they look inconsistent. The authors may use the criterion in (1), or may abandon the modelling. There should be cases that the results from different signals are not consistent with each other, then modelling of cooling history cannot be made from the statistical point of view. Probably Eq 10 would be for this. What are the L values for these?

The reviewer is correct that it is easier to combine some ESR/OSL signals than others. The reason is likely a combination of factors including natural sample variability, experimental data and/or numerical model limitations. In order to treat the data objectively, all numerical modelling was done under the same conditions i.e. with the same initial condition, over the same time-period with the same range of final temperatures and for the same number of iterations. As it was comparatively challenging to fit KRG16-06 fewer cooling histories were accepted after the values of L (Eqs. 9 and 10) were treated with the rejection algorithm i.e. L is contrasted with a random number between 0 and 1, if L is greater the cooling history is retained. The benefit of using this approach is that the full range of possible cooling histories are accepted. If the data could not be fitted, no cooling histories would be accepted, and this is something that we have observed multiple times for OSL data, but not for the samples presented here.

Detailed points Page 2 line 6: “later” should be “at higher doses”

Amended.

Page 3 Eqs.1 and 2: “Ea – μ(Et)” should be “Ea”.

Thank you for spotting this. We have amended the equation.

Page 5 eq. 5: The first term, “Ea” should be “Eb”, second term, “Et – Eb” should be “Eb”.

Thank you for spotting this. Ea should be Eb. Amended. However Et-Eb should not be Eb. Eb is the energy of the particular band-tail state, the total energy to escape the trap is Et-Eb i.e. the trap-depth minus the energy of the band-tail width.

Page 6 line 1: What is n_mod?

We have clarified the sentence so that it reads:

“For each t-T path we calculated a misfit between the final inverted trapped-charge population, n ÌČ_mod,Âň and our forward modelled values, n ÌČ_fwd (Wheelock et al., 2015),”

Thus n_mod is the final inverted trapped-charge population.

Page 6 lines 1-8: What is m? Probably number of traps.

Yes, this is defined on line 6 of the original submission “for m traps”.

Page 6 Eq. 9: Is this summation from 1 to m? If so, it is not clear.

This is summed over m traps. We do not know how to make the nomenclature clearer.

Page 7 line 17: Correct the inequality sign.

Sorry, however we are unsure what the reviewer is referring to. The quartz extracts have a density >2.58 and <2.70 hence 2.58><2.70 g cm-3.

Page 8 line 7: “fitted” is by the least square method? How Ti-Li and Ti-H centers ratio was assumed?

Our measurements were carried out at -150 ïĆřC which meant that we were unable to differentiate between the Ti-Li and Ti-H centres. We have added a sentence stating this explicitly to the text.

“As our measurements were carried out at -150 °C, it was not possible to differentiate between the Ti-H and Ti-Li centres, and consequently they have been treated as a single centre.”

Page 10 line 3: What is “signal intensity experiment”?

The signal intensity experiment is described in section 4.3.1 (page 9, line 14 of the original submission). It comprised measurement of how the signal intensity changed as a function of changing preheat treatment.

Page 10 line 3: Is “plateau” preheat plateau?

Yes, although not in the usual sense. It is a plateau in signal intensity with changing preheat temperature. We feel this is clear from the sentence “The signal intensity experiment indicates a plateau for the Ti-centre of sample KRG16-104 up until 160 °C”

Section 5.1: One example of observed ESR spectrum should be shown together with a fitted spectrum.

We have added a new figure 2 showing an ESR spectrum for sample KRG16-06 and how the Al and Ti-centres were fitted (attached to this response).

Page 10 line 17: Correct the values and/or sample number.

Thank you for spotting this. Text updated.

Page 10 line 19: KRG16-112 is not listed in the Table.

No. This sample was used only in the dose recovery test. Therefore, it is not possible to include its details in this table. We amended the text on page 10, line 19 of the original submission to read “full dose response and isothermal decay was not measured for sample KRG16-112 and it is not included in Table 1”

Page 10 lines 20-23: Section 4.3.1 probably says the authors adopted regenerative protocol, but the dose response in Figs. 3 are additive dose.

The reviewer is correct and we tried to be explicit about this in the text. We measured the De values of our samples using a regenerative protocol as detailed in section 4.3.1. However, for measurement of the dose response into saturation we used an additive dose protocol. We have clarified this in the final sentence of section 4.3.2:

"Using a new aliquot of each sample, dose response was measured using the same measurement protocol, but omitting the zero-point measurement step, i.e. in an additive dose response protocol."

Page 13 line 1, Fig. 5: The signals seem to reduce too much. Please check the number in horizontal axis.

We re-checked these calculations and the figures are correct. Both the band-tail states model for luminescence and the GAUSS model used here for the ESR samples predict lower thermal stability than a single first order kinetic model.

Page 13 line 26, "consistent": Please describe how consistent

Qualifying how consistent the data are is difficult within the remit of this study, and is the focus of currently ongoing work. Ito et al. (2013; 2017) and Spencer et al. (2019) reported extremely young U-Pb and Zircon (U-Th-He) ages for this site. The fact that the luminescence data are not saturated is consistent with this. We have tried to give further information, whilst avoiding a lengthy discussion on this topic, which we feel is outside of the remit of this study, by amending this sentence to:

"The data inversions reveal that rates of rock cooling in the Hida range of the Japanese Alps are consistent with previous investigations that indicate rapid rock cooling (Ito et al., 2013; 2017; Spencer et al., 2019)."

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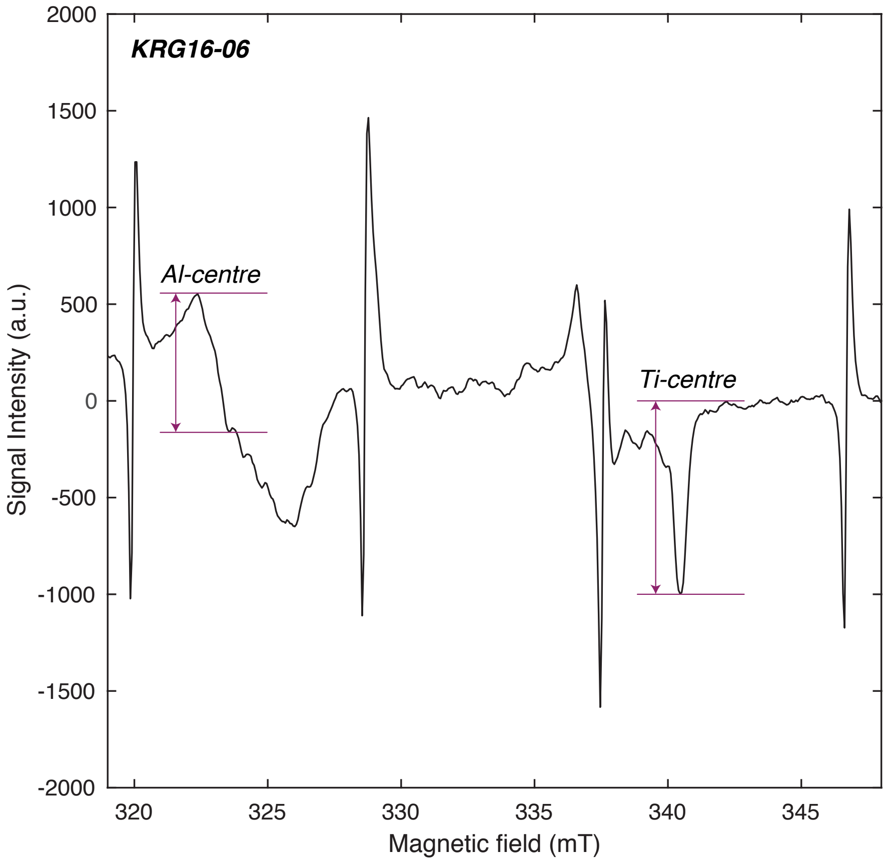

Fig. 1. ESR Spectrum (new Fig 2 in revised text)