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Interactive comment on "A closer look at IRSL SAR fading data and their implication for luminescence dating" *by* Annette Kadereit et al.

Anonymous Referee #1

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The manuscript :

The manuscript under review presents a series of laboratory measurements intended to test the dependence of anomalous fading rate on the thermal environments of the IRSL measurements. Some of the conclusions deduced by the authors are shown graphically in the abstract; over short-term delay times they observe an increase of IRSL due to a delay in the cooling of the sample holder to reach the read-out T requested in the sequence (my understanding. . .), followed by a sharp decline in IRSL that is apparently tempering out at longer time delays. The authors thus propose that strict thermal conditions are required in AF studies and among other arguments would also suggest preheating irradiated aliquots after the pause in time necessary for assessing fading, which sits against the original protocol set up by Auclair et al.

Introduction to the review:

Before giving my appreciation of this manuscript, I would claim that I am a specialist in anomalous fading of feldspar luminescence. My group has carried out thousands of AF tests and we have been challenged quite a few times with scatter, lack of reproducibility, apparent trends between aliquots etc. Overall, we have at times observed plateaus or slight increases of IRSL during the early part of the AF experiments which we attributed to recuperation. Is this due to the thermal effects described here? Maybe but I do not think so (see comment below). However, this short-term malign behaviour seems to disappear for extended time delays and the rule of thumb is to try to stay away from very short delay measurements. This being said, let me remind every laboratory practitioner involved in feldspar luminescence that the assessment of g values involves measurement of minute luminescence intensity differences between aliquots and over an extended period of time. This is a very difficult task indeed.

The actual review

I must first admit that even though I know a lot about AF, I had to read the paper over and over again and I am not sure yet that I do understand exactly what the authors have been doing and why. In terms of format, the manuscript requires extensive rewriting and graphs revision before resubmission. The authors may wish to revise the document following the propositions below or at least provide arguments to support the claims carry by the paper. At this stage, I consider the manuscript should not be accepted for publication.

First of all, I have serious concerns about the scientific arguments of the manuscript.

1. When engaging in a scientific endeavour, one needs to have strong evidence that there is a large issue on hand that has been overlooked by the community. Here, there is a pervasive reference to a paper by Rhodius et al that would have shown unequivocally fundamental failures about the protocol of Auclair et al for measuring AF. Just for a quick reminder, the latter is one of the most referred papers in AF since its publication.

There are probably hundreds of publications in which this protocol has been used and found reliable. The rare papers that do not consider the AF g values measurements as accurate using Auclair et al are those dealing with high-temperature pIRIR (in which are small ca 1% g values reported. . .are they real?) and those for which applications of g values to the uncorrected ages results in age overestimations. It is not this reviewer job to judge these contributions but I know by experience that this is observed mostly in the context of possible partial bleaching of feldspar IRSL such as for fluvial and alluvial sediments. In the case of the Rhodius paper, the measurements are carried out on rock slices, a difficult and relatively new application for which some aspects of age determinations could be questioned. 2. The proposal of Auclair et al to preheat just after the irradiation is to try to get the charge distribution as close to that of the natural as possible during storage. To preheat after storage thermally transfer electrons back in the dated trap. This is easily observed. Also, you may want to measure AF using short-shines so you need to preheat first as well (eg Huntley and Lamothe, 2001). 3. In science, if one wishes to test a protocol, the way to do so is to change only one variable at the time and compare results before and after a qualitative or quantitative change. We are faced here with an arsenal of methodological changes that do not allow the reader to make his own idea about the results of anyone experiment. In that sense, the methodology is not designed to allow drawing unequivocal conclusions. Therefore, please simplify the experimental tests and organize them so the argument of the experiment is structured. I understand that Figure 1 is intended to explain the readers just that but unfortunately, this figure does not seem to do the job. 4. If you wish to test new methodologies for assessing AF, you need 1) a monomineralogical sample, coarse grains K-feldspars in the best case; 2) a feldspar showing unequivocal fading, and 3) a bright thus highly dose sensitive feldspar. The samples used in this study are just not appropriate. IRSL emission from polyminerals may be from K-feldspar but also from albite or from another low-K feldspar even maybe from clay minerals. Samples are dim except for sample 713, a coarse grains K-fds extract shown on figures 7 and 8. The log decay is very clear therein and there is no short-term increase. . ..

5. For the three stages of luminescence decays: plateau or increase, then decrease then plateau again. . .there are several examples of anomalous fading decay curves you could find in the literature (Huntley and Lamothe, 2001; Huntley and Lian 2006 and several others. . .) for which there is absolutely no evidence for cessation of fading. From the early work of Wintle and Visocekas to the more recent contributions of Huntley (see his paper in 2006 about the t-1 law), the dependence of the decay of luminescence intensity over the log of time has been clearly demonstrated. Therefore these three stages if observed are due to hardware-based temperature variability for the first part and absence of long storage times for the fading "flattening". . .or the decline could be an artifact for a non-fading feldspar. . .whatever the cause, this fading structure has nothing universal. There could be contexts in which one may have issues in detection of AF decay as the non-fading component becomes dominant but fading continues. . . There is no such thing as the nonsensical expression "expiring of fading" as written on line 450; quantum mechanical tunnelling does not take breaks. . . Among technical changes that are requested: 1. Some sentences are difficult to understand due to poor English grammar (eg lines 350 to 355 as an obvious example). 2. The experiments are difficult to follow because the authors have decided to use acronyms of their own for describing their protocol. The use of terms such as LAB or NRM for normal Lx and Tx is very confusing. I would also require that the authors use Lx/Tx as the y axis instead of relative intensity (it is a sensitivity-corrected signal) for most of their figures. Use delay instead of pause (or storage). 3. Every figure should show only one stimulation time range (0-10 sec is fine), the use of three time ranges crams the graphics. 4. Each value needs to be properly calculated by subtracting the early part of the shine down by the late light, as done everywhere else. I do not get here the argument of why one decides to change a universal measurement protocol. 5. For the x axis, you are required to use a log of (time/tc) in which tc is approximately the half irradiation time plus the time between irradiation and measurement. You then get the zero point right and should be able to properly test decay log-linearity. 6. The extra-heating on another position: why? To get the heating plate always a bit hotter for a delay as it would be for a prompt

measurement? Then is this not a problem of hardware as the thermocouple is not doing its job? 7. The experiment of having a read-out temperature lower than the lift-up T may be interesting but this is not a routine way to measure luminescence, contrary to the impression we get from reading this part of the manuscript. 8. It is not a good idea to make any luminescence measurements at room temperature as this T may be different from day to day, hours to hours, minutes to minutes. . .always use some higher than RT temperature to measure IRSL. We use 50C, some have used 32C in my lab for a while, the idea is to control the temperature. . . 9. The conclusion for figure 1b is that extrapolation to extended times results in a large reduction of the signal. . . is this a problem? This has been known for decades (see Visocekas for extrapolation to the age of the Earth). . .the argument normally is that after some time the decay from the lab dose is the same as that in the field. . .reaching thus a state of quasi-equilibrium (see Lamothe et al 2003). 10. You need to properly refer to those early workers who have observed the relation between temperature and IRSL emission in feldspar, Bailiff and Poolton, Duller . . .this property has been known for some times. Along the same lines, it is a common practice in the Auclair et al protocol to run several prompt measurements to fix clearly the zero point on the (log of time) x axis. Our lab and others in the world have been doing this in routine for years. I would ask the authors to remove their strange claim that they have discovered this procedure. 11. I should point out that if you measure fading on a set of MAA, you need to subtract first the natural signal. . .I cannot see if this was done in the papers for which there was some "problems" (Lang, Rhodius, Kadereit. . .).

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