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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.

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Dear Anonymous Referee,

I appreciate the thorough and critical comments on our manuscript, to which I may respond as part of the open discussion encouraged by Geochronology.

When Annette approached me with early results in 2017 that would later be part of this manuscript, it became, very soon, clear to me that a publication of these results will become tricky. But still, I am happy to see these challenging comments, because what we need is a proper discussion and an exchange of arguments.

You open the comments with a request for an "extensive rewriting and revision of graphs". This is a bold statement, and it indicates that we somehow failed to lay out

reasoning and results convincingly in the reviewer's eyes, and we will certainly try to do better. Putting this aside, I have to admit that I sometimes struggled with these rather general comments. They read clear and convincing in the first place, but sometimes they lack the link to our manuscript and our results. Nevertheless, I tried to address your concerns with this quick response so that we may clarify some points before we prepare an improved version of the manuscript.

1 Response to (1)

I found the comments from that point very difficult to address because they do not refer to the results presented in our manuscript. The bottom line seems to be that you are unhappy with the repeated reference to Rhodius et al. (2015) while the paper by Auclair et al. (2003) is *"Just for a quick reminder, the latter is one of the most referred papers in AF since its publication."* I do not intend to play down the impact of the work by Auclair et al. (2003). It was, and is, a solid piece of work, a fact which is not questioned by our work. Do you expect us to add something in particular to the text?

2 Response to (2)

I am not sure whether I got the point. Do you want us to discuss the possibility of thermal transfer? You refer to Auclair et al. (2003) who wrote that *"g-values that are underestimated, probably as the result of thermal transfer."* This is something we mentioned very early in the manuscript in Sec. 1.4 and, of course, later before Sec. 4.2.

3 Response to (3)

Here you wrote that *"In that sense, the methodology is not designed to allow drawing unequivocal conclusions."* I admit that I can understand that the presented experiments appear rather dense, and perhaps we should try to simplify the writing further. But the here made general statement does not provide much guidance.

For instance. In Sec. 3.1 the header states that we compare "IRSL readout without vs. IRSL readout with thermal assistance". We then present results from experiments for a DA12 and a DA20 luminescence reader (Sec. 3.1.1 and Sec. 3.1.2). Sec. 3.1.1 is structured as follows:

1. We provide a context for the loess sample used in our experiment
2. We point out that, with the same sample MAA and SAR shows different behavior in terms of measured fading
3. We illustrate the effect of different pause (delay) times
4. The we apply IRSL at room temperature and with thermal assistance (heating)
5. We present the results

Again, I admit that it takes time and needs a careful reading of the text to follow the experimental design. So, of course, we are happy for any hint to improve the readability without neglecting essential technical details.

4 Response to (4)

Your comment claims that our samples are just not appropriate for our experiments. This argument, however, twists the aim of our manuscript. What we did was to test and question the way fading is commonly measured. We questioned whether the SAR approach is the most appropriate way to do so, given our results, and measured with the samples at hand. We did not examine fading and we did not claim that our results change underlying physical assumptions.

One could say that we used the wrong samples, and because of this, we measured what we measured. But what does it imply for all the fading measurements out in the wild using 'non-perfect' samples? What we found, we found highly reproducible, and you are right, maybe this is all just related to our samples, and our measurement conditions caused this effect. But still, the impact is real and would bias fading measurements in the way they are currently done.

> *"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"*

Dim does not mean unsuitable and should not be understood that way. More important, only the Mexican lake sample (HDS-1712) was a rather dim (Figs. 5 6), and all samples are fine-grain samples.

A plateau would be best visible with more data points at the beginning, which was not the case for this experiment. However, you can still see a plateau and an overshooting (e.g., Fig. 8f). However, we will try to make this clear in the figure caption to avoid this misunderstanding.

5 Response to (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.

We did not question this in our manuscript.

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.

This is somehow similar to my response above and fully in line with our argumentation: Whatever it is, it has nothing to do with 'true' fading.

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 tunneling does not take breaks.

I do agree and we did not claim this, we just repeated observations and this has something to do with the way it gets measured and is not related to 'true' fading.

6 Technical issues

These are valuable comments and we will address them separately in more detail. We understand that the experiments are difficult to follow, they are indeed complex and we will try to further simplify the text.

A few preliminary questions/comments here:

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).

Normalizing signals is useful to compare results with different L_x/T_x values and nothing special. For the moment, I cannot see what we would gain from plotting L_x/T_x values, except that it would be harder to compare the curves. Please correct me if I fundamentally overlook an aspect that needs to be pointed out by plotting L_x/T_x values. The term 'pause' was chosen because it is a 'pause' that is requested in the sequence editor and not a 'delay' (which does not mean that it cannot be changed).

Every figure should show only one stimulation time range (0-10 sec is fine), the use of three time ranges crams the graphics.

This would be only meaningful for curves where the 1-10 s range matters while the rest does not.

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.

What would be *'everywhere else'* and *'universal measurement protocol'*. To avoid misunderstanding, I am sincerely not sure what is meant here. Do you want to imply that there is something wrong in particular with our results? What would (probably) change?

> *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.*

I do remember that we discussed this, and I even had in mind that we double-checked this, but it does not change the results. Anyway, of course, I will recheck it with the other authors.

> *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*

No, as written in the text, to have thermal assistance due to, if you want to say so, 'cross-talk' of the heating. It is a rather strange experiment, I do agree, but it does not make a claim on the thermocouple.

> *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.*

This experiment was performed to show the influence of possible heat assistance, varying/decaying with delay time. We will clarify this in the text.

> *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.*

The temperature in the room was controlled, given its location in the basement, and stable during the time of the experiments. We cannot exclude minor temperature differences though.

> *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).*

I don't see a problem, but we will discuss this. However, the result was striking, because Lang et al. (1996) could produce MIS 2 – MIS 5 IRSL-ages which were in correspondence with the ^{14}C ages (of course where such a match was possible). If fading would be have been an issue such results would have been unlikely. Maybe something else went wrong, and they got the results in agreement by chance, but for obvious reasons, such an assumption would not be a sensible approach.

> *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.

Of course, we should adequately cite previous work, and we will recheck this part. To my understanding, we did not claim that we have discovered this but pointed this out (again). The problem with these 'common' procedures that may have been used or not in different labs is all the same: I do agree, many have seen this problem before, indeed, this is nothing new, but we should discuss what does it mean for the current approach to measuring fading.

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. . .).

Rhodius et al. (2015) did not apply MAA, but SAR. Further, they used the Auclair et al. (2003) protocol, but also a variant of it. Presented in their publication are both results. For the other articles, we refer to our supplement (supplement 1) were presented the MAA fading test procedure applied by Lang et al. (1996) who had published their papers in renowned peer-reviewed journals. For constructing the MAA dose-response curve you always use the laboratory dose on top of the natural dose. It does not seem mandatory to first subtract the natural signal. Although this might lead to possible stronger fading (theoretically), the subtraction would lead to larger errors following Gaussian error propagation, which in turn could camouflage true fading. There seem to be pros and cons to this issue, which, however, is not the focus of the present manuscript.

We are, I guess I can speak for all authors of this manuscript, looking forward to continuing this discussion.

Best regards,

Sebastian Kreutzer

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