**RC1**: 'Comment on egusphere-2025-4186', Anonymous Referee #1, 20 Sep 2025
In this manuscript, the authors report on <<Anomalous fading correction in luminescence dating – a mathematical reappraisal>>. In my opinion, this manuscript is very interesting, as it addresses age correction in relation to the anomalous fading effect. The complexity of the expressions is not strictly mathematical but rather based on extended algebra, which makes them both accessible and applicable. I am confident that future readers will enjoy the manuscript and benefit from its content. Therefore, I recommend its publication in its present form.

We thank the reviewer for their positive assessment of our work.

**RC2**: 'Comment on egusphere-2025-4186', Anonymous Referee #2, 30 Jan 2026
This project is an important step forward in developing closed form expressions for values and uncertainty propagation for common fading corrections. I imagine this represents a significant amount of work to parse these equations into their present forms. Additionally, I enjoyed the having the historical progression of fading expressions laid out clearly. My comments below are less focused on these expressions, and more related to the editorial tone of the first half of the manuscript, which I found distracting. I also disagree with the authors' assertion that Visocekas meant to describe tunneling as a power-law phenomenon. Overall though, this is manuscript should be of great interest to the luminescence dating community.

We thank the reviewer for their positive assessment of our work.

Line edits: l.27-31: The tone is unnecessarily pejorative. There is no need to characterize the broader community as confused, ignorant, and progress-avoidant with respect to fading phenomena. Nor to discredit efforts to provide transparent fading correction packages, such as those in the R 'Luminescence' package, for example, which implement several published correction routines.

Amended as proposed. Whilst we never referred to our community as “confused”, “ignorant”, and “progress-avoidant”, we appreciate that at times the tone of the manuscript should have been moderated. We have rephrased the text throughout and in particular the opening paragraphs (lines 26-100).

l.38-46: The authors are shadowboxing a bit here. No one in the luminescence dating community is hampered by Wintle failing to develop a mathematical expression for anomalous fading in 1973.

Whilst we agree, it is nevertheless interesting to see how prevalent the mathematical inaccuracies are throughout this particular sub-discipline of luminescence dating. We have rephrased the text in this part of the manuscript following our response to the previous comment.

And Huntley and Lamothe's statement is true: their g-value correction, as formulated within Huntley and Lamothe (2001), is only appropriate for the linear portion of the dose response curve. In Lamothe et al. (2003), for example, they extend the framework to deal with samples nearing dose saturation, but reiterate that the expressions laid out in the appendix of Huntley and Lamothe (2001), apply only when luminescence intensity is proportional to dose. That is, the linear portion of the dose response curve. At higher doses, intensity is no longer linearly proportional to dose and therefore this correction would be inappropriate. This was echoed in Huntley and Lian (2006): "Huntley and Lamothe (2001) showed how to correct ages for tunneling for samples which are on the low-dose linear region of the dose response, but we do not yet know how to correct ages for older samples which are on the non-linear region of the dose response curve."

> Amended as proposed. Although H&L's linearity prerequisite is neither underpinned by any mathematics nor does it stand the test of experiment (cf. Lamothe et al., 2003; this work Fig. 6), we accept the reviewer's point that linearity is a valid ad-hoc assumption, making later inferences building on it true within their framework. We have therefore removed our statements criticizing the linearity assumption.

"which nevertheless became a legitimate excuse for when corrected ages fall short of their independent constraints." This seems to ignore the subsequent paper, Lamothe et al. (2003), which extended the g-value framework to the laboratory dose-response measurements, their so-called "dose-rate correction equation." On a related note, I am surprised to not see this Lamothe et al. (2003) correction engaged with more thoroughly in this study, as this was one of the approaches advocated by the original others when dealing with high-dose regions. And subsequent comparisons between nearest-neighbor models and g-value models have compared Kars et al. (2008) to Lamothe et al. (2003). For example, in Kars and Wallinga (2009).

> Amended as proposed. While Lamothe et al. (2003) demonstrably did not introduce any new model or maths, we agree with the reviewer that it is a milestone interpretation of H&L2001 that deserved more thorough attention than we had initially given. Consequently, we added a whole new section 3.2 and figure 3 (lines 294-357) exploring and explaining the handful of divergent methods, which came to be referred to as the "Lamothe et al. (2003) correction" (now also included in our final roundup of corrected ages vis-à-vis independent constraints, in Fig. 5c).

l.73-77: The rescaling of kappa is reported in the R Luminescence package documentation for the calc_FadingCorr function, though I agree with the authors that this expression is difficult to find.

> Amended as proposed. We thank the reviewer for drawing our attention to this reference. Incidentally the code gives the formula without derivation, citing a private communication, one and a half decades after Huntley and Lamothe's (2001) paper (lines 80-84). This reinforces our point about grey literature surrounding fading and fading correction.

l.82-82: What about the semi-analytical solution described in Jain et al. (2012) (doi: 10.1088/0953-8984/24/38/385402)?

Suggestion not implemented. Jain et al.'s (2012) semianalytical solution does not have an explicit irradiation term (or stage), which makes their treatment irrelevant to the argument we're making.

l.85-94: Here also I find the tone here to be extraneous. Impugning past workers ("taken as unquestionable truths" "to the best of its authors' understanding") is neither helpful nor appropriate.

Amended as proposed. The rephrased statements are in lines 31-32, 92-93. Again, our spotlight is on the ambiguity and opacity of the seminal papers, and not on the inevitable different interpretations.

l.110-117: I disagree with the authors' assertion that the "proliferation of these faux units in the literature ("% [loss] per decade")...leaves little doubt that "logarithmic decay"...was in fact a misnomer." Visocekas (1985)'s Fig. 2 plainly shows decay that is logarithmic, not power-law. Maybe one could argue that there is ambiguity in the phrase "% loss per decade." For example, the phrase could be interpreted to mean "% (of original quantity) loss per decade" or "% (of remaining quantity) loss per decade." The authors view the latter concept as more useful, which is fine and the merits of this view should be developed. But based on the description, mathematical expressions and figures of Visocekas (1985), he intended to describe decay as logarithmic and he meant--unambiguously, I think--the former concept. More critically, redefining kappa as the slope on a power-law plot, rather than the slope on a logarithmic (% remaining vs. log(t/tc)) plot, and then describing this expression as kappa "sensu Huntley and Lamothe, 2001" has the potential to deepen confusion.

Amended as proposed. We thank the reviewer from stopping us from ascribing intent to either Visocekas, or conflating our definition with that of Huntley and Lamothe, 2001. However there is no ambiguity whatsoever between Visocekas' $t_c \Delta I / I_c \Delta t$ and our $(t \cdot dI)/(I \cdot dt)$, which are the finite and infinitesimal measures of the same quantity. We have reworded our analysis more carefully (lines 119-120, 125-127).

l.127-129: "This 'feature' of the model bothered Visocekas (1985)..." I am not convinced that it did bother him. From his p. 524: "The case of large storage duration t which, from equation (3), would lead to negative values for the TL remaining, is the most easy to deal with...At a sufficient long time the holes [within which all nearby traps have been emptied] will start to intersect. When this occurs the centers are no longer independent and the (1/t) law does not apply. The fading rate approaches zero as the spheres fill the entire space and no trapped charge remains." Based on passages like this, I interpret his view to be that logarithmic decay is a physically meaningful description, but only relevant when recombination centers are independent. It is a fair criticism that this leaves the behavior at long storage times undefined.

Amended (lines 133-136). We respectfully disagree with the reviewer's assessment of Visocekas' manner of addressing negative concentrations: exactly half of Visocekas' 1985 paper's maths are invested in mitigating negative

TL values, unfortunately in an irreproducible manner (e.g. the term $\log(1+t/\theta)$ in his Eq. 7 must be omitted in order to reproduce $F$ in his Fig. 3). In practice, these expressions cannot be found in any later fading-correction approach.

l.132: "Eq. (5) is the physical model that Visocekas probably stived for" The physical model described in his papers was of logarithmic decay. If the authors prefer another model, they should argue its benefits. The implication that the authors succeeded where Visocekas failed is gratuitous.

Amended as proposed. “Contrary to the heuristic Eq. (4), Eq. (5) is a physical model, where concentration remains positive n>0 at all times”. (lines 139-140).

l.140: There is a factor of 100 missing from the definition of g-value.

Amended (lines 144-155). We respectfully disagree: hardcoding units into equations goes against all metrology principles of BIPM. In our initial submission we omitted this factor 100 as subtly as we could, without highlighting that neither of the seminal texts (Visocekas, 1985; Aitken, 1985; Huntley and Lamothe, 2001) aligns with basic BIMP standards. However, since the reviewer’s confusion on this matter is likely representative of that of other luminescence practitioners, we added a focused discussion regarding the malpractice of unit hardcoding into formulas and are explicit about the exclusion of the factor.

l.183: Please expand on the meaning of 1.8. Ex., from Jain et al. (2012).

Suggestion not implemented. We hold on to our comprehensive explanation, already given in the original manuscript: “note that 1.8 is a totally arbitrary constant, that improved the approximation in the few scenarios considered by Huntley (2006)”

l.205-209: "such a definition is broad enough" I do not follow this statement. Surely D_e / Ddot_lab and t_lab = 30 days are very different numbers in this context? Please clarify.

Amended (lines 224-230). The entire paragraph was rewritten for clarity. Furthermore, we realized that the correct tc should have been D_e / 2 Ddot_lab, as per the commonly used “half irradiation time”. The factor of 2 propagates in a straightforward manner into Eqs. (9b) and (10), and we remark on how any other approximation of tc will affect these equations (lines 254-255).

Eqs 10, 11; 16, 17: These equations are very useful contributions!

We thank the reviewer for recognizing the value in these expressions.

**CC2**: 'Comment on egusphere-2025-4186', Sebastian Kreutzer, 10 Feb 2026 reply

Dear Benny, Dear Georgina,

Thank you for this interesting manuscript. I think you discuss some important aspects of the rather convoluted realm of fading correction. I appreciate that you have provided a comprehensive overview and mathematical clarity. I agree that the subject can be daunting and that information is distributed across several articles. As I am not engaged in a formal review, I will cherry-pick some paragraphs from the document I found less clear or misplaced.

We thank the reviewer for their positive assessment of our work.

- The introduction comes across a little bit rough and to me they feel rather as an accusation of severe negligence in past work instead of a warranted neutral approach. Once the reader made it past the introduction, the manuscript is much for fun to read; the discussion certainly stands out here.

  Amended as proposed. We have reworded our intro (lines 26-99) but maintain that the mathematical inaccuracies have trickled down from the field's seminal papers, and that this needs to be mitigated in a transparent manner going forward. We future-proof our own treatise by inviting you, and everyone else, to attack our own mathematical inconsistencies as harshly as one can, as soon as they are found (lines 653-656). Our aim is neither to wear a righteous halo nor trash our predecessors, but solely to raise the bar for mathematical literacy, without which the field will likely continue to be stuck.

- On multiple occasions you are referring to "grey" literature, without ever writing what you have in mind. Perhaps you are referring to a particular Excel sheets/program you came across or some kind of instructions you consider at odds. As for those "grey" literature/tools, I can tell that I have seen many of them in various stages. All of them, however, only tried to implement the peer-reviewed models and only one I saw had a (minor) flaw. However, I would not consider the circulation of measurement protocols and spreadsheets "black-box" solutions in general. The opposite, usually they are quite thoroughly inspected by the people that use them. Either way, there are no "theoretical gaps" that led to the spreadsheets and software. There is simply the implementation of proposed models. Nothing more, nothing less.

  Amended as proposed. We have removed the contested wording ("black box", "theoretical gaps"), and have thanks to your comment narrowed the problem down to "severe algorithmic opacity" of the original papers (lines 75-87), which has given rise to demonstrably divergent implementations – as strongly exemplified by our new exposition and discussion of the "Lamothe et al., 2003" correction scheme (lines 293-356).

- You correctly stated the rescaling of g-values with different references times is somewhat non-obvious in the original article, however, no "reverse-engineering" is required. The code was a various of those “grey” literature spreadsheets I came across, and we have it also in the R package ‘Luminescence’ since many years, where the code is fully accessible and transparent.

> Amended (lines 79-85). Ironically, the very code and comments of “calc_fadingCorr.R” of the R package Luminescence only further reinforces both of our claims re: grey literature and the necessity to reverse engineer the opaque H&L2001 algorithm. Code lines 245-248 contain a key comment on the recalculation of g-value “thanks to the help by Sébastien Huot, e-mail: 2016-07-19”, using a formula that doesn’t appear anywhere else, so it’s prudent to assume that calc_fadingCorr.R incorporated rescaling of g-values only after 2016 (half a decade after the R package was developed, and a decade and a half after H&L2001’s publication). Code lines 168-199 contain a “check” of the formula vis-à-vis H&L results, which is exactly the same reverse engineering and sanity check that we demonstrate in our article, in order to decipher what Huntley and Lamothe actually meant.

- Another statement (lines 143 to 144) flags the lethargy to report the t_c value. While I agree that this could be done better, in many cases it simply does not matter because we have the g-values reported normalized to 2-days. A situation where somebody would assign ‘ka’ instead of ‘s’ to tc appears made up with little practical relevance.

> Amended as proposed (lines 158-164). Our rather misplaced statement re: tc units arose from the irreproducibility of the best-fit g-values in Fig. 11 of Thiel et al. (2015), where we suspected that the mismatch might have arisen from mis-assignment of units to tc. However, the reviewer is correct that units shouldn’t matter here. Motivated by the reviewer’s comment, we dug deeper and discovered that while Thiel et al. (2015) reported all their g-values as 2-day normalized, they are demonstrably scaled to the shortest-fading-measurement-duration, the so-called tc-normalization (~10 minutes rather than 48 hours).

- More science related, with reference to Fig. 1b, I am wondering how big the problem really is. Are samples with fading rates >5%/decade really something we suggest using to build robust chronologies?

> Suggestion not implemented. Our paper’s sole focus is on having self-consistent maths for a range of signal decay models, and by no means do we want to go into the rabbit hole of ad-hoc practical thresholds. Concerning the latter, the meaning of “robust chronologies” is arbitrary in itself: while the reviewer mentions a reasonable/conservative g=5% for sediment dating purposes, values of up to g=45% could be meaningfully utilized to screen samples for thermochronological signals (Valla et al., 2016). Similarly, fading for luminescence surface exposure dating may be of even lesser significance (Sohbati et al., 2018).

- When I look at Fig. 4c it appears that the current Huntley/Kars approach does not perform so badly and Fig. 5 shows that differences will likely not change the general outcome and geoscientific interpretation. Better accuracy is always welcomed; though. My impression is that you must have come to the same conclusion given lines 517 to 520 in the discussion.

  Retained as originally written (lines 517-520). We totally agree with the reviewer.

- What I find more puzzling than the question of which model was used is what you show in Fig. 4a. The real problem there does not seem to be the fit, and then correction but the scatter of the data for the g-value measurement. To me this is the elephant in the room that should be, at least briefly, addressed and discussed but this aspect does not receive the appropriate attention in the manuscript (afterthought note: it indeed does seem to bother only me, but not the reviewers).

  Amended as proposed (469-473). This issue bothers us as much it bothers the reviewer, so we have added a brief but critical mention of this problem: "the scatter of fading data around either of the models in Fig. 5a raises serious methodological doubt regarding the quality of the fitted dataset itself; regretfully, transparent discussion on the design of fading measurement experiments has been uncommon since Auclair et al. (2003) [...]".

- There seems to exist an odd understanding about how fading measurements can be carried out, and although your manuscript does not spare the readers with critics, you leave this out and instead even echo this procedure in Fig. 4. What is the rationale of having short-term fading measurements over only a few hours with a few data points instead, for instance, at least five days and more data points? I am aware of the usual explanation that would block the measurement equipment too long. But this is just a cheap excuse without merit. In some readers it is only less convenient to perform in other machines, but proper measurement design enables you to measure much longer times without easting up reader time. Certainly, this is not on you, but I wonder whether we put a lot of attention on the post-processing of data, instead of a proper measurement design. Note: This should not give the impression that "all" fading measurements are flawed per se, I just think (this includes me) that we can do better. See also https://doi.org/10.5194/gchron-2020-3. Also apparently we never managed to get it formally published, the measurement results are regardlessly valid.

  Amended as proposed (469-473). This issue bothers us as much it bothers the reviewer, but we cannot cite a withdrawn manuscript. Our criticism is thus narrowed down to the following: "regretfully, transparent discussion on the design of fading measurement experiments has been uncommon since Auclair et al. (2003)".

In summary, I think you could make a good manuscript excellent and gain broad reach and appreciation with a few changes that likely won't take you much time. Once the manuscript is fully published, we will certainly implement your suggestions in 'Luminescence'. Having said that, thanks once more for the effort and the nice work.

Sebastian Kreutzer, Heidelberg, February 10th, 2026.

Thank you again, Sebastian, for your critical reading of our manuscript, and your very useful comments.