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Interactive comment on "Towards an improvement of OSL age uncertainties: modelling OSL ages with systematic errors, stratigraphic constraints and radiocarbon ages using the R package "BayLum"" *by* Guillaume Guérin et al.

Ed Rhodes (Referee)

ed.rhodes@sheffield.ac.uk

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General comments and observations

I think this paper represents a very useful opportunity to re-examine the nature of uncertainty in OSL dating, in particular the degree to which a formalized treatment of systematic errors can lead to significant enhancement of chronological information, and improvement in the quantification of overall age uncertainty. I feel this is a significant step forwards, and there are several aspects I particularly find attractive.

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Firstly, I find very appealing the notion of carefully quantifying all the aspects of the data that feed into several OSL age estimates from a single site, and then performing a single analysis that takes account of all of these.

Secondly, absolute clarification of the shared, part shared or unshared aspects of possible error contributions helps researchers carefully consider ways to develop chronologies, for example with collection of further samples, or ways to improve future dating campaigns, besides improving the uncertainties for existing datasets.

The description of the covariance matrix I found particularly lucid, and the formal declarations of the different contributing components form a useful record. The paper evolves around particular samples from the important Palaeolithic site of La Ferrassie measured in Bordeaux at the IRAMAT-CRP2A laboratory, and carefully steps through the introduction of more constraints and application of greater degrees of information (stratigraphic and relating to shared error terms). There is an understandable concentration in the particular aspects of the techniques used in the dating of these samples, but in later sections of the paper, I was made to feel more confident that I could extend this approach to my own data using some different dose rate approaches.

I have taken a quick look at the guidance notes provided as Supplementary Materials, and these look excellent, clear and very full - really quite an impressive degree of documentation and provision of components. I have not tried to run these yet, but I do intend to; I recently (re)installed R (after a break of several years without it) to interpret OSL/IRSL data, so I feel this will not be too great a step.

In summary, this is a very useful addition, both as a specific set of tools, but also as a prompt for readers to consider error and uncertainty contributions in detail. Possibly the use will be limited to a few specific groups at first, but to me this paper clearly points the way forward. Reading the paper was surprised me that I was not more challenged, in terms of understanding, maths or concepts, and I congratulate the authors on their clarity. I have a series of specific points for consideration listed below, but in general, I

find the paper very clear and with a good narrative that develops throughout.

Some specific comments

Line 35 Add “the user” before "to reproduce. . .”

Line 38 “allows the dating of”. Note Huntley et al. never refer to “OSL dating” but use the term “optical dating”.

Line 42 Replace “important” with “significant”?

Line 49 I think it would be useful to add “at 1 sigma (68% probability)” or similar here, as many geochronological techniques quote their uncertainties at 2 sigma.

Line 50 Note that there are many ways in which a single OSL sample may correctly not be associated with a single age estimate; for examples samples collected across unobserved stratigraphic boundaries. If the “unit of analysis” is to be a single sample (rather than a single depositional event) then some clarification of the assumptions regarding how this relates to depositional events or post- depositional mixing may be important.

Lines 50-51 Why are field observations or measurements e.g. in-situ gamma spectrometer measurements that play a very important role in reducing uncertainties specifically excluded here? Why is it important to define the “system of analysis” as the laboratory. I withhold judgement on the importance of this, but I highlight here the absolute paramount importance of stratigraphic information based on detailed specific field observations both for lithostratigraphic and morphostratigraphic relationships between different sedimentary units and luminescence samples. I note that this aspect is discussed towards the end (lines 651-655).

Lines 52-57 Yes, I agree with this definition. But, I note that random errors in calibration are not the only contributors to systematic error (line 54) but are included. Other contributors to this error can include mistaken application of the (somewhat complex) theoretical conversion between dose deposited in different materials, or application of

attenuation models.

Line 96 Note that there are some instances where radiocarbon dating and luminescence DO share systematic error, for example transport in drilling mud of organic material and sediment grains from one horizon to another during coring; in this situation, both techniques can provide similar erroneous age error of the horizon with material transported into it. This might seem like a rather specialised example, but it is a situation I encounter regularly (at least potentially, meaning I must consider it), and can happen by other processes (e.g. animal burrows, as are common in cave environments). I suggest simply introducing the word “usually” or “typically” after “which”.

Line 98 I find the division of uncertainties into just these two categories (systematic and random errors) potentially confusing, and it seems, outdated. The authors already gave an example of where random measurement errors of calibration standards become a systemic error for that laboratory; this can work at many scales, including site or catchment specific errors (for example behaviour of a mineral group in one region affecting the assumptions embedded within the SAR protocol). I note that this type of effect has been discussed within the Bayesian OSL literature for almost two decades, so it seems slightly odd to ignore it here, at least with a comment or reference.

Lines 99-101 Great, this document with application examples is warmly welcomed!

Line 175 I think it would be useful to outline a few characteristics of the Cauchy distribution here for readers, as it is not a term in general circulation within the luminescence dating community, as far as I am aware. I would add that it is also known as the Lorenz distribution, and that it represents a distribution derived from the ratio of two normal (Gaussian) distributions.

Lines 205-208 This is an important and exciting result (the better agreement with independent age estimates). Can the authors tell us what the main reason for this is? I wonder whether it involves the use of KDE (which effectively gives all results a similar weighting) in contrast to the central age or central dose methods that weight the re-

sults inversely to their measurement uncertainties. A brief explanation of the observed difference, even if this is just a probabilistic inference would be useful here (e.g. “We suspect that main reason for this difference in combined dating results is. . ..”).

Lines 210-212 It is important to say “stratigraphically above”, not simply “above”, as archaeological contexts are often not flat, and often have significant steep features such as pits dug by people for fires, as middens, or for burials, as well as collapse structures or other “natural” features. The application of the “principle of superposition” being applied here requires certain assumptions to be met, and I think it is worth stating this fact, even if the assumptions themselves are not listed. The current presentation, with no mention of the principle involved, no mention of the assumptions required to be met, and the use of the word “know” is too strong, in my opinion. This is the first introduction of the incorporation of stratigraphic constraints in the paper, and this is one of the most important aspects of Bayesian methods in the application of chronological techniques, so I think it’s important to be quite rigorous here. To clarify, I strongly advocate the use of such additional, stratigraphic information in building sediment chronological models, but when introducing readers to these ideas, I also advocate careful attention to detail.

Line 219 Where you say “1 values” this is a little confusing to read, and I misunderstood this initially. I suggest changing it to with “the first row contains the value 1 in each column”.

Lines 220-229 Yes, I will need to read the Markdown document to follow what this means; it is a little hard to comprehend simply by reading the text, as acknowledged by the authors’ comments. On line 220,I suggest that it is vital to use different terms for stratigraphy and age (in part because they go in opposite directions). Here, you refer to the “lower age bound” meaning “younger age constraint” but this could easily be misunderstood by readers as “the lower stratigraphic constraint on age”; this is confusing as low in age = high in stratigraphy (when not inverted etc), so I suggest reserving the spatial terms “low” and “high” for stratigraphy, and using “younger” and “older” when you mean age.

Line 242 “Should not allow solving” needs changing, as it is incorrect English. But I don’t understand what is meant here, so I can’t easily suggest alternative wording. Please clarify and reword.

Lines 241-267 This is a very fair and useful introduction to this issue, and many of these issues have clearly been discussed in earlier publications by this research group. I still find the term “systematic errors” as used here somewhat dangerous, as they may be only systematic to this particular group of samples (and therefor probably don’t count as true systematic errors). Perhaps I am being too pedantic, but it would be clearer to call these uncertainties “site-systematic” or similar. This is a key issue, and one that it is important for readers to grasp, so I don’t want to change the dynamic of the text or narrative flow here.

Line 278 I think the comma after “matrix” is wrongly placed? I think the word “that” could be inserted at this point to help clarify this sentence.

Line 371 I’m not clear what “(resp. Th)” means. Is this “with respect to thorium”?

Line 373 I think this should be “. . .two systematic sources. . .” at the end of the line. Line 379 This is probably a good idea to use a single fixed value for quartz, but I think the amount should possibly be varied as a function of the grain size used?

Line 390 This section seems to omit discussion of the obvious solution to this problem that has been widely used for very many decades – the use of a portable NaI or similar calibrated gamma spectrometer. I think this section needs some work to generalize it for a wider audience and focus less specifically on those techniques used at IRAMAT-CRP2A and include discussion of methods that have been developed and applied specifically to reduce the introduction of such systematic errors (e.g. direct measurement of Th230, U238 and K using small samples for beta dose rate determination, for example using ICP-OES/MS or NAA which significantly reduces this sort of error). OK, I see that this discussion appears starting at line 558, so perhaps simply include a reference or pointer to that discussion in this section?

Lines 397-425 This feels like quite a lot of discussion about water content without really discussing some of the important issues. First, although modern values are indeed widely used (and in some cases this may be appropriate), workers often also commonly model past water content, and occasionally water content changes. This is not well represented here it seems to me, and there is little guidance on how to include the different uncertainty aspects involved in that modelling. I note that water content uncertainty represents a good example of a "middle ground" error type; highly sample-systematic, partly site-systematic, and not at all truly systematic (i.e. certainly does not affect every sample to the same degree) and with a different impact on different samples even when the changes are similar.

Lines 427-8 I note that the labelling for equations is somewhat irregular; some equations have numbers, others do not (e.g. all the water content discussion equations).

Line 426 Did I miss it, or was theta defined somewhere?

Line 435 Missing comma after "beta dose rates"

Lines 416-451 I find this description and explanation very clear! This gives me additional confidence and enthusiasm to attempt this approach myself, which is great, I feel.

Lines 569-571 I disagree that counting methods such as beta counting do not have a systematic error that can be quantified and applied. In fact, in my limited experience (i.e. reports from others), G-M beta counting is dominated by systematic not random (i.e. counting) errors, and suites of samples measured using this technique probably have significant group-systematic error introduced that could be subsequently calibrated, and the value included in the covariance matrix.

Lines 577-580 It may be hard to isolate, but there certainly is a very significant systematic error introduced by the threshold technique; in fact, the counting uncertainties are commonly negligible (e.g. many hundreds of thousands of counts even for short mea-

surement with small diameter crystal). However, the conversion to dose rate depends critically on several things – the dose rate value of the sites used in the calibration; the uncertainties in these values are significant for the calibration holes located around the Massif Central, as an example. Secondly, the location of the threshold energy can be tricky, as most NaI systems are quite temperature-dependent, so it is necessary to locate and fit specific emission lines, and in low dose rate sites (such as in limestone caves), these can be hard to locate. Thirdly, damage to the crystal can gradually change the spectrometer sensitivity over time. These effects can be time- or site-systematic, but can readily be quantified by regular re-calibration.

Lines 587-8 I'm not clear what is meant by the phrase "attenuation in grains implies that something other than the grains does not attenuate radiation". Please clarify.

Line 618 Parentheses open but do not close.

Line 622 Replace "equivalents doses" with "equivalent doses".

Lines 665-682 Yes, I firmly agree with these conclusions!

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