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Interactive comment on "Luminescence properties and dating of proglacial sediments from northern Switzerland" *by* Daniela Mueller et al.

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Contribution summary

The manuscript presents a dating study from a palaeovalley (Rinikerfeld) from northern Switzerland. The authors retrieved eight samples from a drilling campaign for luminescence dating. Prepared were either the quartz, feldspar or the polymineral fraction using three different grain-sizes. The preferred grain-size fraction was altered with the sedimentological environment. Along with the chronology, the study aims at providing better insight into the luminescence characteristic to assess the potential for further luminescence dating studies that region.

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Recommendation

I suggest that the manuscript changes a little bit the story and becomes transferred to another Copernicus journal, e.g., E & G (https://egqsj.copernicus.org).

Justification

The manuscript presents a concise luminescence-dating study for different minerals, including tests, such as preheat/dose recovery tests or thermal transfer tests from a drilling site in the foreland of the Swiss Alps. All tests are reasonably explained, justified, and they help to support the chronological findings.

However, the manuscript claims to target, specifically, luminescence properties of "proglacial sediments from northern Switzerland", but it remains foremost (and nothing is wrong with it!) a luminescence-dating study. The manuscript conclusion by the authors may best reflect this assessment. The performed tests and measurements are not particular, but something luminescence-dating studies present all the time to increase the confidence in the results. The study on eight samples characterises, as a byproduct, to some extent the luminescence properties of those samples. Nevertheless, it does not investigate luminescence properties in general for the area or directly compares findings from a large dataset (e.g., as a meta-study).

Hence, the exciting part of the study is the chronology itself concerning the palaeovalley. This is the story the manuscript should exploit and detail further. Currently, only a few lines (including the conclusion) wrap the setting of the site and its geomorphological and geological background. Thus, it falls short and more context would also help to understand the chronological findings better.

At the end of the introduction the authors wrote that they "asses" luminescence properties of the samples from the site, but again, it does not evolve beyond standard tests.

Therefore, I suggest that the authors alter the story a little bit towards a geoscientific focus, add some details as requested below (e.g., dose rate) keep what they have and submit the manuscript to, e.g., E & G (https://egqsj.copernicus.org).

I left a couple of comments below. Mainly referring to some glitches here and there, except the missing results and discussion on the dose rate (the water content is discussed though) and the sketchy discussion on the fine grain quartz age underestimation, nothing critical.

General comments

1. The manuscript reads clear, and the preparation of figures and tables is good. Besides, the authors use many abbreviations in the figures that remain unexplained on top of rather short figure captions. The latter is not necessarily bad, but the manuscript may want to address a broader audience. Currently, the apparent target audience is readers with a background in luminescence dating working in the Swiss Alps. However, other readers may want to have a look into the article as well. Hence, figure captions and abbreviations should elaborate a little bit more, and figures should be more self-explanatory.

2. The manuscript oddly seems to focus a lot on the differences between coarse and fine grain quartz results, towards an interpretation of an age underestimation of the fine-grain quartz fraction compared to the coarse grain fraction. Indeed, such an age underestimation has been reported in the literature, and I do not doubt these findings for the particular sites.

   However, in the presented manuscript, only for one out of eight samples, a comparison of both grain size fraction is presented. Both numerical results overlap within uncertainties. All other samples report results, either for the coarse grain (top of the composite profile) or for the fine-grain quartz fraction (lower part). The author's statement on a potential age "underestimation" seems to be trigger by an age inversion in the profile. This age inversion is also present for the fading corrected fine-grain polymineral fraction. Why does this suddenly lead to the conclusion that the fine-grain quartz ages are underestimated (in comparison to the quartz coarse grain ages)? I got the impression that the authors had this idea of a fine-grain quartz underestimation in mind and then tried to see this pattern in their data. It is one possibility that deserves to be discussed, weakly supported by the data though.

3. Technical detail: The chosen format to apply units is odd, e.g., the authors wrote "471.2 ± 47.1 and 525.8 ± 53.2 ka" instead of "471.2 ± 47.1 ka and 525.8 ± 53.2 ka". There is a general pattern in the manuscript and it should be corrected throughout. If I understand the author guidelines of *GChron*[1] correctly, it should even read, e.g., "(525.8 ± 53.2) ka" because the linked SI brochure refers to the *Guide to the expression of uncertainty in measurement*; I might be wrong. Besides, I suggest to round values to meaningful digits. If the age uncertainty is around 10% the number after the digit does not tell much.

Detailed comments

Abstract

1. Line 17–18: The statement that the fine-grain quartz ages are underestimated compared to the coarse grain ages does not seem to be supported by the data for three reasons: (1) The authors do not systematically compare coarse grain

[1] https://www.geochronology.net/for_authors/manuscript_preparation.html

and fine grain quartz ages, they do this for sample RIN13 only. (2) For this particular sample, coarse and fine grain quartz ages overlap within 1σ. (3) Table 1 marks the coarse grain age as "underestimated due to grain size-dependent D_e underestimation", not the fine-grain quartz fraction.

It does not mean that the fine-grain ages are per se not underestimated (e.g., compared the general pattern observed and compared to the profile figure with corrected feldspar ages). Still, the abstract should reflect the essential outcome of the study with regard to the data.

2. Line 19–20: That the dating reveals a rapid deposition during the (at least) MIS 6 was first mentioned in the Conclusion. Doubtlessly, it had no particular relevance to the authors, given the overall scope, but the abstract leaves the reader with the impression that this point will be detailed in the manuscript.

Main text

1. Line 49: "Feldspar" $\rightarrow$ "feldspar"
2. Line 55: Please report g-values normalised to two days or report the t_c value otherwise it will be impossible for readers to compare these values with other findings from the literature.
3. Line 63: I am not sure whether Thiel et al. (2011) should be mentioned as well here (for the 290 $^\circ$C)?
4. Line 73: Remove "Scientific"
5. Line 101: ca 10^6 should suffice.
6. Line 110: Add references for the calibration quartz

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7. Line 115: Better "when comparing", because this is what people do when they double check your protocol parameters.

8. Line 131: "gamma-ray"

9. Lines 131–132: There is something wrong with the citation chain:

   For the fine grain quartz fraction Buechi et al. (2017, p. 57) wrote: *"For fine-grained quartz an a-value of 0.04 $\pm$ 0.02 has been incorporated to account for the variability of the values reported in literature (Rees-Jones, 1995; Mauz et al., 2006; Lai et al., 2008)."*.

   For the polymineral fraction Buechi et al. (2017, p. 57) reported: *"The effect of alpha irradiation was considered with an a-value of 0.05 $\pm$ 0.01 for PM fractions (Preusser, 1999b; Preusser et al., 2001)."*.

   Where Preusser (1999b) is the here cited Preusser (1999). It was Preusser et al. (2001) who reported a-values for the polymineral fraction as quoted in line 132.

   Contrary, Preusser (1999) reported four IRSL a-values without uncertainty with a mean of 0.05 and a standard deviation of 0.00 (all values show 0.05; their Table 2). More important is that the applied protocol is not similar to what was applied by the authors here.

   In Sec. 4.2.2, the authors detail various possibilities and discuss whether the selected a-value is justified. My impression is that the here applied a-values were used, because they had been always used for samples from that region. This might be justified, but it also shows that it should be re-measured at some point. In either case, the chosen values need the proper reference.

10. Line 130: U, Th, K concentration values were deduced from gamma-ray spectrometry but only summarised values are presented. What about radioactive disequilibria?

    Your environment is undoubtedly very challenging regarding the dose rate, so maybe you can present a few more results regarding the nuclide concentrations? For example, as a plot normalised to Th-232 (cf. Guibert et al., 2009), this would give a good indication. If this appears to be too much, the authors can copy and paste the data from the VKTA into a supplementing document and add one sentence to the main text addressing the possibility of radioactive disequilibria.

11. Line 133: Gaar et al. (2013) confirm Huntley and Baril (1997); which is very reassuring. However, (1) they report $12.9 \pm 0.4\,\%$ and (2) they argue for the application of the 95 % confidence interval for the potassium concentration (citing Huntley and Baril 1997), means ca $12.5 \pm 1\,\%$. Since the authors cited both references, they should make elaborate why they did not follow the suggestion by Gaar et al. (2013).

12. Lines 140–180 (Sec. 3.1 Performance test): It should read “tests” and please add further subsections so that the results for the quartz and the feldspar/ polymineral, can be more easily separated.

13. Line 164: It is not really a different “preheat behaviour” but a completely different design of the heating element and the thermo couple and its feedback electronic. So perhaps: “different technical design”?

14. Line 198: “Chinse” $\rightarrow$ “Chinese”

15. Line 230: The obtained overdispersion value also depends on the initial σ_b; if set. Was it different from zero?

16. Line 237–238: I am not sure whether CAM is the most suitable model. The authors should double-check the findings by Heydari & Guérin (2018). I also suggest adding one or two dose-response curves from the lower part of the profile.

17. Lines 257–258 ("However, if ..."): The D_e is not a good indication because it is a function of the dose rate and should only be used if the dose rate is homogenous over the profile. Besides, the fading corrected feldspar age appears to be also slightly younger (within 2σ ok) for the Kars model. My point: If the authors want to keep that argumentation, they should extend the description of the environmental setting and the dose rate. Ages should not be disconnected from the sedimentological environment. For example, why did not a "facies change" (maybe it is not) cause all these "problems"?

18. Lines 261: The purple density curve in RIN13 looks somehow skewed.

19. Lines 269–270: Preusser et al. (2014) wrote that they followed Auclair et al. (2003). Perhaps the latter one is the better reference to cite, or at least in combination with the first. Besides, it appears that Preusser et al. (2014) did not normalise their values to t_c as done by Auclair et al. (2003). In that case, the g-value will be slightly different than "expected". The authors may want to add a plot showing their fading measurements; then it should become clear. Additionally, Preusser et al. (2014) measured only three points on the time axis. With regard Kadereit et al. (GChron discussion 2020)[2] the obtained g-value might be somewhat arbitrary, and so would be the following fading correction.

    Nevertheless, I did not make this a major point for two reasons: (1) The manuscript by Kadereit et al. will likely be rejected and not become published (though the discussion is online and outlines the general problem). (2) Fading measurements and corrections are a tedious business. The approach chosen

[2] https://gchron.copernicus.org/preprints/gchron-2020-3/

by the authors might be ok; it might be not. Without further age information, in particular, in the lower part of the composite profile, it is impossible to say.

20. Lines 273: Unfortunately, the function the authors applied to corrected the ages after Lamothe et al. (2003) for fading has a (recently discovered) bug (https://github.com/R-Lum/Luminescence/issues/96). The consequence of the bug is that the uncertainty of the fading corrected ages is lower than it should be because the error of the g-value goes into the calculation with a weighting that does not seem to be justified. Of course, this is nothing I hold against the authors. I just wanted to mention it here.

21. Line 275: Please mention the t_c value along with your g-value, otherwise they are not comparable. Please add throughout the manuscript.

22. Lines 295–298:I was wondering whether the D_0 criterion has any substance after the value became corrected for fading after Kars et al. (2008)? With the correction, the D_e is deduced from a new, simulated, dose-response curve. The D_0 of the simulated curve should be the new reference, not the faded dose-response curve. Did I overlook something?

23. Lines 335–336: I do not agree that based on these findings, the logical conclusion is that coarse and fine grain quartz ages are different. The profile may have some hiatus going along with the age inversion. The reason for this age inversion is not necessarily grain-size related.

    Do the authors have granulometric data from the core?

24. Lines 340–341: The last point appears like an appendix in this sections and it leads to nothing further. Is this maybe some kind of leftover from a discussion the authors wanted to engage but did not?

25. Lines 343–365: This is a helpful discussion of different scenarios and justified. However, it should engage a more general discussion on dose-rate scenarios (which does not exist yet).

26. Lines 366–389 (4.2.2 Alpha efficiency values and age determination): This subsection renders a potentially fascinating discussion. The problem I have with this section, in particular the first part, is that it does not read clearly but mixes different aspects. For example, after reading the section, my conclusion was that the chosen a-value of $0.05 \pm$0.01 is the less justified value. The reasoning is that somehow all goes back to Preusser (1999) and Preusser et al. (2001) Although for Preusser et al., 2001 I am not sure whether it does not resembles values from Preusser, 1999 (?).

    Means, in the worst case, the selection bases on four values with rather low a-values, e.g., for the polymineral and feldspar fraction. By contrast, the majority of the other articles would favour higher values. Besides, Schmidt et al. (2018) presented an extended dataset of a-values (IRSL and pIRIR$_{290}$), though the focus was pIRIR$_{290}$, which was not measured here.

    Of course, it does not mean that the value is wrong, but the arguments presented by the authors indicate that (as even alluded in the manuscript) that they should remeasure the value.

27. Line 373–374: Please correct the reference or the a-value (see above)

28. Line 391: Something is missing in the section title. Perhaps: “Quartz age grain-size dependency” or “Grain-size dependency of the quartz ages”

29. Lines 391–403 (Sec. 4.2.3): The section is very brief and, in my opinion, does not add to the understanding of the “age discrepancies” (if there is any, see comments above) and it does not discuss the quartz grain-size dependency as announced in the section title. Instead, it provides a brief, selective review of other

findings, and it concludes that the lowermost two samples should be regarded as minimum ages. I would support the conclusion, but not the reasoning.

30. Line 400: Timar-Gabor et al. (2017) wrote:

> *On the other hand, the age discrepancy of SAR-OSL ages previously reported for Romanian and Serbian loess for ages beyond ∼40 ka (equivalent doses >∼100 Gy) was also found to be characteristic of Chinese loess. It is thus believed that this is potentially a global phenomenon, affecting previously-obtained chronologies worldwide, and further increasing concerns for the accuracy of silt-sized SAR-OSL ages in this high dose range.* (Timar-Gabor et al., 2017, p. 470).

Timar-Gabor et al. (2017) expressed a guess or hypothesis as part of the conclusion. This conclusion, however, should not be become some statement on the "pattern around the globe'. At least the cited study does not provide the data to it.

Furthermore, Timar-Gabor et al. (2017) refer, first of all, to own observations from Romanian and Serbian loess comparing 4–11 μm (fine grain) and 63–90 μm (their coarse grain). This is not similar to what is presented in the manuscript for *GChron*.

31. Lines 411: The conclusion should reflect the results and discussion of the manuscript. The depositional history was not discussed in the manuscript and came here by surprise.

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Figures and tables

1. Figure 1

   - Do the authors have other ice extent data to show? Perhaps the LGM ice extent is a nice to have, but of limited relevance given the age results.
   - “A.” and “B.” is part of the figure, consequently those lettering should be part of the figure caption.

2. Figure 2

   - An y-axis unit (core depth) is missing.
   - I would be good to have some photos showing the core log to better understand the setting. Also, the authors may want to indicate where one core ends and the next starts.
   - What does the upper x-axis (“C, Si, Sa, Gr, Co, Bo” on top the core log) labels? Probably it is obvious, but it is not to me and maybe other readers are not familiar with it as well.
   - The age comparison should be based on 95 % confidence intervals. However, I guess the graph will not scale very nicely given the two lower polymineral ages. This can be fixed by using a non-continuous scale.

3. Figure 3

   - The figure would benefit from more details in the figure caption. Readers not familiar with luminescence dating may struggle to understand the figures. For example: “M/G” probably means measured to given dose, “PH” means preheat etc.

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- The inset legend in all the figures in the right column is unnecessary because only one type of data is shown in all figures. It would suffice if the figure title (or subtitle) says "thermal transfer test".
- It is not clear what the data points are displaying. A single measurement with uncertainties? An average with the error bars showing the standard deviation (of the mean)?

4. Figure 4
    - The mineral fraction is missing in the figure caption
5. Figure 5
    - Same as above, the mineral fraction is missing in the figure caption
6. Figure 6
    - Same problem as Fig. 3. The figure captions should explain used abbreviations (e.g., "DR")
    - The solid line the curve is not really showing the "given dose decay" but the "luminescence signal decay" or shine-down curve of the natural signal. It is a proxy for the "given dose decay", but is not a "dose decay".
    - "Natural decay" might lead to a wrong understanding by others who work with dating methods relying on the "radioactive decay" of isotopes. Perhaps: "natural shine-down curve" or something similar.
    - $D_e(t)$ plot was used by Bailey et al. (2003) to identify the partial resetting of the luminescence signal. They shifted and extended the signal integrals slightly at the end. Probably it was not done here, but the figure caption should detailed what was done so that the figure becomes immediately understandable.

- Y-axis labelling should be added to the figure in the right column.

7. Figure 12

   - Proper x-axis labelling is missing or figures should align more closely.
   - Was a similar bandwidth used for all kernel density curves?
   - RIN2: Table 1 reads 158.4 ± 4.4 Gy instead of 158.4 ± 4.3 Gy in the figure (minor detail, since I argue for meaningful rounding above).

8. Figure 13

   - 95 % confidence intervals should be used for the age comparison.
   - There is somehow a typo in the figure caption: It reads "Accepted ages are presented with 1σ uncertainty as point symbol.", however, there is no "point symbol" in the figures.

9. Table 2

   - What meant is the internal K-concentration, it should be written.
   - "De" should read "D_e" (subscript "e")

Personal note to the authors

Dear Müller et al.,

I can imagine that you do not agree with my suggestion to transfer the manuscript. To avoid the impression that I am "against" your manuscript, I may add that I sincerely believe that every luminescence-dating study deserves to be published; given that it is free of significant mistakes. Luminescence dating is way too costly to ignore the data

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or let them disappear in a drawer. Your study indeed, should be published. However, I think for *GChron* it would need way more tests (e.g., TR-OSL, TL with trap parameters etc.) and a larger dataset. This is nothing I can ask of you. To the contrary, you probably have the perfect for, e.g., E & G with a ready to go chronological part if you extent the geomorphological/geological part.

Nonetheless, to ease the minds and to avoid a heated discussion: If the editor believes that the manuscript is most suitable for *GChron*, I will certainly not further argue against a publication in *GChron*. Moreover, I consider my suggestions as a piece to an open discussion, which is not carved in stone.

Conflict of interest

I have no conflict of interest to declare. I am not a beneficiary of the suggested references to be cited. Naturally, the authors are free to reject my reference suggestions.

Sebastian Kreutzer – July 21, 2020

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