

Dear Christina,

Thank you for submitting this substantial piece of work to GChron. While your paper has improved following the revision, I have a number of minor comments and a few major issues. Sorry it took me so long to get back to you. I hope these will clarify the paper.

Major issues:

1. The current structure of the paper is that Section 3 is the method and Section 4 is the results. However, in Section 4, the description of the method and the results are mixed up in many places (I will mention these in my detailed comments below). Either move the sentences to the correct place, or change the structure of the paper.
2. While I understand that the luminescence characteristics of limestone and volcanic rock gravels differ, the various assumptions and treatments you employed are neither adequately justified nor explained. This includes the fading correction method: limestone rock was corrected using the Huntley and Lamothé (2001) method, while volcanic rock was corrected using the Kars et al. (2008) method. The rocks are similar in age and fading rate, so there is no reason for the methods to be different.
3. The next problem is the internal K content. The K content of the limestone gravels was assumed to be  $12.5 \pm 0.5\%$ , without this being explained (the information was given in the footnote of the table), whereas for the volcanic rocks it was assumed to be  $10 \pm 2\%$ . Such assumption is normally made for K-feldspar extracts. In this paper, all samples are polymineral mixtures. This would not have much influence on the total dose rate for volcanic samples, but it could significantly alter the dose rate and ages for limestone samples. As they have access to an SEM-EDX, I recommend measuring the K content. If not, please make a careful and reasonable assumption.
4. The bleaching environment of playa lakes is expected to be similar at the two sites. I think something does not fit: you use the IR50 signal with the low-temperature preheat and still have to use MAM for the limestone, whereas the pIRIR290 signal is likely to be well bleached for the volcanic rocks. The large OD could be an artefact of fading correction and the characteristics of the low dose rate. I am not convinced that MAM should be required.
5. The current length of the paper is of 57 pages, which I think it is really long. Could you consider more diagrams to be moved to the supplementary? For instance, the dose rate comparison between ICP and gamma could be moved.

Here I list my minor comments (partly overlap with the major issues).

Line 22: Infrared signal – please specify the temperature

Line 23: Mass numbers are either written like C-14 (here) and U-238 or  $^{90}\text{Sr}$ . Please make them consistent throughout the text and figures (including supplementary).

Line 26 and 32: It is pIRIR in all other appearances, not PIRIR. I don't think 'pIRIR' stands for 'post infrared infrared', but rather for 'post-IR IRSL', so please include 'stimulated luminescence'.

Line 40: for readers who are not familiar with Great Basin pluvial lakes, I suggest adding a few more words about the location.

Line 54, “the ~16,000 year 1522 m above sea level (asl) Lake Coal highstand”: please check the sentence. Perhaps some prepositions are missing. A reference is needed for the expected age of ~16 ka (or are both the OSL ages and the 16 ka assumption from the same paper?).

Line 59: “this” → age discrepancy, to make it clearer?

Line 77: lab → laboratory is better.

Line 95: please spell out NV.

Line 96: isn't precipitation normally expressed in mm?

Line 101 and 113: The period of lake level high stand was referred to “~16000 years ago” (Line 101) and “between ~15000 and ~18000 years ago” (Line 113). Neither of them is really consistent with the two calibrated radiocarbon chronology, appeared in the following sentences, 15,873-16,281 cal yrs BP and 16,938-17,649 cal yrs BP.

Section 2: When the authors refer to ages (e.g. lines 27, 101–103, 113–119 and 125, also line 54-56 in section 1), they are expressed in different manners (e.g. cal BP years, years ago and ka) with varying number of significant digits and with and without uncertainty. Sometimes, cal BP was used without mentioning that it was a radiocarbon age. Please try to use a consistent style of referring ages. I think it would be helpful to add a table summarising all previously reported radiocarbon and luminescence ages, highlighting the problems, including all the relevant information (e.g. dated material, calibration method – I would even recalibrate them using Intcal20). And make sure that BP is only used for radiocarbon ages.

Fig. 2: Could you include more information in the map (e.g. extent of lake highstand and geomorphic features explained in Section 2 – which are currently hard to follow) or prepare a better geomorphologic map? Please also add “Coal valley”, “Cave Valley” and “Lake Valley” in the map.

Table 1: Where does the estimated age of 16,900–18,000 years come from? It was explained in Section 2.2 that the Cave Valley highstand was not dated but assumed to have been dried earlier than Lake Carpenter.

Line 135, “volcanics and tuff”: Tuff is also a type of volcanic rocks.

Fig. 3: I do not think this map, which is almost unreadable, is useful in this paper. There is no legend for the map (the colour code between left and right parts are similar, but there is no 1 to 1 relationship). The study area is not shown. How about creating a geologic map of the same size as the image of Fig. 2 and put it side by side within Fig. 2?

Line 156 and 167 about the sampling elevation: remove “~” as the elevation is very precise.

Line 146: Again it is better to add a table including all previously reported ages. I think that it is more important to note which calibration curve was used, not the program. Age unit is needed after 16000.

3.1: Please add more info about the sampled gravels, e.g. the size of the gravels, rock type (how many limestone and volcanic rock crusts from each site). Could you specify the type of the volcanic rocks and minerals within?

Line 203 (Title of 3.2): I think that “for Equivalent dose ( $D_e$ ) measurement” should be removed, because this section also includes the preparation of dose rate samples.

3.2.1: I assume that you sieved the prepared samples before luminescence measurement. There is no information about the grain size of the luminescence samples.

Line 230: e as subscript for  $D_e$ . Please check this throughout the paper.

3.2.2: Are gravels of volcanic rocks from all three locations?

Line 270 "K-feldspar": I don't think that you can assume the signal originated from K-feldspar, but could be from any kind of feldspar.

3.4.1:  $\ln/T_n$  and  $L_x/T_x - n$  and  $x$  should also be in subscripts. Also the temperature for the pIRIR protocol (290) should be in subscripts. Please correct these throughout the paper.

Line 282: The authors used IR50 protocol with a very low preheat temperature (160°C 10s), which is contrasting much more stringent pIRIR290 protocol. Please add a short explanation for the selection of the protocols. And I do worry about the thermal stability of the IR50 signal after the preheat of 160°C for 10s.

3.4.2: How many aliquots/grains were measured? Only the accepted number of aliquots/grains are given in Table 4.

Line 289: Please indicate how long were the samples bleached before irradiating them for the dose recovery test. It is unusual to use pIRIR stimulation for bleaching because samples are not supposed to be heated. Are you sure about the validity of the test using heated bleaching?

3.6: It is not mentioned how the internal dose rate of feldspar was calculated. It is later revealed that you used 12.5% of internal K, which should not be used, because the samples were not density separated. This could make a significant influence on the dose rate, due to the low dose rate of the samples. See my comment in 4.4.4.

Line 326: 3 +/-5% means that there is minus water content...

Fig. 4a: please indicate the position of gravel samples in the photo. Fig. 4b: I am not sure what the authors want to show using the photo?

Table 3: this is good. Could you also provide similar table for other sampling locations (at least for Cave Valley even though the structure of the sediments was not clear)?

Table 4: Rock 3 has the fading rate result but was marked as "no signal", which is not consistent.

Line 379-383: these sentences are the method of the dose recovery tests, not "result" and should be moved to 3.4.2.

4.3.2: the grain size information of the measured fraction appears for the first time here and it is very difficult to keep track. Could you mention this earlier at the section of sample preparation and also add this in Table 4? The very large fading rate of Rock 9 can be mentioned.

Line 395-397: These two fading rates are obtained by single aliquot and single grain measurements, which is not obvious from the text (can only read from Table 4). Does the weighted mean fading rate mean that you analysed fading rate for each aliquot/grain and take the mean? Another thing you could do is to calculate a single fading rate using all L/T data (e.g. using R luminescence package).

4.3.5: I disagree with using the individual fading rates of the grains for the fading correction. Did I understand right that the OD values were calculated for the individual fading corrected ages? I think that the large OD values are originated from grains with higher fading rate, and if so, it is incorrect to attribute these large OD values (derived from the fading correction) to incomplete bleaching. The comparison of Rock 2 with different grain sizes (one calculated using the mean fading rate and the other from individual fading rate) supports this. The sigma-b value for MAM was obtained from the dose recovery test, which indicates the minimum OD. Given the low dose rate of the limestone

samples and the normal distribution of Rock 2, I think the signal is well bleached and the mean De should be used, at least for this sample. Please calculate the age distribution using the mean fading rate.

Fig. 8: the label (CDM and MDM) are different from the caption (CAM, MAM).

4.4.1 and 4.4.2: This comparison of the performance of the protocols was not mentioned at all in the method section and should not appear for the first time in the result section. I now understand that the choice of the high temperature pIRIR was made primarily from the fading rate and it should have been mentioned earlier.

4.4.1: Regarding the result of dose recovery test, apart from the fact that pIRIR290 signal was satisfactory, the mean dose recovery ratio and residual for the three protocols are not provided.

4.4.2: Here only pIRIR180 and pIRIR290 were compared – what happened to pIRIR225?

4.4.4: It does not make sense to use  $10 \pm 2\%$  K here for volcanic rock samples, whereas  $12.5 \pm 0.5\%$  was used for feldspar in limestone samples. If you had an access to SEM-EDX, you could have measured K content of feldspar grains (see e.g. Maßon et al., 2025). I do not understand what exactly you did but these supplementary figures are not useful. I do not think the sentences of line 560-569 is helpful, which is based on different setting. But here, it is true that K content is a minor problem, because the grain size is small and the total dose rate is high, but could change the dose rate of the limestone samples (larger grain size, low dose rate).

4.4.5: The method of fading correction should be moved to 3.5. Why Huntley and Lamothe (2001) was used for limestone samples, and here you applied Kars et al. (2008) correction? As the age range of these two sets of samples are the same, the correction method should also be the same.

Figs 10 and 11: Could you include the mean De and its uncertainty in the figures? There is no De axis scale for Fig. 10d.

Line 655 and 794: How exactly were the MAM ages calculated? Can you really do it using such a small number of samples?

Reference:

Maßon, L. A. E., Riedesel, S., Opitz, S., Zander, A., Bell, A., Cieszynski, H., and Reimann, T.: How much K is OK? Evaluating different methods for K-concentration determination and the effect of the internal K concentration on feldspar luminescence dating, *Geochronology*, 7, 475–492, <https://doi.org/10.5194/gchron-7-475-2025>, 2025.