Dear Associate Editor,

Thank you for your careful review, which has led to significant improvements to the manuscript. We address your comments in bold below.

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.

Restructuring has been addressed following your recommendations below.

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 Lamothe (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.

Originally, we anticipated that the volcanic rocks would have very high or saturating De values near the centre of the rocks, so we applied the Kars et al. (2008) fading correction model. This turned out not to be the case. We have now re-analysed the data from the volcanic rock samples using the fading correction model of Huntley and Lamothe (2001) for consistency with the limestone ages.

The text in Sections 4, 5 and 6 and the Abstract have been updated to refer to the new calculated ages.

3. The next problem is the internal K content. The K content of the limestone gravels was assumed to be 12.5 ± 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 ± 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.

We realised during the course of this peer review that our dose rates did not include an internal dose rate contribution from K. This is the result of a calculation error while applying the dose rate model of Reidesel and Autzen (2020). We have now rectified this.

We agree that, especially in light of data presented by Maßon et al. (2025) that our assumed internal K concentrations for our polymineral samples may be too high. However, after revisiting previously published studies where polymineral samples were dated we find that either, no information is provided about the internal K content used/assumed (e.g., Zimmerman, 1971, Archaeom.; Long et al. 2014, Boreas; Veres et al. 2018, QSR; Schatz et al. 2012, Quat. Geochronol.; Lukas et al. 2012, Quat. Geochronol.; Buylaert et al. 2007, Quat. Geochronol.; Buylaert et al. 2011, Geochronometria), or, commonly used published values were applied in absence of direct measurement (e.g., 10 ± 2% in Reimann et al. 2015, Quat. Geochronol.; 12.5 ± 0.5% in Frouin et al. 2017, Quat. Geochronol.; 12.5 ± 0.5% in Le Beon et al. 2018, Tectonophysics). We agree that measurements using an SEM-EDX would be informative (unfortunately we do not have access to an SEM-EDX), but a true assessment of the internal dose rate of measured grains should also examine which grains are contributing most to the luminescence signal as measured using our detection window. Such experiments are beyond the scope of our study. Until this matter can be further investigated, we've defaulted to the lower commonly assumed value of 10 ± 2% from Smedley et al. (2012) for both our limestone and volcanic rock ages.

The text in Sections 4, 5 and 6 and the Abstract have been updated to refer to the new calculated ages.

Also see our response to Comment #49 below.

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.

After corrections for the internal dose rate were made (Comment #3, above), our calculated CDM ages for all three limestone rocks now agree with the independent age control, suggesting that the limestone surfaces may have been well bleached prior to burial. We also show below that the large OD is not an artefact of the fading correction and we address this comment more fully below (Comment #41).

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.

We have moved Tables 5 and 7 to the Supplementary Materials, and have deleted Fig. 3 (see Comment #20, below). Additional requested data and tables have been allocated to the Supplementary Material (see below).

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

We are numbering the Editor's comments below so that major revisions can be cross-referenced to the manuscript with Track Changes on.

6. Line 22: Infrared signal – please specify the temperature

Done.

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

Done.

8. 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'.

Done. I've corrected "PIRIR" notation in text and figures of the main text as well as the Supp. Mat.

9. Line 40: for readers who are nor familiar with Great Basin pluvial lakes, I suggest adding a few more words about the location.

Done. We start the paragraph with the statement "During the Pleistocene epoch, pluvial lakes covered vast areas of the arid western United States, including Nevada, western Utah, southeastern Oregon, and eastern California. Most of the pluvial lake basins are found within the hydrologic Great Basin, an area of closed drainage encompassing about 520,000 km^2 (Reheis et al. 2014)."

10. 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?).

We've updated this sentence to read: "Previous research in Coal Valley, Nevada yielded post-infrared infrared 225°C (pIRIR225) ages that severely underestimate the expected age of the 1522 m above sea level (asl) Lake Coal highstand, which has been dated to 15,873-16,281 cal yrs BP using radiocarbon dating (Wriston and Adams, 2020)."

11. Line 59: "this" → age discrepancy, to make it clearer?

Added “age discrepancy” for clarity.

12. Line 77: lab → laboratory is better.

Fixed.

13. Line 95: please spell out NV.

Done.

14. Line 96: isn’t precipitation normally expressed in mm?

Converted to mm.

15. 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.

We’ve done the following to clarify the independent age control for our study sites:

Under section“2. Study area and sample sites”, we start the 2nd paragraph with the following statement:

“During the last major pluvial period in eastern Nevada, pluvial lakes reached their highstand between ~15,000 and ~20,000 years ago (see highstand ages for lakes Carpenter, Spring, Railroad, Jakes, Newark, Diamond, Franklin, Clover and Waring in Munroe and Laabs (2013), their Table 2).”

We correct former line 101 to read:

“Coal Valley, Cave Valley, and Lake Valley in Lincoln County, all contained pluvial lakes (Pluvial Lakes Coal, Cave, and Carpenter, respectively; Mifflin and Wheat, 1979) and marsh systems during the late Pleistocene (cf. Wriston and Adams, 2020).”

We correct line 113 to read:

“Geochronological and geomorphic evidence from Coal, Cave, and Lake valleys suggests that pluvial lakes in these basins reached their last highstand sometime between ~16,000 and ~20,000 years ago (Table 1 and Table S1).”

16. 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.

We now summarise all relevant previously published ages specific to our sample sites in Table S1 in the Supp Mat., and calibrate all C-14 ages using IntCal 20.14c.

17. 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.

We now include a new Figure 2 that shows pluvial lake highstands in each of the sampled lake basins. We refer the reader to Supp Mat Figs S1-S4 for zoomed in imagery of relevant landforms in Section 2 of the main text.

18. 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.

We've updated this age estimate based on the updated calibration of the Lake Valley (Carpenter Lake) highstand using IntCal 20.14c (Tables 1 and S1). We now estimate the highstand of Cave Lake to have occurred between 18,000 and 20,000 years based on regional lake highstand chronologies (Munroe and Laabs, 2013), as well as GIS computer simulations by Duke and King (2014).

This is stated in the 3rd paragraph of Section 2.

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

Changed to "volcanic rock".

20. Fig. 3: I do not think this map, which is almost unreadabl, 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?

We've deleted the geological map and refer the reader to the geological maps for each sampled site in the Supp. Mat. (Figs S5-S7). This will also shorten the length of the paper.

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

Done.

22. 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.

Done.

23. 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?

All sampled rocks were gravel-sized rocks. At Coal Valley clasts ranged in thickness (i.e., the shortest dimension that was cored in the laboratory) from 18.5 mm to 41.9 mm and were all composed of limestone. Clasts collected in Lake Valley were also all limestone and similar in dimension. At Cave Valley, the clast lithologies were dominated by intermediate volcanic rock at CA21P1 and ranged in thickness between ~15 and ~30 mm. The majority of clasts collected at CA21P2 were a similar size to those at CA21P1, but consisted of darker more mafic or basaltic varieties of volcanic rock. The specific mineral composition of the volcanic rocks was not measured.

We've included this in Section. 3.1. Due to light exposure considerations, the dated rocks could not be photographed prior to processing, however the volcanic rocks are shown in the Supp Mat post-processing. Rock thicknesses are also tabulated in Tables 4 & 6.

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

Done.

25. 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.

We've added that the detrital sediment from limestones was wet sieved to available grain size fractions between 63-250 µm. The specific grain size used varied from rock to rock, but is specified for the dated samples (Tables 4 & 5).

26. Line 230: e as subscript for De. Please check this throughout the paper.

Done.

27. 3.2.2: Are gravels of volcanic rocks from all three locations?

We've clarified in sections 3.1 and 3.2 that the gravel clasts collected from Coal and Lake valleys were all limestone rocks, while the clasts collected in Cave Valley were all volcanic.

28. 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.

We agree and have changed "K-feldspar" to "feldspar".

29. 3.4.1: Ln/Tn and Lx/Tx – 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.

Done.

30. 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.

We've added the statement: "The IR50 protocol includes low-temperature (160 °C) preheats prior to measurement of the regenerative and test doses, as these have been found in the past to reduce or remove recuperation and improve dose recovery test results (e.g., Neudorf et al., 2015). The pIRIR$_{290}$ protocol was adapted from Thiel et al. (2011). Because preheats must be equal to, or higher than pIRIR stimulation temperatures, this protocol includes a high preheat of 320 °C."

Work by Murray et al. (2009, Radiat. Meas.) demonstrates that, for K-feldspars at least, there appears to be no change in De for preheats ranging from 80 °C to 320 °C. This suggests that either the range of traps contributing to IRSL in this temperature region are all geologically stable, or, more likely, that the dosimetry trap is not significantly eroded by preheating up to 320 °C.

As shown in the subsequent sections, both protocols pass dose recovery tests.

31. 3.4.2: How many aliquots/grains were measured? Only the accepted number of aliquots/grains are given in Table 4.

The total number of grains measured are now included.

32. 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?

We've now added the following details about our sample bleaching procedures:

“Polymineral grains measured at the single-grain (or micro-hole) level in single-grain discs were first bleached simultaneously for 1000 s using IR diodes, then stimulated individually (in each hole) for 2 s each using the IR laser. It was found that this second bleach with the IR laser was necessary as the shadowing effects of the holes in the single-grain discs prevented full depletion of the IR_{50} signal in all grains. A similar approach has been adopted for single K-feldspar grains by Feathers et al. (2019).

For the pIRIR protocols, the pIRIR signal was depleted using a pIRIR L_n measurement (Table 2). Because the purpose of our dose recovery tests was to test the suitability of SAR, rather than measure a residual signal, a pIRIR bleach was used to target the pIRIR traps and avoid leaving a slowly bleaching part of the signal that can remain if the sample is only exposed to daylight or a solar simulator for a finite period of time (Li and Li 2011).”

33. 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.

See our comments under #3 above. We’ve added this statement at the end of Section 3.6:

“The internal K content of the sediment and rock minerals measured in this study could not be quantified, so grain (or rock mineral) internal dose rates are assumed to be 10 ± 2% following Smedley et al. (2012). We recognise that this value may be high for our polymineral samples (Maßon et al., 2025), and that future work should investigate the K contents of grains and minerals that contribute significantly to the luminescence signals detected using our detection windows.”

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

This is a typo and the error should be 50% of the water content (i.e., 1.5% water content). We’ve adjusted it accordingly and this new error is taken into account in all age calculations.

35. 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?

The sampling locations are now indicated, and an explanation for Fig. 4b is now included.

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

Yes, we've added the corresponding information for site CA21P1 in Table 3.

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

We've checked our data and corrected the typo.

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

This has been moved.

39. 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.

We've added the grain size information to Table 4 and clarified this in the text in Section 4.3.1. We've focused the discussion in 4.3.2 only on the samples chosen for dating and do not mention Rock #9.

40. 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).

We've clarified which are the single-grain and multi-grain aliquot measurements in the text. The goal was to correct each single-grain De value with the fading rate measured from that particular grain, as grain-to-grain fading rates vary significantly. The weighted mean for the sample is the weighted mean g-value calculated from all g-value measurements.

41. 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 fading correction procedure does not increase the OD of the De (or age) distribution. It actually decreases the OD because individual grain final age errors are increased by the H+L2001 fading correction (Fig. S13) (also see Neudorf et al., 2012, Radiat. Meas.). The resulting OD value for the final fading-corrected age (or fading-corrected De) distribution is actually slightly smaller than the non-fading-corrected data. For example, for Rock 2 (63-90um) in Fig S13, the OD is 76 ± 7%

(n=60) before fading-correction, while after fading-correction it is 67 ± 12% (n=60 before removing a high outlier). Also, all grains/aliquots with fading rates equal to or greater than 10 %/decade are rejected from the distribution (these are not included in the fading-corrected data) because they cannot be handled by the Huntley and Lamothe (2001) model.

42. 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 sample (rock 2, 90-125 um), where the fading-correction was done using a mean fading rate showed no significant change in OD after correction as expected (29+/- 6% to 30 +/- 6%). This can be interpreted to reflect the fact that all grain De values were corrected by the same amount (not different amounts due to different grain-specific fading rates). The non-corrected and fading-corrected datasets for Rock 2 (63-90 um fraction) both show a slight positive skew, which may be indicative of partial bleaching within this grain size fraction. The more symmetrical age distributions obtained by the 90-125 um of the same rock is interesting and may indicate that this part of the sample was well bleached. The reasons for the different shaped age distributions are not immediately clear but may be attributed to the tendency for finer grains to clump together in nature preventing full light exposure.

As already stated in Section 4.3.5, we emphasize the need for further research in understanding bleaching processes in rocks: "Given that most rock dating studies derive ages from rock (primarily granite, sandstones, quartzites, and volcanics) slices, rather than single-grains or small aliquots, the relationship between single-grain/aliquot age distribution shapes, OD, and depositional process for rocks is not known and has not been examined in the same way these relationships have been examined for traditional sediment dating studies (e.g., King et al., 2014). We provide preliminary interpretations of these data that should be solidified by future testing."

43. The sigma-b value for MAM was obtained from the dose recovery test, which indicates the minimum OD.

The sigma-b values were obtained from dose recovery test results that are not corrected for fading because we assume negligible fading during the short time between sample irradiation and measurement. Of course some fading will occur during this time, and this could, in theory actually lead to a slight increase in OD. Because fading corrections do not significantly increase OD (shown above), and because some fading may occur between irradiation and measurement in a dose recovery test, our sigma-b values should not be considered minimum values.

44. 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.

We've removed the MDM age for the 90-125 µm fraction from Rock 2 from the results, as we agree, the symmetrical distribution suggests complete bleaching of this sample.

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

This has been fixed.

46. 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.

We now explain the rationale for testing both the IR50 and pIRIR protocols in Section 3.4.2. We've also changed the headings of Sections 4.3.1, 4.3.1, 4.4.1, and 4.4.2 to include the word "results" to differentiate from "methods".

47. 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.

All dose recovery test results are now tabulated in Table S6. As mentioned above, a residual PIRIR290 signal was not measured. This is because we find the definition of "residual signal" to be vague as the size of the residual signal, however slow it bleaches, depends entirely on the length of time one exposes a sample to a light source. Such data does not provide useful information on bleaching rates of gravel rocks in pluvial lake environments. We emphasize the need for future work to investigate the bleachability of luminescence signals in gravel rocks in pluvial lake environments; such work would entail more involved experimentation to establish bleaching rates in gravel sized rocks, ideally in subaqueous conditions, along with predictive modelling. It is possible (or perhaps likely?) that the time it takes to bleach a gravel-sized rock is longer than a human lifespan, and of course any bleaching experiment we may be able to conduct in the lab.

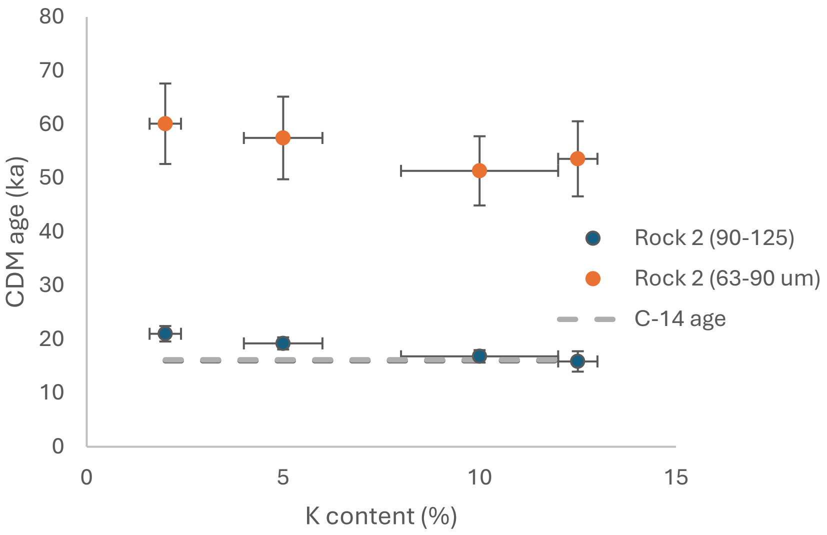
48. 4.4.2: Here only pIRIR180 and pIRIR290 were compared – what happened to pIRIR225?

Although the sample-average PIRIR290 fading rates were generally low, individual aliquot fading rates could still vary between 0 and ~12 %/decade. So we opted for the highest temperature (290) pIRIR protocol to minimise fading, and thus fading

tests using the pIRIR$_{225}$ protocol were not pursued. We've now clarified this in Section 4.4.2.

49. 4.4.4: It does not make sense to use 10+-2% K here for volcanic rock samples, whereas 12.5 +-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).

As mentioned in our response to #3 above, we've now re-calculated all ages to include an internal dose rate that was previously missing due to a calculation error. After including the internal dose rate into our limestone rock calculations that assume an internal K content of 10+/-2%, the CDM ages of Rock 2 (90-125 um) and Rock 18 (180-250 um) now overlap with independent age control at 1 sigma (Fig. 7, Table 5). A sensitivity test to view the impact of our assumed internal K contents is attached using the two grain size fractions from Rock 2, and K contents of 2, 5 and 10% (including 20% relative error), as well as the oft cited 12.5 ± 0.5%. The 90-125 um fraction has a symmetrical De distribution suggesting that it is well bleached. This shows that higher assumed K contents (10 and 12.5%) yield calculated ages closer to the independent age control than lower K contents, and could reflect a brighter signal emanating from grains with higher K contents in our detection window.



We have deleted the discussion concerning grain size and K contents in Section 4.4.4, as well as Figs S20-S22 in the Supp. Mat. and now include this statement in the methods Section 3.6 as per our response to #3 above:

"The internal K content of the sediment and rock minerals measured in this study could not be quantified, so grain (or rock mineral) internal dose rates are assumed to be 10 ± 2% following Smedley et al. (2012). We recognise that this value may be high for our polymineral samples (Maßon et al., 2025), and that future work should investigate the K contents of grains and minerals that contribute significantly to the luminescence signals detected using our detection windows."

50. 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.

Originally, we anticipated that the volcanic rocks would have very high or saturating De values near the centre of the rocks, so we applied the Kars et al. (2008) fading correction model. This turned out not to be the case. We have now re-analysed the data from the volcanic rock samples using the fading correction model of Huntley and Lamothe (2001) for consistency with the limestone ages.

We have also adjusted the text in Section 4.4.5 accordingly.

51. 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.

We've added the mean and uncertainty values in Figs 10 and 11 (now Figs 9 & 10) and fixed the scale.

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

We now use a CDM weighted mean of all rock surface ages plotted in Fig. 15 (now Fig. 14).