Comments	Responses
Major comments	
1) From the application of a Minimum Age Model (here bootstrapped MAM) to both quartz OSL and feldspar pIRIR signals the reader would infer that both the feldspar pIRIR and the quartz OSL signals are partially reset or show (high) dose tails (possibly due to mixing). However, at least 3 samples (NCL-11171 28,29 &30) have an quartz OSL over-dispersion (OD) of ~15% (see Fig. 5) and this is identical to the input OD of a well-bleached sample for BsMAM modeling. So, one would consider these samples as well-bleached for quartz OSL. Do the BsMAM and the CAM (or weighted or unweighted means if the authors prefer) give the same answer as the BsMAM for these samples? If the BsMAM works for well-bleached material both models should return identical results and the authors should demonstrate this. The CAM results should be listed and compared with BsMAM. Actually, the authors themselves allude on samples with well-bleached quartz OSL characteristics (see lines 394-395); please show the quartz OSL De distributions for all samples in Supp Info.	You are correct in that the three samples provide the same CAM and BsMAM provide the same results (agreeing with 1-sigma error) as we used the systematic approach of assigning a meaningful sigma_b value to our analyses outlined by Chamberlain et al (2018) . We shall provide the CAM results as commented, and demonstrate that for well-bleached samples results for CAM and BsMAM are in agreement. Also, we would like to point out that signal bleaching in our setting is most likely function of soil mixing intensity. The grains get surfaced by either natural bioturbation or anthropogenic ploughing. Thus, the dose tails mentioned by the reviewer are most likely a result of incomplete mixing rather than due to mixing as suggested by the reviewer. We will include a conceptual diagram to clarify our points on how soil mixing affects bleaching and dose distributions.
Would it be possible to discuss the average IR50 results in this paper too? If the IR50 signal is sufficiently reset which is definitely possible for the three samples mentioned above, these samples are likely to give IR50 ages smaller than quartz OSL (because of fading). The samples that are less-well bleached for both IR50 and pIRIR180 signals will tend to give ages equal to or larger than quartz OSL. I miss a discussion at the level of the average behaviour (CAM, weighted mean) in this manuscript.	We will include the analysis on IRSL signals as suggested. We have a similar expectation that IR50 ages will provide younger ages when compared to pIRIR175 ages. For well-bleached samples, as mentioned above, the BsMAM age from pIRIR175 provide same results with the CAM age from quartz OSL. We will report the results in the revised manuscript.
This leads me to the proposed research question: 1) How can well-bleached grains be identified for feldspar single-grain pIRIR dating? In my view, to answer this question one needs some form of independent age control. I cannot find that in this paper, especially because the authors believe that the quartz OSL ages should also be inferred from a MAM approach. The best option in the case one does not have independent age control, would be to use a well-bleached, unmixed quartz age (from CAM) and compare with the MAM age of feldspar pIRIR (filtered or unfiltered).	We agree that a truly independent age control would be ideal to answer the proposed research question. However, dating plaggic soil through other means is problematic. However, we have shown that quartz OSL for three samples are well bleached (low overdispersion and BsMAM in agreement with CAM results). We argue that the results on these samples are highly robust, and provide good age control to test our feldspar single grain dating.
2) I cannot seem to find the radionuclide concentrations, used water contents and the total dose rates in the paper. These data are crucial to calculate luminescence ages and should be tabulated.	We shall provide the data essential to calculate the dose rate in the revised version.
Minor comments	
Suppl Mat A.1 (Table): suggest to change cutheat to preheat. Cutheat refers to immediate cooling after reaching temperature but test dose preheat here has duration of 10s.	We will change the term in the revised version.
Line 17: humans, remove second recently,	We will remove the second 'recently'.
Line 36: has created	Will be corrected as commented.
Line 39: factor in the creation of anthrosols?	Will be revised as commented.
Line 56 (caption): at Braakmankamp	Will be corrected as commented.
Line 79: remove full stop after question mark	Full stop will be removed in the revised version.

Line 102: northern	Will be corrected as commented.
Line 145: place at a site	Will be corrected as commented.
Line 159 (caption): in areas with coversand	Will be corrected as commented.
Line 162: At all depths	Will be corrected as commented.
Line 169: gleiing	Will be corrected as commented.
The K-feldspar grains were not etched. Did you take into account an external alpha contribution? If so, how large is it?	The external alpha contribution was taken into account with the assumption of 0.05±0.025 Gy/ka.
Line 246-247: These contrasting effects, von Suchodoletz	Will be corrected as commented.
Line 252: Poolton et al. looked at elevated temperature IRSL but not post-IR elevated temperature IRSL? Please check, if not pIRIR, then remove ref.	You are correct; we shall remove the reference.
Lines 285-286: not logical after previous sentence in which it is stated that TT is very small or negligible (at least I cannot see a trend). There is more scatter in the results but this is not necessarily due to thermal transfer? Can also be sensitivity changes not full accounted for by test dose? Please rephrase.	We agree that there is a larger scatter, rather than a small TT, and will rephrase this.
Line 324: majority of the samples	Will be corrected as commented.
Line 343: Fig. 7a	Will be corrected as commented.
Line 346: remove second full stop	Will be corrected as commented.