Geochronology Discuss.,
https://doi.org/10.5194/gchron-2020-2-AC2, 2020

Interactive comment on "Extended range luminescence dating of quartz and alkali-feldspar from aeolian sediments in the eastern Mediterranean" *by* Galina Faershtein et al.

Galina Faershtein et al.

galaf@gsi.gov.il

Received and published: 17 March 2020

We thank the anonymous referee #2 for the positive and constructive review and address specific comments below.

38-40: It might be clearer to list the references with the technique list: "(TT-OSL; Wang et al., 2006a)..."

Done.

42-48: Other primary limitations include a low signal-to-noise ratio (TT-OSL) and the long period of time required to bleach all three signals relative to conventional BLOSL.

In the manuscript we present some examples for limitations of the different extended range methods. The referee suggests adding additional limitations. In our respectful opinion, the main idea is clear without adding more disadvantages of these methods. In addition, the signal to noise ratio of the TT-OSL varies from sample to sample and we don't see is as a limiting factor.

78: Capitalize "K"

Done.

83: Unclear here whether TT-OSL and pIRIR signals are dated with SAR or MAAD protocol. Please rephrase for clarity.

The TT-OSL and pIRIR250 ages are obtained with SAR protocol. It was clarified in the manuscript.

112: Please list what is meant by 'sensitized aliquots.' Are these simply discs that have been through the SAR cycle, or does this mean something else?

These are discs that went through several SAR cycles. It was clarified in the manuscript.

112-116: It would be nice to show the fading data (and fitted functions for g and rho). Could you include these in the supplement please? Also, to be clear, is the fitted value rho or rho'? From the caption of Fig. 9 it seems like rho'.

The average g-values and 's are presented in Table 5. In addition, fading data of all measured aliquots was added to the supplementary.

125-127: Please justify why this approach is preferred. If another sample were more variable in DRC or Ln/Tn values, one might expect an approach like this to produce bias.

We applied the calc_Huntley2006 R function using average Ln/Tn and the combined DRCs for all samples. As explained in the manuscript, this approach resulted in almost identical results as when applying the function on all measured aliquots (0-4 % difference). In our opinion, the variability on the DRC and Ln/Tn is embodied in the combined DRC and average Ln/Tn. Therefore, this approach is appropriate and time saving. The comparison made for sample KR-1 was added to the supplementary.

140: I am confused by this sentence. Maybe change from "is comparable" to "should be comparable" as it seems that you are referencing another dataset. I am unclear on the meaning of this statement: "...it is expected that DRCs constructed for different samples would be comparable as well." Comparable to each other? Comparable to the natural DRC? Both? Also, "the MAAD DRC is comparable to the natural DRC" is a bit ambiguous. Does this refer specifically to your DF-13 data? And are you comparing your data against data from Ankjaergaard? Are you interpreting DF-13 data with the help of conclusions from Ankjaergaard? Or are you simply restating a conclusion of Ankjaergaard? Please clarify.

Ankjærgaard et al. (2016) showed that a MAAD constructed DRC is comparable to a combined natural DRC for the Chines loess. Following these results, it is expected that MAAD DRCs of different samples of the same source would be comparable. The sentence was rephrased for clarity.

145: My understanding is that the 160 Gy added to RUH-180 is not an actual dose given in the instrument, but rather a number added to the x-coordinate of the data. While I think this is a clever thing to do (and really like your suggestion of treating these data similar to RF), neither Fig. 2 nor the text make this clear. For the text, please clarify that the data are shifted but the actual given doses range from 0 to 200? Gy. Likewise, Fig. 2 should be reworked to avoid the false impression that the samples were given doses of 160 to 400? Gy. Perhaps the use of an arrow, or a secondary inset x-scale for the red boxes.

Sample RUH-180 received doses of 0 to 200 Gy. The data points of this sample were shifted by 160 Gy on figure 2. This was clarified in the manuscript and the figure caption.

165-166: "saturates at 700-800 Gy" Is this D0? 2D0? "saturates" in this context is an ambiguous concept.

The laboratory DRC of pIRIR250 reaches 2D0 at 700-800 Gy. This was clarified in the manuscript.

234ff: While I basically agree with your assessment that saturation occurs around 2 m and 6 m for OSL and the others, it might help to be slightly more quantitative, if possible. For example, why not saturation at 4 m for VSL? That datapoint has 1-sigma overlap with the lowest sample in that profile.

The concept of saturation profiles (plotting natural signals vs. depth) is qualitative due to variation in dose rates. Fitting the data with a certain function would by misleading. For the VSL signal the saturation level is harder to determine as this signal is rather noisy. Although the Ln/Tn level at 4 m overlaps with the Ln/Tn levels at 6 m and 11 m it dose not overlap with the lowermost sample (15 m). Discussion regarding the saturation depth of VSL was added to the manuscript.

261: A third option would be significant erosion which strips off material down and exposes the old, saturated units. This seems incompatible with your 'clay from the surface' hypothesis though.

This option was added to the manuscript.

283: "fading rates increase over geological time" I'm confused by this statement. The functional form of both the Huntley and Lamothe (2001) and the Kars et al. (2008) would yield the opposite response following a lab dose–a decreasing rate through time–either as a simple logarithmic decay or as a sigmoid (in log-x space). If instead you mean that fading rate should increase with geologic dose, i.e., that only unstable sites remain open, then that makes senese. But how this relates to your argument is not clear to me.

Fading rates can increase at high absorbed doses as was demonstrated by Huntley and Lian (2006) and Wallinga et al. (2007). This process is considered by the fading correction of Kars et al. (2008).

296-298: Here and in Fig. 8, I think the argument that 'age mirrors dose rate' is a little misleading. Earlier in the manuscript you seem to indicate that samples below 6 m are close to saturation for TT-OSL and that these TT-OSL ages should be treated as minimum ages. If this is the case, then a) this should be clear in Fig. 8 (currently there is no indication that TT-OSL samples below 6 m are minimum ages), and b) the comparision you really make is between dose rate and 2D0/Ddot or similar (e.g., time until near saturation). This relationship is informative for characterizing samples but not for providing a depositional timeline, as would currently be interpreted from Fig. 8. Fi.g 10 does a better job at representing this.

As explained in the manuscript TT-OSL De values of all samples below 6 m cluster at 400-500 Gy but resulting in apparent TT-OSL age increase with depth due to decrease in the environmental dose rates. The ages discuses in the text and presented in figure 8 are calculated ages (Table 3) and not 2D0 ages. Hence, comparison to the dose rate trend is adequate. It was added in the caption of figure 8 that all ages below 6 m should be treated as minimum.

319: "signal loss"

Done.

327-328: How similar were the growth curves of the KR samples? Was this examined in order to justify using a common MAAD curve for all KR natural signals?

Interpolation of natural signals onto a MAAM DRC of a modern sample was done based on the results of Ankjærgaard et al. (2016), who showed that a MAAM DRC of a modern sample is very close to the natural DRC of their samples. Laboratory VSL DRCs were not constructed for the KR samples as discussed in the manuscript. Nevertheless, comparison of MAAD DRCs of two samples is presented in the manuscript (Sect. 2; figure 2), strengthening the interpolation approach.

359: "one can expect the A and upper B horizons to be kept relatively bleached all the time." This may be the case, but the portion of grains that are fully bleached due to bioturbation is likely to depend upon the local plants and animals.

That could be true but cannot really be quantified. As a first-order approximation we assumed that vegetation type and cover did not change significantly over time.

Tables 1, 3, 5: Unconventional to give dose rates as microGy/a. Please consider using milliGy/a instead (better yet, Gy/ka, given that ages are reported in ka and doses in Gy).

The dose rates were changed to Gy ka-1 in all tables.

Fig. 3: "OSL signal and DRC are modified from Zilberman et al. (2007)" Please describe this modification, here or in the main text.

The signal and DRC were not modified. This was corrected in the figure caption.

Fig. 9: Are the Ln/Tn error bars shown? Please include these if not.

The error bars for the Ln/Tn are shown in the figure.

Interactive comment on Geochronology Discuss., https://doi.org/10.5194/gchron-2020-2, 2020.