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

Anonymous Referee #2

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Dear Authors and Editor,

This manuscript presents an interesting comparison of three long-range luminescence chronometers: TT-OSL, VSL, and pIRIRSL. While the authors do determine age saturation for the oldest sediments, they also extend the existing depositional chronology at this site. Overall the manuscript reads well, is scientifically robust and presents novel results. I have provided some comments below which I hope will prove useful to the authors.

538-40: It might be clearer to list the references with the technique list: "(TT-OSL; Wang et al., 2006a)..."

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Discussion paper

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.

78: Capitalize "K"

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

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?

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'.

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.

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.

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.

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

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.

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.

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 sense. But how this relates to your argument is not clear to me.

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.

319: "signal loss"

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?

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.

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).

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

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

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