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Interactive comment on "Regional Pliocene Exhumation of the Lesser Himalaya in the Indus Drainage" *by* Peter D. Clift et al.

Anonymous Referee #1

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General Notes: The authors use εNd values and U-Pb zircon ages in sediment cored from the Indus submarine fan and the offshore Laxmi basin to constrain shifting source areas in the western Himalaya. They compare their results to δ13C, apatite fission track, sedimentation rates from prior publications. The data in the paper is valuable and worthy of publication, but some of their interpretations are problematic. The authors use the percentage of total zircon age population to determine which source is eroding most rapidly at different points in time. Based on this they argue for a previously unrecognized Quaternary exhumation event within the inner Lesser Himalayan Sequence (iLHS). The authors acknowledge that this interpretation of their data can only be made consistent with published bedrock zircon (U-Th)/He data from same region if iLHS rapid exhumation was not accompanied by its exposure at the surface.

This would require rapid erosion of the overlying outer LHS and/or Greater Himalayan Sequence depending on the geometry of the underlying thrust or duplex. There is no indication of this rapid erosion of overlying material present in their data. Furthermore their interpretation of a Quaternary iLHS exhumation event is inconsistent with data from Najman et al. 2009 (who the authors cite). Najman et al. (2009) show that the LHS in this region was exposed at the surface and contributing sediment to the foreland basin by 9 Ma. The results of Najman et al. (2009) are consistent with the widely recognized 10 Ma thrust wedge reorganization during which the Main Boundary Thrust (MBT), the fault underlying the LHS, became active across most of the Himalaya. It should also be noted that LHS age consistent zircons are well known components of the granites of Nanga Parbat. The increase in LHS age consistent zircons at 1.9 Ma may reflect the well documented post 3 Ma rapid exhumation of Nanga Parbat e.g. Koons et al. 2002 (and many other papers). Alternatively the increase in LHS age consistent grains at 1.9 may be driven by rapid exhumation of the proximal foreland basin that accompanied the Quaternary initiation of faulting along the Main Frontal Thrust in the area. Since the LHS is the nearest bedrock to the foreland basin it follows that its detritus makes up a significant proportion of the immediate foreland basin. The onset of MFT faulting and accompanying exhumation of the foreland basin should be considered an alternative driver of the shift in percentage of zircon ages at 1.9 Ma. The detrital zircon analysis in this paper would benefit from a more rigorous statistical treatment. I recommend something along the lines of DZ Mix (Sundell and Saylor, 2017). At the very least the authors need to ensure that their interpretations of the DZ data are plausibly consistent with published bedrock thermochronology from the area.

Detailed Notes: Page 1 Line 13 – authors state that a decrease in εNd values correlates with increasing abundance of >300 Ma zircon grains. Since no correlation coefficients are calculated I suggest replacing "correlates with" to "corresponds with" or "coincides with". Page 1 Line 14 – authors state that the increase in >300 Ma zircon grains precludes large-scale drainage capture as the cause of a long-term decrease in εNd values. I realize that this is an abstract and space is limited but it is not clear why an increase in zircon grains whose age is consistent with Himalayan bedrocks warrants such strong language. Without further explanation it seems more correct to say that the increase in >300 Ma grains suggests increasing Himalayan contribution, or suggests that large scale drainage capture is not the cause of the εNd decrease. Based on my evaluation of the reported zircon ages the increase in >300 Ma grains precludes drainage capture or reorganization of syntaxial or Karakorum draining rivers. However it could be explained by drainage capture or reorganization of rivers draining southern Tibet and the High Himalaya, specifically changes in the upper reaches of the Indus, Sutlej, or Chenab rivers where they flow orogen parallel across Tethyan rocks in southern Tibet. Page 1 Lines 16-19 – Authors use the increasing percentage of >1500 Ma grains in the post 1.9 Ma sediment samples to suggest a previously unrecognized episode of LHS exhumation in the western Himalaya. This is an exciting prospect. Maybe I'm missing something, but can't the shift in relative abundance of >1500 Ma grains also reflect decreasing exhumation rates in the Karakorum and Kohistan regions as well? Also, I think a discussion of lag times between exhumation and deposition in the fans is need as well. Especially when we are discussing rivers with headwaters on the Tibetan plateau, these rivers flow through multiple sub-basins where long term (>100 ky) sediment storage has been predicted (Blothe and Korup, 2013). I realize that 100 ky is not long compared with million year timescales but it at least warrants discussion. Page 1 Line 25-27 – Authors state that foreland basin sediment provides an incomplete sedimentary record of orogenic unroofing since it is dominated by more local sediment sources. While this may be true of the most proximal coarse grained deposits more distal fine grained foreland basin deposits should reflect a more complete picture of the fluvial sediment load. Maybe it a failure of imagination on my part, but I don't see why distal foreland basins deposits should differ greatly from the submarine fan deposits analyzed here. The only additional sediment source the fan is sampling is the foreland basin deposit itself. Page 1 Line 28-32 – The Authors argue that basin deposits are the only complete record of the long term exhumation of mountain ranges, which is true. However the events with which they are most concerned

happened in recent geologic time (Miocene and younger) these tectonic changes in the thrust wedge are well documented in studies of bedrock exhumation. Any claim of rapid Quaternary exhumation of the LHS should be evident in bedrock studies as well, but to my knowledge no such study has found evidence for this. Page 2 Lines 10-15 – The authors acknowledge that zircon U-Pb dating is only possible when sediment is sufficiently coarse grained. In their samples this means they could only analyze post 8.3 Ma sediment using this technique. This is problematic. They would like to use detrital zircon (U-Pb) dating to discern changes in sediment source, but are restricted to a very short window of time over which to do so. For example, prior studies have shown that a major foreland propagation event occurred around 10 Ma when the MBT became the locus of active thrusting across the orogen. This event led to widespread exhumation of the LHS. If the authors want to argue for a Quaternary period of rapid LHS exhumation it would be nice to see what the well known $\sim$10 Ma event looks like in their data for comparison. As it stands now we are left to wonder how the 1.9 Ma increase in LHS age consistent zircon grains compares with the well documented earlier event. Page 2 Lines 25-35 – This section is titled "Provenance Methods" but these lines are a discussion of prior studies more appropriate in a background section than a methods section. Page 5 Line 5 – Authors state that the εNd values increase from 17 Ma to 9.5 Ma. This trend is barely discernable in their plot. The long term running average hovers between -9 and -10 over this interval. Page 5 Lines 10-30 – There is a great deal of discussion of the potential effects of paleo-rivers on the submarine fan compositions. It would be helpful to see a figure showing these rivers. This entire discussion could benefit greatly from information on longshore currents in the region. Without this the reader is left to wonder what effect longshore drift, or storm events may have had on submarine sediment distribution. Page 5 Lines 38-42 – The authors state that young zircon grains (< 25 Ma) are restricted to the Nanga Parbat massif in the Indus catchment and then go on to acknowledge that zircon of 1850 Ma and 400-500 Ma age are known to be common in the Massif as well. This has been recognized as a LHS contribution to the granites of Nanga Parbat. This seems to be an alternative explanation for their proposed young LHS exhumation event. Especially since Nanga Parbat has been recognized as one the most rapidly exhuming place on Earth. Page 6 Line 30-35 – The authors state that the increase in LHS age consistent zircons can only be achieved by preferential erosion of the LHS. However such an increase could also be achieved by rapid exhumation of the most proximal foreland basin deposits. The strongest increase the authors report is at 1.9 Ma, right around the time when the Main Frontal Thrust began to rapidly deform and uplift the foreland basin. The authors state previously that foreland basin deposits are dominated by proximal sources. . . in this case the LHS is the most proximal bedrock unit. The rise in LHS age consistent grains may actually reflect the onset of MFT deformation in the region. Page 7 Line 26-30 – The authors note that despite a well documented exhumation event in the K2 region of the Karakorum they do not see a signal of this enhanced erosion in their samples. This is a common theme of this manuscript. The same is true of the 5 Ma to recent rapid erosion of the Nanga Parbat massif which does not show up in their samples (at least not in their interpretation). These examples should be viewed as red flags that the dataset presented here does capture the complete exhumation history of the region. Whether it is due to sediment residence times in upstream sub-basins, some sample bias introduced by hydrologic sorting, or some other unknown cause it seems clear that the dataset cannot be interpreted as a complete record of western Himalaya exhumation. Page 8 Line 30-33 – Authors argue that their data shows the iLHS was not significantly exposed at the surface until 3-4 Ma. However, published zircon (U-Th/He) data cited in the manuscript shows that the iLHS was being rapidly exhumed by 11 Ma (Colleps et al. 2018), and that it was exposed the surface and contributing grains to the foreland basin by 9 Ma (Najman et al. 2009). The authors simply state the exposure of the LHS happened later that recognized in previous studies but do nothing to reconcile their dataset with those prior publications.

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