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Interactive comment on "Timing of exotic, far-travelled boulder emplacement and paleo-outburst flooding in the central Himalaya" *by* Marius L. Huber et al.

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First of all, we thank Paul Carling for his interest in our study and also for taking the time to share his insightful suggestions and comments. This reply is aimed at providing key responses and details on the main points that were raised, in the spirit of Esurf discussions. These points will be addressed in more details in future iterations of this manuscript together with the points raised by other reviewers.

Perspective & paleo-hydraulics:

The first series of comments raised by P. Carling is related to the transport mechanisms

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Discussion paper

that mobilised these boulders. It is clear to us that at least some of these boulders have been transported over long distances. The lithology of the largest boulder in our dataset, for instance, can only be found outcropping 13 km upstream. This is the case for many of the surveyed boulders. It is true that the exact provenance of these boulders remains unclear, the fact that some of these boulders were previously incorporated in moraines before long-distance transport is indeed possible (as suggested in the comment), especially if these boulders are entrained by glacier lake outburst floods (GLOFs) as we suggest is a plausible explanation. However, there is a lack of evidence to directly support this hypothesis and, if correct, it would not change the conclusions of our study. Nonetheless, for the sake of completeness, we will mention this hypothesis in the discussion of our results in the revised version of the manuscript.

We do however not think that glacier transport to the location of deposition (< 1000 m a.s.l.), being it during the LGM or during earlier glaciations, is likely. There are very limited traces of the extent of the glaciations before the LGM on the southern flank of the central Himalayas (Owen & Dorch, 2014) but glacial chronologies from the lower relief, higher-elevation regions north of the range suggest that glaciers' extent during the Quaternary were not much larger than the LGM (Owen, 2020).

The hydraulics responsible for the transport of these boulders is necessarily poorly constrained as these types of flows are not frequently observed or instrumented. We, therefore, agree with P. Carling that the paleo-flow estimates we derive should be taken with caution but we sincerely believed this caution was conveyed by our manuscript; for instance, through the use of three available transport laws and our reporting of a broad 2 orders of magnitude range of what discharges might have been. We will emphasize these limitations further in the manuscript and will also refer to the references we might have missed that are cited in the short comment. It should nevertheless be noted that paleo-discharge estimate is not the finality of our work and that these estimates were made for comparison with available modern gauging data. This comparison suggests that paleo-discharges were most likely significantly larger than what is observed during typical monsoons. And in some way, this confirms the obvious, since the fact that these boulders have been exposed for several kyrs in the river bed suggests that no recent flood has exceeded the flood that emplaced them.

We are not aware of creep processes that could explain slow boulder transport over long distances during monsoonal flows in such environments as is suggested in the comment. A travel distance of 10 km (shorter than generally observed from our data) over 5 kyrs would require about 2 m of movement per-monsoon season. This seems unlikely to us and would almost certainly result in greater dispersion of the exposure ages reported in our study.

As for the boulder size estimates, tape measurements have been made in the field when possible but these have proven challenging. Satellite imagery was therefore judged to represent a more homogeneous alternative. Overall boulder size estimates remain first-order estimates but these do not nuance our conclusion that emplacement has most likely occurred through exceptional hydraulic events.

Cosmogenic nuclides:

It is indeed true that our exposure age estimates represent minimum ages as is frequently the case with cosmogenic nuclides exposure dating and in the manuscript we acknowledge that erosion may lead to younger apparent ages. However, the general effect of surface erosion on boulder ages is relatively limited, especially for younger boulders. A surface erosion rate of 10 mm/kyr, as suggested, will change the calculated exposure age of ca. 150 years for our 5 kyrs boulders and about 1000 years for our older 12-15 kyrs boulder (Martin et al., 2017 - https://crep.otelo.univ-lorraine.fr). Surface erosion is therefore unlikely to significantly affect our exposure ages. As mentioned later, toppling of the boulders or the breaking of significant parts of the surfaces, if undetected during sampling, can affect boulder ages more significantly, but this would also result in stochastic ages.

It is possible that some of these boulders suffer from cosmogenic inheritance, i.e. pre transport 10Be build-up linked to earlier exposure. This would result in boulder exposure ages significantly older than their true emplacement age. We do not observe such a pattern in our data as it is very unlikely that several boulders have received the same amount of pre-exposure, especially given the different lithologies implying different sources. Even though we do not favour the hypothesis that the boulder transport occurred slowly, over long time spans, such a mechanism would likely result in wide exposure age distributions, as boulders rotate during transport, successively exposing different sides for various amounts of time.

Even though the number of samples is limited in the Sunkoshi reach, we still identify three boulders out of five with ages compatible within uncertainty with the ages found by more boulders in the Trishuli, the remaining two being much older. It would have been beneficial to sample more boulders but we nevertheless believe that the suggestion of events occurring both in the Sunkoshi and the Trishuli around 5 kyrs is a robust finding of our study.

We hope that we have provided some constructive comments that help clarify the manuscript under discussion. As mentioned earlier we will incorporate these into an eventual future revision.

Marius Huber, Maarten Lupker & Sean Gallen on behalf of all co-authors

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