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Earth Surface Dynamics
Discussions

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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Perspective

Huber et al (2020) present a potentially valuable contribution to determining the rate and timing of transport of large boulders in a montane environment in the context of landscape evolution. However, herein I propose that a more cautious and nuanced approach might enhance the interpretations advanced. The initial motion and transport of large boulders in fluvial environments is poorly understood, but is an important topic of research related to channel and valley evolution. Indeed, the corollary is that identification of exceptionally large boulders that have not moved is also important as these

indicate an upper limit to palaeoflood competence (Carling & Tinkler, 1998; Carling et al., 2002).

Palaeohydraulics using boulder data

As noted by the authors, large boulders can be delivered to the valley bases by earthquakes and other gravitational slope processes. These slope-derived boulders have the potential to be transported by monsoon floods or by lake-outburst floods. Some rockfall blocks may remain in situ until abrasion and weathering reduces their size sufficiently for fluvial transport. Nonetheless, in the present examples, the boulders are many kilometres downstream from known outcrops of the given lithologies, so they must have been transported by ice or by water. Huber et al (2020) argue that the boulders must have been water-transported to their present locations as they are outside of the known last glacial ice-limits. Whilst this is a reasonable assertion, it is also possible that some of the boulders could have been transported by ice to moraines upstream (e.g., LIA, LGM) and then transported from the moraines to their current locations by fluvial processes. In addition, consideration needs to be given to the possibility that the boulders were transported by an early and more extensive glaciation, but the authors provide no information on the ice-limit of glaciations prior to the last. In the circumstances, the distances of travel induced by floods reported by the authors require recalculation or qualification. It would be useful to have the positions of the glacial maxima, if known, indicated on Fig. 1.

I assume boulder sizes were also measured in the field but the text suggests all dimensions were obtained from Google Earth imagery. There is no way to know the resolution of Google Earth imagery at a given site, which can range from 15m to 0.15m. Calibration of pixel resolution against objects of know size in the field would have been useful, as would publication of the dates of images used. Imagery also can provide only the planview size of boulders with the vertical dimension having to be estimated. Determining boulder volume in such circumstances is a well-known problem and use of laser-scanning would have produced more reliable boulder dimensions. That being said, the limitations of the Huber et al. sampling methodology are not unusual and I do not wish to dwell unnecessarily on the sampling of boulder dimensions, but prefer to focus on the entrainment calculations and estimates of palaeodischarges, as well as the dating issue.

It would have been useful to see a fuller appreciation of the three entrainment equations utilized, with an explanation for their selection. The Costa (1983) empirical method for boulders in shallow flows has stood the test of time as it is equivalent to applying the Shields equation with an average value of the Shields parameter (Ït’) of 0.04 (s.d. of 0.011 for n = 40) for gravel in the size range 20mm $\leq$ D $\leq$ 5000mm (Carling & Fan, 2020). The use of this equation is an extrapolation in the analysis of Huber et al. (2020). Theoretical considerations have concluded that a Shields parameter of 0.04-0.05 applies to well-embedded large gravel clasts, including boulders (Lamb et al. 2015; van Rjin, 2019; Dey & Ali, 2019), but large boulders may be entrained for Ït’-values < 0.04; down to 0.01 for example (Carling & Fan, 2020). Protruding boulders in particular tend to have lower Ït’-values. In addition, boulders protruding from the flow and subject to obstacle-induced standing waves, have radically different entrainment conditions in contrast to fully-submerged boulders (Carling et al., 2002a & b). Scour in gravel surrounding boulders is far more intense for large protruding boulders than for deeply-submerged boulders (Schlömer et al., 2020) promoting undermining and motion of large protruding boulders (Clark, 1996). Boulder protrusion and undermining might be a particular issue, where there is a ‘dramatic topographic gradient’ as indicated by the authors resulting in steep channels subject to rapidly changing periods of aggradation and degradation of alluvial bed levels, also noted by Huber et al. (2020).

Clark (1996) used a simple force-balance for fully-protruding clasts with a drag coefficient of around 1.18 or less. This selection of drag coefficient values can only apply to fully-submerged boulders in deeper flows (Carling et al., 2002b). This appreciation explains why the Clarke function plots below the Costa function in Fig. 3A. Nevertheless, Clarke was concerned that some of his boulders may have been transported by

debris-flows, in which case the entrainment threshold calculated for clear-water using his equation might readily be related to observed boulders that are too large to have been moved by water. Consequently, extreme caution is needed when applying the Clarke analysis.

The Alexander & Cooker (2016) analysis was most welcome as it incorporates the small impulse factor due to floods arriving as a bore front, as well as flow instability. It is well-known in coastal science that very large shoreline boulders have been observed to move during relatively small storm-wave impacts that exert a large impulse. In these cases the application of the traditional force-balance approach for steady flow is not suitable and rarely predicts boulder entrainment accurately. Nonetheless, the Alexander & Cooker analysis applied to the data of Huber et al. (2020) lies below that of Clarke (1996) in Fig. 3A, as it should. Given the issue of imperceptible downstream creep of large boulders due to undermining, it is possible that the boulders observed were moved down valley under conditions whereby small values of Ït’ pertain, or the boulders could have been emplaced by debris-flows which Huber et al. (2020) do not consider.

The arguments above explain why the velocity ranges estimated subsequent to application of the three different entrainment equations differ systematically. Taken together the velocities reported by Huber et al. (2020) range between 4.3msˆ-1 and 17.3msˆ-1. However, velocities in alluvial rivers rarely exceed 3msˆ-1 (e.g. Jia et al., 2016), with flow in steep bedrock gorges occasionally reaching 8 to 10ms-1 (Barnes, 1956; Pielou, 1998; Whipple et al., 2000). Natural dam failures can result in short-duration, modelled high-velocities (15-19msˆ-1: Alho et al., 2005; Carrivick, 2007; Carrivick et al., 2013) in upstream reaches which tend to reduce downstream as the floodwaves attenuate. Thus, although Huber et al. (2020) in section 2.3 provide some caveats as to the discharge estimates they regard discharges up to 10ˆ5 mˆ3sˆ-1 as acceptable. From the arguments I outline above the discharges at the higher end of the range calculated by Huber et al. (2020) should be viewed with extreme caution. In making this assertion, I are not suggesting that floods of that magnitude have never occurred due to dam-breaks events. Indeed, the comparison of discharges advanced by Huber et al. (2020) with other large floods (c. 105 m3s-1) reported in the literature does indicate some precedence, but these earlier outbreak flood estimates, of course, are subject to considerable uncertainty. Despite relatively low velocities occurring during monsoonal floods in contrast to outbreak floods, the long time-periods (basically post-glacial) considered means that over thousands of years boulder creep becomes an important transport mechanism in steep mountain valleys, as well as debris-flow. In summary, it remains an open question as to whether large boulders can be transported by monsoon floods or whether exceptional outbreak floods necessarily have to be invoked.

Cosmogenic dating

The application of cosmogenic nuclides to date the deposition of the boulders raises a few issues that should prompt further consideration by the authors. There are four main points.

1) In the absence of burial, exposure ages should always be regarded as minimum ages.

Unless there are some preserved surface forms on these boulders, such as glacial striations, then one should assume that some fraction of the nuclides that arrived with each boulder have been lost via erosion. At what rate? It would be good to see a simple sensitivity analyses applying plausible surface erosion rates for this environment: 1 mm/kyr, 2 mm/kyr, 5mm/kyr, 10 mm/kyr? etc. The question is: what surface rate of erosion is required to move outside of the mid-Holocene window? How plausible is that?

2) Intermediate storage and accumulation of nuclides between source and sink.

Some of these boulders are big: Trishuli valley, ten boulders ranging 8.5 to 18.6 m;

Sunkoshi valley, six boulders ranging 4.5 to 29.9 m. These exhibit very long boulder transport distances: 22-46 km in Trishuli valley, and 11-17 km in Sunkoshi valley. Boulders that exhibited signs of post-depositional movement were not sampled; however, it is the transport history of these boulders that could be the problem here. It would be good to see some explicit acknowledgement of the possibility that some fraction of these boulders accumulated nuclides at some point upstream of where they were sampled. As noted above, these are very long transport distances. To expect a single transport from source to sink is simply not plausible for blocks of this size. It is more likely that these blocks have taken a few rest stops along the way where they might have been buried or eroded in situ, if close to the channel.

A simple test could have been to sample the underside of a few of the tabular blocks to determine whether they spent much time the other way up (e.g. Fujioka et al. 2015). Intermediate storage might, for example, be an alternative explanation for the two blocks in Sunkoshi with high nuclide abundances. It may be possible that boulder erosion during transport may have removed any inherited nuclides accumulated during intermediate storage steps. However, it would be good to see some consideration of this scenario in the manuscript. The authors do not air any of these potential doubts; I suggest a more candid discussion would be welcome.

3) Age clusters

Regarding the Sunkoshi data shown in Fig. 4, how is it that a single exposure age forms an ‘age group’. These are single data points. The clustering of exposure ages around 4.5-5 ka is not convincing in Sunkoshi given the small number of samples, which is too few to establish any statistical significance. The Trishuli cluster is more convincing. One important thing that has been learned about Himalayan valleys over the past decade is that valley floor elevations fluctuate by tens or even hundreds of metres over relatively short intervals (e.g. Schwanghart et al. 2015; Munack et al. 2016). Boulders sampled up to 18 m above the channel in Trishuli valley suggests that there might be scope here to bury clasts for significant periods within the valley fills.

Perhaps an alternative explanation of your age distributions is that these nuclide concentrations reflect ‘exhumation’. That is, the nuclides accumulated in the boulder surfaces as they were exhumed from beneath a deeper thickness of valley fill. The exhumation itself might be a pervasive erosion of the valley floors resulting from the postglacial decline in sediment supply. Such a scenario would yield the kind of spread with vague clustering. It would be interesting to investigate this further. Exhumation might explain why there are no boulders with higher nuclide abundances. If these boulders are the product of outburst floods, why do we have such a short record in these valleys? Where are the boulders deposited by previous outburst? All washed away?

4) Lower nuclide concentrations, why not younger floods?

In Section 4.3 it would be better to simply have the ages reported, rather than the special conditions under which some data are preferred over others. It is a bit unclear why samples with low nuclide abundances are dismissed as somehow problematic when they might just easily be the result of more recent floods?

Specific issues: In line 201, I do not follow why this low concentration ‘only allows to determine a maximum exposure age of 0.49 ka’? Finally, I am a bit unclear on how the probability distributions for individual ages are calculated in Fig. 4 and what this means exactly. Perhaps some more detail is required here.

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