Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2017-66-RC2, 2017

Earth Surface Dynamics
Discussions

Interactive comment on "Evidence of, and a Proposed Explanation for, Bimodal Transport States in Alluvial Rivers" *by* Kieran B. J. Dunne and Douglas J. Jerolmack

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Received and published: 23 December 2017

The manuscript addresses a question that emerged in the past half century from studies of hydraulic geometry sensu largo (Parker et al. 2007, 2008) and downstream fining (see Frings 2008 for review). The two main claims are: 1) that gravel-bed river have sediment mobility near the threshold for motion and sand-bed rivers are in the suspension regime; 2) that channel geometry is a result of the threshold of bed material in gravel-bed rivers and of cohesive bank material for sand-bed rivers. These claims are based on reanalysis of existing hydraulic geometry data and data of rivers along the gravel-sand transition.

The manuscript is mostly based on review and replotting of existing data. The manuscript is clearly written and the data is well-presented in the figures. However, the manuscript is flawed in a number of aspects: a large number of essential references are missing in the presented datasets and in the text; the insights presented are not novel and that based on a downstream fining river is flawed; some of the reasoning about experiments is based on only a few points while many more are available in the literature. Below these points are detailed.

It is not exactly clear what precisely the argument of the authors is. It seems to be that "there are mainly sand-bed, suspension-dominated rivers and gravel-bed, bedload-dominated rivers because of the scarcity of the intermediate sizes on this planet and because the typically available floodplain materials are strong enough to withstand the typical bed shear stress in sand-bed rivers but not in gravel-bed rivers." The arguments revolves around the scarcity of sediments between sand and gravel, discussed by the authors, and the scarcity of sediments between sand and floodplain fines, not discussed by the authors. If this is correctly represented then these ideas have already been published in many papers over the past decades and are therefore not novel. If not correct then the argument is not clearly enough presented.

An elegant way, in principle, to look at the relation between bed mobility and channel geometry is the dataset of Singer (2010). This shows a gradual downstream reduction in bed shear stress but a zone with a bimodal reduction in median particle size and in excess mobility due to patchiness of bed material (also found in Paola and Seal 1995). However, the authors here invoke this observation to argue for bimodal behaviour in the entire dataset and this argument is flawed. Here, and on P3 L14-16, important references are missing, such as Yatsu (1955), Paola and Seal (1995) and work by Frings (review in 2008). The large body of literature on downstream fining bracketed by these references show three things. First, many fluvial sediments are bimodal because of the nature of sediment and the manner in which it wears down. The effect is that there are very few rivers with dominantly fine gravel in their bed. Second, in a bimodal sediment distribution where the two modes gradually change in height, as in downstream fining where the finer mode increases in abundance as the coarser mode reduces, the median grain size flops suddenly from one mode to the other where the mean size would not. This means that the representation of downstream fining by the median size is misleading the authors to believe in the bimodal behaviour. Third, many gravel-sand transitions are rather gradual, except in many relatively small rivers (Frings 2011). This is partly due to patchiness, bend sorting, and the transition from clast-supported to matrix-supported sediments. Elsewhere it has also been argued that many rivers show a third mode of sediment: the silt and clay that form the floodplains. Usually the material between that mode and the sand mode is scarce too (see Kleinhans 2010 for review).

The systematic trend of increasing bankfull Shields number with decreasing grain size is disputed on the basis of two pieces of evidence related to published experiments. The first is that seepage channels formed in experiments with sand cluster at the threshold for motion, and the second is that other sand-bed channels in the laboratory likewise are at the threshold for motion. But this misses the point of scaling in experiments entirely. The Shields number is the relevant scale for sediment mobility, and in experiments that scale down the size of systems in nature by orders of magnitude, of course the sand is near the threshold for motion because it simulates gravel and even at sand size needs steeper slopes than in reality. This is why other experiments with self-formed seepage channels were conducted with low-density sediment (Marra et al. 2014), and consequently the mobility ranged from 0.044-0.68.

Furthermore, there are a large number of other experiments with self-formed channels (compiled in Kleinhans et al. 2014 Fig. 9 and Table 1 and some more in 2015) that show a range of mobilities in the context of a large river dataset that also show a gradual transition from bedload to suspended load dominated behaviour. This includes high-mobility experiments with sand and floodplain-forming low-density sediment (Paola group, Minneapolis) and with materials that strengthen banks (famous experiments of Hoyal, ExxonMobil) (see Kleinhans et al. 2015 for references). The main point is that the nature of the bank material determines the channel geometry. This is essential for bar and river patterns and was incorporated in the hydraulic geometry in Kleinhans et al. (2015) because correct scaling of the bank material in experiments leads to desired channel aspect ratios. The qualitative understanding behind this work goes back at least to Ferguson (1987), and shows that the experimentally inferred relation of the straight-meandering-gravel transition as a function of slope by Schumm and Kahn (1972) is wrong and the bank stability is key. Furthermore, the natural and experimental channels adhere well enough to the hydraulic geometry relations of Parker et al. (2007) when accounted for the strength of the banks (Kleinhans et al. 2015), which allowed application of the Parker relation to bar theory. It is already well-known that the absolute dimensional shear stress in gravel-bed rivers is much larger than that in sand-bed rivers despite the lower sediment mobility, so that bank material unstable in the gravel reach can be stable in the sand-bed reach. The hypothesis on P10 L14-16 has already been voiced in a large body of bank erosion literature underlying the BSTEM model by Simon et al. (2000 and later references). This argument has not become more precise by the scatter plot in Fig. 7. Also the authors do not present new data or new insights; for example P10 L27-32 states that information about bank material is usually lacking and this is precisely why Kleinhans and van den Berg (2011) had to introduce that counterintuitive streampower measure and why Eaton and Church (2007) developed a rational regime theory that can be used to invert to bank stability measures from observed channel dimensions. These papers and many others already make that point; however, these references are lacking here.

The final remark of the discussion is somewhat simplistic: "Some of the scatter in hydraulic geometry scaling plots may be due to stochastic fluctuations around the mean behavior." Here, stochastic is a rather dirty term for a number of well-known sources of variability. Apart from the fact that vegetation creates its own patterns with its own scales on the banks, leading to variations in channel dimensions, the balance between bank erosion and floodplain formation is not always in equilibrium because of the specific length and time scales of bank failure and floodplain / levee formation. The assumption of the authors is that measured channel dimensions in their compiled dataset are due to such variations, but it is quite likely that the original authors of the datasets underlying this compilation already averaged out such variation. In fact, this is certainly the case for the Kleinhans and van den Berg (2011) dataset and in the experimental datasets from our laboratory, that were also taken up in the compilation of Metivier, and are therefore possibly part of the present compilation.

Finally, the authors present their data online, which is commendable, but entirely unreferenced, which is unacceptable, not only because it does not pay proper credit to the original owners. These data originate from many sources and likely the workers used somewhat different methods which may strongly affect the scatter and trends. Furthermore, the seepage and experimental datasets are sorely lacking numbers, which are available in the recent literature.

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