Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2017-66-RC3, 2018

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

Anonymous Referee #3

Received and published: 7 January 2018

I have reviewed the paper, and the subject is quite an interesting one. However, the analysis seems to be fundamentally flawed to me, and ignores a vast body of work that has been trying to make sense of reach-scale channel geometry for many decades. The paper relies far too heavily on work done by a small group of authors. As a result, much of the insight that the paper does produce I feel has already been better explored and presented by previous authors. Because this paper is not based on a fair and balanced understanding of the previous work, it is not suitable for publication, in my opinion.

Critical Issues: On page 5, line 10, you state that you assume the Chezy friction factor

is constant. I can see no possible justification for this assumption. Looking at Ferguson 2007 and Eaton and Church 2011, it is clear that Cf varies dramatically with relative roughness, and that changes in relative roughness also produce order of magnitude changes in the bedload sediment concentration generated at the same critical dimensionless shear stress. These scale variations are fundamental, and need to be included in the numerical analysis. This is one of the reasons that Millar took the numerical approximation approach that he did, and then generated power law equations using a Monte Carlo modelling approach (see Millar, 2005). This assumption appears to me to fundamentally undercut the entire following analysis.

Specific comments: Page 1, Line 15: you claim that the empirical hydraulic geometry equation are robust, and show remarkably little variation in the exponents: this is a stretch. log log plots hide the true scale of differences between datasets; the lumped data includes systems where W/d ratio declines downstream, and where it increases downstream; it includes systems where depth is nearly constant, and ones where it changes as expected. You also fail to observe that simple Froude scaling nearly explains the trends that are found in nature, which indicates that most of the variance can be explained simply by the size of the system, not any particular organizing principle. This is well described by Eaton in the Hydraulic Geometry chapter of the Treatise on Geomorphology. In fact, there are significant variations in the exponents, when they are compared against the Froude scaling exponents, and I believe this is well described by Eaton and Church (2007) in JGR.

Page 1, Line 20: you attribute the concept of the dimensionless discharge to Metivier (2016). This is certainly not the primary source for a dimensionless discharge. Andrews used it in his 1984 paper (though there is an unfortunate typo in the final version, he used exactly this definition of a dimensionless discharge).

Page 2, Line 1 It is remarkable that you do not refer to Ferguson's classic work on the basis for regime theory. I believe that this is still one of the best summaries of the problem, and provides a much more satisfactory statement of the problem than the authors provide here.

Page 2, line 10: The "ground state" approach by Metivier sound exactly like the threshold channel approach used by many previous authors, including Lane, 1955; Henderson, 1966; Stevens, 1989.

Page 2, line 2: the discussion of the stable channel paradox seems to be to miss the point. The key thing to realize is that the shear stress acting on channel banks is lower than the average boundary shear stress, and that the shear stress acting on the bed is higher than average. This particular issue was very well addressed by Rob Millar's implementation of a regime theory that considered bank strength, using the shear stress partitioning approach by Knight, Flintham and Carling. I recommend reading Millar 2005 and the numerous relevant papers cited therein. I believe that Millar's contribution has adequately asked and answered the questions that this paper attempts to answer, and that the data analysis presented herein does not provide any additional insight to the problem. In any case, I do not see the justification for publishing this analysis without acknowledging the previous work!

page 3, line 11: you state that there is no accepted model for the equilibrium geometry of rivers far above threshold. I think is is an unfair and inaccurate representation of the current state of the science. There are models (like Millar and Quick, 1998) that successfully predict the geometry of such streams. They are published, and have been successfully tested, yet you appear to disregard all of these models without even bothering to mention them.

Analysis in Fig 2: does this really tell us anything that we do not already know from Church's (2006) exploration of the plotting positions of various rivers? While the plots are somewhat different, I do not see what novel insight they provide, particularly given the scale distortions introduced by the inappropriate assumption that Cf is constant!

Page 6, lines 1 to 5: You text is not an accurate representation of the bank strength issue. With respect to vegetation, the effect on relative bank strength is fundamentally

scale dependent (see Eaton and Giles, 2009, Eaton and Millar, 2017), which you fail to mention (and which also introduces significant scale distortions), and the effect on channel with is close to a linear one. As banks become very erosion resistant, then changes in width are reduced, because the channel is able to reach the hydraulically optimal form, beyond with narrowing does not produce any increase in bed shear stress (basically this is the geometry predicted by Wobus 2004 for erosion into bedrock).

Page 6, line 9: your use of Schumm's M findings is a poor choice, since that analysis is a classic tautology. Clay and silt are never found on the channel bed, so Schumm's index (which is the percent silt and clay averaged over the entire channel boundary) builds in the width depth ratio; therefore it cannot be used to make a meaningful prediction of the width depth ratio. Simons and Albertson (1963) do manage to make some progress on the sedimentological controls for canals, however.

Page 6, line 15: the authors present a hypothesis about what controls bank erosion, but that hypothesis was advanced previously by Nanson and Hickin, and is the basis for the modesl developed by Millar and Quick (1993) and by Eaton (2006).

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