

Interactive comment on “Morphodynamic regime change induced by riparian vegetation in a restored lowland stream” by J. P. C. Eekhout and A. J. F. Hoitink

S. Dixon

s.j.dixon@bham.ac.uk

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I enjoyed this paper and it is valuable as a relatively uncommon study of post-project river restoration monitoring. However there are a few things which I think could be changed relatively easily to improve the manuscript and one slightly larger issue around interpretation which I think needs to be addressed.

The larger issue is around the interpretation of the role of vegetation in mediating and controlling the morphological change, as opposed to developing coincidentally with a change in regime. The authors are right to highlight other works demonstrating the role of vegetation in stabilising banks and floodplain surfaces, but I don't feel they demon-

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strate this is what has occurred in this study. The river restoration project finished in October 2011 and from the days since completion in the paper vegetation began to develop in May. It would seem a natural process that vegetation growth and establishment would be suppressed in Autumn and be much more prevalent in Spring, so this is an unsurprising finding. The key issue though is the lack of channel equilibrium before the start of the study; looking at Fig.6 the DOD appears to show in the first few weeks a large net aggregation in the whole channel suggesting the restored channel is not competent to transport its sediment load (probably due to increased sinuosity equalling a decreased bed slope and thus lower energy channel), furthermore in the second DOD (day 93-133) there appears to be further aggregation restricted to the upper portion of the channel, this would indicate that the decrease in bed slope through the reach associated with re-meandering the channel is causing deposition as sediment-laden water flows into this reach. This deposition has then been instrumental in triggering the cut-off described in the text. Once this initial period of adjustment to the new (imposed) channel morphology of the restoration has taken place the channel is closer to equilibrium and thus morphological change is less. The establishment of vegetation is thus coincidental with expected adjustment to the new post-restoration morphology and not a driver or control of it, or subsequent equilibrium. In Tal & Paola (2007, 2010) the flume is already at a dynamic equilibrium braided planform under a constant discharge before the addition of alfalfa or varying discharge, therefore they are able to attribute changes to the vegetation, comparisons between this study and Tal & Paola (2010) in pg 726 ln 13 are therefore not justified in the context used.

Such an interpretation would need to be addressed and refuted specifically in the manuscript in order for the findings to be valid, for example pg725, line 9 makes no account of any expected adjustment to a new channel form but rather implies the lower cohesive strength of the vegetated banks is driving change, which fails to account for increased deposition during this period.

I will not address specific writing points with the text, however I would like to make a

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general comment that sentence structure, especially in the introduction is perhaps too short with linked ideas bridging multiple sentences leaving many starting with "These", "They", "This", etc. Linking ideas more strongly by reiterating the subject specifically, or by using semi-colons would make the structure clearer.

Around pg714 line 26 I think a stronger case could be made for the novel nature of the work. Authors are perhaps selling their work short. There are two issues; i) I don't feel a convincing case has been made that their work is that novel in relation to DoD application to morphological change; however I DO feel this case is there to be made. There is a line from Croke et al 2013 specifically calling for more DoD studies which strengthens the case. ii) there are few published studies of detailed post-project morphological monitoring, therefore a case can be made on the value of this work purely on that basis (rather than just the technical methods), but is missing.

Site Description: pg715 ln 22 "mildly sloping" - better description needed pg715 ln 23 - better characterisation of underlying geology needed pg716 ln 6 - description of sediment fill needed (from floodplain, clay plugs, gravel??)

pg719 - there does not appear to be any discussion of error propagation within the DoDs. Each point is +/- 0.02m and although change of 0.04m has been excluded there remains effective error bars on each cell within each DEM (and thus each DoD), therefore there will be uncertainty in each calculation of net and gross morphological change and this needs to be addressed.

pg 719 ln 11 - I think this needs to be reworded, as currently it implies a lack of planning!

pg 720 ln 4 - what is the resolution of the vegetation data (relative to the DEMs)?

pg 721 - shear stress has been calculated as reach averaged and time averaged, whereas most erosional work will occur during high flow events, by averaging authors are losing the temporal extremes in shear stress which actually drive erosion. An acknowledgement of this is needed, or an explanation of why this is not important.

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Furthermore I cannot see any explanation of how shear stress was calculated for the cutoff channel or floodplain.

pg 723 ln 4 - Are results statistically significant? pg 723 ln 11 - needs statistical analysis or plots needed to demonstrate this relationship pg 723 ln 12 - justification needed for why this analysis by bins of 5% has been used

Interactive comment on Earth Surf. Dynam. Discuss., 1, 711, 2013.

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