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Earth Surface Dynamics Discussions

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

J. P. C. Eekhout and A. J. F. Hoitink

joris.eekhout@wur.nl

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We would like to thank the reviewer for the comments, which have improved our manuscript considerably. We appreciate the reviewer thinks that this is a relatively uncommon study of post-project river restoration monitoring. A detailed response to each of the issues raised by the reviewer is provided below.

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 demonstrate 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.

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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.

We agree with the reviewer that the data provide insufficient support to assign riparian vegetation as the primary driver of the changes in morphological response. Indeed, the channel was initially in a state of morphological disequilibrium. This may be held responsible for processes related to the occurrence of the chute cutoff and bank erosion in the downstream half of the study area during the initial six months after construction of the stream. We have revised the manuscript in this direction, emphasizing the rate in which a reconstructed lowland stream adjusts towards a new equilibrium. We have changed the title, the abstract and the conclusions accordingly. We have removed the reference to the laboratory experiments.

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

We have made editorial changes to address this comment, improving the readability.

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

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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.

Thanks! We have changed the last paragraph of the Introduction into: "A field study is presented, based on 14 high-resolution morphological surveys over a period of almost two years, which makes this study among the highest temporal resolution DoD-analysis to date. The study focusses on a reach-scale study site, covering three meander bends and the adjacent floodplain. Morphological and terrestrial ecological data are combined, under varying discharge conditions. The field study is performed in the context of stream restoration. Stream restoration projects are rarely subject to monitoring schemes combining morphological, hydrological and ecological surveys, although there are exceptions (e.g., Gurnell et al., 2006)."

Site Description: pg715 ln 22 "mildly sloping" - better description needed

Changes sentence to: "The study area is located in a lowland catchment, which implies a mild bed slope."

pg715 ln 23 - better characterisation of underlying geology needed

Changed "drifting sand" to "aeolian-sand"

pg716 ln 6 - description of sediment fill needed (from floodplain, clay plugs, gravel??)

Added sentence: "The sediment fills originated from other parts of the study area, where excess sediment was available from the construction of the new channel."

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.

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We have combined theory by Legleiter and Kyriakidis (2007), Heritage et al. (2009) and Milan et al. (2011) to produce the DEMs and DoDs, adopting a channel-fitted coordinate system. In the revised manuscript, we have offered more details of the processing methods, which are elaborately described in Eekhout and Hoitink (submitted). Although we are confident this is a state-of-the-art manner of effectively filtering out much of the error from the original data through the interpolation routine, a rigorous propagation model of the remaining error is lacking. In the revised manuscript we write: "The difference values exceeding the LoD are typically one order of magnitude larger than the standard error in the individual measurements, and the interpolation routine is designed to filter out error from the raw elevation data. This, and the fact that the aggregation of individual measurements tends to further average out much of the error, implies a high degree of accuracy of the resulting volumetric rates of change, although a rigorous method to quantify the remaining uncertainty is lacking.".

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

Due to a malfunctioning pressure sensor, water level data was unavailable in the first 103 days after construction. We have added this sentence to section 3.4.

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

The aerial photographs were taken with a 10 cm (day 188) and 25 cm (days 289 and 636) resolution. We have added this sentence to section 3.5.

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.

We have included 25% and 75% quantiles obtained from the Shields stress time-series

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to Figure 10 of the revised manuscript. These quantiles provide information on the temporal variability of the Shields stress.

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

In the revised manuscript, we have excluded the analysis of the four geomorphic areas. Indeed, estimating the Shields stress at local scale would be better in that case, but reliable Shields stress estimates are extremely difficult to obtain in shallow areas such as the floodplain and cutoff channel in the present study.

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

After reconsideration, we have removed this analysis from the revised manuscript. We now fully focus the manuscript on the morphological development in relation to riparian vegetation development. An analysis of water level variation contributes too little to this topic.

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