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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

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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 nice and detailed monitoring project. A detailed response to each of the issues raised by the reviewer is provided below.

This paper describes an in-depth monitoring project on a lowland stream restoration project. Monitoring consisted primarily of repeat RTK-GPS surveys coupled with hydrologic monitoring, one aerial photograph, and repeat photos from the ground level. The authors use DoDs to determine morphologic change, sepa-

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rated into geomorphic regime, and relate the overall change by regime to hydrologic drivers. This is a nice, detailed monitoring project of a remeandered site.

Like other reviewers, however, I have problems with premise that vegetation is the primary driver for the change in stream behavior over the 1.5 years following project completion. The authors see a lot of change initially and decreasing amounts of change through time, with more muted response to high flow events in year 2 as compared to year 1. This decrease in change and muted response to floods in year 2 is attributed to vegetation growth. You would expect the same kind of signal even without vegetation growth, due to the disequilibrium associated with the remeander project. Deposition in the channel followed by a bend cutoff indicates that perhaps the designed channel was unable to transport its imposed sediment load and thus adjusted. To me, this attribution of change to vegetation is the biggest short-coming of the paper. Fundamentally, although it may still be the case that some of the stability attained in the system is driven by vegetation, the authors need to address the possibility that the system is simply adjusting over time to the impulsive nature of the restoration project including the cut-off that occurred shortly after the project was completed.

We agree with the reviewers that assigning riparian vegetation as the primary driver of the change in morphological response is insufficiently supported by the data. Indeed, the channel was initially in a morphological state of disequilibrium. This apparent disequilibrium 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 reoriented the revised 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.

In addition, given that the premise of the paper is that riparian vegetation growth

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is altering channel behavior, the density, type, and changes in riparian vegetation are not addressed well. In fact, I see no mention at all as to what kinds of vegetation were established at the site. The color-shaded "vegetation density" data in Figure 10 is qualitative and comes from photos shown in Figure 9. There is no mention of the species that are present, whether or not they were planted, how they vary across the reach, how they change seasonally, etc. In addition, there is no quantitative "vegetation density" metric that can be compared with geomorphic change. There is only a general sense that vegetation was not present and then it slowly grew back. The NDVI would have been helpful here, but there is only one photo that was analyzed, which gives only two points in time (t=0 with no vegetation and t=289 days).

We agree that more information on the vegetation was lacking in the original manuscript. In the revised manuscript we have added data from a parallel study on the riparian vegetation development in the study area. This explains the additional co-author. We have added the methods to section 3.5 and the results to section 4.2. Species-specific characteristics were added to Table 2. We have added a paragraph to the Discussion section explaining the type of vegetation (herbaceous) and we have discussed how this vegetation type may play a substantial role in stabilizing channel banks in lowland streams.

We have also added two additional aerial photos to the revised manuscript, allowing to increase the spatial understanding of the vegetation growth. We added an aerial photo taken at day 188. This photo did not include near infrared data, so no NDVI could be obtained from the photo. We added a third aerial photo (day 636) containing near infrared data and determined the spatial NDVI. The temporal resolution of the aerial photos was too low to quantify vegetation growth during the study period. For this information we can only rely on the qualitative interpretation of the terrestrial photographs (Figure 8). The additional aerial photographs do increase understanding of the rate in which riparian vegetation grows after reconstruction measures have been

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implemented in lowland areas by showing the spatial expansion.

Additional comments: 1. This is a very rich dataset in terms of morphologic change. 2. I like Figure 11 and think it shows good evidence for a change in behavior between times 1-5 and 5-13.

Thanks for your supporting remarks about the presented dataset and interpretation.

3. In terms of shear stress, the authors use a time-averaged, reach-averaged shear stress based on one cross-sectional survey (for A and P) and the reach-average slope. Yet most sediment transport occurs during peak events in isolated locations. Given that they have detailed gauging records and detailed DEMs across the reach, I think a better treatment of shear stress could be done.

We have included 25% and 75% quantiles obtained from the dimensionless bed shear stress (Shields stress) time series to Figure 10 of the revised manuscript. These quantiles provide information about the temporal variability of the Shields stress, capturing peak events.

4. I have trouble with the idea of bank erosion calculated in terms of channel widths/year over time scales much shorter than that. I realize the idea is to standardize between measurements taken over different time intervals, but using a temporal ruler that is longer than the sampling period is misleading. Perhaps channel widths/day is a more defendable rate. Likewise, this rate is being applied to bank erosion in meters, but aren't you calculating bank changes in m3? How did you get back to change in meters?

We agree, channel widths/day is a more defendable rate. We present the results in this way to make it comparable with other studies. Most studies are presenting bank erosion with a width change per time period. The volumetric change is presented in the manuscript as well (Figure 6).

5. Bounding boxes were set up to define channel bank, bed, floodplain, and

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cutoff channel (Fig. 3). Were those bounding boxes adjusted through time as the channel shifted?

The segregation of the study area into the four geomorphic zones was done for each individual morphological survey, so indeed the bounding boxes were shifted. We emphasize this in section 3.3: "For each DoD, the morphological activity was quantified for the study area as a whole, and for isolated geomorphic zones. In each individual DEM, the study area was segregated into four geomorphic zones: channel bank, channel bed, floodplain, and cutoff channel."

6. p. 725, line 8-10 implies that the observations made in the two stages of behaviour indicates that riparian vegetation can have a substantial influence on time that elapses before an equilibrium is reached. Given the difficulties in attributing the two-stage behaviour to vegetation growth alone, I don't think this statement holds.

As stated earlier, we have withdrawn the causal relationship between riparian vegetation growth and morphological regime change in the revised manuscript.

Other minor comments: 1. P. 712, line 8: add in the word "through" as in "maximum coverage halfway through the survey period"

Added.

2. P. 713, line 20 should include Tal and Paola (2007)

Included.

3. P. 719: Was the RMSD and volumetric change calculated only on areas that were above the LoD? How was the uncertainty propagated through to the volumetric change measurements?

Indeed, we have calculated the RMSD and volumetric changes using measurements exceeding the LoD only. We have combined theory by Legleiter and Kyriakidis (2007),

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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.".

4. P. 720, line 11 should reference equation 3

Changed to Eq. (3).

5. P. 721, line 12: specify "survey 5-13", not just 5-13.

Added "survey".

6. P. 724, line 1-2: "riparian cover started to decrease". Was this because it was winter? It would be nice to include actual dates on figures and in the text so that the seasonality of flows with respect to the seasonality of vegetation can be seen.

We added actual dates to Figures 6, 8 and 9 to include the seasonality to the analysis of the results.

7. P. 724, last line: "accidental peaks in discharge" This is an odd term, and I'm not sure what it means. High-intensity precipitation events lead to peak discharges in unregulated catchments, too. Could you make this more clear?

Indeed, this is a common response of catchments in other climatic regions. We re-

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moved this sentence from the revised manuscript.

8. P. 725 – I think it needs to be clear in the paper that the regression lines presented in Figure 11 are for the time periods 5-13 only. It is in the caption, but should be in the text, too.

We have added the following sentence to the Discussion section: "The linear regression model was established for the period after riparian vegetation emerged (survey 5-14) only."

9. P. 726, middle paragraph, I agree with another reviewer that this comparison between the field site and experiments by Gran and Paola, Tal and Paola, and Braudrick et al. is not warranted as those systems achieved equilibrium before vegetation was established and the energy of the system did not change. I think to make that comparison, you would need to watch the field site for several years with no vegetation while it attained equilibrium, and then add vegetation.

We agree on this point. Therefore we have removed the comparison with these experiments from the Discussion section.

10. Minor comment on figures – can you add an arrow showing direction of flow? It is Hereby on Figure 1, but not the others, and I think it would help.

We have added arrows to Figures 4, 5 and 7.

References:

Eekhout, J. P. C. and Hoitink, A. J. F.: Importance of backwater effects to the occurrence of a chute cutoff, Journal of Geophysical Research, submitted.

Heritage, G. L., Milan, D. J., Large, A. R. G., and Fuller, I. C.: Influence of survey strategy and interpolation model on DEM quality, Geomorphology, 112, 334–344, doi:10.1016/j.geomorph.2009.06.024, 2009.

Legleiter, C. J. and Kyriakidis, P. C.: Forward and inverse transformations between

cartesian and channel-fitted coordinate systems for meandering rivers, Mathematical Geology, 38, 927–958, doi:10.1007/s11004-006-9056-6, 2007.

Milan, D. J., Heritage, G. L., Large, A. R. G., and Fuller, I. C.: Filtering spatial error from DEMs: Implications for morphological change estimation, Geomorphology, 125, 160–171, doi:10.1016/j.geomorph.2010.09.012, 2011.

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