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Interactive Comment

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 study is valuable, considering not many stream restoration projects have been monitored with this amount of detail. A response to each of the issues raised by the reviewer is provided below.

The paper reports on the morphological evolution of a restored reach of a lowland stream in the Netherlands. The study is valuable, as not many river restoration projects have been monitored after realization with this detail (but see Gur-

nell et al., 2006). The paper is presented in a clear way. However, in my opinion, there are a few points that need addressing, before publication of the manuscript.

I do agree with the interactive comment by S. Dixon that the main issue is about the interpretation of the morphological changes in relation to vegetation establishment. I am not sure that vegetation is the main factor in controlling the observed changes in morphological evolution. My impression is that the main interpretations and conclusions, as presented by the authors (including the title of the paper), is not supported by the observed data. The first period after the restoration is probably affected by the decreased sediment transport capacity of the new channel.

After a careful reconsideration of our findings, we agree with the reviewer that the data do not assign riparian vegetation as the primary driver of the change in morphological response. In the revised manuscript, we stress that the channel was initially in a morphological 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. We reoriented the revised manuscript in this direction, emphasizing the rate in which a reconstructed lowland stream adjusts towards a new equilibrium. We accordingly changed the title, the abstract and the conclusions.

Moreover, it looks like the chosen meander wavelength was not in equilibrium with the morphological conditions of the reach. Meander wavelength is usually 15-20 times channel width, which in this case means about 100-120 m (e.g. Seminara, 2006). The initial wavelength is shorter and the cutoff increased it to about 20 widths (if I am correct measuring on the maps). These two are probably the main reasons for the initial greater morphological changes.

We doubt that the meander wavelength simply tends towards 15-20 times channel widths. Indeed, the meander wavelength of the meander bend that was cutoff increased to a value around 60 m ($\sim$ 10 channel widths), but the meander wavelength

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of the downstream located bend decreased to 20 m ($\sim$ 3 channel widths). In low-energy circumstances in lowland areas, multiple equilibria may exist, featuring varying wavelengths.

In addition, the study considers a period of 1.5 years, with only one growing season for the vegetation and is therefore rather short in order to infer about equilibrium and the effect of vegetation. The vegetation shown in Figure 9 is mainly herbaceous, annual vegetation. Do you expect woody vegetation (riparian trees) is going to develop in the next years? In the paper, time is measured in days after project conclusion and there is no discussion about seasonality of vegetation growth (e.g. Mahoney and Rood, 1998). The last larger flood (after about 440 days) occurred in a moment where vegetation was strong. What would like to be the effect of a flood in winter or early spring (day 600?), when the vegetation dies off?

We extended the dataset with one morphological survey and two aerial photographs, allowing to increase the spatial understanding of the vegetation growth. The dataset now contains two growing seasons. Furthermore, we added vegetation data from two field surveys to the revised manuscript, on the species specific characteristics of the riparian vegetation 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. The type of vegetation (herbaceous indeed) and the role this vegetation type have in stabilizing channel banks in lowland streams is discussed in the Discussion section. Previous research has shown that herbaceous vegetation may be more efficient in stabilizing the top 30 cm of the soil than shrubby and woody vegetation (Wynn et al., 2004). The characteristics (Tab. 2) show that the dominant species are perennial vegetation, rather than annual vegetation. And root depths may vary between 10 and 100 cm. Therefore, we argue that the herbaceous vegetation may have a significant control over the bank stability in lowland streams, where channel depth is around 0.5 m.

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Other specific comments: - About NDVI computation: please report on the spatial accuracy of the aerial image in the text (at present it is mentioned only in figure caption). NDVI has been widely used to quantify vegetation occurrence, but commonly the threshold value is larger than 0 (about 0.1-0.2). Did you assess this threshold, comparing the NDVI classification with field measurements and/or visual interpretation of the aerial image? Wet sand is likely to have associated values of the NDVI of about 0.1-0.15.

We have added the following sentence: "The obtained NDVI values within the study area are in a range between 0 and 0.2, which is associated with bare soil (Holben, 1986). Nevertheless, the aerial photo and field observations show that riparian vegetation was present in the study area, where higher NDVI-values corresponded to more abundant riparian vegetation. Further analysis of the aerial photographs only considers the areas where positive NDVI-values are obtained."

- About computation of the bed shear stress: a spatial and temporal average of the bed shear stress has been computed, considering one cross section (at the water level gauge) and the entire period between consecutive bed surveys. This estimate is quite rough, as it does not consider the transversal variability of the bed shear stress and the fact that sediment transport is not a linear function of the shear stress. Is the cross section at the water level gauge similar to the bed topography in the study reach? Would it be possible to compute the shear stress in the different areas (channel, banks, floodplain, and chute cutoff)? It would be much better to relate the RMSD to this local estimate of the shear stress. Bed shear stress in the floodplain (particularly for periods with lower discharge) is definitely lower than that in the channel, and this could modify the relationships reported in Figure 11. Furthermore, a dimensionless version of the bed shear stress would be better, as it would allow the comparison of these results with other case studies.

We agree that a dimensionless version of the bed shear stress allows to better compare

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the results with other case studies. Therefore, we have replaced the bed shear stress with the dimensionless bed shear stress (Shields stress). Indeed, the time-averaged Shields stress at the cross-section of the water level gauge is a rough estimate of the temporal and spatial variability of the local Shields value. We have included quantiles (25% and 75%) obtained from the Shields stress time series to Figure 10 of the revised manuscript. These quantiles provide information on the temporal variability of the Shields stress. Estimating Shield stresses in extremely shallow flows over floodplains and in the chute channels is cumbersome. In the revised manuscript, we focussed this analysis on the entire study area and excluded the separate analyses for the four geomorphic areas.

- Moreover, a local estimate of the bed shear stress would allow the investigation of the relationship between inundation frequency and bed shear stress. Vegetation growth is affected by flow level, but sediment transport has probably a stronger effect on vegetation removal.

This would be a very interesting analysis. However, after reconsideration, we have removed the analysis of inundation frequency and spatial vegetation development from the revised manuscript. We focus the manuscript on the morphological development in relation with riparian vegetation development. An analysis of inundation frequency in relation with riparian vegetation development may be too far from this topic. Apart from this, we think that it may be difficult to obtain a trustworthy value for the Shields stress in shallow areas, as stated in the previous comment.

- Is there any information about the return period of the floods that occurred during the 1.5 years of this study? This is relevant in order to discuss about morphological changes in the floodplain, and to frame the observed morphological changes in a broader context, particularly when discussing about equilibrium.

We included the return period of the high discharges in periods (1-2) and (9-10) to section 4.3: "In Fig. 9b, two periods of extremely high discharges occurred, i.e. in *period (1-2) and period (9-10), featuring discharge peaks with a return period of 120 days and 180 days per year, respectively."*

- p. 726, lines 5-10. I am not sure this sentence is correct. I would say the opposite, i.e. initial changes are driven by disequilibrium of the configuration, whereas in the second part there are typical meander processes, with most of the changes occurring in the channel and in the bank area.

We agree the processes that occurred in the initial period are associated with the morphology being in a state of disequilibrium. We changed the Discussion section accordingly.

- p. 726, lines 13-17. I do not agree that this case shows a shift from a high energy system to a low energy one. First, energy is the same; if vegetation has an effect on energy, it is probably an increase in the main channel (narrower) and a decrease in the floodplain (due to increased roughness). Second, the chute at the beginning is due to lack of equilibrium in the sediment supply and, possibly, a wrong wavelength.

We have removed the cited sentences from the Discussion section, including the references to the laboratory experiments.

- p. 726, lines 23-26. Authors do not consider timing of flooding, that is crucial in determining channel evolution, as well as growing season of vegetation. A major flood in spring or early summer could remove most of the vegetation, delaying vegetation establishment by one year.

We have replaced these sentences by: "To some extent, this duration can be manipulated by changing the season in which the new channel is planned to be constructed. This has an impact on the duration of the pre-vegetation period, and on time the channel forming discharges may cause morphological changes."

References:

Holben, B. N.: Characteristics of maximum-value composite images from temporal AVHRR data, Journal of Remote Sensing, 7, 1417–1434, doi:10.1080/01431168608948945, 1986.

Wynn, T. M., Mostaghimi, S., Burger, J. A., Harpold, A. A., Henderson, M. B., and Henry, L. A.: Variation in Root Density along Stream Banks, Journal of Environmental Quality, 33, 2030–2039, 2004.

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