Hydrol. Earth Syst. Sci. Discuss., 7, C5231–C5234, 2011
www.hydrol-earth-syst-sci-discuss.net/7/C5231/2011/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Macroinvertebrate community responses to a dewatering disturbance gradient in a restored stream" *by* J. D. Muehlbauer et al.

Anonymous Referee #2

Received and published: 8 March 2011

General comments:

This paper uses an ecosystem scale manipulation of hydrological perturbation to study the response of macro-invertebrate communities. At this scale, the possibilities of doing such manipulative experiments are rare. Moreover the particular geomorphic nature of the Timberlake mitigation site allows controlling to a great extent the habitat heterogeneity effect. The data presented in this paper are thus of particular importance for gradient ecology.

The paper is well-structured and written in clear and fluent English. The introduction

nicely sets the theoretical frame of the study and discussed its potential implications for practice. The methods are of high scientific quality although a few points need clarification. Results are clear, concise and nicely discussed. However one interpretation made by the authors is problematic. Supplementary material needs to be completed.

Specific comments:

Wetted perimeter is a central metric in this study. I, personally, feel unable to recalculate it based on the explanation given. The authors discussed these metrics and provide two references, but these came only in the discussion section and do not provide substantial explanations of the methodology. Moreover, figure 1, which present the gradient used throughout the paper, use percentage change in wetted perimeter and depth. Since the study focuses on a perturbation gradient, I suggest adding raw data of these two metrics at the different sampling dates as supplementary table, as well as the distance to the downstream pumps for each site.

Water quality measurements consisted in single spot samples. This is certainly not the best method to account for the changes experienced by macro-invertebrates, but I think it is reasonably fair to give a qualitative picture of the changes occurring along the gradient.

The authors provide a single reference as example of the identification facilities they used. However they state that the level of precision (i.e. family, genus, species…) of the identification depended on those facilities. I think the authors should provide a detailed list of the literature used for species identification related to each major taxa (e.g. Diptera) that could be included in the species abundance table. It is difficult to figure out how the authors have assigned each species to a functional group (i.e. Hydrophyte-associated, Swimming, and Benthic) and how the coding of the variables was done. Since species may often be considered in two groups at the same time (e.g. habitat change for reproduction, change in behavior during maturation), this issue is of particular importance when studying the functional response of communities. Stating clearly which taxon belongs to which functional group in the species table would help the reader understand the study.

Introduction, p. 9603, l. 20 “community response would be due strictly to changes in metrics like channel depth or water quality”. In my opinion, this statement is unfair at this stage of the paper. The authors are right in assuming that water related metrics will have the strongest influence on communities. However the authors consider that community response is either due to changes in water related metrics or refugia effect. They did not consider that communities may change due to biotic interactions, species colonization, seasonal effects, and many other potential effects. However, NMDS results tend to confirm the author statement. As a result, modifications of the lines 18-20 (Introduction p. 9603) would be, in my sense, sufficient to address this issue. Methods, p. 9606, l. 27: Authors did not use the “control” site for the invertebrate analyses because it was “radically different from the other 5 sites even before the dewatering”. It is not clear what radically different means. It could represent change in species composition, abundance, richness… To avoid such confusions, the authors may provide species abundance table for each sampling events as supplementary material, as well as the “preliminary analysis” that motivated the decision. Moreover providing data showing that the dewatering-rewetting treatment had no effect on the control site will confirm the choice of not using it in the analyses.

Results, p. 9610, l. 16: The notation “native to distinct habitat” that refers to what I called functional groups is confusing since it may be understood as a biogeographical concept.

Discussion, p. 9611, l. 13-14: I do not think it is fair to interpret the result in contradiction to the test result based on a small difference. The authors interpret dewatering as a pulse-type disturbance that provokes an increase in community dispersion, although no data clearly support this statement.

Technical corrections:

Methods, p.9603, l. 23-24: Typing (already noticed by the authors).

Discussion, p. 9613, l. 23: Typing: “aquatic biotic”.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 9599, 2010.