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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.

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We thank "Anonymous Referee 1" for his or her review of our manuscript. Several critical points are raised in this review, which we will address here roughly in the order in which they were presented.

Methods
The reviewer is largely correct in his or her assessment of the experiment; namely that it occurred once, over one month, and that water quality and macroinvertebrates were collected as "spot samples" (we refer to these in the text as "sites.") He or she is

incorrect in referring to the macroinvertebrate sampling as "benthic kick samples." In fact, as noted in the text, sampling was done with a handheld net and encompassed the benthos, water column, bank edges, and even terrestrial habitat within reach of the net. This was in line with the state water quality bioassessment protocols we emulated (NCDWQ 2006, cited in the manuscript References). Cross sections were also measured once, as the channel banks were not actively adjusting (pers. observation). We devote substantial effort (1/4 of the text body) to the discussion of our methodology, and it is unclear to us what additional detail in this context is being requested by the reviewer. As for the criticism of the experimental design and timing (what we assume the reviewer refers to as "effort"), we are forthright in the Discussion and Conclusions as to the limitations of this study. It is our opinion that the patterns described in this manuscript (especially the dispersion and recovery patterns) are interesting and novel enough to warrant publication in spite of these limitations. We regret that the reviewer apparently feels differently.

NMS
The reviewer mentions that, in his or her opinion, the results section is lacking in detail, as it is mostly based on NMS analyses. We agree the results are largely NMS-based, but disagree that this is in some way inadequate. NMS is a highly defensible method in community ecology. To quote McCune Grace from their textbook, which is the standard in ecological community analysis (2002, full citation available in the manuscript References), "Nonmetric multidimensional scaling is the most generally effective ordination method for ecological community data and should be the method of choice, unless a specific analytical goal demands another method." Again, it is unclear based on the review what additional information the reviewer would find more acceptable.

pH and water quality
We state, and the reviewer notes, that pH ranged "from neutral to very acidic." The reviewer felt this miscategorized the data. We disagree. As shown in Table 1, pH values ranged from 3.8 to 6.89. Although 6.89 is not truly neutral (7.0), we feel our

characterization of the pH values was not flippant. We fully agree with the reviewer's statement that pH values less than 4 generally affect stream biota adversely. That is why we include the data in the manuscript, fit it to the ordination results, and display it (along with other major water quality parameters) prominently in Tables 1 and 2 and (where they were significant) in Figs. 2 and 4. However, as Table 2 and the biplots in the Figures show, those water quality parameters (particularly pH and ORP) were overwhelmingly related to the minor ordination axis (Axis 2), which accounted for only 18% of the variation in the community ordination (in comparison to 64% for Axis 1, as discussed in the Fig. 2 legend). This is why commentary on the pH values was largely left out of the Discussion, which was focused instead on habitat parameters that lined up well with most of the variation in the community data. We recognize how this omission can lead to confusion, and will correct it in future manuscript versions.

PERMDISP

Also in the Results section, the reviewer mentions that PERMDISP dispersion values are provided without any significance tests to elucidate whether the values differ from one another. This is incorrect. As noted repeatedly in the section, Table 4 provides such significance test results. However, Table 4 only provides t and p-values (and, to avoid redundancy, does not list the actual dispersion values, which are listed in the text), so we can understand how this has led to confusion. This will be cleared up in subsequent manuscript versions.

Dewatering gradient

At several points during the review the reviewer suggests an impression that the dewatering gradient was either insignificant or otherwise deserving of less importance than we give it in this paper. We strongly disagree with this assertion. As indicated by the percentages shown next to each site name in Figure 1, a change in wetted perimeter and depth of 85-95% occurred during the dewatering at the most extreme end of the gradient (our "extreme" site), and the dewatering effect in terms of these two metrics decreased relatively monotonically to the least-impacted end of the gradient (the "very

small" site). It is unclear to us what further justification of a dewatering gradient is necessary than the reported percentages of these metrics, which directly depict the relative presence or absence of water. If additional reviewers have suggestions about ways to state this point more clearly, we would of course be happy to incorporate them.

Study rigor

Finally, the reviewer criticizes an overall lack of rigor in the study, which we attribute from his or her comments to be directed towards assertions that 1) this study only looked at macroinvertebrates in terms of biota and 2) habitat conditions are underdescribed. In terms of biota this study does, quite explicitly, only consider macroinvertebrate response, which can clearly be seen from the title so as to deter readers who have no interest in reading about macroinvertebrates. It is common for restoration practitioners to look at macroinvertebrate response as one of the few or the only indicators of "ecosystem health," so using these organisms in this study seems highly salient. We do not understand how such an approach lacks rigor, or what additional biotic responses the reviewer would have liked us to consider.

The decision to focus on habitat in terms of wetted perimeter and depth are also discussed in detail in the Introduction, Methods, and Discussion. Bed and bank conditions were functionally homogeneous throughout the study reach and in-stream refugia such as logs were absent, resulting from the site's recent agricultural history. This, in our opinion, is one of the aspects of this study that make it unique. As we state in the manuscript, we believe the ability to explicitly consider community response only in the presence of hydrologic habitat components (wetted perimeter and depth) and in the absence of commonly cited geomorphic controls on communities (bank refugia, pools, LWD) is one of the more interesting components of this study. How this approach lacks rigor is, to us, unclear.

Again, we thank the reviewer for his or her review and for bringing some points of correction or clarification to our attention. We hope this response assuages or removes the reviewer's major sources of criticism or confusion. We invite additional reviews and look forward to incorporating these and subsequent comments into the manuscript.

Post script: Related revision

Responding to this review made us notice a typographical error in our manuscript. The first 2 sentences of section 2.1 (page 9603) were jumbled. They should read: “This study was conducted at the Timberlake mitigation site, located near the Albemarle Sound estuary on the outer coastal plain of North Carolina (Fig. 1). Timberlake is a 1000 ha former corn/soybean field, and has been a site of riverine/wetland restoration and mitigation activity.” We hope this revision will clarify this section for future readers, and we regret the error.

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