Biogeosciences Discuss., 5, S597–S603, 2008
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Biogeosciences Discussions



# *Interactive comment on* "Surface flow types, near-bed hydraulics and the distribution ofstream macroinvertebrates" *by* M. A. Reid and M. C. Thoms

**Anonymous Referee #3**

Received and published: 19 May 2008

Overall, this is an interesting contribution to the literature on near-bed flow regimes and macroinvertebrate responses to them. The manuscript is well-written and concise. I appreciated the use of the PRIMER-based multivariate analyses as a complement to the (overused) CANOCO analyses which are so popular with some ecologists. The two main issues I have with the ms in its current form are:

1. There needs to be some critical appraisal of the likely generality of these findings to other rivers
2. Some more information needs to be presented to allow readers to fully under-

stand the inter-relationships between the flow variables and the taxa that are responsible for the *significant* patterns between the flow types.

The other major issue I have concerns the treatment of variables in the multiple regression analyses. I treat each of these issues below.

## 1 Appraisal of the generality of these findings

By directly characterising near-bed velocity and turbulence conditions, and relating them to readily observable flow conditions, the authors have made a substantial advance over the earlier proposals of Davis and co-workers (Davis Barmuta, 1989; Davis Growns, 1991; Robson, Chester Davis, 1999) and Young (1992). I was surprised, therefore, to find that (some of) these contributions were neither cited nor discussed in this paper. Consequently, I think the larger implications of this research could be developed more in this paper by these authors.

Several studies are cited on p. 1188 to the effect that substrate characteristics don't explain all of the community patterns, and that near-bed flows could provide something "extra". While I am a convert to that idea, I think the authors probably have sufficient data to move the debate forward, and explain how they would use near-bed flow measures combined with surface flow features to update or improve on Davis Barmuta (1989) and Young (1992) attempts to use substrate and flow depth characteristics to classify near-bed features. I propose this cautiously (after all, I was a co-author with Davis and worked with Young), but I think these authors can improve on what we attempted.

Not everyone will have access to a hand-held acoustic Doppler meter, so the promise of this research is that a version of the Newsome and Newsome classification of surface flow types might be a useful, generally applicable way for other researchers to use. To



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me, it seems that the Newsome and Newsome scheme may split flow types too finely in the high-energy category (USW, CSW  BF, among others), and so macroinvertebrate communities may not discriminate between them (which is what the authors conclude in ll. 22-28, p. 1187). So, in effect, I'm asking the authors to stick their necks out and tell us which flow types are well-separated faunally and on the basis of near-bed flow measures and which are not. At the moment, that level of synthesis is lacking in the discussion: while these conclusions might be deduced from the results and some of the discussion, I'd like to see a bold set of proposals for the rest of us to test in our own streams, and perhaps some guidance about what experimental manipulations might prove most productive to try to untangle the potentially conflicting results the various (but still rare) field-based studies of near-bed flows and benthos.

## 2 Improving the information content

This won't be popular with the editors, I'm sure, because I want to see more data, but here goes.

First up, there are 6 flow variables presented in Table 2 and described in the Methods. I would like to see a scatterplot matrix of these variables so that readers can appreciate how interdependent these are (I'd also take the square root of the variance measures to convert them into standard deviations, i.e. express the variation in the units of the original measures). This display would let readers judge the potential for redundancy in these predictors, as well as give us a feeling for the way these variables co-vary. If it doesn't look too cluttered, using different symbols for each flow type in this scatterplot matrix would be very useful too.

The conclusion that USW, CSW  CF had "...conditions [that] are suboptimal for all taxa" (p. 1187 ll. 27-28) exemplifies my need for more information on the taxonomic bases for the community differences that were observed. The diagnostic information



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for this statement is simply absent in the ms in its current form. In PRIMER-land, SIMPER can be used to provide listings of taxa that are good discriminators between various pair-wise comparisons that are important to the final conclusions that the authors would like to draw. For example, this would mean amalgamating levels of the "flow type" factor to just reflect the significant or important community differences between flow types (e.g. comparing BSW and CF combined v. other "high energy" flow types). I don't think that presenting all such pair-wise comparisons would be warranted, however. The alternative to this would be to replace Figure 3 with a bi-plot type display, where taxa are superimposed on the sample-unit ordination to indicate which samples are characterised by those taxa. Either way, this information would allow readers to appreciate if all taxa did "avoid" the high-energy flow types (i.e. no taxa discriminated for these flows in SIMPER comparisons, or none were correlated with their position in ordination space). Figure 5 might be more compactly displayed as a table (and these are just estimates of marginal means anyway).

These recommendations mean finding more space in the ms. I don't find Figure 1 necessary provided latitudes and longitudes are provided on p. 1179. Unless bi-plots are used, the MDS displays in Figure 3 are not informative. The ANOSIMs provide the significance tests in the text: what use are low-dimensional displays unless they are substantially enhanced with diagnostic information or other aids such as convex hulls which enable readers to discern differences more readily? I spent more time with and got more information from Figure 4: an interesting way to display compactly the relative differences of the flow types on the three sets of criteria examined in this ms.

## 3 Multiple regressions

(p. 1181, ll. 6 8211; 9; Figures 5, 6  7; p. 1184, ll. 13 8211; 21)

While the following may not change any of the authors' qualitative conclusions, I did



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find the standard of reporting of these analyses wanting 8211; especially if I wanted to make useful comparisons with future studies.

First the methods do not make it clear if any of the regressions used transformed versions of the explanatory (independent) variables, and some of the relationships shown in Figure 7 suggest that log-transformations might improve the linearity of some relationships. Such a transformation might make biological sense: animals are likely to respond to proportional changes in many environmental variables rather than straight arithmetic differences. Importantly, we need to be told how the final regression models were derived: stepwise automated procedures? Some sensible, a priori sequential entry or removal of variables? Were any interaction terms tested? (I suspect not, given the size of the data set.) Which explanatory variables had quadratic terms tested? What diagnostics were examined to determine the robustness of the model selection procedure and final model?

I ask these questions because of the following features discernable from the figures, and the lack of information provided in the narrative of the results. First, the smooth curve in Figure 6(b) and the second row of panels in Figure 7 is clearly not a simple quadratic polynomial. Only one regression coefficient (apart from the intercept) is given for Figure 6(b), which implies a straight line. Are all these displays actually "back-transformed" from linear regressions on log-transformed explanatory variables? Second, some of the quadratic trends in these figures seem to be driven by a small number of data points, and the turbulence panels in Figure 7 show two values with very high leverage. I'm sure that leverage and influence diagnostics would highlight these features. I'd also be interested to know why the authors found no turbulence readings between ca. 750 and 1500. Is there any chance that these points result from some problematic ADV readings? Third, I realise that Figure 7 presents only *marginal* scatterplots, but then the narrative does not tell us clearly whether these were the only variables remaining in the model after some selection procedure, or whether the models were in fact more complex – in which case tabulating regression coefficients and



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standard errors would be more informative about how dependent variables responded in a "partial" fashion to the explanatory variables that were retained. Finally, in Figure 6a, the caption does not tell us what the parameter names mean; I presume "b1" is the linear term and "b2" is the quadratic.

## 4 Minor issues

p. 1180: please state mesh size of Surber sample net. What is the effective mesh size of the live-sorting method adopted? Was the remainder of the sample preserved after live-picking so that the total abundance could be determined in the lab for subsequent computation of the proportion picked?

p. 1180, l. 28 Missing date of Nikora and Goring reference; anything else missing from this sentence?

p. 1183 Figure 2: what does a negative value of Vy signify? I presume negative Vx means "upstream", negative Vz means "towards the centre of the earth". Does the slight tendency for BSW to have negative values of Vy mean anything?

p. 1190. ll. 7 ff. may have some impact on torque perceived by an animal. Anyway, torque, drag and lift on an animal are probably combinations of the three components, and would there be reason for them to be linear combinations as implied by the analytical methods used here?

## 5 References cited

Davis, J.A. Barmuta, L.A. (1989) An ecologically useful classification of mean and near-bed flows in streams and rivers. Freshwater Biology, 21, 271-282.

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Robson, B.J., Chester, E.T., Davis, J.A. (1999) Manipulating the intensity of near-bed turbulence in rivers: effects on benthic invertebrates. Freshwater Biology, 42, 645-653.

Young, W.J. (1992) Clarification of the criteria used to identify near-bed flow regimes. Freshwater Biology, 28, 383-391.

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Interactive comment on Biogeosciences Discuss., 5, 1175, 2008.