Hydrol. Earth Syst. Sci. Discuss., 11, C164–C167, 2014
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Hydrology and Earth System Sciences
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HESSD
11, C164–C167, 2014

Interactive Comment

Interactive comment on "Dissolved and particulate nutrient transport dynamics of a small Irish catchment: the River Owenabue" *by* S. T. Harrington and J. R. Harrington

Anonymous Referee #1

Received and published: 24 February 2014

General comments:

Harrington & Harrington provide a classical evaluation of hydrochemical time series with respect to nitrogen and phosphorus species being mobilized as result of event-based and seasonal processes. The manuscript is concise and well structured. The used approach does not provide much novelty, but the presented findings add some interesting aspects concerning the importance of different N and P species, especially the particulate ones, in relation to the prevailing hydrologic conditions. One concern is that the sampling interval of 1 week may not be sufficient to capture the considerable temporal variability, in particular of P species, and to calculate annual loads

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and species balances. Two (of many) events were sampled at some higher resolution. The authors conclude from the data that flood events drive annual loads of P. As a consequence of their findings, they should more thoroughly discuss the representativeness of weekly samples and the resulting uncertainties. Furthermore, a more thorough consideration of the hydrological conditions during sampling could provide some more information about the processes responsible for P and N mobilization.

Specific comments:

p. 110, l. 24: Reduction of temperature, productivity and the mass of benthic communities may result from increased sediment loads but you would expect the opposite as consequence of high nutrient concentrations.

p. 114, l. 5: “Peak discharge” needs some statistical classification: Is that the highest discharge ever measured since 1956, the mean annual peak flow, . . .?

p. 115, l. 10: dt is not the whole interval of integration (t2-t1) but a time increment of (in the ideal case) infinitesimal length.

p. 117, l. 15: The minimum proportion of TDN (20%) and maximum proportion of PN (84 %) do not sum up to 100 %. Is that a matter of rounding?

Figure 3: The authors have explained in the introduction that the method of differences to determine particle-associated species may be critical, in particular if adopted for low concentrations. This seems to be the case during some periods shown in Figure 3 when TDN are above TN concentrations. Please comment on these problems and the associated uncertainties.

p. 117, l. 22: “. . .61 % of all samples being dominated by TDP” is unclear: Does that mean, that in 61 % of all samples TDP was higher than PP or that the mean proportion of TDP in all samples was 61 %?

p. 118, l. 2: Are “nutrient parameters” concentrations or loads? In the former case: Why were “nitrogen parameters” (l. 12) calculated using Eq. 2 which yields a mass flux?

p. 119, first paragraph: Are nitrate concentrations given as NO3- or NO3–N? On p. 117 you state maximum concentrations for TDN of 8.3 mg/l, here you report maximum nitrate concentrations of 7 mg/l. If the latter corresponds to 1.6 mg/l NO3–N: Which N species is responsible for the remaining 6.7 mg/l TDN?

p. 120, ll. 19-20: Increases in total concentrations primarily driven by increases in particulate species? This is of course true for P, but for N the increases in PN are, according to Figure 4, rather weak and only slightly influence the TN concentrations. In my opinion, Fig. 4 rather stresses the difference in N and P, the latter being much more strongly transported associated with particles.

Figure 4: It would be helpful to show the whole hydrograph of the event instead of only a couple of discharges at the times of sampling. The characteristics of the event with the steepness of rising and falling limb and the timing of the peak discharge reveal loads of information about hydrological processes that should be further evaluated for the interpretation of N and P mobilization processes.

p. 121, l. 1: The fact that TN is diluted raises the question where this diluting component stems from. The authors answer this question partly in l. 7, using increased P concentrations as tracer for a soil-derived runoff component. This is quite interesting as it could mean that soils were more or less depleted in N during the period of the event. Further, this would contradict the expectation that high nitrate concentrations in winter are due to the flushing of soils. This point could be discussed more deeply here.

p. 121, l. 8: The authors state that TP loads are determined by infrequent events, which is reasonable. It raises, as already mentioned in the general comments, the question to which extent it is possible to derive annual loads and balances for individual species from weekly samples (as done in the following paragraph). As part of the discussion a table showing the range of discharges encountered during sampling would be valuable.

p. 122, ll. 1-9: These comparisons with other catchments are quite interesting, but it is not clear what the purpose of this is. It appears as if the authors try to justify their findings by showing that other scientists found similar results. It is, however, difficult to compare yields of catchments that are characterized by different climates, geology, hydrology, and land management.

Technical corrections:

p. 113, l. 1: plural s in sediments missing

p. 114, l. 18: TRP = total reactive phosphorus?

p. 119. L. 16: This should read "N" parameter, I guess.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 109, 2014.

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