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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Hydrological controls on DOC : nitrate resource stoichiometry in a lowland, agricultural catchment, southern UK" *by* Catherine M. Heppell et al.

Anonymous Referee #2

Received and published: 15 May 2017

General comments

This is an interesting and well-written paper that will be of interest for many in the hydrological community. The research questions investigated are relevant for managers of lowland agricultural catchments and the focus on DOC:Nitrate ratios is novel. The overall quality of the research is high, although occasionally there is a tendency to drift towards speculation (e.g. with reference to the different flowpaths operating in the catchments) without presenting all the necessary data to support these statements.

There are two key drawbacks to the paper at present that require addressing. The first is that nutrient samples were collected at 48 h intervals, which means that many short-term storm events may potentially have been missed and thus nutrient loads

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underestimated. As no discharge time series are presented, it is not possible to say whether this is the case. The authors should recognise this explicitly in the text.

Secondly, the authors discuss at length how changes in C:N stoichiometry may limit the potential for in-stream uptake of inorganic nitrogen, but give far less attention to the equally important role of hydrological residence times in influencing these processes (e.g. Zarnetske et al, 2012, WRR). A better discussion of how these respective reaction and transportation factors interact to influence reach-scale nutrient transformation rates may be warranted here, particularly given the apparent importance of high discharge (and therefore velocity) periods for nutrient mobilisation and export.

Specific comments

L25: Climate change is undoubtedly important, but is not the only potential driver here. Land use change and management practices will also influence DOC and N dynamics. L39: 'Storm events' is perhaps somewhat misplaced in this context. Many readers will interpret this phrase as meaning short-term (hours) intense rainfall events that result in rapid changes in streamflow and biogeochemical dynamics. Yet the frequency of sampling in this study (48 h) is not sufficient to capture this variability. I suggest recasting this sentence (and similar others throughout the text) to clarify that 'storm events' relates more generally to the wetter conditions experienced during autumn and winter months. L92: I agree with the authors that more research into DOC dynamics in lowland agricultural streams is important. The Introduction as a whole is rather long and could be shortened considerably. As it stands, the key arguments do not stand out clearly. L127 and 139: It would be useful to state in this paragraph that baseflow index indicates the groundwater contribution to streamflow. Also, the authors should provide more justification for their prediction regarding the link between BFI and NO3. L141: This sentence seems repetitive of the start of the previous paragraph. L176: The start of Objective 3 seems repetitive of Objective 1. L225: Was the Manta 2 cleaned at any point during the study period? If so, at what frequency? Did this affect the results and if so, was a correction applied? L228: Can the authors confirm no sample

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degradation occurred in the time between collection and analysis? Two weeks is a long time for samples to sit in an unpreserved state. L303: When comparing 48 hr nutrient samples with 15 min Q values, were instantaneous Q values at time of nutrient sampling used? Or were these integrated over longer time period? L381 and 465: Care is needed here to avoid placing text in the Results that would be better suited to the Discussion. L445: Are these trends descriptive only or can they be quantified? L462: A discharge time series would be nice to prove that 'autumn storms of intermediate discharge' are really storm events (as mentioned earlier) and not just seasonal shifts in baseflow. Throughout the Results section there are references to different years (e.g. L462) but this is not evident in the figures. L556: Some discussion of other land use types in the catchment and their potential influence on DOC would be useful here. Also, what potential is there for instream production? L612: Given that the discussion focuses heavily on flow pathways within the catchment, it would be helpful to show the rapid response of EC to rainfall events to support this statement. L664: This text could be expanded a little to place the results of this study in a wider context and make comparisons with previous research in this field.

Technical corrections

L127: Not just the UK. L135: Provide indicative range. L145: Define meaning of letters in equation. L154-160: Suggest splitting this very long sentence. L171: Clarify whether three or six sub-catchments are involved in the study. L195: Ref to support this? L215: Provide number of points and R2 value for stage-Q relationship. L266: How often were samples retrieved? L281: Provide precision and LOD information for autoanalyser and TOC-L. L291: Check reference date. L327: Provide indicative number of samples for those included in the analysis. L368: State type of correlation analysis used (Pearson or Spearman) L510: Does "the data" refer to EC-Q relationships? L571: Need to clarify here that the absolute concentration will change but the flow-weighted concentration won't (see Basu et al 2010 GRL) L573: By whom? Citation needed. Fig 1: Sites AS and GN seem in the same place. Also, can differences in baseflow indices be indicated

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