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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Quantifying the nutrient flux within a lowland karstic catchment" *by* T. McCormack et al.

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GENERAL COMMENTS

The manuscript presents an interesting combined monitoring-modelling approach to investigating nitrogen (N) and phosphorus (P) fluxes through a Karstic system of ephemeral lakes (turloughs) in Ireland. Three years of monthly surface and groundwater monitoring data does provide an insight into the temporal and spatial variability in hydrochemical flowpathways, and nutrient storage capacities of these systems. In modelling nutrients, the authors treat N and P as conservative tracers, suggesting that a mismatch between modelled and observed data could indicate "non-conservative behaviour", i.e. process interactions.

Whilst the observational data is interesting, and the modelling approach unique, I would like to see additional detail on a) model structure, b) model calibration procedure b) model accuracy. As there is no indication even of hydrological model performance, in its present form one could not ascertain how much of any difference between observed and modelled loads is due to modelling error, and how much is attributable to "non-conservative behaviour", e.g denitrification or biological uptake. It is also somewhat unclear as to where any direct comparisons are made between observed and modelled behaviours. On first examination it seemed as though this might be achieved on page 112 through the use of a mass balance, however the authors mention on line 12 of that page that the mass balance is "calculated using the aid of the hydraulic model". In general it could be made clearer as to when the authors are delivering observed, and when they are presenting modelled data.

Finally, many of these results have previously been presented in McCormack et al (2014), including some of the graphs (Figure 8 of this manuscript is Figure 13 in the 2014 paper). The differences in alkalinity between turloughs, the hydrological monitoring and modelling, and even some of the nutrient data have been published in the Journal of Hydrology. The manuscript would benefit greatly from focusing on the new data and findings, and referencing that which has been previously published. As a whole, there is some very interesting data presented on potential volume-dependent nutrient storage capacity of ephemeral lakes (predominantly presented on page 115 and 116). With some clarification in key areas, this could be a valuable manuscript.

SPECIFIC COMMENTS

1) The model's performance needs to be assessed. There is no evidence provided in this manuscript that Infoworks CS Wallingford, as applied to these ephemeral lakes, successfully represents hydrological or chemical behaviours. Previous applications (Gill et al, 2013; McCormack et al, 2014) have quantified hydrological success of the model, but the ability of this model to represent systems varies dramatically with individual rainfall events, and its performance must be analysed with every application

(Artina et al, 2007). Thus, a direct comparison between model and observed data is required (R2, MAE, NS). Whilst I appreciate that, given the conservative treatment of N and P, a direct comparison with water quality would not serve as an assessment of model calibration performance, a hydrological analysis is at least necessary.

It would be helpful if the authors clarified that two different hydrological calibrations of Infoworks have previously been applied to these systems, first by Gill et al (2013) who quantified model performance over 2007-2009, and subsequently by McCormack (2014), where model performance was assessed over 2010-2013. It is unclear which version of these hydrological models has been used here, or if it has been re-calibrated. Please could we have some indication as to which it is. In addition, whilst a few equations are provided in this paper, a comprehensive summary of how the model works is missing. Some form of conceptual figure would be helpful here. The example given in Gill et al (2013) is excellent, and some variation on this would be nice. Readers of this manuscript will not necessarily have read all of the authors' past papers, and it is necessary to give sufficient information for the research to stand alone. For instance, the hydrological component of Infoworks is substantially more sophisticated than it is given credit for in this manuscript.

2) A hypothetical scenario, of injecting high nutrient concentrations into one of the rivers, is modelled through the system. The justification given for this modelling exercise is that it gives "a useful conceptual distinction between how flow-through and surcharge tank turloughs should behave". Unfortunately, without the aforementioned provision of model performance statistics, and further information on calibration procedure, the readers cannot determine whether these models are capable of simulating turlough behaviours. Somewhat confusingly, the authors do make a visual comparison between the hypothetical nutrient plumes (model scenarios) and observed data, even though as they correctly indicate in the manuscript, there is no reason for modelled nutrient behaviours of a hypothetical large plume injection to match observed data.

3) There is a general tendency to introduce new data and results within the discussion.

For example, some of the most interesting findings on nutrient storage capacities of ephemeral lakes by volume, and calculations of denitrification rates – are presented in the discussion on pages 115-116, as are results from the mass balance results (page 112). Please introduce this information in the results section.

4) Page 112, lines 7-21: This is very confusing. The abstract and conclusion suggest that this mass balance should have been calculated using observed data, for comparison with modelled estimates of nutrient loads. In accordance with this, the conclusion states that "as a result of loss of N within turloughs, the gain in nutrient loading observed. . .was found to be lower than expected. . .an increase of 36% rather than 85% as predicted by the model".

This appears contrary to the description in the results section, however, where it is written (page 112, line 11) that this balance is calculated using the hydraulic model, and that the "36%" represents a (modelled) increase in TN loads within the Kinvarna spring, as compared to (modelled) input concentrations of the river. The "86%" as described on page 110, line 15, appears to represent the proportion of TN within the spring that originates from diffuse sources (again modelled). Therefore the description in the results suggests something quite different from that in the abstract and conclusion, i.e. that 85% of the 36% increase is due to diffuse groundwater influx, all of which is modelled, and wouldn't infer any added information about upstream processes. I absolutely agree with the use of observed data in generating a mass balance to compare with the model outputs; but to highlight the purpose of using Infoworks, and the general value of manuscript conclusions, the methods and results really do need clarifying.

It is also important to use all inputs within the observed mass balance. From the description, the authors make it sound as if they are using only river loads as inputs (page 112). Inclusion of groundwater, atmospheric deposition, and direct-lake additions (e.g. cattle manure and the abattoir mentioned on page 106) as inputs would also be necessary. Perhaps spring outputs are 36% higher than inputs because several nutrient sources were excluded from the equation. If groundwater constitutes 85% of inputs to

the spring, its inclusion within the input of a mass balance would immediately highlight your losses within the system, in a direct and much more simple quantification.

In summary, I am somewhat perplexed as to what added value the inclusion of nutrients as conservative tracers in Infoworks CSmodel brings to this study. N and P are not conservative, and I find it unlikely that either would pass through an entire system, karstic or otherwise, without interacting in some way with their environment (Kilroy and Coxon, 2005). It would therefore be helpful if the authors could highlight the added value of their "conservative-model minus observed data" approach, as compared to a more direct and simplistic all-inclusive mass balance approach (measured inputs-measured outputs).

TECHNICAL COMMENTS

In general the manuscript was well written with no obvious typing errors that I could see. The structure of the results and discussion could be improved(results are presented in the discussion and vice versa). Both are split into an unnecessary number of subheadings, which perhaps would benefit from being consolidated into "observed", and "modelled" data, and subdivided into "hydrology" and "water quality". This clear partitioning between the different types of data might facilitate understanding.

Abstract:

1) It is not inherently clear from the abstract what the purpose of the research is. Some suggestion as to why the findings are important e.g. "results suggest that ephemeral lakes are important nutrient stores" etc might highlight the value of the research. Previous work by these authors (Gill , 2013; McCormack,2014) did make these links, and it would be good to remind readers why this avenue of research is being pursued.

Introduction and area description:

1) Could you include the definition of turloughs, and their ecological significance in the introduction rather than waiting until the area description.

2) Page 96, line 18: "chemically aggressive" – I'm not keen on personification of chemical reactions. Perhaps replace with corrosive or something similar?

3) Page 97, line 2: "these inherent unpredictable flooding patterns" – presumably not unpredictable, as they are modelled in the manuscript. How about "variable" or "sporadic flood events"....

4) Page 98: Could you quote some previous research/references to demonstrate why we begin this study believing that Coy, Garryland and Caherglassaun are "surcharge tank systems"? These assumptions are maintained throughout the manuscript, so justifications are necessary.

5) Page 98, line 18-20: "The Gort Lowlands catchment has been modelled successfully using Infoworks CS, a hydraulic modelling package..." please indicate what is meant by successful (model performance statistics). Perhaps here would be a good opportunity to introduce the two different hydrological models that have been used in this catchment, over various time periods. Please be clear that the 2010-2013 hydrological model of these turloughs has previously been published, with the alkalinity data, and that this part is not novel. Unless of course this is a new calibration, with new data. In which case – can you demonstrate that please.

Methods

1) The modelling needs much more detail. It may have been discussed in Gill et al (2013), but its application here still needs to be justified. There is no indication here as to whether this model is suitable for use in these systems. Performance, calibration data, parameter information. All are missing.

2) Page 100 (and 103): This alkalinity data has previously been published. Please quote your papers.

Results

1) Pages 102-103: The authors should consider re-writing the alkalinity section. This

data is already presented in McCormack, 2014 (though is not referenced here). In this manuscript, the differences between the turloughs are presented in a very confusing manner. This is not helped by the poor quality of Figure 3, which would benefit greatly from topographical data, or, preferably some indication of subcatchment/watershed boundaries. In summary I gathered from the information presented that:

a) Coole and Blackrock have inflows from surface-streams b) Coy, Caherglassaun and Garryland are isolated from surface-flow, and are instead fed by sub-surface surcharge tank systems. c) The direct impact of the river is supported by comparing turlough stage and alkalinity. Alkalinity in Coole and Blackrock responds rapidly to changes in stage, whereas alkalinity in surcharge-tank fed turloughs respond more slowly. Here low alkalinity water from rivers slowly becomes enriched in bicarbonate from groundwater recharge. d) The Coy (surcharge tank-fed) and the Blackrock (river fed) turloughs are situated closest to the Owenshree river. This river has the highest alkalinity, and accordingly so too do these turloughs (slightly). e) The other three turloughs, despite being situated closer to rivers with much lower alkalinity, are not significantly different in pH to the upper turloughs. This is attributed to influx of high alkalinity from the Conteen catchment to the south, dissolution of limestone, and groundwater additions.

This information is really not that clear from the manuscript in its present form, please could you re-write.

2) In general, please select either mg/l or ug/l for N and P, and please be consistent. Comparing “large” variations in ug/l with “small” variations in mg/l is not really valid. How significant are the differences (when using the same units?).

3) Page 106, line 8: “which suggest that the turloughs act as sources rather than sinks of nutrients”. This is unfortunate terminology; really the data suggests additions directly to the lake from alternative sources, and/or nutrient retention by lake sediments/biota. A lake is rarely a “source” of nutrients. The authors correctly go on to suggest multiple reasons for the higher lake concentrations (diffuse and direct nutrient inputs, internal

loading – atmospheric deposition would be another one). In most instances, the nutrients did not originate from within the lake, and it would not be considered the source.

Discussion

1) The use of observed data (I think) to calculate denitrification rates is excellent. Pages 115 line 10 to page 116, line 19 are highly interesting. The information provided on nutrient storage capacity by lake volume is valuable, and perhaps the authors might want to focus more on this data.

Figures and Tables

Table 1: please choose either ug/l or mg/l for nutrient concentrations and be consistent.

Figure 1: the colours are very confusing. The turloughs are the same colour as both "pure-bedded limestone" and some of the volcanics. Could this be improved please.

Figure 3: the addition of some defining features (topography, roads, landmarks etc) would be very helpful here. Specifically, the addition of watershed/subcatchment boundaries. The authors talk of "upper" and "lower" turloughs, but this is difficult to envisage, having no idea of site elevation.

Figures 4-11: all look like they have been copied and pasted directly from an excel file. Some formatting here would be nice. A darker axis (black instead of gray), removal of grid lines, removal of "shading" on the legends.

Figure 8: this has already been published (McCormack et al., 2014). Please reference your previous work.

REFERENCES

Artina, S., Bolognesi, A., Liserra, T., and Maglionico, M. 2007. Simulation of a storm sewer network in industrial areas: comparison between models calibration through experimental data. Environmental Modelling and Software. 22: 1221 - 1228

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