We thank the reviewers for their insightful and constructive comments and provide responses to the comments below. The reviewer comments are in black text and our responses are in blue text.

**Referee 1.**

* Alterations in revised manuscript based on Reviewer 1’s comments are highlighted in Yellow

**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.

Text has been added into Section 2 to further describe the model and it’s calibration (specifically the calibration used for the model used in this paper). Some R2 values from the previous papers have also been added to the text.

* Figure 3 has also been modified to provide more information on the model.

In terms of individual rainfall events and their effect on the model, the nature of the Gort Lowland system, as with most other turlough systems, is that long term rainfall patterns and their cumulative effect are of far more importance than individual rainfall events. So as pointed out by the Reviewer, the Infoworks modelling software’s ability to represent a system does vary with individual rainfall events, but in this particular model, these variances and fast fluctuations are damped out in the turlough/tank system over the period in which they flood (typically weeks).

* The sensitivity of the turloughs to rainfall patterns of different durations was investigated as part of my PhD (*Ted McCormack, 2014, Quantifying Nutrient Dynamics through a Lowland Karst Network*). In a sub-study, it was found that the modelled turloughs showed very little alteration in flooding pattern when rainfall was changed from hourly data to daily averages. For Coole and Caherglassaun turloughs, even using 5-day hourly averages caused little alteration in flooding pattern.
* Some text has been added at the end of Section 2 to reflect this issue.

1. 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.

As referred to in the previous response, the calibration statistics have now been added in the text (in Section 2)

It is unclear what the Reviewer means with the comment ‘the authors make a visual comparison between hypothetical plumes and observed data’. Figures 9 and 10 (which it is assumed the comment is about?) are not displaying any observed data. The plots display concentration, load and volume, all of which was derived from the model. The purpose of these plots is not to prove a correspondence between observed and modelled nutrient concentrations at a particular period. Instead, the purpose is to illustrate how the model predicts conservative pollutants/nutrients to act in a hydrologically calibrated system, and how the different types of modelled turloughs (surcharge/flow-through) react to the pollutants. Then these findings are discussed in the context of the observed data (end of Section 4.3.1), but are directly compared, as the model does not account for any complex gain/loss transformation processes occurring in reality.

* 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.

The nutrient mass balance calculation has been moved to the Results section. The authors consider however, that the section on denitrification rates is difficult to place in the Results section as denitrification is not discussed as a process until the Discussion section. It would seem strange to discuss it briefly in the Results and then not mention it again for a number of pages. Also, the denitrification calculation is brought up in the Discussion as a means to validate the discussion and the idea that nutrient loss could be due to denitrification

1. 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).

The model allows for the estimation of a nutrient loading from autogenic sources. Without the model, concentration of autogenic groundwater could be known but the quantity of this recharge entering the system would not. Thus the model allows for a hydrologically calibrated spatial and temporal estimate of nutrient flux into the karst conduit network. This key ability of the model is mentioned/emphasised in the abstract and discussed further in Section 4.3.2

The concept of the nutrient mass balance section is to use as much of the collected observed data which can be used without the aid of the model\* to make an input-output comparison. Then the model, and its ability to estimate autogenic recharge, is used to make another input-output comparison. The difference between these input-output comparisons is an indication of non-conservative processes or direct lake additions.

So for the observed input-output comparison, the spring output load is the net result from the contribution of nutrients from allogenic sources, the direct lake additions, and non-conservative processes that have taken place within the system. The model, on the other hand, takes into account the influx of nutrients from autogenic/diffuse sources (based on observed groundwater values and modelled groundwater recharge) to provide a value for output load at Kinvara.

So the input/output offset from the observed results approach indicates how groundwater influx, direct lake additions and non-conservative processes are affecting the output, whereas the input/output offset (as calculated by the model) indicates the change in load due to groundwater influx only. Hence, the different input/output offsets from these two approaches indicates the possible effect of non-conservative processes and direct lake additions on the system.

As explained it Section 4.5.2 it is thought that denitrification can cause significant N- losses in these turloughs and as such, denitrification is thought to be the most important process occurring, but not the only process. Other process causing addition of nutrients are certainly taking place such as atmospheric deposition or direct lake additions but these processes are deemed to have significantly less impact than denitrification.

This section has been edited for clarity and moved to the results section, as you suggest in another comment. The reclarification is much shorter in the manuscript than it is here in an effort to reduce the overall manuscript size.

\*Unfortunately Kinvara discharge cannot be measured directly and so its discharge has been based on previous simulations from the calibrated model (McCormack et al, 2014).

**Technical Comments**

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.

Some additional text has been added to the abstract as recommended.

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.

Text has been altered accordingly.

1. Page 96, line 18: “chemically aggressive” – I’m not keen on personification of chemical reactions. Perhaps replace with corrosive or something similar?

Text has been altered accordingly.

1. 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”..

Text has been altered accordingly.

1. 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.

These assumptions are based previous studies by Gill (2013a, 2013b). A reference to these studies has now been added into the text.

1. 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.

Text has been added in this section to further describe the model and it’s calibration. A schematic of the model has also been added to Figure 3.

**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.

Performance statistics have been added (in response to the Reviewer’s previous points) which are for the same calibration, and modelling period as this paper. These statistics prove the justification of the modelling approach. However, the authors see no merit in going into further detail about the hydrological model (i.e. repeating previous work) as it is all contained in the cited publications and will cause the manuscript to be unnecessarily long. However, if the Editor deems such detail is required we are happy to include an additional section.

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

This data has mostly not been published. In my previous paper I only discuss alkalinity in Caherglassaun, Kinvara and groundwater as a means to better calculate Kinvara discharge. In this paper the alkalinity is used for analysing the turloughs themselves. Nonetheless, a reference in the groundwater section has now been included.

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.

See previous response regarding the past publishing of the alkalinity data.

Regarding the text in the manuscript, the way in which the Reviewer describes it corresponds with what we wrote in the manuscript more or less; hence, the authors are not certain as to where the confusion lies?.

The section is broken into 4 paragraphs. The first gives a brief introduction. The second paragraph deals with the alkalinity values in a catchment context, comparing the mean river values to the mean turlough values and hypothesising as to why Coole, Garryland and Caherglassaun turloughs have higher values then would be expected from the rivers. The third paragraph discusses the different alkalinity behaviours within the turloughs due to their different hydrological functioning and presents these differences in Figure 4. The final paragraph discusses how alkalinity behaves differently in other more autogenically fed turloughs.

That being said, the clarity has now been improved by updating Figure 3, as well as some general text updates.

1. 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?).

Units have been amended to remove any inconsistencies.

1. 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.

Text has been altered accordingly.

**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.

The authors agree with the Reviewer’s points regarding this section and have brought more focus to it in the abstract. Aside from that we would prefer not to add more text as the word count has already increased based on previous comments.

**Figures and Tables**

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

Table has been altered accordingly.

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 has been altered to improve clarity.

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.

The catchment boundary of the Kinvara Springs has been added to Figure 1 and elevation data has been added to Figure 3. Further details (roads, landmarks etc) were not added as the figures would appear over-cluttered.

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

Figures have been improved accordingly.

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

your previous work.

Reference has now been added.