We thank the two anonymous referees for their comments on this paper. We have considered their comments and make the following responses (in blue), which will be incorporated into the final version of the paper.

Referee 2

General Comments

1 *The Sampling frequency of one month in karst terrain:*
Temporal high-resolution monitoring has proofed that in karst systems significant chances in nutrient transport often occurs in a time frame of hours to days (e.g. Lloyd et al., 2016; Mellander et al., 2013; Pu et al., 2011). The authors state that 'Monthly sampling of turloughs was deemed to be adequate to characterize the system as water is typically retained in the turloughs for long periods. However, for the rivers, monthly sampling only offers a snapshot of concentrations at the time of sampling. '(P 10243, line 5). Personally, I do not agree that the assumption 'monthly sampling of turloughs is adequate enough' can be made without any evidence. For example, if you look at the fluctuating stages over time (Fig. 4) (or the changes in the mean volume of the turloughs over time in Fig. 7 – or Fig. 14 in Gill et al., 2013, for the changes of the turlough water levels in the previous years) than it is more than likely that due to monthly sampling intervals a lot of significant concentration changes of the nutrients (due to dilution or mobilisation processes) are missed. The question is how reliable the temporal resolution is regarding the interpretations made in the manuscript. A higher frequency of sampling over time or e.g. the use of passive diffusion bags that are recording mean values of nutrient concentrations would be more reliable.

Response

The authors accept that nutrient transport in karst systems (particularly springs and other conduit-type sampling points) is often much too fast for monthly samples to capture. However, we believe that this is not the case in turloughs, especially the turloughs of the Gort Lowlands. These turloughs have been studied from a water quality perspective by different research groups since 2006. Over that period there have been several targeted studies on turloughs which have sampled at a much higher frequency. Indeed, the early research carried out by Gill (2005 to 2009) into these turloughs specifically evaluated nutrient concentrations fluctuations both temporally and spatially in order to optimise sampling methodologies (contained in Gill's PhD thesis (2010)). From spatial perspective more than 40 samples were taken in each turloughs at a range of locations and depths which showed the turloughs could broadly be considered fully mixed. Equally, samples were taken every 2 weeks from January to April in 2009 which confirmed that nutrient concentration levels only varied gradually. This data will be presented in Supplemental Information, but an example graph for Caherglassaun turlough is shown here in Figure C.

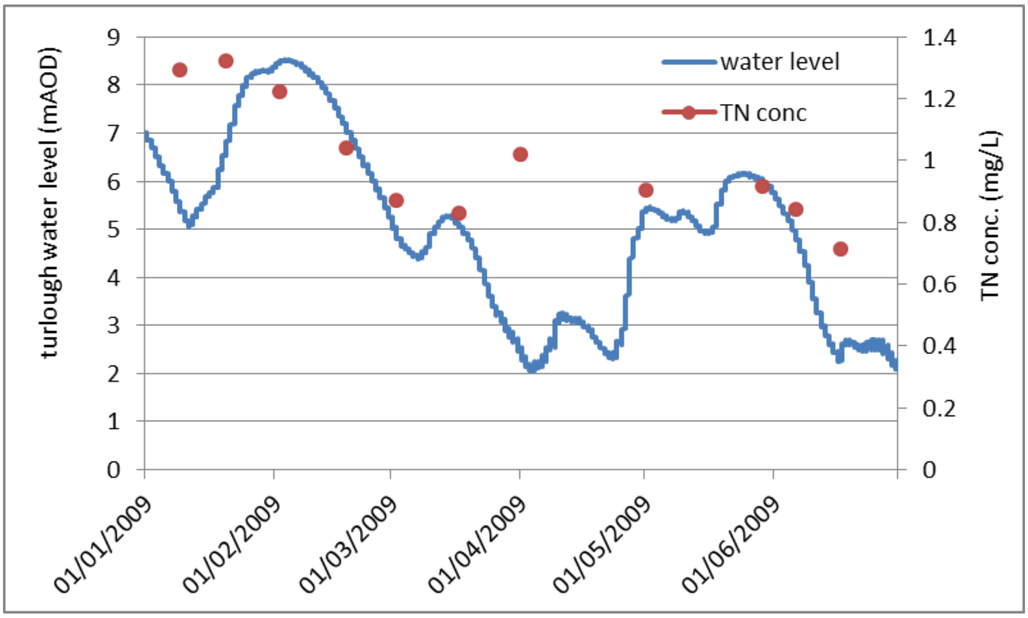

Figure C

Equally, another extensive research project into turloughs entitled *Turlough Hydrology, Ecology and Conservation* (Waldren et al., 2015) used monthly sampling of turloughs to study interrelationships between hydrology, invertebrates, plant communities and algae. This was a multi-disciplinary project carried out by environmental scientists, engineers, zoologists, botanists and ecologists and resulted in several peer reviewed publications a range of ecohydrological issues (e.g. Cunha Pereira et al., 2010, 2011; Kimberley et al., 2012; Porst et al., 2012 etc). Further unpublished data (in Waldren et al., 2015) as part of this project, has revealed similar patterns in 2006/07 in nutrient concentrations during recession (as to the patterns we are presenting in our paper) both for turloughs in the Gort lowland chain as well as for other turloughs outside of this area (particularly surcharge tank-type turloughs), which were studied as part of the extensive research project. An example, for the 2007 N profile in Caherglassaun is given in Figure D.

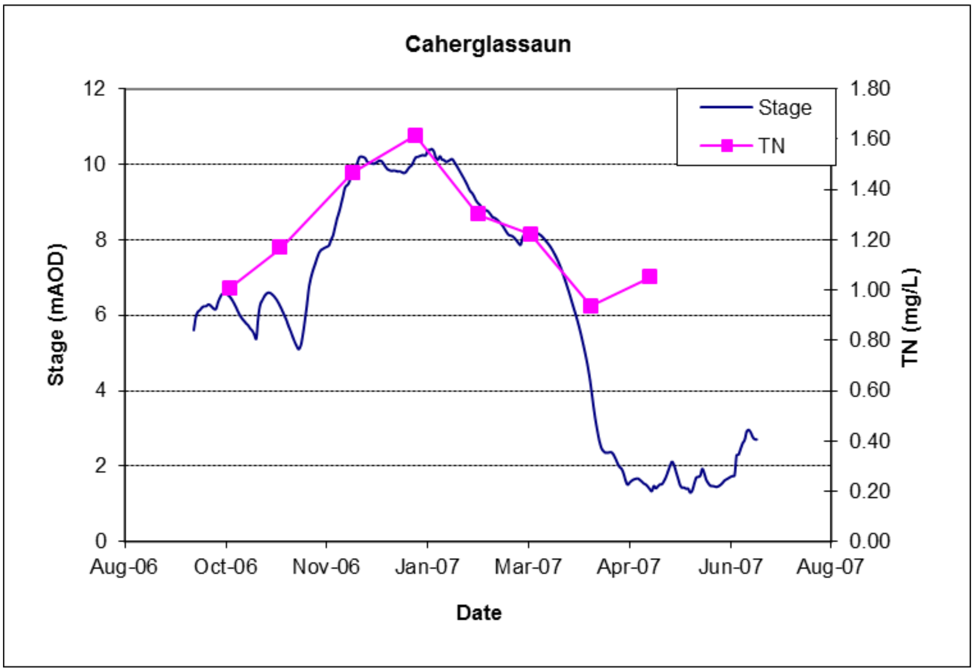

Figure D

While the water level (and nutrient load) in the turlough may change significantly over the flooding season, the nutrient concentrations do not due to the large dilution and dampening effects imparted on the inputs by each turlough (in addition to the cumulative dampening impacts of any upstream turloughs).

The only turlough that does not have any attenuation/dilution before water reaches it is Blackrock turlough. Thus, out of all the turloughs it is likely that the monthly sampling frequency is least robust there. However, this was addressed in the manuscript; for example, it is pointed out that denitrification can't be calculated in Blackrock turlough.

2 *In relation to the sampling frequency the validation of the model:*
In Fig. 9 (and in the manuscript itself) the validation of the model with the field data is missing. It should have been easy to plot the simulation plots together with the real nutrient concentrations. I also doubt that the model input of the river (SA1) in Fig. 8 is realistic in relation to the time chosen. The shape looks familiar as proven in a lot of studies, but the time frame is unusual (e.g. steady increase of concentration for 3pprox.. 1 month) (e.g. compare with Bende-Michl et al., 2013, or Schwientek et al., 2012). In Gill et al., 2013, there are 'final calibrated model results for Owneshree river section' for the area shown which also leads to the interpretation that the concentration input signal should be different. In general, the question remains: Could one sample once a month representative enough for the continuous, modelled curve?

Response

As per the response to Referee 1, observed (normalised) results will now be added to Figure 9. It should be stressed that the model is not validated in the common sense for nutrient data. The model is already hydraulically validated, as shown in previous journal articles (Gill et al., 2013; McCormack et al. 2014). In this study, we are using the model (which assumes conservative nutrient behaviour) to present how conservative nutrients would behave in the turlough system when it is modelled as a network of pipes and tanks. The flow-through turloughs behave as expected by the model, but the fact that the surcharge tank turloughs do not, must therefore indicate other attenuation processes. These processes are then discussed in the Discussion section.

Regarding the model input signal in the river, we accept that it is unrealistic when compared to typical river nutrient signals. The reason for using that signal was to an attempt to incorporate recorded measurements into the model, even though the sampling interval was lacking. However, we acknowledge that this input signal was not realistic and so have changed the signal to a more realistic one (shown in Fig E).

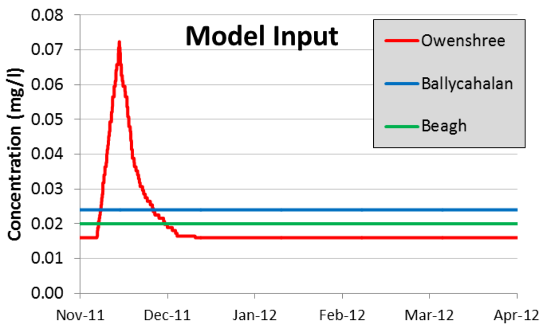

Figure E

This signal has been input into the model and the following results were obtained (Figure F):

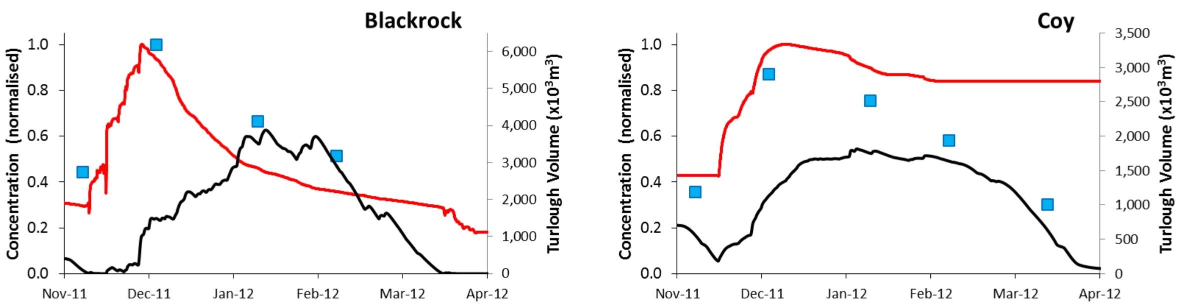

Figure F

3 *The interpretations that were made based on the available dataset*

In general, the author's choice of the graphic depictions of the available dataset makes it complicated to follow the author's interpretations in the text. As example: âˇA ´c Page 10244, line 5-8 (in relation to Fig. 7): 'These spikes could be due to the increased sensitivity of the turloughs to their river inputs during dry periods. During these periods, the turloughs have less capacity to dilute any incoming nutrient plumes and so spikes in nutrient concentrations should be expected'. The interpretation would be more qualitative if the precipitation dataset would be included in Fig.7. âˇA ´c Page 10233, line 22-24: 'The peak in P in July 2012 (Fig. 6) (which was also seen to a lesser extent in the other two rivers) occurs during forestry fertilization season of April- August (Teagasc, 2015) and coincides with a period of heavy rainfall.' It would have been great to have seen the precipitation data for this area plotted in Fig. 6 to be able to verify this thesis with real data.

Response

These plots will be altered to reflect the Referee's valid comments. Precipitation will be added to Figs. 6 & 7.

4 One main interpretations of the study is (page 10222, line 17-22) 'Denitrification during stable flooded periods (typically 3-4 months per year) was deemed to be the main process reducing nitrogen concentrations within the turloughs whereas phosphorus loss it thought to occur mostly via sedimentation and subsequent soil deposition. The results from this study suggest that, in stable conditions, ephemeral lakes can impart considerable nutrient losses on a karst groundwater system.' One example for denitrification is shown in Fig. 10. First of all, it should be recognized that the amplitude of Total N between Point A and B is very low (max. 0.5 mg/l). In addition, there is a sampling interval of one month in a karst catchment (see my previous comment to 'a) the sampling frequency of one month in a karst terrain'). And in general, I miss error bars in the diagram that show the expected accuracy of the method chosen for the analysis of TN. In my opinion, all these facts lead to a high uncertainty of the interpretation.

Response

We acknowledge that the amplitude between points A and B is low, but we are confident in our analysis and quality control methods. Duplicate, and sometimes triplicate samples were recovered and analysed separately. Standard solutions were also tested with each batch to ensure quality control. Samples which did not adequately match the standard or had excessive differences between duplicates were omitted from the dataset (resulting in a number of missing datapoints in the 2011-2012 dataset).

See plot below (Figure G) for 'Figure 10' with error bars (standard deviation) added. This will be added to the manuscript.

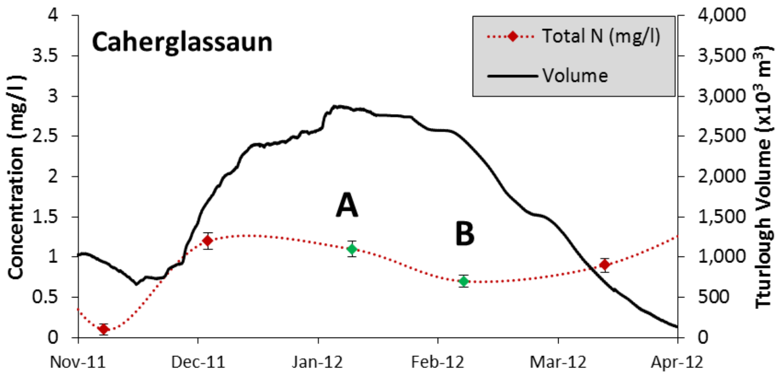

Figure G

We have answered the query regarding the sampling frequency in comment 1.

We must stress that we are not intending to give an exact denitrification rate from the turloughs. Instead, we are trying to prove that it is the most likely cause of N loss. All possible mechanisms were discussed and it was deemed to be most likely to be denitrification. This was then backed up by Fig 10 which showed N loss to be within the range of expected N loss from denitrification in lakes.

Thus we postulate that denitrification is likely to be the predominant (but not only) cause of N loss in the lakes.

Specific Remarks

The specific remarks will be addressed in the updated manuscript.

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