Response to Reviewer 2

Thanks for the detailed comments. We are providing the response to each comment.

I think that this logical scheme must be better explained. These are some considerations of mine for the authors: (a) Point 1) is at the base of all the work: if there is no correlation between precipitation and simulated loadings, it is senseless to develop a prediction model based on precipitation forecasts. I think that this point must be emphasized.

Figure 2 shows this point – correlation between observed precipitation and simulated nutrients is statistically significant over all the stations.

(b) Observed precipitation is not used in the two models as predictors. Thus why are relevant grid points chosen passing by correlation between observed and forecasted precipitation? Why don't you choose relevant grid points by selecting the grid points with PC1 most correlated with TN loadings? In this way there is no need to take into account the errors of precipitation predictions, because the calibration is done directly on forecasts.

Given that observed precipitation and simulated nutrients are significantly correlated (Figure 2), it is natural to expect the same set of grid points will be significantly correlate if we were to select it based on the correlation between the forecasted precipitation and simulated TN loadings. We tried this earlier itself, the selected grid points remains the same.

Adding more PCs resulted in improvements in the adjusted R^2 of the stepwise regression. Most of the sites required only 1-3 components (Table 5) with the exception being very few sites (8, 10, 11). Of those three, only station 10 shows significant correlation in predicting the nutrients.

(c) Why do you check correlation between PC1 and stream flow if stream flow is poorly considered in the discussion?

It only shows that we are establishing climatic information is perceived across all basin-level hydroclimatic attributes. For instance, if we see PC1 is significantly correlated with precipitation and loadings, but not with streamflow means that our developed statistical relationship is not making physical sense. Though we don't see this over all the 18 stations in connecting climate to nutrients, we have seen such discrepancies upon linking climate with groundwater (Almanaseer and Sankarasubramanian, 2012). http://ascelibrary.org/heo/resource/3/jhyexx/361?isAuthorized=no

(d) I think that you should spend few words in section 2.4 in defining PCs components of ECHAM4.5, and define better what are the low-dimensional components of precipitation forecasts? The reader who does not know ECHAM4.5 must know what are PCs component without going to the references, because they are the primary information for the article comprehension.

We can add the following in section 2.4.

Principal components basically signify the reduced dimensions of the gridded precipitation forecasts. Given that the precipitation forecasts from the selected grid points are spatially correlated, it is better to obtain low/reduced dimensions of the forecasts using principal component analysis.

(e) The sentence "To summarize, Fig. 2 and Table 3 provide the scope for using the low dimensional components for using : : :. "(line 10/13 page 10943) should be better explained, such as "Since variance explained by: : : is high, and correlation between: : : is high, as possible to see in Fig. 2 and Table 3, building predictive models with low dimensional components of climate forecasts can be a powerful tool for predictive TN loadings: : :.."

We agree with your comments. It could be revised as follows:

Given that PCs of precipitation forecasts are significantly correlated with the observed precipitation, streamflow and TN loadings, predictive models of TN loadings can be a powerful tool to obtain season-ahead nutrient forecasts.

Section 4.3 Role of antecedent flow condition. I think that the role of antecedent flow condition must be better put in light. It seems like a secondary result of the work, but it is very important. I think that this must be anticipated in the introduction, so that the reader can be better oriented reading the paper without finding the "surprise" of the antecedent flow conditions.

We agree. Reviewer 1 also has suggested. As per this, we intend to move this into Discussion and move Section 4.4 before Section 3.1 to improve the logical flow of the manuscript. We will also emphasize it more strongly in the introduction.

Section 4.4 Source of climatic information: : : Also this should be anticipated in the introduction, with emphasis of why it is important to consider ENSO.

We are planning to move this before Section 3.1, so that we can emphasize the source of interannual variability in TN loadings upfront in the manuscript.

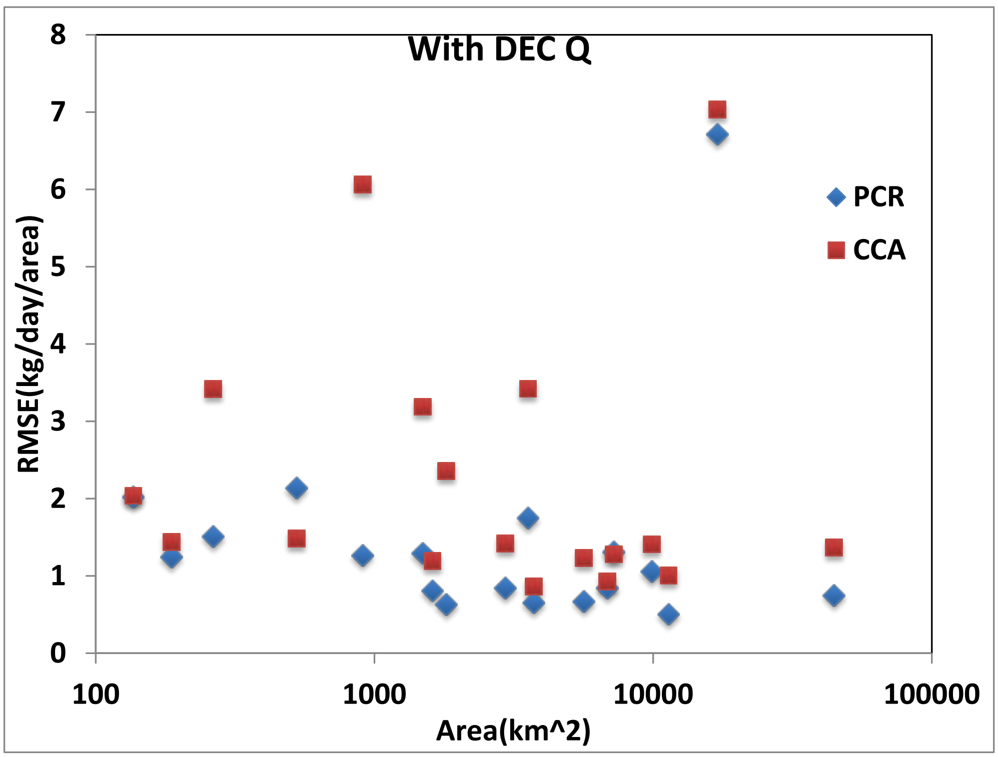
In the paper the authors do mainly two things: illustrating two models and applying them two 18 basins. Nevertheless, the possible applications of the predictive models are stressed, but:
(1) Results and performances of the two models are not compared and poorly commented.
(2) The performance of the two models on the different basins can be improved
Concerning this I make these considerations:
(a) Have you considered summarizing Fig.s 2, 4, 5, 6, 7 in one or more tables? This can provide a synoptic view of the performance of the models site per site, and can put better in evidence sites where performances of models are better and where they are worse. This can also be a good tool to give guidelines to the comments on the sites. Maybe using letters A, B, C, D instead of symbols.

We prefer having them in figures. It is better, since one can clearly see that most of the coastal watersheds perform better under both validation methods.

We are also making specific improvements in moving sections 4.3 and 4.4 to improve the presentation.

(b) Are morphologic parameters (first of all, the area) of basins influent in model performances? Is there a correlation, at least qualitative, between the area and models performances? The same can be said also for years of observation in table 1.

We did not see any relationship between the drainage area or any other morphologic parameters (e.g., slope). We are plotting here the RMSE shown in Table 6 as a function of drainage area.



One reason for limited relationship between drainage area and model performance is that we consider only 18 stations. It is difficult to explain spatial variability in the reported skill based on 18 points. However, we see ENSO playing an important role in influencing spatial variability, but we don't see any role for basin characteristics. This is because at interannual time scale, hydroclimatic processes play a more critical role in explaining the interannual variability than basin characteristics.

(c) Do the models behave well for the same basins or not? What can be inferred from this? Is one of the two absolutely the better, or the performance is dependent on the particular case? Why in your opinion?

We discuss this in detail in Sections 4.1 and 4.2. Our observations are as follows:

In general, PCR performs better than CCA. The reason we wanted to include CCA is because of Figure 8 which shows that regionally the dominant source of variability is from ENSO. This could be seen in Table 6 in which loadings are estimated using both precipitation forecasts and streamflow. By developing PCR, we better explain the variability in simulated TN loadings through a step-wise regression model.

(d) Consider separating the validation procedure of the two models from the results of the model for all basins (section 4.1 & 4.2), so that a space is dedicated to a more schematic illustration of results.

We are planning to remove the box-plot and plot the median of the R^2 as a map so that it is consistent with results from section 4.2.

The introduction provide a good motivation for the importance of predicting TN loadings, on the other hand, I think that that section must answer to these questions:
(a) Why do you predict TN loadings in winter? Is this season particularly important, or is it for data availability in order to test the goodness of the models?

Given that streamflow peaks in winter over the Southeast US, it has been shown that loadings are at its peak during the same season (Muller and Spahr, 2006). From the perspective of winter forecasts too, this is a season that has significant skill in predicting observed precipitation by the ECHAM4.5 model (Devineni and Sankarasubramanian, 2010). Hence, we focus our analysis for the winter season over the Southeast US.

We can include the above points in the introduction.

(b) Why are low dimensional models so important? This can strengthen the originality of the model.

Low-dimensional models provide a simple way to recalibrate the forecasts and develop a predictive model whose predictors are orthogonal to each other (Wilks, 1995; Sankarasubramanian et al., 2008). Given that we use only fewer components, it also avoids over-fitting.

(c) Which are the main sources of nutrients in a watershed and how do they reach water courses? How do precipitation can influence nutrients concentration in JFM? (in other words, briefly a discursive phenomenology should be provided)

These are virgin watersheds whose flows are minimally impacted by anthropogenic influences. The primary source of total nitrogen is from non-point that includes fall foliage and post-harvest agricultural lands (Muller and Spahr, 2006). Thus, increased flow during the winter season primarily carries the TN loadings from fall foliage and agricultural lands.

Section 2.3 Simulated nutrient database In my opinion something has to be said about a0, a1, a2: : : and other parameters of LOADEST of Table 2, without making the reader find the references to understand the table. Or better, are the numerical values of a0, a1, a2, strictly necessary?

We will include the equation in Section 2.2.

$$\ln(\hat{L}) = a0 \ + a1 \ln\ (Q) + a2 \ln Q^2 + a3 \sin(2\pi dtime) + a4 \cos(2\pi dtime) \ \ldots(1)$$

Dtime is the centered decimal time in years as defined in Cohn (1992) and in Runkel et al., (20035)

Section 4.1 Line 2 page 10947 "The entire procedure (i) – (iv) is repeated 100 times: : :" Why 100 times? Is there a particular reason?

Anything above 30 times is good, since the estimated statistics (R2 and RMSE) approach normal distribution based on central limit theorem.

Abstract line 16 LOADEST and WQN are undefined before, who reads the only abstract may not understand the meaning.

Thanks. We can correct this in the revised manuscript.

Section 5 line 6 page 10952 "Thus, the intent of the study is to understand: : :rather than developing a skilful nutrient forecasts using low dimensional model". I think that this must be corrected in something like: "The intent of the study is to develop a skilful nutrient forecasts using low dimensional model and, in addition understand how well climate and basin condition: : :.". And I also think that this could be moved to the introduction of the paper. In fact most energies are spent in the paper to describe the models, and this is good, but few is spent on the results comment (as I written before). The sentence put in that way seems a bit in contradiction to how the paper is written. I think it can be transformed in the way I suggested, with adding, coherently, improvements to results comments for the basins and to comments and comparisons of the two models.

Based on the comments, we are planning to reorganize the manuscript substantially. We are planning to move Section 4.4 before Section 4.1. The section on adding streamflow will be moved within the discussion section. We also plan to add additional table on false alarms and missed targets of the nutrient forecasts to indicate how they predict the observed events.

We hope all this modification will improve the flow of the manuscript.

In my opinion the limits of these models have to be acknowledged: they can be calibrated once series of prediction forecasts and nutrient loadings are known. If they are calibrated for a basin, they cannot be exported to other basins, and, in addition, they cannot be predictive if anthropogenic actions (dams, use soil change: : :) are made in the watershed.

We agree. We will include this in the discussion section. This is our response to the comment.

PCR models cannot be transported to other basins. However, CCA model could work well if they fall within the cluster.

To develop urban watersheds with significant anthropogenic influences, the developed model could be used to predict the upstream loadings for the undeveloped segment of the watershed. However, to estimate the loadings leaving the fully-developed watersheds, one needs to know the loadings from the point sources so that net loadings leaving the watershed could be estimated. This also gives potential to combine weather and seasonal precipitation forecasts to develop nutrient management strategies for controlling point (weather time scale) and nonpoint (seasonal time scale) sources.

References:

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