Reply and Rebuttals

We sincerely appreciate the reviewer's constructive and careful review. Reviewer #1 fully understood the merits of our study and gave us many positive and constructive comments. He/She raised two main concerns in this study. One is the dependency between urban and population which might result in the colinearity in our model. Thus, the reviewer asked us to clarify the necessity of the variable, urban. Secondly, the reviewer also suggested us to refine the crop types, instead of using a more general term, agriculture, to reduce the large variation of agricultural yield factor.

In accordance with the comments, we compared different structures of the models and evaluated the contribution of urban in model performance. We also performed the VIF which is widely used in multiple regression approach to determine the colinearity. We sub-classified agriculture in accordance with crop types to improve the model performance which might be of use for further management. In this revision, we followed reviewer's suggestion to remove global mean method in our flux estimation. The revised fluxes are similar to what we presented before; thus, the story is consistent. Additionally, we invited a native speaker to refine the English to improve the readability. Below, we listed the point by point reply for the comments.

General comment(s):

The manuscript (MS) reports on "area based export coefficients", called here yield factors, for different land-uses in a subtropical mountainous catchment. The approach chosen is interesting and the calculation procedure is fine. An amended MS can be accepted.

Reply:

We are glad that the reviewer appreciate the merits of this study and we revised this manuscript in accordance with the comments.

Comment(s):

1. A big question mark has to be placed to role of the interdependence between the building and population density term.

Reply:

Thanks for bringing up this question so we have the chance to clarify the dependency between building (hereafter, "building" was replaced with "urban" following another reviewer's suggestion) and population density in our model. The dependency or colinearity between variables in generic multiple regression approach is always an issue in high priority. There are two main purposes for multiple regression. One is to determine the possible causal relationship. For this purpose, the dependency between variables must be examined and excluded carefully for avoiding incorrect interpretations. The other purpose is to do estimation based on the variables. For this purpose, the dependency between variables is not restricted. For examples, some variables used in GLOBAL-NEWs, NANI and PLOAD are more or less dependent. Although the dependency is not seriously restricted, the VIF (variation inflation factor) is widely suggested to determine the dependency between variables.

In replying this question and other relevant comments that reviewer brought up below, we re-designed three models. Model A, which could be regarded as the original one, considered 5 land uses (forest, paddy, cropland orchard, and urban) with population. In Model B, urban was eliminated and only 4 land uses (forest, paddy, cropland, and orchard) were evaluated with population. And in Model C, population was removed while urban was taken back in for comparison. The performance and VIF of variables in the three models were discussed.

The yield factors of the individual land use, model performances, and VIF of variables in the three models were listed in Table 1.

Table 1. Yield factors, VIF values, and performances of the three models

Type	Forest	Cropland	Orchard	Paddy	Urban	Population Density	R^2
	μM	μM	μM	μM	μM	kg-N capita^{-1}	
Model A (with Urban)	14.9 (12.7~19.3)	93.3 (33.1~157.3)	116.8 (67.1~192.4)	1337.2 (889.7~1675.7)	1388.4 (1240~1442.3)	1.7 (0.8~4.3)	0.82
VIF	7.11	3.90	1.70	1.32	6.93	4.71	
Model B (Without Urban)	41.3 (28.2~42.5)	153.9 (74.4~216.9)	28.2 (14.5~31.7)	1228.7 (934.3~1234.5)		25.4 (24.4~25.1)	0.59
VIF	2.89	2.74	1.50	1.22		1.15	
Model C (Without Pop.)	16.9 (13.1~20.0)	70.5 (24.2~96.8)	209.2 (107.9~205.9)	1530.5 (1087.6~1477.8)	1476.4 (1445~1477.9)		0.83
VIF	3.95	2.92	1.63	1.30	1.70		

Number in parentheses indicates the first-to-third quartile range of the yield factor.

In this experiment, Model A and C outperformed Model B significantly. It might indicate that the inclusion of urban rendered better explained variance than when it was excluded. Meanwhile, the yield factors estimated from Model A was more reasonable than that obtained from Model B. For example, the yield factor of orchard from Model B is too low to be reliable. The yield factor of orchard should at least be higher than that of forest. Besides, the value of DIN loading per capita estimated from previous studies ranges from 2.0 – 7.0 kg-N/Cap/yr (Thomann 1972 cited by Valigura et al., 2001). Although these values were estimated from riverine DIN export reversely, it could not really reflect the real N footprint in human society (We will discuss this issue in another comment below). Since our study focused on the riverine DIN export, the estimated DIN per capita loading should be comparable with the range.

As for the determination of colinearity, we performed VIF analysis in our Model A to investigate the dependence between variables quantitatively. In general, VIF > 10 indicates harmful collinearity and that variable should be excluded (Stine, 1995; O'brien, 2007; Liao et al., 2012). Table 1 shows that all VIF values are less than 10 in our study revealing that the colinearity between variables is not harmful though the VIF values for forest and urban are as high as 6.0 and the VIF of population density is 4.41. In fact, the correlation coefficient between urban and population is up to 0.97 (seeing Table 3 in our previous study, Lee et al., 2014). Such high correlation will have to result in colinearity, whereas our study which separately considered the runoff variance in space and time increased the applicability of the model. Additionally, considering the two variables simultaneously, the intensity of urbanization (i.e., concentration of human activities and settlements) can be better represented. Based on the two reasons described above, we kept urban in our revised model. We added this table as Table 6 and summarized the above descriptions in our revision. [Line: 350-371]

Reference:
Liao, D., and Valliant, R.: Variance inflation factors in the analysis of complex survey data, Survey Methodology, 38, 53-62, 2012.
O'brien, R.: A Caution Regarding Rules of Thumb for Variance Inflation Factors, Qual Quant, 41, 673-690, 10.1007/s11135-006-9018-6, 2007.
Stine, R. A.: Graphical Interpretation of Variance Inflation Factors, American Statistician, 49, 53-56, 10.2307/2684812, 1995.

Comment:

2. Data sets gained in the Danshui river catchment have already been evaluated related to other and similar questions by the same author group (Lee et al., 2014). Without reading this publication and others from the other group present, MS remains partially unclear. In general, discussions are founded and they are going into detail, some arguments are questionable. This MS need some amendments and clarifications.

Reply:

Yes, this study and our pervious study (Lee et al., 2014) shared the same dataset. The previous one focused on the dynamic speciation along the river continuum. This study focused on the regulation of land-use, discharge and population for estimating the riverine DIN flux for assessment. In this revised version, through the suggestions and comments of the reviewers, we have done our best to amend and clarify our statements.

Comment:

3. The title is very general. An indication to the region or landscape is needed.

Reply:

To present our content clearly and precisely, now we changed the title to "Inverse isolation of dissolved inorganic nitrogen yield within a subtropical mountainous watershed".

Comment:

4. In the abstract, very precise numbers of the yield factors are presented. Indicate the confidence intervals or variations.

Reply:
In order to describe the yield factors clearer, we added the first-to-third quartile range in our revised abstract and other places in the text.

Comment:

5. The indication to fertilizer application rate, line 13 page 450, cannot be taken from discussion in 4.3.

Reply:
We did not describe the fertilizer application clear enough. In this revision, we re-wrote the discussion section and clarified the fertilizer application rate. [Line: 318-321]

Comment:

6. As discussed later, the value reported for DIN per capita loading cannot be regarded as a realistic value or as an effective coefficient after treatment.

Reply:
Thanks for the comment. The concept of "DIN per capita loading" is indeed more complicated than expected. Many studies used the riverine DIN export to estimate the DIN per capita loading reversely as we did in this study (Alexander et al., 2002; Van Drecht et al., 2003). However, the estimated DIN per capita loading from this approach only represented the human DIN emission expelled to river systems. In other words, other human emissions treated via different processes for food production or energy were not taken into account. In a recent study, Leach et al. (2012) developed a calculator to estimate the N foot print from human activities and society development. They concluded that the N per capita emission could be up to 40 kg-N/cap/yr in U.S. showing the complexity of N cascade. In this regard, the DIN per capita loading derived from riverine DIN export should be recognized as a part of DIN per capita emission. Precisely, the so-called "DIN per capita loading" is the riverine DIN output from N per capita emission after a series of processes, as reviewer recognized. To clarify it, we used the term, riverine DIN per capita loading in this revision.

Specific Comment:
1 page 451, line 29, include the full names of the models and their important references.
Reply:
As reviewer suggested, we put the full names of the models and the references in this revised version [Line: 65].

Comment:
2.1 To put all agricultural used land in one category can be questioned. When discussing N export, the fertilising intensity of the land is the classification criteria. Fig. 6 in Huang et al. HESSD (2012) is indicating the very large differences in N yields in a nearby catchment for different activities. By

the way, this inhomogeneity in the class agricultural land may induce the large variability of the yield factor shown in Fig. 8.

Reply:

Indeed, classification is an art of trade-off or generalization for applicability and accuracy. Less discriminative classification may increase the applicability for other regions, whereas decrease the accuracy, and vice versa. Indeed, the agriculture types (e.g. crop type and fertilizer application) are quite different in intensively cultivated regions, like eastern China and Taiwan. In this revision, we sub-classified the agriculture into three sub-categories (paddy, cropland, and orchard) and applied it to the new model. All relevant sessions of the text, tables, and figures were modified based on the new results. As expected, the new results showed that the yield factor of paddy was higher than the others. However, the yield factors of cropland, and orchard were much lower than that of paddy showing the differences among the land uses. We added this in results [Line: 245-247] and related discussion [Line: 312-315].

Comment:

2.2 At the end, state that data set used is discussed in Lee et al. (2014) from the point of view of speciation of DIN and dynamics and the flow regime is treated in Huang et al. EMA (2011 or 2012), check correct year.

Reply: Thanks for the reminder. We cited the two references in section 2-2 [Line: 134].

Comment:

2.3 In principle, the discussion of the methods to estimate the riverine DIN yield (often called load) is correct and fine. However, the need of Figure 2 is questionable. In the figure a reference flux is cited which is not discussed in this script, but in Lee et al. (2009). In this publication methods are discussed in details and used for data gained in the same catchment.

Reply:

We agree with reviewer's point of view. The figure was removed.

Comment:

2.4 The principle of model approach is fine, however between the land use property P building and the population density D there is a significant and strong correlation, see table 3, Lee et al. (2014). Therefore, the 2 terms in equation 2 are not independent. This means that the human emission (waste water input) is split in 2 parts. Calculation later on is proving this effect, see p 465, line 9-11. If the building term is omitted the capita loading factor increases from 0.49 to 3.5 kg N/cap y, which is probably more realistic. This result contradicts the statement on line 13, page 457. By the way, in literature there exist models on the approach based on equation 1 that include point and diffuse sources. Such a comment would complete the introduction.

Reply:

The issue of dependence/co-linearity between urban and population density was discussed in our reply to comment 1 above. The yield factors of urban and per capita were revised. As reviewer suggested, we also explained each item in equation 1 and pointed out the difference between point and non-point sources. In this revision, we clarified the point and diffuse sources in equation 1 and completed the introduction [Line: 165].

Comment:
3.1 Since data set used here is almost identical to that in Lee et al. (2014), result table and the discussion of the observed concentrations are similar to that paper. Same similarities hold as in 3.1. Some differences can be noted, e.g. partition to season in this MS.
Reply:
As reviewer suggested, we diminished some parts similar to our previous paper and highlighted the differences in dry and wet season [Line: 232-238]. Meanwhile, we addressed more on the model in this revision.

Comment:
Table 4 indicates large differences between the wet seasons 2002/2003 and 2004. How does this fact influence yield factors?
Reply:
Compared to our previous method (Huang et al., 2012), this new method deals with the annual variation of streamflow. In other words, the new proposed yield factors for the land uses could remain relatively consistent in annual basis. For example, the performance in validation was compatible with the calibration (see Fig. 6), though the seasonality could not be performed well. We addressed this in section 3-2.

Comment:
3.3 As figure 8 indicates, some yield factors exhibit large variations. It would be adequate to state this, e.g. as ±. As consequence numbers have to be rounded.
Reply: We modified fig. 8 (fig. 6 now) to accommodate the sub-classification of agriculture and we also rounded the numbers and stated the variations of the yield factors.

Comment:
4.1 This is a detailed discussion with figures, partially different and partially similar to Lee et al. (2014). The C-Q relationship is treated extensively.
Reply:
The revised section 4-1 was re-written thoroughly. We diminished the parts similar to our previous study and focused mainly on the differences.

Comment:
4.2 Here or somewhere else, some characterization of the wastewater emission has to be stated. What is the percentage of treated wastewater and what kind of treatment? By the way, organic carbon wastewater treatment plants only reduce the N loads slightly. When discussing the agricultural yield, issue mentioned in 2.1 has to be considered. Lee et al. (2013) reveal nicely the influence of typhoons to yields in a similar catchment. Do you recognize such an influence in the Danshui data set?
Reply:
Wastewater treatment in Taipei City was briefly introduced in section 2-1 [Line: 99-100]. Typhoon, as an important forcing affecting DIN concentration and flux, has been well discussed in Huang et al. (2012) and Lee et al. (2014), whereas in this study runoff issue in landuse-based inversed isolation model and the yield factors was focused. Note that the whole discussion was thoroughly re-written to discuss the controlling factors clearly, see revised section 4-1.

Comment:
4.3 For numbers see remark 3.3. Here, possible reasons for the variation of the agricultural yield factor are discussed. Why not taking the consequences and put 2 different agricultural terms in the model?
Reply: As suggested, in this revision, we classified agriculture into 3 sub-categories. All relevant part in the tables, figures, and text through the whole manuscript were modified.

Comment
The questionable assessment of the capita yield factor is mentioned in remark 2.4. Explication on page 465, line 6 and 7 is unclear.
Reply: This comment has been replied before and section 4-3 has been re-written to clarify the assessment of the capita yield factor.

Comment
Line 13, what are the premises?
Reply: The sentence was removed.

Comment
4.4. Scenario projection is a nice exercise with data gained.
Reply: Thanks.

5. Statement page 467, line 2- 4 would only be realistic if the waste water treated would exhibit an extreme high N removal rate, see also remark 4.2.
Reply: We added a sentence, "Besides, there are three sewage treatment systems, the total nitrogen removal ratio is ~72% and the NH_4 removal ratio is ~87% (Taipei city report, 2014)." To describe the

wastewater treatment in revised section 2.1. [Line: 100-101]

Comment

Table 3 and 4. State the meaning of ± values. Probably they are different.

Re: As suggested, we clarified the meaning of ± values which represented the standard deviation of the DIN concentration and flux, respectively.

Comment

Figure 1, Correct spelling “legend”.

Station numbers are hardly readable. River names cited in the text are not indicated in the figure. Eventually state in the legend, red points reflect the city Taipei with x millions inhabitants.

Reply:

We re-drew figure 1 to increase its readability. The elements that reviewer suggested were added in legend and stated in text.

Comments for figures:

Figure 2 may be deleted, If kept define “reference flux”.

Reply: As suggested and discussed before, we removed this figure.

Figure 3 To less explications in the script; therefore questionable.

Reply: We added more descriptions to make it informative and clear.

Figure 4 Add analogous figure with population density.

Reply: As suggested, we added an analogous figure of DIN concentration against population density.

Figure 5 is not so meaningful. Better design see Fig. 3 in Lee et al. (2014).

Reply: The figure was revised according to reviewer’s suggestion.

Figure 7 and 9 the log-log scale demagnifies the discrepancy between estimation and simulation. A bar diagram (100% are estimated values) would indicate better the degree of concordance.

Reply: As suggested, we used bar diagram to represent the degree of concordance.

Figure 8 indicate the type of box plot.

Reply: Corrected

Figure 11 what is the meaning of numbers beside station numbers?

Reply:

The caption was revised as “The blue dots and values are the sub-catchments which are located at the corresponding coordinate of the scenario experiment and the estimated yields.”