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Discussions

Interactive comment on "Inverse isolation of dissolved inorganic nitrogen yield for individual land-uses from mosaic land-use patterns within a watershed" *by* Y.-T. Shih et al.

Anonymous Referee #1

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Comments to HESSD 12, 449-487, 2015

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

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into detail, some arguments are questionable. This MS need some amendments and clarifications.

An amended MS can be accepted

Deficiencies noted, explications and changes needed - The title is very general. An indication to the region or landscape is needed. - In the abstract, very precise numbers of the yield factors are presented. Indicate the confidence intervals or variations. The indication to fertilizer application rate, line 13 page 450, cannot be taken from discussion in 4.3. 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. 1 page 451, line 29, include the full names of the models and their important references. 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. 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. 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. 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.

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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. 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. 3.2 Same similarities hold as in 3.1. Some differences can be noted, e.g. partition to season in this MS. Table 4 indicates large differences between the wet seasons 2002/2003 and 2004. How does this fact influence yield factors? 3.3 As figure 8 indicates, some yield factors exhibit large variations. It would be adequate to state this, e.g. as $\pm$. As consequence numbers have to be rounded. 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. 4.2 Here or somewhere else, some characterisation 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 inffluence in the Danshui data set? 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? The questionable assessment of the capita yield factor is mentioned in remark 2.4. Explication on page 465, line 6 and 7 is unclear. Line 13, what are the premises? 4.4. Scenario projection is a nice exercise with data gained. 5. Statement page 467, line 2- 4 would only be realistic if the waster water treated would exhibit an extreme high N removal rate, see also remark 4.2. Table 3 and 4. State the meaning of $\pm$ values. Probably they are different. 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. Figure 2 may be deleted, If kept define “reference flux”. Figure 3 To less explications in the script; therefore questionable. Figure 4 Add analogous figure with

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population density. Figure 5 is not so meaningful. Better design see Fig. 3 in Lee et al. (2014). 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. Figure 8 indicate the type of box plot. Figure 11 what is the meaning of numbers beside station numbers?

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