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Discussions

Interactive comment on "Effects of mountain agriculture on nutrient cycling at upstream watersheds" *by* T.-C. Lin et al.

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The authors present a study based on two years of weekly stream water and precipitation chemistry measurements made in mountainous Taiwanese catchments. The authors have collected an interesting and potentially very useful data set but I have a number of serious reservations that I hope can be addressed.

The authors present a study that potentially has broad scientific significance and will be of interest to a wide audience working on catchment-scale element cycling and eutrophication problems.

In its present form, it is hard to judge the scientific quality of the manuscript. Specifically, not enough information is provided about the methods used to calculate element

fluxes.

The overall presentation quality is fair but I believe that could be improved to good or excellent if the authors are able to incorporate my suggestions below.

The authors report concentrations and fluxes from streams in four catchments and two precipitation monitoring stations. Agricultural land use ranges from almost none to approximately 22%. As there are three headwater catchments and one mesoscale catchment (see Figure 1), I wonder if this paper might be better focused only on the headwater catchments.

Reply:

One of the main findings is that a forested watershed downstream from a watershed with substantial agricultural cover has much lower concentrations of nutrients (by more than 70% for nitrate). The result also demonstrate that the inclusion of the large watershed, F1, revealed that monitoring in large watersheds maybe insufficient, particularly in agriculture-intensive region with heavy fertilizer application. Although, the strong dilution and landscape configuration can efficiently remove the nutrient, the heavily pollution may occur in tributaries.

The quality of the written English is generally very good, with a few exceptions. For example, p 4787, l. 28, “scarifying” is used in a very unusual manner and p 4790 l 27, the authors probably meant “without any preservatives”. I suggest that, if the paper is eventually accepted, the authors retain the services of a professional English language editor to ensure that all word choices are appropriate.

Reply:

We changed “scarifying” to “sacrificing” and “without any preserves” to “without any preservatives”. We carefully checked the English throughout the manuscript and had an Ecology professor, Dr. Craig E. Martin, at the University to proofread the manuscript.

I am a little confused about the overall purpose of this paper. The authors note that

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agriculture is increasing in rugged mountain landscapes, yet it seems that any increase in agricultural area is forbidden in the reservoir catchment where they are working. Furthermore, the authors note a lack of published research on agriculture in montane areas, yet present numerous citations to earlier work in the study basin. I believe there is relevance to the work presented here as I think we do need to know more about the possible effects of agricultural intensification in humid montane environments but I would like the authors to clarify their focus.

Reply:

It is correct that no additional increases in agricultural area is allowed at the study site. “However, existing agricultural area is still having an impact at the study site, and throughout many (sub)tropical mountain areas around the world, agricultural area is expending. Therefore, our goal is to use the study site as a case study to illustrate the effects of agriculture activities on nutrient cycling in (sub)tropical watersheds with rugged topography and high precipitation.” It is also correct that quite a few studies have examined nutrient efflux and sediment output to the Feitsui Reservoir. “However, no studies have examined the effects of agriculture activities on nutrient cycling through both streamflow and rainfall”. “The Feitsui Reservoir is a rare case among (sub)tropical mountain watersheds where the effects of agriculture on nutrients have been intensively studied, and with the addition of this study, we believe the knowledge about the reservoir can be very informative to other less-studied (sub)tropic mountain watersheds.” [The descriptions were added to the revised manuscript].

The authors do not provide enough information to assess the credibility of their flux estimates. They note that precipitation and streamflow were obtained from the Central Weather Bureau and Water Resource Agency of Taiwan (p 4791, l. 1-5) but provide insufficient information to interpret flux calculations. Specifically, I would like to see additional figures which present (i) monthly precipitation for each study month from the three rain gauges (mm/month) and (ii) monthly runoff (mm/month) from the two discharge gauges.

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Reply:

We moved the description of how we constructed nutrient fluxes from the Discussion to the Methods, and added more details to clarify our calculations of nutrient fluxes. We also added a table to show all flux values, and a figure to display the monthly precipitation and runoff from the rain gauges and discharge gauges.

I would also like more information about how the flux calculations were performed. I assume for the precipitation fluxes, weekly values were estimated by multiplying the concentration in a precipitation sample by the depth of precipitation over the previous week and then aggregating to monthly or annual scales. There are a number of different ways in which stream fluxes could have been estimated and I would really like this to be clarified. Such clarification is especially important given the extremely high fluxes reported by the authors. If the flux numbers are correct, they are really quite remarkable. The information on mean streamwater and rainfall chemistry is interesting but I do not think worth two of the four figures in the paper. This information could be summarized in a table and plots provided of concentrations and fluxes over time.

Reply:

In response to this and the previous comments, we added more details to our description on the calculations of the fluxes. Specifically in the revised manuscript we stated that "Weekly element fluxes through rainfall or streamflow were derived by multiplying element concentrations and rainfall/streamflow quantity of the same weekly precipitation/streamwater samples, which were then aggregated to a monthly scale. For weekly samples that continued into the next calendar month, we divided the weekly fluxes between the two months in the ratio of their rainfall/stremflow quantities. To properly display the values of all elements on one figure, we expressed some elements in 10 or 100 base units (e.g., 100 base unit for P). Unfortunately, when we were double-checking the flux values, we found a calculation mistake. The values for P output fluxes were taken from the values in 100 base unit rather than the original values. This

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is the reason for the much higher P outputs compared to inputs. We fixed this mistake and revised P output input ratio in the revision. Following the reviewer's suggestions, we now summarized streamwater and rainfall chemistry in a table (the list of the inputs and outputs of N and P) and a figure (the temporal pattern of nutrient concentrations and fluxes).

I am quite skeptical about the authors' assertion that erosion could have been responsible for higher phosphate concentrations in the F2 catchment (p 4793). I have no doubt that steeper slopes will, all other things being equal, have greater rates of erosion. Higher rates of erosion might explain higher concentrations of particulate phosphorus. However, the authors report dissolved phosphate concentrations from filtered samples. If higher rates of erosion will in fact lead to higher phosphate concentrations, the authors need to do a better job of explaining and justifying this phenomenon.

Reply:

We agree that erosion is one of the main drivers for increased particulate phosphorus. However, many studies have shown that watersheds with high erosions can lead to increased dissolved phosphate concentrations as well (see examples listed below). To strengthen our discussion on the potential link between erosion on phosphate concentrations, we added several new citations in the revised manuscript, particularly the studies in Taiwan as they are highly relevant to current study.

Lee, T.Y., Huang, J.C., Kao, S.J., Tung, C.P. (2013) Temporal variation of nitrate and phosphate transport in headwater catchments: the hydrological controls and land use alteration, Biogeosciences, 10 (4): 2617-2632, doi: 10.5194/bg-10-2617-2013. Zehetner, F., Vemuri, N.L., Huh, C.A., Kao, S.J., Hsu, S.C., Huang, J.C., Chen, Z.S. (2008) Soil and phosphorus redistribution along a steep tea plantation in the Feitsui reservoir catchment of northern Taiwan, Soil Science and Plant Nutrition, 54 (4): 618-626, doi: 10.1111/j.1747-0765.2008.00268.x. Green, M.B., Finlay, J.C. (2010) Patterns of hydrologic control over stream water total nitrogen and total phosphorus ratios, Bio-

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geochemistry, 99: 15-30, doi:10.1007/s10533-009-9394-9.

I am also quite skeptical about the authors' proposed link between agricultural land use in the catchment and precipitation chemistry. Volatilization of ammonia from livestock (or perhaps fertilizer) is a well documented phenomenon. The authors report elevated levels of ammonium sulfate, urea and calcium ammonium sulfate (p 4794, l 9-11) in precipitation. I am quite concerned that what the authors are actually reporting is contamination of their precipitation samples. Did they weight the filters before and after to rule out presence of large amounts of particulates in the precipitation samples?

Reply:

As the reviewer correctly pointed out, volatilization of ammonia from livestock (or fertilizers) is indeed well documented. In our analysis all samples were filtered through 0.45-μm filter papers, therefore it is unlikely that large amounts of particulates from volatilization of ammonia would present in the filtered samples.

The flux numbers are very difficult to understand. Please consider a table which presents fertilizer inputs, atmospheric inputs, harvest outputs and runoff losses for agriculture and forest land cover types in each study catchment. I read the paper several times and could not work out the numbers to my satisfaction.

Reply:

We added more details in the revised manuscript to clarify our calculations of fluxes of N and P. We also added a table of the values of fluxes to make it easier to follow the numbers and calculations.

Finally, I would like to thank the authors for the opportunity to review this thought provoking paper (is it a positive term?). I hope that they will find my comments useful as I believe they have a potentially important contribution to our understanding of human impacts on water quality.

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