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Discussions

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

Anonymous Referee #2

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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.

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Interactive Discussion
Discussion Paper

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.

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.

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

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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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.

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.

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?

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.

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Finally, I would like to thank the authors for the opportunity to review this thought-provoking paper. 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.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 4785, 2015.