Review of "Riparian and in-stream controls on nutrient concentrations along a headwater forested stream" by Bernal and others

Presented data and descriptions include interesting and important information. The aim and approach to evaluate the riparian and in-stream controls presented in this manuscript is challenging and attractive. Therefore, the document itself is valuable, even though this is a report of a case study at particular middle scale catchments. However, there are several weaknesses for considering a publication as an original article in the current version. I hope following comments will help the authors to improve the contents and descriptions.

Comments:

1. As the authors stated in the first paragraph of the discussion section, the novel point of this paper was statistically quantitative analysis of sources of nutrients in the stream water. However, in the conclusion section and abstract, their descriptions on relative contribution of riparian and in-stream processes was still very qualitative. More quantitative expressions on relative contributions were favorable.

2. P11600, L17-20: This type of hypothesis has been commonly recognized by many researchers in the field of river ecosystems. Not only your proposal.

3. In the methods section, it would be helpful for readers to show a conceptual diagram or picture examples expressing the spatial distributions and scales of riparian forests (zone), riparian-stream interface (zone?).

4. How does the climatic seasons correspond to the vegetated/dormant seasons? Is the vegetative season rainy season or high flow season? And also, doesn't the seasonal variation in discharge rate affect those of the nutrient concentrations and fluxes?

5. P11602, L14-16: In the discussion section, the authors discussed about the influences of the N2 fixing trees and the N dynamics of soil microbes on the nutrient input from the riparian zone to the stream. Those processes are usually most active at the organic horizon and the near surface part of the mineral soils. Was the

sampling conditions which made the influences from those soil horizons minimal appropriate to investigate the direct effect of those N dynamics?

Related to this question, in order to elucidate the controls by the biogeochemical processes of riparian zones comprehensively, relative degree of impact under the low flow condition should be estimated compare to that under the high flow or storm conditions.

6. What was the specific benefit to use the CI value (considering the NH_4^+ concentration) for evaluating redox condition instead of DO?

7. A table for summarizing definition of each variable (Q_{gw}, Q_{spf}, F_{spf}, C_{sw}, etc.) would be very helpful for readers.

8. More detailed explanations of stream water sampling should be needed. Because, solute concentrations are often different with parts of cross section. Effect of the riparian groundwater might be evaluated differently at the bank side and the center of the stream.

9. Sub-chapter 4.3 included some discussions based on observed results. Some contents in this section might be categorized into the discussion chapter.

10. The first paragraph of the Chapter 5 was redundant, and some contents overlapped with the introductory section.

11. P11613, L25- P11614, L1: The authors cited the paper Asano et al. (2009) to show an example indicating a reduction in the variability of solute concentrations as catchment size increases. Then, they stated that their case did not show the decrease in the CV of stream solute concentration along the reach. However, the discussions in Asano et al. (2009) did not tell the variability of solute concentrations decrease with the distance from the headwater in a SINGLE PARTICULAR stream. The "variability" in their paper indicated a variability of solute concentrations AMONG

THE MULTIPLE DIFFERENT STREAMS WITH SAME CATCHMENT SCALE. The discussion was totally different with that in this manuscript.

12. P11614, L407: It is generally accepted that a major source of phosphate in stream is chemical weathering of the bedrocks. And also, phosphate in the vegetated floor and surface soils are usually recycled tightly within the plant-soil internal cycling. If the authors want to conclude that the controlling factor for stream phosphate concentration was the "hill slope water source", they had to explain how different the hill slope water (groundwater?) input was from the water input from the riparian zones. Citations of Asano et al. (2009) was not appropriate, because they did not discuss the sources of solutes using riparian-hill slope comparison. Their concept was that the stream solute concentrations could be explained the mixing of shallow and deep groundwaters both from hillslopes.

13. P11614, L8-14: The authors concluded that the drop in the specific discharge and the Cl^- concentration along the reach were due to increase of evapotranspiration. This explanation might be doubtable. Could the evapotranspiration rate vary significantly within a few-km scale? Did vegetation type change from headwater to valley bottom significantly?

14. P11615, L4-L28: The discussions of this part were confusing. It seemed that the longitudinal pattern of NO3- concentration had two phases: decrease from (0-3000 m) and increase (3000-3700 m). But, the ratio of the N2-fixing trees increased monotonically from headwater to valley bottom. To explain the longitudinal pattern of NO3- concentration sufficiently, they should show the more consistent logic to apply to both decreasing and increasing phases with certain evidences. Probably, there might be two different controllers

15. P11615, L17-19: On English expression at the following part:
"…we could not establish any positive relationship between the proportion of N2-fixing tree species and stream NO3- concentrations along the reach."

Any relationships can not be “established” by researchers. They can only observe and find the relationships.