

## ***Interactive comment on “Quantifying nutrient fluxes in Hyporheic Zones with a new Passive Flux Meter (HPFM)” by Julia Vanessa Kunz et al.***

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The article describes implementing a combination of two passive methods to determine joint water, nitrate, and phosphate flux in a depth profile in a stream bed. Water flow is quantified using a previously published empirical relationship between alcohol dilution from activated carbon, while chemical fluxes employed ion exchange resins. The method was tested both in the field and laboratory.

The work was well described, and appeared to be carried out with attention to many important potential interferences (e.g., attentive to the fact that many brands of ion exchange resin have significant background concentrations of nitrate). In the end the results were believable and encouraging, which is in keeping with the fact that the base methods have been well developed in previous studies. To be clear, the implementation

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in the hyporheic setting is novel and represents a solid advancement.

The paper was well written, with only a few places where I encountered grammatical usages which I found to be slightly awkward (noted on the attached pdf). With respect to brevity, I found the introduction and discussion to be overly detailed - the role here is to point out the need for this method in the context of previous work. It should be a matter of just one or two sentences to lay out the well-documented importance of hyporheic processes. At the same time, since this is a methods paper, I found that the description of related methods (e.g., heat pulse or passive heat transport for flow) to be lacking specific attribution of relationship to the new method in terms of precision, advantages and disadvantages. Thus I found the paper to bely the authors interest in flow processes over methods, which was at odds with the goal of the paper. On this same point, I was expecting that the heat-flow methods described would be reported in the results, which I did not encounter.

Overall, I think this paper is well deserving to be published with minor revisions to attend to the issues I have noted.

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Please also note the supplement to this comment:  
<http://www.biogeosciences-discuss.net/bg-2016-334/bg-2016-334-SC1-supplement.pdf>

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