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Discussions

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

Anonymous Referee #1

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Content

The article describes a very interesting passive approach to determine and couple interstitial water flow and nutrient transport in the hyporheic zone, the hyporheic passive flux meter (HPFM). The method is based on alcohol dilution from activated carbon and ion exchange resins. They firstly tested the ion exchange resins to obtain the most appropriate one. Secondly tested the HPFM in the field and compared the results with a most commonly used method such as pore water sampling. The presented approach is very interesting since it reduces temporal variability and the sampling effort, and it is very relevant because couples nutrients and water flow, to obtain hyporheic fluxes.

General comments

While interesting and novel, I have five major concerns as summarized herein. First,

I have concerns about the approach in the field test. The number and distribution of the HPFMs in the field seems to be done assuming a very homogeneous hyporheic zone, however this hardly ever happens. As a consequence the high spatial variability arises among all the measurements. There is not enough replication for basic statistical tests, and therefore, the comparison with the reference method, the pore water sampling (MLS), is very difficult to interpret. At least the analysis of the data should be done using those layers that have been measured as replicated presenting means and standard deviations/errors. And when possible perform statistical tests that proof or not the differences.

Second, the data from the HPFM should be explored with more detail and use the information provided by the coupled information to obtain more accurate information. In its present form the usage of the data is slightly superficial. A part of showing whether the HPFMs worked or not, the manuscript should also show which information can be obtained with them.

Third, the growth of biofilm appears to be very significant in the HPFMs both in the laboratory columns and in the field. This could have strongly influenced the results and should be taken into account by the future or potential users of the HPFMs compared with other methods based on diffusion; however no data on this aspect are shown.

Fourth, while the abstract is quite direct, the introduction is too detailed what makes the reading confusing and lacks of a clear and direct objective. The methods section provides numerous and useful details as should be in a methodological manuscript, however they could be arranged in another way more intuitive that eases the reading. In general terms, the manuscript would be more convincing if it were presented as a comparison with MLS.

Fifth, the text is well written in general however a revision of expressions and grammatical mistakes is needed.

For Specific comments see Supplement to this comment.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-334/bg-2016-334-RC1-supplement.pdf

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