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Biogeosciences Discussions

Interactive comment on "Hydromorphological restoration stimulates river ecosystem metabolism" *by* Benjamin Kupilas et al.

Anonymous Referee #1

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General comments

The aim of this manuscript was to use ecosystem metabolism as a measure of ecosystem function in response to river restoration. The authors estimated ecosystem metabolism over 50 days within a mid-sized river reach that contained moderate (R1) and substantial (R2) restored reaches and a degraded (unrestored) (D) reach. The idea of using ecosystem function, especially ecosystem metabolism, as a response to river restoration is compelling, and in the direction where restoration work/research is needed and likely headed.

Using open channel diel changes in dissolved oxygen, the authors used a 1-station approach to estimate ecosystem metabolism. As both restored (R1 and R2) and unrestored reaches are in succession, the 1-station approach results in the dissolved oxy-

gen signal integrating beyond the study reach of interest. Unfortunately, the authors cannot parse out restoration effects on ecosystem metabolism because the estimates of GPP and ER encompass all of the river reaches. If I understand correctly, the authors have the data to use a 2-station approach to estimate ecosystem metabolism. I strongly urge the authors to estimate reach specific ecosystem metabolism in order to quantitatively parse out the effects of restoration on ecosystem function.

Specific comments

L31 – Hydromorphology is introduced here and mentioned throughout, but not defined. I think it would help the readers to clearly define hydromorphology early on in the manuscript. Specifically, why or how is hydromorphology linked to ecological status (as mentioned in the next sentence).

L58 – I suggest changing 'It measures. . .' to 'Ecosystem metabolism is a measure of the production. . .'

L65 – Missing a 'have' – 'The majority of these studies 'have' focused. . .'

L66 – Dodds et al., Freshwater Science – estimated ecosystem metabolism in the Mississippi River. . . reference to this study could be included. Also – Hall et al. L&O, measure GPP in the Colorado River.

L84 – What do the authors mean by 'hydrodynamics'? Beaulieu et al. 2013 measured ecosystem metabolism in an urban stream where storm events and periods of low to now flow had a strong effect on GPP and ER.

L91 – The authors predicted that the restored reaches would result in higher biomass of primary producers. I am assuming this is due to increased light availability because of the widened channels. However, increased biomass of primary producers can also indicate eutrophication. I think within the context of this study, the authors attribute increased macrophytes, etc.. to be a positive response to restoration (increased ecosystem function). But, increased macrophytes due to eutrophication or

due to light alleviation can likely have opposite ecosystem function outcomes, I think it would be beneficial to mention or briefly discuss eutrophication versus a positive ecosystem function of increased autotrophic biomass.

L101 – km to km2

L181 – L185 – I do not follow the reasoning for not using a 2-station approach to estimate ecosystem metabolism. What was the travel time between the two stations? As mentioned within the general comments – without having a reach specific estimation of ecosystem metabolism, the authors cannot infer differences in GPP and ER to restoration efforts.

L205 – Using night-time regression is ok here. However, I do agree with one of the open comments posted for this manuscript – given the high productivity in this river, it might be possible to model GPP, ER, and k. There are several packages becoming available that may help model ecosystem metabolism from oxygen data, including a 2-station approach. One package that is currently in development is StreamMetabolizer, for instance (http://usgs-r.github.io/streamMetabolizer/) or another package from Halbedel and Buttner 2014 (Methods in Ecology and Evolution).

L225 – ANOVA with repeated measures is not appropriate for time-series data. I suggest using an analysis method that includes an auto-regressive term, perhaps generalized least squares or something similar (i.e. see Zuur, A., Ieno, E. N., Walker, N., Saveliev, A. A., & Smith, G. M. (2009). Mixed Effects Models and Extensions in Ecology with R. The Quarterly Review of Biology (Vol. 84, pp. 574–405). Springer Science & Business Media. http://doi.org/10.1086/648138)

L228 – For future analyses, the authors should justify why they would exclude days during or after storms where GPP was zero. The variation in daily rates of ecosystem metabolism within one river often exceeds or is the same of that from multiple sites (see Hall 2016, Metabolism of Streams and Rivers. In J. B. Jones & E. H. Stanley (Eds.), Stream Ecosystems in a Changing Environment (pp. 151–180). El-

sevier. http://doi.org/10.1016/B978-0-12-405890-3.00004-X). I think excluding these days, other than not being able to estimate GPP or ER (which can occur with high stream discharge events) would skew the results by artificially reducing the variance.

L266-268: Why not report ranges of NEP here? Also, I understand the utility of GPP:ER ratios – but NEP can be informative as actual flux values.

L336: Given the methodological approaches using the 1-station method of estimating metabolism along with the use of ANOVA, differences in metabolism amongst the reaches is not presently supported.

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