Biogeosciences
Discussions

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

Benjamin Kupilas et al.

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We would like to thank the three anonymous reviewers and Robert Pennington for their helpful comments. We believe that our manuscript has been substantially improved by the suggested re-analyses and changes to the text and figures, and address these changes below in a detailed point-by-point response to the comments of reviewers 2 and 3. Our final responses to reviewer 1 and Robert Pennington have already been published in previous comments. Furthermore, we add our revised manuscript as supplement to these responses.

Detailed response to reviewer 2 COMMENT This manuscript addresses a question of high importance – how might we use measurements of ecosystem processes to monitor the health of rivers? While there has been a push to include process measure-

ments with other monitoring efforts (including those of restored sites), data showing how process measurements can actually be used to inform monitoring in rivers are few. Here the authors estimate ecosystem metabolism and hydromorphic characteristics of connected river reaches with (n=2, "R1" and "R2") and without (n=1, "D") a history of restoration efforts and provide evidence for higher rates of ecosystem metabolism (gross primary production and ecosystem respiration; GPP and ER) in restored versus degraded reaches. What this difference means in terms of "good" or "bad" rates remains unclear without longer-term before/after data. And while the motivation for this research is very timely and will be of broad interest, a few assumptions behind methods and presentation of results limit the impact of the manuscript in its current form. RESPONSE We agree with the reviewer that the incorporation of ecosystem processes in river monitoring is of high importance and we think that our revised paper will be of broad interest. By estimating metabolism with the 2-station method for the combined restored reach R1+R2 (see Fig. 1 in response to reviewer 1) and by comparing ARIMA function estimates (a) along the restored river section and (b) between the upstream degraded river and the restored reach R1+R2 (see Fig. 2 in response to reviewer 1), we believe that river metabolism increases due to restoration are now well supported in the revised paper. As for the referee's question as to what higher rates of GPP and ER mean in terms of "good" and "bad": The restored sections (R1 and R2) have a higher structural quality according to the national river habitat survey protocol, and their better (=closer to natural conditions) morphological condition compared to the degraded "control" section has been characterized throughout the manuscript. As discussed in lines 371-374 of the original manuscript, higher rates of metabolism and the occurrence of dense macrophyte stands in restored river reaches may also increase the assimilation of dissolved nutrients (Fellows et al., 2006; Gücker et al., 2006) and the sedimentation of particulate nutrients (Schulz and Gücker, 2005), thereby positively affecting water quality. Accordingly, increased rates of GPP and ER should indicate a positive response to restoration in the case of the investigated river, as they occurred in reaches closer to local reference conditions. However, a general

classification of metabolism increase as either "good" or "bad" in terms of "natural" and "unnatural" conditions is currently hardly possible as there is only limited knowledge about natural geographical gradients in river metabolism (especially for mid-sized and larger rivers) as discussed in lines 347-356 and 376-380 of the original manuscript. This highlights the importance of coupling functional measures, such as metabolism, with assessments of ecosystem structure in different biomes and ecoregions. COMMENT It is not clear from the descriptions in main text and supplements why the two restored reaches were separated from one another – beyond the fact that they were perhaps separate projects? While restoration effort was indeed larger in R2 than R1, it was not drastically so, especially given the larger reach area in R2 than R1. Further, using a single-station metabolism approach to estimate GPP and ER does not honor the boundaries set by the authors in naming R1 and R2 given the overlap in O2 footprints upstream of researcher-defined reach boundaries. Based on author responses to earlier comments, it appears that combining R1 and R2 is a more suitable approach, despite losing a reach "replicate" of sorts. I hope the manuscript will be revised accordingly and the authors will confirm that combined R1-R2 rates are reasonable given what R1- and R2-only rates were. RESPONSE The larger area of R2 compared to R1 is a consequence of restoration effort (widening of the river channel and amount of soil removed) - accordingly, it is important to separate R1 and R2 to explain the local situation with two reaches restored with different restoration effort to the reader, even if these reaches do not match the dimensions of our experiments. Restoration is most often implemented in short river stretches of approximately 1 km (as described in lines 35-36 of the original manuscript). The experimental reaches, therefore, reflected these typical spatial scales and we were able to combine the assessment of reach-scale structural characteristics with changes in metabolism. Despite the mismatch between lengths of river reaches evaluated and reaches exclusively affected by restoration, we found significant effects of reach-scale restoration on whole-river metabolism using the 1-station technique. We also stressed in the original manuscript that the used 1-station technique measured long upstream, degraded river reaches in addition to the

river reaches of interest, and that the presented metabolic rates (measured with the 1-station method at the end of each experimental reach) and their successive increase have therefore to be considered as qualitative indicators of metabolism increase due to river restoration, rather than as the exact rates in the experimental reaches. In response to the reviewers' concerns, we additionally calculated the metabolic rates of the combined reach R1+R2 with the 2-station method, and obtained metabolic rates that can be directly compared to metabolic rates of the upstream, degraded river (measured at station D with the 1-station open channel method). Results obtained with the 1-station and the 2-station method often agree well (e.g., Bernot et al., 2010; Beaulieu et al., 2013). Thus, we quantitatively supported our previous qualitative findings by estimating metabolism with the 2-station method for the combined restored reach R1+R2. We would like to stress that the good agreement of results of the 1-station and the 2-station method in restored reaches, i.e. that both clearly suggested metabolism increases due to restoration, may be an important finding for agency efforts to monitor restoration outcome, because the 1-station method may be more practical for routine measurements, while the 2-station technique is often considered a research method that is too complex for such purposes. Therefore, we believe that it is important to show and compare results from both techniques in our study. COMMENT Time series analyses could be a more powerful way quantify differing/strengths of controls on metabolism in R vs D reaches during the 50-day deployments. A more sophisticated analysis would enhance the contribution of the paper beyond means/ranges of GPP, ER, NEP. See Roley et al 2014 Freshwater Science for an example of this and a citation of general interest. If not used here, this is at least worth a mention for future research avenues. RESPONSE We changed our statistical approach and now use an autoregressive approach to address the autocorrelation issue of time-series data (see response to reviewer 1 for a more detailed description). However, this new approach only resulted in minor changes in results. COMMENT The importance of autochthonous production for ecosystem functioning" (19-20) is a very context-dependent statement that should be expressed with caution. What does an increase in GPP mean

for ecosystem health? We see this response in the R2 reach (but D was 2nd highest in macrophytes, not R1?), and higher GPP is sometimes used as a sign of ecosystem degradation: excess primary production in response to nutrient loading. Units of N are in mg, so there may be environmental issues requiring mitigation beyond physical restoration by widening river channels. Without "pre" data for R reaches, one could argue that the restoration project provided more light + warmer temperatures needed for enhancement of macrophytes beyond "natural" conditions. RESPONSE We express increased abundance of macrophytes as positive effect of restoration (see Lorenz et al. 2012 J. Appl. Ecol.) rather than using it as indicator for high nutrient load and eutrophication. Restoration aimed to establish near-natural hydromorphology and biota. Thus, increased occurrence of macrophytes is positive effect in terms of near-natural state of the investigated river. See also response to comment of reviewer 1: We agree and discussed this issue in more detail in the revised paper. In the present case, there are no point or diffuse sources that lead to eutrophication effects in this river section. Thus, metabolic responses should be solely due to river restoration, i.e. wider channels (-> more light availability), shallower channels (-> better habitat for macrophytes), and lower current velocities (->less hydraulic stress) as also discussed in lines 70 of the original manuscript. In R1 other autotrophs (e.g., periphyton) which have not been subject to mapping may have stimulated metabolism. COMMENT L2 - No comma needed after "Both" RESPONSE We agree and corrected this in the revised paper. COMMENT L - 49-50 – Should include a "but see: : :" citation here to acknowledge that there have indeed been several previous studies even if this issue is not well-studied or settled. RESPONSE We have added this to lines 53ff of the original manuscript. COMMENT L - 53 – Delete "only" RESPONSE We corrected this in the revised paper. COMMENT L - 109 – It would be useful to give time frame of/since restoration here, not only in supplement. RESPONSE We agree and added this information in the next paragraph of the revised paper. Please see also response to reviewer 3. COMMENT L - 130 – What about water chemistry? Any differences among reaches? If restoration is indeed altering nutrient retention or removal, that should be reflected in downstream concen-

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trations. RESPONSE It may well be possible that changes nutrient concentrations are measurable along the >2 km of restored river. However, we did not measure nutrient concentrations along the studied stream section, and therefore cannot comment on that. COMMENT L - 183-4 – Vague RE: which methods as written. Why not restate and give equations for this, k, and base metabolism calculation to allow readers to better assess the methods within the manuscript itself? RESPONSE We agree with the reviewer that it is interesting to have the mentioned equations and added them to the manuscript. COMMENT L - 225 – Good to see that the authors will update their statistical analyses in a revised manuscript. RESPONSE The detailed results from these analyses are in our response to reviewer 1. COMMENT L - 227 – Data from flood events are one of the most exciting things we can learn about from longer time series. I urge the authors not to exclude them from their analyses. RESPONSE We agree that floods and storms are an important part of environmental variability. However, GPP was not detectable during storm events, and we cannot be sure whether GPP was really zero or very low, or whether high flows may have prevented the detection of GPP. As reviewer 1 also stated, this often occurs with high stream discharge events. Thus, we show the data and discuss them in terms of environmental variability, but believe that it is more adequate not to analyze flood data statistically in terms of restoration effect. COMMENT L - 273 – "returned" RESPONSE We agree and corrected this in the revised paper. COMMENT L - 347 – Give some numbers to support this comparison in the main text. Fine to keep the citations/table in supplement. RESPONSE We agree and added more detailed information in the discussion. COMMENT L - 369 – "near-natural" is a very vague description and does not seem to be supported with data. RESPONSE We removed the term "near-natural" conditions ("Thus, the restoration of short river reaches may have positive effects..."). COMMENT L - 380 – "as a functional indicator". RESPONSE We agree and corrected this in the revised paper. COMMENT Table 2 – Check units. Superscripts for m didn't appear in my version of the text. RESPONSE In our version of the manuscript superscripts are correctly represented. We confirmed this in the final version. COMMENT Fig 2 – Needed? There are

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many figures, and I didn't feel that this was needed for main text. Supplemental figures are nice. RESPONSE We believe that these figures are important to understand our rationale. Moreover, as Biogeosciences is an online journal without figure limits, we thought that this would not be a problem. COMMENT App S5 – Possible to include Q for context? See also Genzoli & Hall 2016 FWS, Davis et al 2012 RRA, Dodds et al. 2013 FWS, Hall et al 2015 L&O. RESPONSE Yes, we included Q in spreadsheet (a) "river characteristics" of Appendix 5.

Detailed response to reviewer 3 COMMENT This manuscript examines the response of ecosystem metabolism to river restoration by comparing ecosystem metabolism among three river reaches of a mid-size river: a degraded (unrestored) reach (D), a moderately restored (R1), and a substantially restored reach (R2). The use of ecosystem metabolism to determine the effects of river restoration is fairly novel. In that sense, the manuscript represents a relevant contribution to the challenge of incorporating measures of ecosystem functioning to river monitoring, and to river restoration in particular. The manuscript is well structured and written. In general, the results are clearly and transparently exposed, limitations indicated, and details nicely presented in the appendixes. My main concern with this manuscript relies on the fact the sampling design of this case study may not be the most appropriate for correctly answering the important question of whether river restoration caused a significant change in ecosystem metabolism. Ecosystem metabolism was measured in only one river, only after river restoration, and only during a certain period of the year (summer). Ideally, such a question should have been addressed by measuring in several rivers, before and after restoration (BACI design), and considering several periods of the year. None of these criteria is fulfilled, and therefore, the strength of the results and its potential extrapolation to more general responses are limited. This should be at least more explicitly acknowledged in a revised version of the manuscript. RESPONSE The comparison of restored with upstream degraded "control" sections (space-for-time substitution) is commonly used to quantify restoration effects as data on pre-restoration are rare (e.g., Hering et al. 2015 J. Appl. Ecol., Jähnig et al. 2011 J. Appl. Ecol.). Time of measure-

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ments is in accordance with the WFD compliant sampling period for structural measures such as macroinvertebrates and macrophytes. Consequently, our results can be linked to these measures (see lines 315-318). The measurements are extensive and time consuming and it was not the aim of our paper to replicate measurements in several rivers – this would have required a much simpler method, thus gaining limited insights into the underlying mechanisms. We would like to stress that our manuscript is the first measuring metabolic changes of medium-sized rivers following restoration. The lack of available data - especially for mid-sized and larger rivers - is discussed in lines 351-356 of the original manuscript.

COMMENT I agree with the comments that have arisen in the open discussion regarding the use of the 1-station method. As indicated in those comments and responses, the authors should incorporate the 2-station method for the restored reaches (R1 + R2) to make their statements more robust. These limitations in the metabolism estimations together with the issue in the general sampling design (previous paragraph) and the statistically significant but relatively minor changes in metabolic fluxes in restored relative to degraded reaches, makes the conclusions of a clear effect of the restoration on ecosystem metabolism not as clear as pointed out by the authors. RESPONSE Please see our response to reviewer 1. We do not consider significant GPP increases from ≈5 to ≈7 g DO m-2 d-1 and significant NEP increases from ≈-2 to ≈0 g DO m-2 d-1 as minor changes from an ecosystem perspective.

COMMENT I also think that the last part of the discussion could be greatly improved by making more explicit recommendations and by being more convincing about the advantages of incorporating metabolism and other functional measures to river monitoring.

RESPONSE We agree with the reviewer that advantages of functional indicators for river monitoring are manifold and should be mentioned in studies, which are using functional measures in applied river research. However, we feel that this topic - and ecosystem metabolism in particular - has been widely discussed within the literature

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(see also references presented in line 382 of the original manuscript) and that a summary of the benefits of metabolism as a functional indicator is more appropriate as presented in lines 380 - 387 of the original manuscript. We think that a more extensive description would make the manuscript lengthier without giving new information.

COMMENT L - 17-18: Unclear sentence. Rephrase.

RESPONSE We changed the sentence as follows: "Restoration increased autotrophic processes, as indicated by higher GPP:ER rates measured at restored reaches".

COMMENT L - 23-24: Any hints that this is occurring at the study site?

RESPONSE "High rates of metabolism and the occurrence of dense macrophyte stands may increase the assimilation of dissolved nutrients and the sedimentation of particulate nutrients, thereby positively affecting water quality." – This is our interpretation of possible consequences of the observed high metabolism rates, although we did not measure nutrients permanently. To highlight the speculative nature of this sentence we use the word "may".

COMMENT L - 61-64: "natural changes" and "land-use change" are confusingly used in this sentence.

RESPONSE We think the difference is clearly described by giving examples in the original manuscript (see lines 62 and 64). Natural changes refer to floods and droughts (e.g., Uehlinger, 2000) and land-use change refers to differences between pristine and agricultural streams (e.g., Gücker et al., 2009; Silva-Junior et al., 2014).

COMMENT L - 79: Any reference?

RESPONSE "Moreover, the reconnection of rivers with their floodplains by creating shallower river profiles and removing bank fixations may enhance inundation frequency, and hence resource transfers from land to water" - This is our interpretation of possible consequences of the hydromorphological restoration (reconnection of river and floodplain by creating shallower river profiles and removing bank fixations) al-

though we are not aware of a reference. To highlight the speculative nature of this sentence we use the word "may".

COMMENT L - 89: Do you mean "contiguous" instead of "continuous"?

RESPONSE We agree and corrected this in the revised paper. COMMENT L - 91: Here I miss some predictions regarding the expected differences between R1 and R2. It seems important to justify the examination of two levels of restoration.

RESPONSE We expected (i) hydromorphological river characteristics, i.e. habitat composition and hydrodynamics, to change following restoration, with the magnitude of change depending on restoration effort (e.g. width and diversity of the river channel, and abundance of primary producers, as well as sizes and locations of transient storage zones in the two restored river reaches compared to the degraded reach).

COMMENT L - 110-115: I suggest including here information on when the restoration was done. It seems important to know how much time has passed from restoration to measurements.

RESPONSE We agree and added this information in the revised paper. See also response to reviewer 2. COMMENT L - 138: Unclear at which flow conditions these measures were done. RESPONSE We agree and added this information in the revised paper.

COMMENT L - 195: It seems odds that some measures were done in 2013 and others in 2014. How may this have influenced your results?

RESPONSE The purpose of our study was to quantify structural changes following restoration at the reach scale and examine the related effects on metabolism. Considering that there were no major meteorological differences between these years, that structural differences between D, R1 and R2 (effort) were very similar between years, and that measurements were performed in the same season at similar discharge conditions, we believe that interannual variability is a minor confounding factor in our

study.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-431/bg-2016-431-AC1-supplement.pdf