Biogeosciences Discuss.,
doi:10.5194/bg-2015-645-AC2, 2016

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Are flood-driven turbidity currents hot-spots for priming effect in lakes?" *by* D. Bouffard et al.

D. Bouffard et al.

marie-elodie.perga@thonon.inra.fr

Received and published: 11 April 2016

R2-1 Firstly, the incubations were carried out in October with river water that was not turbid and likely had a very different composition of DOM and dissolved nutrients compared to during the flooding event in May. The authors themselves acknowledge this discrepancy, and argue that the aim was rather to test the responses of lake water from the different hypolimnetic layers to river water, regardless of the composition of the river water (page 13, lines 5-9). I agree that the results have some value in this regard, but they are still very unrepresentative of the context of the field observations. This makes one wonder why the respiration assays were not carried out on more occasions, at least some of them involving flood-like conditions?

Actually, in terms of nutrient and C concentrations, river water was not that much differ-

Printer-friendly version

Discussion paper

ent between the May flooding event and the following October (see P10, L5-10) but we understand the reviewer's concern. This is for the exact same reason that we specified in the original manuscript that Âń this experiment did not intend to mimic conditions during the flood but instead to investigate the variability of the metabolic processes in the different hypolimnetic layers" p13, L.7-9. Ideally, the experiment should have been conducted during the studied flooding event, but as we emphasized in the introduction, based on available background, a respiration effect could hardly be anticipated. Bioassays were justified by the immediate, natural and straightforward critics we got when sending around an early version of the manuscript to colleagues for advices, i.e. the supposed-to-be refractory nature of allochthonous organic matter inputs would hamper fast and significant respiration within the lake. This is indeed the most common and shared belief in global literature on the topic. The flood we had been studied was of exceptional amplitude (a 50-yr return time at least for the Dranse river) and waiting for another year would not have anyway reproduced the field conditions. The point was then to investigate the processes underlying the observed field results, and we were lucky enough that even for different flowing conditions, bioassays results reflected very well the field conditions. This stresses out the fact these processes might not be exceptional, instead their overall contribution to the lake O2 budget gets more significant in flooding conditions. Shall we revise this manuscript, we would better emphasize that point. We agree though, and this is the next step of this ongoing work, that tests at different seasons would be quite informative.

R2-1. A circumstance that the authors put forward, is that the October river water conveniently had the same DOM concentration as the river water, so that the dilution with lake water did not cause an overall difference in DOM (page 10, lines 1-4). However, I do not see how dilutions in the 10 to 100-fold range would cause drastic enough differences in DOM concentration to make such incubations invalid, even if the river water would have had a much higher DOM concentration compared to the lake water. It should be possible to normalize observed oxygen consumption rates to DOM concentration to obtain comparable metabolic activity measures between waters, for

Printer-friendly version

Discussion paper

example.

As the reviewer might have noticed, this clearwater lake has very low DOC concentrations (about 1 mg/L). If the flooding river waters were as rich as 3-4 mg/l (which remains though a low concentration), even the 10% treatment might have changed the initial DOC concentrations by 30%-40%. In such a case, normalization would have been very helpful. In the present case, DOC concentrations were 0.8 and 0.7 mg/l for the lake water, and 0.75 mg/l for the Dranse river, which, accounting for the accuracy of the TIC-TOC equipment, might not be even significantly different. Normalization by DOC concentrations provides fully similar results. Yet, shall this representation (Fig 1 R2) give more trust to our results, it could easily substitute it to the actual figure in a revised version of the manuscript.

R2-2. I commend the authors on submitting a manuscript, that is obviously the result of good and thorough work, less than a year after a major field campaign, and less than 6 months after experimental work. Yet, I can't help to wonder how much better the manuscript could have been if the authors would have waited until the next spring, and carried out additional respiration assays during more representative conditions. I would not let this be a ground for rejection, as there can be a number of valid reasons why such a delay in publication is not acceptable, but I recommend putting less emphasis on the incubation results, as they do not fit well in the context of flood-driven turbidity currents and they do not prove the occurrence of priming effects (see below).

See reply to the previous comments. Our point of the experiment was more to test the process, that to mimic the environmental conditions of the field survey. We yet still believe these are crucial.

R2-3. Second, I am not entirely convinced that the incubation experiment in fact indicates a priming effect, since the increased oxygen consumption in the 1-10% river water in lake water mix is compared statistically to oxygen consumption in lake water alone. More appropriate in my opinion would be to compare to an expected oxygen

Printer-friendly version

Discussion paper

consumption, adding the oxygen consumption of each part of the mix together. The authors do make such a comparison in the discussion, but only of a few examples are given and there is no statistical testing to support claims. See specific comments below for more detail.

We do contest the absence of statistical testing since the O2 consumption curve over time were statistically compared between all treatments using ANCOVA (P 16, L16-24-28), such as final O2 consumption after 92h (l 21) and results are non-ambiguous. Comparing to expected O2 consumption is yet a possibility (see figure below done for un-normalized O2 consumption as DOC-normalized consumptions provide fairly similar results) and leads to the exact same conclusions : there is a substantial respiration overyield when mixing a small fraction of the Dranse water to the lake water at 100m depth. Interestingly, a 50-50 mix results in an underyield respiration. This proportion is far above what could be expected within the lake but would deserve further investigations. We believe this figure (Fig 2 R2) is redundant with the rest of the ms but could easily replace and strengthen text P 15 l 1-10.

R2-4. Third, I can see alternative explanations than the priming effect for any disproportional increase in oxygen consumption when river water is mixed with lake water compared to when they are incubated in isolation. The authors mention nitrification (page 10, lines12-17), and increased respiration of particulate carbon such as microbial biomass is another possibility. A budget of dissolved OM in the incubation flasks would have been a way to confirm that the observed differences in oxygen consumption were indeed a result of respiration of DOM, as the authors suggest. Yet, TOC concentrations appears to not have been measured after the incubations, or the data is not shown. Similarly, it would have been valuable to measure dissolved nutrient concentrations both before and after incubations to rule out the influence of other processes, such as nitrification or fertilization effects.

We agree that full demonstration for priming effect requires that the fresh organic matter is added to the incubation pre-filtered for microbial inoculum, and also that a C

Printer-friendly version

Discussion paper

mass balance (or better an artificial isotope tracking) is being done. We have been yet quite careful about these limits in this original version of the ms and these are explicitly mentioned (L16-18 p15) and discussed. The unfiltered additions are a more realistic representation of what happens in nature and C mass balance is sometimes uneasy because of the low DOC context of these waters. Increased respiration of particulate carbon would have been an explanation in the case that POC was high in the Dranse river water used for bioassays. However, for both lake and river waters, POC concentrations are basically beyond detection limits (<0.1 mg.L-1). We can still add these specifications though in a revised version of the paper. We could however reasonably rule out a fertilization effect (p10 L5-10) such as potential nitrification by restricting the data analysis to 92H. After that delay, there were obvious patterns for nitrification occurring in some vials (clear breaks in the oxygen consumption dynamics see Fig 3 R2).

R2-5 Another interesting aspect that is discussed in the manuscript is the inoculation of distinct microbial communities by the river water, or the exposure of the river DOM to distinct lake communities, that could change the OM degradation rates due to functional differences of these microbial communities (page 15, lines 16-19). This possibility could be ruled out by sterile filtration of either lake or river water prior to incubation. I am not suggesting that the authors should have done this, and they would probably have had to include a measure of microbial biomass to account for differences in respiration due to microbial biomass alone, but if they would perform similar experiments in the future it is a possibility worth considering.

We thank the reviewer for his/her wise suggestions, and these are indeed supporting ongoing research.

R2-6. All in all, it appears to me that the results of the incubation experiments performed in this study are a bit too preliminary to add much of an explanation to the field observations and to suggest that the priming effect is important in this context. The priming effect has received significant attention in aquatic ecosystems in the last

5 years, and so far the reports from different aquatic ecosystems on its importance are contradictory. The concept of the priming effect seems to be attractive to aquatic scientists, but to demonstrate priming effects experimentally is not trivial. This study adds to the body of literature that reports results suggestive of priming effects, without actually demonstrating it. Although it is a worthwhile addition to the discussion on priming effects, my opinion is that potential priming effects should not be the main message of this manuscript. Either the incubation experiments can be cut out altogether (and hopefully be included in an exciting follow-up study where they are repeated with more rigour) or less emphasis is put on the results of these experiments, which includes changing the title and shortening the discussion. If the authors decide on this alternative, and keep the incubation experiments in the manuscript, please acknowledge the limitations of your approach more clearly in the text.

We still believe that the bioassays, although not intended to mimic the full field conditions, are required to nail down flood driven respiration as a plausible process. Cutting them out would be a real weakness as most readers would only not believe in high and fast respiration of allochthonous organic matter, as this is the most shared belief in current literature. Experiments have been conducted in the light of the paper main hypothesis, i.e. they have been focussing on O2 consumption rather than C mass balance. We fully agree that these are not enough to fully demonstrate 'priming effect' and we took real care in the first version not to claim we did. Most of the limitations mentioned by the reviewer were already thoughtfully discussed in the original version but we could try to emphasize these limits a bit more in a revised version. Maybe keeping 'priming effect' in the title, although it is worded as a question and preceded by 'potential' is a bit too provocative and we would, if required, change it. But there are in this paper, strong evidence that this is a process that could take place in lake depths.

R2-7 Figure 6: This figure does not alone illustrate the presence of any priming effect since it is unclear how the observed increases in respiration differs from what you would expect when you mix the highly respiring river water with the relatively inactive

lake water. If you use for example the end-point measurements, you would expect that an addition of 10% of river water would respire 10% of the oxygen that river water alone respires (that should be about 0.3 mg O2/l according to the y-axis values I am reading out of panel b). The 90% of lake water should respire 90% of the oxygen that it respires alone (roughly 0.7 mg O2/l) that makes 1.0 if you add them up. This is indeed lower than the âĹij1.5 that you observe, but is it significantly lower? I can't tell from the top of my head how you would go about to test this in a statistically sound way, but the additive effect is what you should be comparing to, not the baseline lake respiration (as in results). Perhaps it would help to provide the expected respiration as a separate line but I fear that the plot would be too messy. You could instead choose to plot the time points separately as barcharts, with a bar representing the expected additive oxygen consumption next to the observed bar for every treatment. Alternatively you could plot every treatment separately across time in a multi-panel figure.

See figure 2 and associated reply (R2-3)

Remaining specific comments are specifications, editing or suggested improvements that will be integrated in an amended version of the ms.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2015-645, 2016.

Printer-friendly version

Discussion paper

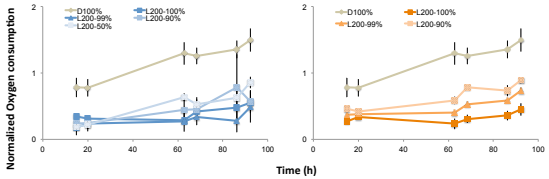


Figure 1R2. Normalized Oxygen consumption (molar ratios; µmol O_2 per µmol of initial DOC) in the bioassays

Fig. 1. Figure 1R2. Normalized Oxygen consumption (molar ratios; μmol O2 per μmol of initial DOC) in the bioassays

Printer-friendly version

Discussion paper

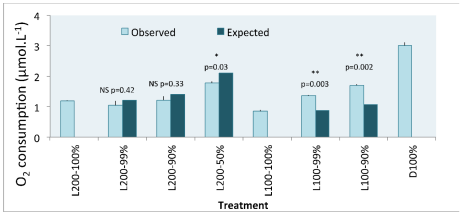
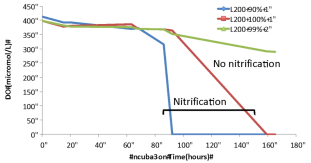


Figure 2R2. Expected (based on a mixing model) and observed O2 consumption in the bioassays of mixed lake and river waters.

Fig. 2. Fig2R2

Printer-friendly version

Discussion paper



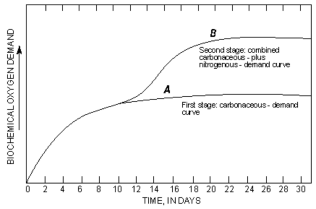


Figure 7.2-1. Biochemical oxygen demand curves: (*A*) typical carbonaceous-demand curve showing the oxidation of organic matter, and (*B*) typical carbonaceous- plus nitrogeneous-demand curve showing the oxidation of ammonia and nitrite. (Modified from Sawyer and McCarty, 1978.)

Figure 3R2 a. DO concentrations over time in the example vials. The abrupt break in the slope after 80-100 h in L200-90%-t1 and L200-100% t1 are typical for nitrification processes as specified in Figure 3b.

Fig. 3. Fig3R2

Printer-friendly version

Discussion paper