Biogeosciences Discuss.,
doi:10.5194/bg-2015-645-RC2, 2016

Biogeosciences
Discussions

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

Anonymous Referee #2

Received and published: 26 February 2016

This manuscript deals with the effect of flood driven turbidity currents on lake oxygen distribution and microbial respiration. It describes an extreme event in May 2015 in Lake Geneva, when heavy precipitation caused turbid river water from rivers Rhone and Dranse to push into the deeper layers of the lake. The authors have taken the opportunity to sample at selected sites in the lake that are differently affected by these turbidity currents to establish the depth-distribution of dissolved oxygen, temperature and turbidity. The context is that similar events in Lake Geneva and other lakes that are deeply stratified and experience deep water hypoxia have been hypothesized to replenish oxygen to these deep layers. However, the data to support this assumption are scarce and contradictory, and the authors set out to test this hypothesis in situ during a flooding event. They observed that turbid river intrusions redistributed dissolved oxygen in deep layers, but did not lead to a net oxygen gain. In shallower

Printer-friendly version

Discussion paper

layers, an intriguing net oxygen deficit was observed, which they interpret as a respiration over-yielding caused by priming effects, whereby riverine DOM stimulates a surplus degradation of lake DOM. In order to test this hypothesis, they performed dark respiration assays, where they mix riverine water with lake water from different depths at different mixing ratios. Similar to their field observations, they detected increased oxygen consumption when riverine water was mixed with upper hypolimnion water.

The manuscript is well-written, the context and hypotheses are relatively clearly laid out and the figures are generally illustrative and clearly presented. The original aim of the study, to test the turbidity-current deep oxygen replenishment hypothesis, appears well-founded and the results seem to provide valuable information in this regard, disproving a long-held belief and adding to the understanding of how extreme events may influence lake ecosystems. As such, the field observations alone are a valuable contribution to the literature. That being said, I am not a physical limnologist or hydrologist, and my ability to fact-check the background and interpretation of the results is therefore limited.

In principle, I am very much in favour of the approach to confirm and mechanistically test field observations by laboratory incubations, and the dark respiration assays are a clever and appropriate way to do so in this case. However, I have a number of reservations about how these incubations were carried out, and how the results from them are presented and discussed.

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? 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 example. 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).

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 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.

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, lines

Printer-friendly version

Discussion paper

12-17), 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.

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.

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.

Specific comments:

Title: How about changing it to something that more closely reflects the results rather than being speculative, for example: “Flood-driven turbidity currents deplete rather than replenish oxygen in a deep stratified lake”

Page 2, line 11: correct to “dense river water”

Page 2, line 13: correct to “ ... balances the force”

Page 4, line 23: What do you mean by western Europe? Many people consider Scandinavia part of western Europe and there are certainly larger lakes in both Sweden and Norway in terms of volume and depth. “western continental Europe” would be more correct.

Page 9, line 13: “... in such large system” should be either: “... such a large system” or “... such large systems”.

Page 9, line 22: “Fig. 6” should be “Fig. 5”.

Page 9, line 25: “lower depths” is potentially confusing, as it can be misunderstood as lower down in the lake, i.e. deeper. Use “shallower depths” instead.

Page 10, lines 22-31: I do not follow the reasoning here. How does an increase in oxygen consumption in 1-10% river water diluted in lake water compared to undiluted lake water indicate a priming effect when the river water alone has a much higher oxygen consumption compared to the lake water? You would expect an increase in oxygen consumption proportional to the amount that was added. If there is a nonadditive (disproportional) effect when waters are mixed, it should be expressed as the difference to the expected additive effect. See also comment to figure 6.

Page 26, line 26: Correct typo to “suspended”

Page 15, line 29: Correct to “ ... true underflow processes”

Page 15, lines 2-10: Here the authors compare the increase in respiration to what you would expect from proportional mixing effects. These calculations should be made in the results in my opinion, and the observed respiration increase should be compared to the expected statistically for all treatments and time points. A graphical representation of these comparisons would also be a valuable addition to the figures.

Page 16, line 8: Which “underlying mechanisms” are you referring to? Could you rephrase to make this more clear?

Page 16, lines 16-17: “... river intrusions in the upper hypolimnion resulted in an increase of BOTH autochthonous and allochthonous organic matter respiration” How can you know this?

Figure 5, legend: You should indicate how many samples these boxes are based on (n=...).

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 ∼1.5 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.

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