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

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R3-1 In the literature the role of such currents have been stressed mainly in low to very low-oxygen hypolimnion, that is not the case here. What would be the effect of the current if the hypolimnic O2 concentrations were below 2 mg/L. I recognize that the authors are cautious about their results and want to show that "turbidity currents (not) necessarily increase hypolimnetic oxygen stocks". So they should better discuss the various situations.

Based on our dataset we cannot predict the effect of the intrusion on an anoxic hypolimnion. The interesting case of oxygen rich river water entering into anoxic water should be investigated separately as the transition from anoxic to oxic water may lead to other processes of larger importance than the priming we have hypothesized. This

limitation can be integrated in a revised version of the manuscript.

R3-2. A recurrent question is the uncertainty of the measurements. There is no mention of the reproducibility and repeatability of, for instance, the O2 measurements; therefore it is difficult for the reader to evaluate if the observed variations are significant.

It is unclear whether the reviewer refers to the field or the lab data. In both case though, the measures have been performed using optical sensors that had been calibrated according to the manufacturer's guidance (Sea&Sun Technology for the field probe, Pre-Sens for the lab bioassays) just before the study. These measures are instantaneous. For the lab measures, the sensor was set on 1 measure/3s and the measures are averaged over 30s. Every treatment in the bioassays has been conducted as triplicates (L15-17, p6). Calibration of the Pre-Sens (two-point calibration DO 0-100%) was tested again at the end of the experiment (<1 week) and showed no drift over the whole duration of the bioassays.

R3-3 I agree with the two anonymous reviewers about the small representativeness of the experiment to explain what happened during the main event.

Rev 1 is actually quite supportive of the bioassays. Indeed Rev 2-3 questioned the representativeness of the experiment, but as mentioned as reply to Rev-2, the bioassays intended to test for the possibility of fast and efficient respiration of allochthonous organic matter in the hypolimnion, more than to strictly mimic the conditions during the floods. We understand the reviewer's concern and this is for the exact same reason that we claimed in the 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. However, without the bioassays, the first critics we got were about the supposed-to-

be refractory nature of allochthonous organic matter inputs that would hamper fast and significant respiration within the lake. 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 flowing conditions. Shall we revise this manuscript, we would better emphasize that point.

Remaining comments are minor and would be addressed in a revised version of the manuscript.

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