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Biogeosciences Discussions

Interactive comment on "Resistance and resilience of stream metabolism to high flow disturbances" *by* Brynn O'Donnell and Erin R. Hotchkiss

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With the exception of the "GENERAL RESPONSE", all of our response text sections begin with "RESPONSE" immediately following reviewer comments.

GENERAL RESPONSE:

This text is included in both responses to reviewers, with specific responses to reviewers below.

Here we respond more generally to questions about why/how we selected isolated flow events and the resulting number of events suitable for our analyses (n = 15 events over

5 years). We emphasize that we focused on quality over quantity when selecting for and analyzing stream metabolism before, during, and after high flow events. Our methods were chosen to address a lingering knowledge gap in our understanding of ecosystem processes: how biological processes (gross primary production and ecosystem respiration, GPP and ER) respond to and recover from discrete higher flow disturbances during storms, how those two processes compare to one another, and which environmental drivers may best explain these dynamics. Potential metabolic responses include subsidy (increasing rates due to higher substrate concentrations), stress (decreasing rates due to physical or chemical disturbances), or no change, which allows for our work to build on concepts fundamental to biogeochemistry and ecology. An additional knowledge gap is how different processes (i.e., GPP, ER) may respond differently to high flows. How we chose to quantify changes in metabolism during higher flow acknowledges the "pulsing steady state" of ecosystems in a novel way. In our revised manuscript, we will better introduce and identify how and where these different concepts apply to, inform, and are answered by our research.

The goal of this work was to assess how metabolism responded to and recovered from higher flow events that were also isolated flow events. Indeed, this decreased the number of suitable events for analysis. But our choice of methods allowed us to focus on response/recovery to discrete disturbances and avoid biased comparisons of pre/during/post multiple high flow (but not isolated) events that encompass time periods that are long enough (e.g., weeks) where pre/post comparisons are less meaningful. Perhaps we could have selected a more pristine stream with less flashy hydrology at the start of this project, but another motivation of our work is to better understand processes in less pristine ecosystems (historically understudied because they are more challenging sites to obtain high-quality metabolism estimates from, another factor that decreased the number of events with appropriate data for our analysis). Despite having "only 15 events", most past analyses included a similar or fewer number of events (e.g, n=10 in Reisinger et al. 2017) over a shorter time period. Our work fills in substantial knowledge gaps: we analyzed across seasons (not only summer months or a short sensor deployment period) and high flow magnitudes (not only base flow or the highest flow disturbances).

After all appropriate QA/QC measures, we had 1375 days of metabolism estimates over 5 years (which were reported in full in O'Donnell & Hotchkiss 2019 Water Resources Research). To calculate resistance and recovery, we needed consecutive days of high-quality metabolism estimates, which further limited the number of high flow events appropriate for our analyses. For example, in 2016 there were 52 (out of 352) days with quality-checked sensor data that had a 50% flow change relative to the day prior. After looking at these 52 storms and selecting those that had 3 days before and 3 days after without any other flow events, we had 12 that were isolated. After quality-checking our metabolism estimates for all of those days, we had 4 high flow events from 2016 that passed all quality-checking steps required for this analysis.

/ end of GENERAL RESPONSE

REVIEWER 2:

General comments: The manuscript bg-2020-304 "Resistance and resilience of stream metabolism to high flow disturbances" by O'Donnel & Hotchkiss analyzes in a third-order stream the response of Gross Primary Production (GPP) and Ecosystem Respiration (ER) altered by disturbances such as isolated high flow events. The study is relevant as it is based on a long-term monitoring (5 years) of GPP and ER, which is critical to decipher seasonal and multiyear variability of stream ecology in the context of climate change. Overall, I found the approach of the study interesting but the authors should explore their dataset further, therefore I suggest major revisions.

RESPONSE: Thank you for your positive impression of our work. We've responded to your suggestions for further explorations below.

Major comments: I was surprised that the authors did not discuss about in-stream net ecosystem production (NEP). NEP is critical to decipher stream ecology as it does

indicate whether an ecosystem is fixing more C than is respiring. The authors showed that ER has higher resistance and resilience in comparison to GPP, thereby should shifted NEP towards heterotrophy (decrease of the GPP:ER ratio). I believe it would be very interesting for the reader to understand/know how NEP is affected by high flow events. I suggest adding figures and discussion about NEP.

RESPONSE: While our aim was to focus on the response and recovery of specific processes (GPP versus ER), we agree that changes in the balance between GPP and ER (i.e., NEP) are an interesting angle for discussion. Most, if not all, of the discussion about changes to NEP will be similar to ER, so we will also be careful to avoid repetition in places where we add discussion related to NEP. We will also refer to our recent paper discussion overall patterns in discharge, ER, GPP, and NEP where this was one of our primary objectives (O'Donnell & Hotchkiss 2019).

The dataset used by the authors is extended in time but the paper lacks of seasonal variability analysis. How GPP, ER and NEP, resistance and resilience are affected by seasons and by year-to-year variability. Indeed, temperature effect on stream metabolism is usually significant. The authors needs justify that the variability induced by the temperature does not overcome the variability induced by flow events. In the revised paper, I suggest the authors adding a figure such as GPP vs ER with points colored according to seasons or river flow.

RESPONSE: This is an interesting question that we hoped to pursue, but one that we found to be beyond the scope of something we could discuss with certainty because of the few isolated storm events with appropriate data and model output for our full analyses (n=15), thus limiting our ability to make concrete conclusions related to seasonality. Despite that, we were able to discuss patterns of GPP and ER more broadly (not for specific flow events) in O'Donnell and Hotchkiss 2019, and will better integrate results from that analysis throughout the paper.

In low order streams, GPP and ER are affected by groundwater inputs, as groundwater inputs are usually significant in such streams. Groundwater exhibit usually low oxygen concentration, which may be problematic when GPP and ER are based on oxygen monitoring. Inputs of low-O2 groundwater in stream can overestimate ER and underestimate GPP. However, the equation 1 does not take into account groundwater inputs. Why? Oxygen measurements during high flow, especially in low-order streams, can give erroneous values, so are the authors sure to measure appropriate values during the high flow events.

RESPONSE: We understand the challenges associated with groundwater inputs and metabolism modeling assumptions (e.g., Hall and Hotchkiss 2017). We will revise the methods to state that we did not see evidence for groundwater inputs in our study reach using conservative tracer additions (not part of this study, but part of other work at this site).

There is some variability in day-to-day metabolism rates; therefore, I do not understand why the authors took the maximum or minimum value of GPP (or ER) from the antecedent range to estimate the resistance. I believe that the median or the mean would be more appropriate. In addition, why the authors used 3 days as the antecedent range. Is it arbitrary?

RESPONSE: Thank you for the opportunity to clarify this. We will update our methods to ensure readers understand that analyzing the 3 day antecedent range was not arbitrary, it was the most appropriate trade-off between keeping our analysis to isolated flow events (the aim of our study) but including more than one or two days to estimate the dynamic range of prior conditions and metabolic rates (e.g., “pulsing equilibrium”, Figures 1,3). If we used >3 days, we would have fewer flow events to analyze. If we used <3 days, we do not have as much information about prior conditions and metabolic rates with which to track metabolic responses to changing flow. We chose the range because this captured the full range of average metabolism estimates in ways that summarizing pre-storm rates to means or medians would exclude.

I have concerns on how the isolated flow events are selected. Indeed, in the figure 2A, I observe that only few high flow events (15 events in 5 years) are actually selected by the authors. In the figure 2C, the authors did not provide statistical analysis on the difference of cumulative daily discharge between all days and isolated flow events. Is it statistically different? Visually, it seems not, considering the high range of cumulative daily discharge during “all days”. If it is not statistically different, it means that the disturbance is the same in both groups. Is there a way for the authors to arbitrary select a greater number of high flow events? As examples, the authors could use maximum daily discharge vs cumulative daily discharge or the change in discharge from pre- to peak-storm flow. By the way, I do not understand why the authors wants to select isolated flow events rather than all high flow events. I believe that estimating resistance and resilience in each high flow events would be much more robust. In addition, the paper aims to study ecosystem response to high flow events, but the paper do not contain figures showing the relation of river flow versus stream metabolism. What is the relationship between river flow and GPP, ER, NEP, resistance and resilience? Resistance represents the change in GPP (or ER) during a change in river flow, so maybe it would be interesting to show ΔGPP (or ER) with ΔQ?

RESPONSE: Please see the general response at the top of this document for more information about how we prioritized quality over quantity in selecting isolated flow events. We will assess differences among isolated flow events and the flow of prior days used for comparison. Figure 2C shows that the isolated flow events we were able to use for this analysis were typical of the range of flow changes we see at this site, and should thus give appropriate insights into the range of potential changes to GPP and ER due to flow disturbances. As mentioned above, we will refer readers to O’Donnell and Hotchkiss 2019 for the paper that discusses all of the flow and metabolism data (the 2019 paper did not analyze how metabolic rates changed during and after isolated flow disturbances, which is the objective of this paper).

I the discussion section, I do not feel that the authors fully responds to their four hypotheses. How can the authors responds to H2 where they actually do not show carbon or nutrients measurements? H0 is strongly dependent on how you arbitrary selected the flow events. To my point of view, with their study design (unless the authors have measurements of carbon and nutrients) the authors can discuss only about H1 and H3. In addition, I also suggest rewriting the Discussion section in a more logical sense following the order of their hypotheses.

RESPONSE: We agree, and as mentioned in our response to Reviewer 1, will revise our manuscript to better align the introduction and discussion topics. While we do not have high-frequency concentrations of carbon and nutrients to compare with patterns in metabolism RE: H2, we note that if metabolic rates did follow a hump-shaped curve as predicted by subsidy-stress, the changes in rates are more appropriate to test metabolic responses than changes in concentrations, as the concentrations themselves have already been altered by any carbon and nutrient uptake that occurred before the sampling point (i.e., concentrations may reflect potential for process, but we cannot know what is missing from carbon and nutrient pools if it's already been removed from the water by biota).

Minor Comments:

L.1: Please, add somewhere in the abstract the ranges of ER, GPP, NEP, resistance and resilience.

RESPONSE: Good idea. We will look for opportunities to optimize the inclusion of summary results within the character limits for the abstract.

L.10-11: You defined the metabolic resistance as the magnitude of departure from the dynamic equilibrium during antecedent lower flows, so why using the words "ER magnitude of departure" to refer to resistance. Better used the word resistance and resilience throughout the text once you have defined those words.

RESPONSE: We wanted to be true to the method used when referring to specific

analyses and results, but will review opportunities to streamline terminology throughout the manuscript to clarify when we are talking about specific metrics and when we are referring to broader topics.

Please add also in the abstract that more ER or GPP is resistant less the magnitude of departure is large.

RESPONSE: We will update the abstract to highlight these results.

L. 69: It is strange to start with H1 and finish with H0

RESPONSE: We will re-order to start with H0, no change, in our revised manuscript.

L.81: Usually precipitation is in mm

RESPONSE: We will change to mm in our revised manuscript unless the journal prefers different units.

L.90: How did you calibrate the different sensors, and how often did you check the calibration?

RESPONSE: Sensors were calibrated every 2-4 weeks (line 96) according to best practice recommendations from the manufacturer or, in the case of the PME dissolved oxygen sensor, with Winkler titration checks of our 100% and 0% calibration solutions. We will add this information about sensor calibration in our revised manuscript.

L.93: can you add the weather station on the figure A1. The figure A1 needs a scale, a geographic footprint.

RESPONSE: Great suggestions. Will will note the location of the weather station in Figure A1 and add a scale bar. We are not sure what the reviewer means by geographic footprint, but perhaps that will be resolved with the scale bar.

L105: Please, specify that you works with gas exchange coefficient not gas exchange velocity.

RESPONSE: We will specify this in our revised manuscript.

L.110: How did you measure the PAR? How did you calculate the average depth?

RESPONSE: Both of these questions are addressed in lines 92-96.

L.119: What are the values of K?

RESPONSE: All K estimates are in our Supplementary Data file of daily metabolism estimates. K for the high flow days analyzed are in Table 2. We will summarize the mean, median, and range of K estimates for Stroubles Creek in the main text of the revised manuscript.

L.133: Please define Qi

RESPONSE: Thank you for catching this. We will clarify in our revised manuscript. Qi is the discharge of the day of interest and Qi-1 is the discharge of the day prior.

L.167:169: For the different variables other than GPP and ER you used the medians from three days prior the flow event for correlations, but for resistance you used the maximum or minimum GPP or ER before the flow event. I believe it would be robust to use the same methods.

RESPONSE: Great point. We used GPP and ER range to account for the variability in metabolism (and to be consistent with how we assessed metabolism for other analyses), but will test for any differences in using medians in a revised analysis for manuscript revisions.

L.180: Is the cumulative daily discharge statistically different between isolated events and other days?

RESPONSE: We interpret this question as asking whether discharge on high flow days for an isolated event was different than lower flow days before/after. We did not test for differences, but can include that analysis in our revised manuscript and will refer to Figure 2 for the visualization of differences.

L.182-185: As mentioned in the major comments please showed how GPP, ER and NEP are affected by seasons and river flow.

RESPONSE: In our revised manuscript, we will remind our readers to refer to O'Donnell & Hotchkiss 2019 for the relationships between Q, GPP, ER, and NEP and for different seasons. We will also better integrate those findings into our revised discussion.

L219 Where can I see that ER was more resistant than GPP. It is on a daily basis? Yearly basis? Multi-year basis? Please give some details, some stats should be applied. Figure 1 do not show your results.

RESPONSE: We will change this citation to direct readers to Figure 5 in our revised manuscript.

L.228: Same comments 228-230: Can you show some results confirming what you stipulate? In the table 4 turbidity seems weakly correlated with resilience of ER and GPP.

RESPONSE: It was weakly correlated across the 15 events appropriate for our analyses. We address this more fully in O'Donnell & Hotchkiss 2019 with daily metabolism and turbidity data and will expand our discussion based on the 2019 paper in our revised manuscript.

254: The authors have a dataset representing 5 years of monitoring so why they cannot answer to this question, at least partly?

RESPONSE: This was beyond the scope of this project and more appropriate for sites with more high flow events that conform to sensor and metabolism QA/QC, hence the call for 'future analyses'.

Figure 5: I am not convinced by this figure.

RESPONSE: It was unclear from the reviewer comment why this figure is not convincing, but Reviewer 1 was also unenthusiastic about this presentation! As described in our response to Reviewer 1, we believe this is an important way to visualize and compare results between GPP and ER, but will explore additional ways to show these results before committing to the figure in a revised manuscript.

Figure 6: Same data as in the table 4, perhaps not relevant.

RESPONSE: We will move either Table 4 or Figure 6 to the appendix in our revised manuscript.

Figure 8: Is there a better way to present these results?

RESPONSE: We are not sure what about this graph did not work for the reviewer, so we do not know how best to respond to this comment. Because we were limited in the data collected and reported by other studies, wanted to be as inclusive as possible, and thought placing our work in the context of other flow-metabolism studies was important, this was the best format to calculate and highlight as many metabolic responses and recovery intervals across studies as possible.

Table 4: Please indicate the p-values, Indeed, two parameters can have a correlation coefficient greater than 0.5 but they are still not correlated together if the p-value is greater than 0.05.

RESPONSE: We will include p-values in our revised manuscript.

Figure A4 to A18: In each isolated flow events: GPP, ER and Discharge can be combined in one figure with 3 axis

RESPONSE: They can, but we prefer to avoid multiple axes on plots whenever possible to avoid potential misinterpretations of data.

Figure A20: To my point of view a figure such as this one showing the seasonal variability of the different parameters (GPP, ER, NEP, reÌĄsistance and resilience) is important and must appear in the main text.

RESPONSE: Please see our comments above RE: challenges with assessing dif-

ferences among seasons (we do not have the data power to do this well). We will make sure we more clearly refer readers to O'Donnell & Hotchkiss 2019, where daily metabolism data (not a subset of data focused on analyzing high flows) are graphed with different symbols for each season.

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