Biogeosciences Discussions

Biogeosciences Discuss., author comment AC2
https://doi.org/10.5194/bg-2021-270-AC2, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Reply on RC2

Tracy E. Rankin et al.

Author comment on "Controls on autotrophic and heterotrophic respiration in an ombrotrophic bog" by Tracy E. Rankin et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-270-AC2, 2022

This manuscript presents data from a field experiment where CO2 fluxes were measured in control, complete vegetation removal, and moss removal plots in an ombrotrophic bog in order to estimate ecosystem respiration. Further, the vegetation removal treatments were used to partition respiration into contributions from autotrophic respiration and heterotrophic respiration. Measurements were conducted across two growing seasons, and respiration measurements were coupled with measurements of environmental variables such as water table height and air and soil temperatures in order to identify drivers of respiration across the growing season and among different vegetation types.

While the objectives of this study and the rich dataset are valuable contributions to the field, I agree with many points made by Reviewer 1 in addressing the statistical weaknesses of this paper. I find six key points that warrant attention on behalf of the authors to improve the strength of this paper's analyses and conclusions.

We thank the reviewer for their comments and suggestions. We will address each one in turn below.

- The structure of the discussion is rather confusing. Perhaps separating the discussion section into environmental predictors of AR vs. environmental predictors of HR, temporal variability in AR and HR, and vegetation type differences in respiration would make for a more succinct discussion that directly relates to your manuscript's stated objectives.

As AR is a residual term (difference between ER and HR), and AR is hence dependent on HR, we believe that separating the environmental predictors of AR and HR into two sections is not a favourable option. Perhaps re-wording the section headings though, and moving up the last paragraph of section 4.1 to right after line 254, would make the flow better?

- As Reviewer 1 suggests, a clearer definition of the methods used as part of the "multiple regression trees" is necessary. Further, I suggest instead using model comparison and selection methods like stepwise AIC comparison of models to identify the suite of variables that best explain HR and AR in bog areas dominated by different vegetation types. This would better allow you to identify the most predictive combination of variables in this system.

As was stated in the reply to reviewer 1, the authors will provide a clearer definition of the methods used, especially with regards to the multiple regression trees. The authors thank the reviewer for the additional suggestions. We will look into conducting the stepwise AIC and will add the results in the revised manuscript if necessary.

- I disagree with the author's discussion of "plant mediated HR" in this manuscript. In the introduction, the author's define plant mediated HR as photosynthesis conducted using CO2 respired by surrounding plants instead of CO2 sourced from ambient pools. This variable is not measured at any point in this study and would require isotopic analyses of plant biomass, assuming that plant mediated HR results in significant fractionation of C isotopes so that photosynthate from plant mediated HR would bear a distinct isotopic signature than would photosynthate from ambient sources. While the authors postulate many credible theories as to why the presence of mosses and the functional differences between shrubs and sedges might alter the physical and chemical properties that influence respiration, these ideas should instead be discussed in a section that is dedicated to describing differences in respiration among vegetation types, eliminating the rather confusing term "plant mediated HR".

As stated in the reply to reviewer 1 comments, instead of using the term "plant-mediated HR'', we will discuss respiration more as an association of CO2 with the structure of the peat. For example, with regard to the mosses, we have recycled C as CO2 that is refixed by the mosses to be used in photosynthesis. We will revise the manuscript accordingly to clarify this.

We will also discuss that there are three sources of CO2 belowground which we cannot discriminate: CO2 that is supplied as a substrate by the vascular plants (priming effect), root respiration itself, and heterotrophic respiration by microbial bacteria, etc. that is not associated with the roots.

- As Reviewer 1 mentioned, the results that the authors report are compelling but insufficient to give readers a clear understanding of how the environmental variables measured here influence respiration. The tables and manuscript text should be amended to include correlation coefficients that report the magnitude and direction of the relationships analyzed in this manuscript, and all results should be reported regardless of whether or not the relationships are statistically significant. Insignificant results are interesting too! Other aspects of the tables are confusing as well. Instead of including 2018 and 2019 data in the same columns with different font faces to differentiate them, consider including separate columns for each year (unless you choose to analyze data from both years together, as suggested by Reviewer 1). I also don't understand what the second row of data under some environmental variable labels (i.e. row 2 of data in Table 2) refers to. Table structure must be amended in all tables in this manuscript to improve clarity.

The authors will revise the tables to improve clarity and as stated in the reply to reviewer 1 comments, we will include in the revised manuscript a table of the additional regression parameters and will separate the two years of data in the table. Adding all of the non-significant data though may make the table too busy and we feel it will not add to the paper as these values will not be discussed in the text. We also drew scatter plots as part of the analysis, and if the editor feels these add useful information, we can include them in a supplemental document.

- The figures in this manuscript are often visually unclear or confusing. In Figure 2, the colors used to indicate drying vs. rewetting points are virtually identical and extremely difficult to differentiate. Perhaps change the size, color, and transparency of the points in this figure to allow readers to see differences near the asymptotes where many points are stacked on top of one another. In Figures 3 and 4, the colors for ER and NEE

are also too similar to distinguish, especially when considering that the figures would be much smaller in the final published article. It is also difficult to distinguish between the blue colors used in Figure 5 for the shrub plots.

We will make all the figures clearer with regards to size and color.

- In Figure 5, why not include error bars for AR contribution data points as the authors did in Figures 3 and 4? While connecting the points with lines across the growing season would help readers distinguish temporal trends in AR contributions among your treatments, I suggest averaging AR contributions in each plot across the growing season and then visualizing differences in AR contributions among growing season years and vegetation types using boxplots. These differences can then be verified using an ANOVA test.

As AR is a residual term, we did not think it was possible to include error bars nor to conduct an ANOVA test, but will add the error bars and results of ANOVA test if this is possible.

- For Table 2, I would prefer to see panels of linear regressions that depict the relationships between respiration components and environmental variables. This table of statistical results can then be moved into the appendix.

Reviewer 1 also suggested this and as stated in the reply, we felt that adding all of the non-significant data would make the table too busy and will not add to the paper as these values will not be discussed in the text, but we can include them in a supplemental document along with correlations.

- An important spatial component of bogs that this manuscript largely ignores is the hummock/hollow variation in microtopography. I would suggest reframing the objectives of this study as analyzing temporal/vegetative variation in bog respiration dynamics to reflect your experimental design more accurately.

We examined patterns of respiration in hummocks, which represent 70% of the bog, and incorporated mosses, shrubs and sedges.

Specific line comments:

Line 121: How much time elapsed between the removal of plant biomass and the installation of root exclosures and the first CO2 flux measurements? Were vegetation removal treatments reapplied throughout the two years of measurements?

We will explain this more clearly in the text.

Line 182: Because hysteresis does exist to some degree, and the amount of hysteresis varies among years, why not use VWC measurements as your variable that represents soil moisture conditions instead of WT height?

We do not have SWC measurements for the different treatments, only the data from the probes near the eddy covariance tower. We could show that the relationship between WTD and SWC are correlated though, and that WTD is thus a reasonable surrogate for changes in SWC, though it is different because of the hysteresis present.

Lines 202-208: Be consistent when reporting p-values. I tend to see 3 decimal places for p-values reported, with exact values used instead of simply reporting significance

thresholds.

We will revise accordingly.

Line 216: There's a small typo here, "mater" should probably be "water".

We will revise accordingly.

Line 222: I do not think that you have the evidence to support your claim that variation in rain events (sporadic rain events) drives greater variation in AR among vegetation types. Furthermore, throughout this paragraph, you should report the coefficient of variation more accurately instead of rounding, as well as p-values and F-statistics stemming from an ANOVA that should be used to properly test the differences in AR contributions among vegetation types or among years. Furthermore, reporting your degrees of freedom associated with the F-statistic in these analyses would help the readers understand how many independent measurements are used in your analyses.

We will revise accordingly regarding the reporting of the statistics. Our comments on the impact of sporadic rain events were speculative and we will make it clear that we are not claiming a cause-effect relationship.

Lines 231-235: This paragraph is unnecessary given the use of subheadings in your discussion.

We will revise accordingly.

Lines 240-243: Perhaps remove reference to Moore et al. 2002 and Stewart et al. 2006 because these studies are not directly comparable to your results given differences in measurement methodology, which you note.

We will revise accordingly.

Line 305: When you say "importance of 70%", what is the statistic that you are reporting here, and from which statistical test is this number derived?

Perhaps the word "explanation" should have been used here instead of "importance"? We will revise accordingly.

Lines 322-330: As Reviewer 1 stated in their comments, the relationship between the environmental variables and respiration components discussed in this paragraph likely stem from non-linear relationships between respiration and soil moisture in particular. Using statistical tests beyond linear regressions would be a more appropriate way to test this hypothesis.

As stated in the reply to reviewer 1 comments, we will discuss this in the revised manuscript. We did test for linear and non-linear relationships over the range of our data. The relationships were linear within that range and therefore appropriate for this particular project. We will also point out that others have found non-linear relationships with a different range of data.

Line 338: Other studies such as Rewcastle et al. 2020 (Pedosphere) use different methods of root exclosures that eliminate the possibility of CO2 flux stemming from residual root decomposition, yet also find rather variable HR rates owing to water table and soil moisture differences irrespective of bog microtopography differences.

The authors thank the reviewer for the suggested citation and we will include it in the

paper, although they are dealing with a forested bog rather than a shrub-dominated bog like Mer Bleu.

Line 331-348: As in other sections of this manuscript, the results that you report must be more specific. Report exact p-values instead of significance, and report p-values even for insignificant results. Results from regressions should include correlation coefficients as well, and results from ANOVA tests should include F-statistics with degrees of freedom to communicate replication in your study.

We will revise accordingly.

Line 354: My understanding of the literature surrounding bog decomposition suggests the opposite, that the high degree of secondary compounds in moss litter inhibits microbial activity, while vascular plant litter and root exudates often have a priming effect on microbial activity in bog ecosystems. Evapotranspiration surrounding vascular plants might also increase oxygen availability by lowering the water table in proximity to deeply-rooted plants, again stimulating microbial activity (further supporting the pattern observed by Zeh et al. 2020).

The other papers cited do suggest what we are arguing, although we have no way of confirming our suggestion, so we will present the alternative (Zeh's paper) as a contrast.

Line 375: I would suggest referencing a study other than Hungate et al. 1997 that confirms this ecological principle in bogs rather than grasslands owing to the complex physio-chemical regulation of the carbon cycle in frequently water-saturated ecosystems like bogs.

We will search for a more recent study that was conducted in a bog and the authors thank the reviewer for the suggestion.