

Biogeosciences Discuss., author comment AC1
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Reply on RC1

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-AC1>, 2021

This study used field measurements of CO₂ fluxes from control and vegetation removal plots to estimate ecosystem respiration, heterotrophic respiration (HR), and autotrophic respiration (AR) in an ombrotrophic bog ecosystem over two growing seasons. The study analyzed the correlations of temperature and water table with respiration fluxes for the two years. The sensitivity of different respiration fluxes to environmental factors is an important question with implications for understanding ecosystem carbon flux responses to changing climate, as is well explained in the Introduction. I thought the study was well designed and produced a valuable dataset for understanding these fluxes and their controls in bog ecosystems.

Thank you for your comments.

In my opinion the statistical analysis portion of the study had some weaknesses that could be addressed.

We will address each of the comments in order.

First, some of the statistical methods are not explained in enough detail in the methods section. In particular, it's not clear how the "multiple regression trees" were conducted or how this method was defined. A full explanation and/or citation for that method would be helpful.

In the revised manuscript, we will insert more details and explanations of the statistical methods that we used.

Second, the statistical methods rely on linear regressions. Moisture interactions with respiration in particular are often nonlinear (a threshold dependence is suggested in the Discussion, for example) so I would recommend testing whether linear relationships are an appropriate model for the processes of interest and, if not, applying nonlinear methods where appropriate.

We will discuss this in the revised manuscript. We did test for linear and non-linear relationships over the range of our empirical observed 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.

Third, it's not clear why the two years were analyzed separately instead of combined as a single dataset. Since it was all the same site and treatments, it would make sense to treat the whole time series as a common dataset and potentially this would give the overall statistical analysis more power. While it is interesting to see if some relationships differed across years, I think a good default assumption would be that the site should behave similarly in different years unless there is a compelling reason to expect otherwise. I suggest conducting the statistical analysis for the whole dataset across both years and perhaps contrasting those results with analyses for individual years if there are significant differences.

We will discuss this in the revised manuscript, but we looked at the relationships across two different years. However, the years were significantly different, and it raises the question if the data can be treated as being from the same population.

Finally, the results of the statistical analysis that are present are very limited. Only statistical significance metrics, coefficients of variation, and R² values are shown. This means that the manuscript never reports the direction or slope of the linear relationships and therefore leaves out a lot of potentially useful information. Statistical significance measures on their own are much less informative if they are not matched with information on how the relationships actually looked. I would recommend at minimum including the linear regression parameters (slope and intercept) in a table. Even more useful would be scatter plots with regression lines showing the data and fit relationships for fluxes and environmental factors (especially if some of the relationships were particularly interesting or significant). Overall, it seems like the study generated a useful dataset but did not fully analyze it.

We will include in the revised manuscript a table of the additional regression parameters. We did scatter plots as part of the analysis and if the editor feels these add useful information we can include them in a supplemental document. We did not include them in the body of the article or as supplemental material originally as we felt they did not add significantly to the results presented.

Other comments:

Line 63-68: This explanation of "plant-mediated HR" did not make sense to me. First it is explained as plants fixing carbon that was recently respired from surrounding vegetation. This isn't HR, it's reabsorption of respired CO₂. And I don't see why this is a problem for calculating ER. From the perspective of ecosystem carbon balance, it shouldn't matter if the carbon source for photosynthesis came from ecosystem respiration or from the atmosphere — aren't they all carbon molecules in the end? Does it make a difference how far they traveled? Later, plant-mediated HR is explained as having to do with root-soil interactions and litter supply, which seems like a different issue from reabsorption of respired CO₂. A different process that could be called "plant-mediated HR" is supply of C to the rhizosphere that is immediately respired by heterotrophic organisms. This explanation is more consistent with the Discussion paragraph on this topic, which is mostly about rhizosphere priming effects. This does seem like an issue for partitioning AR and HR because it is plant-supplied C that would be cut off by removing plants but it is not strictly AR. But this does not fit with the explanation of "plant-mediated HR" in the Introduction text.

We will discuss that there are three sources of CO₂ belowground for 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.

Instead of using the term "plant-mediated HR", we'll discuss respiration more as an association of CO₂ with the structure of the peat. For example, with regards 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.

Line 123-125: The wording here sounds like the vegetation removal happened under dark conditions, but I think what is meant is that CO₂ flux was only measured under dark conditions (not light conditions) in plots where vegetation or mosses were removed. Not that the vegetation removal itself was done in the dark.

We will change the wording in the methods to make it clear that removal was done first then CO₂ fluxes were measured under dark conditions.

Line 124: Plots with mosses removed are later referred to as "shrub-only plots." The same terminology should be used throughout the manuscript.

We will make sure to use the same terminology throughout.

Line 209: The text says that ER and HR were correlated with air and soil temperatures, but based on Table 2 soil T was only significant in one year.

We will make sure to be clear in which year the significant relationships were found.

Line 247: Were the influences positive or negative? And how strong? Only providing statistical significance measures and nothing else leaves out the most important information here

Although it was mentioned in lines 213-215 if the influences were positive or negative, we agree that there was no mention of the strength of these relationships. As stated above, we will include in the table some of the other statistical parameters to show the strength of the relationships.

Line 225: Again, knowing that this interaction was significant is less useful than knowing what the relationship looked like.

Figure 5 can be broken up into two years with a line showing the average in AR contributions so that the variation is more evident, which is actually a suggestion made in another comment below. We agree with this suggestion and will revise the figure as such as well as refer back to this figure in line 225 to make the variations in AR easier to see.

Line 265: The relative influences of soil T and water table on fluxes could be determined from the parameters of the multiple regressions rather than speculating about it based on qualitative looks from the figures as this sentence does.

We will refer back to the statistics when explaining the influences of the environmental variables.

Line 274-275: The relative contributions of AR to ER under different conditions could be shown directly with a scatter plot of the relevant processes, or by referring to parameters of the linear regressions.

As there are already figures of the time series of weather conditions and AR contributions, adding additional scatter plots will not add much to the paper. We will include, though, evidence from the statistical analyses in the text to give more credence to the claims.

Line 276-277: If there is a real statistical connection between AR and environmental drivers, then why would higher variability in environmental drivers cause the relationship to be weaker? Might this suggest that the apparent relationship is due to some other covariate that varies more slowly over the year? Or that respiration responds to environmental drivers at a particular time scale?

You make a good point here! However, with the limited sample size in AR fluxes, the relationship, as it would have been, may not have been captured properly. It may be that the respiration responds at a different time scale than our study period. The way to resolve this would be to use continuous measurements (e.g. automatic chambers), which we do not have for this study. We will revise accordingly.

Line 278-280: A threshold relationship with WT could be shown directly with a scatter plot of WT versus respiration. Also, a threshold response is inherently nonlinear which suggests that linear regression may not provide an accurate picture of the relationship.

We will add a scatterplot as a supplemental figure as stated above, but the relationships were linear within the range of our observed data, so we felt that linear regressions were appropriate.

Line 304: It seems speculative to talk about symbiotic relationships here. The data don't have enough detail to say whether there is a symbiotic component to the observed correlations.

We will move away from using the word "symbiosis" and explain instead that there is a possibility of the mosses and vascular plants having a mutual benefit to one another by their presence in the ecosystem (the vascular plants provide a source of CO₂ that may diffuse through the mosses, while the mosses provide moisture in the water they retain during extended periods of drought).

We do believe with the data we've obtained that this is a valid possibility. But, we will revise the manuscript to be more clear that this is a speculation and that we're not making a conclusive statement.

Line 305-309: This should be in the results section

Agreed, we shall move up to the results section and simply refer to the table in the text here.

Line 315-317: This should be in the results section

Agreed, we shall move up to the results section and simply refer to the table in the text here.

Line 322: Wouldn't this be an interaction term in the multiple regression? The regression would indicate whether the interaction term was significant or not. And conducting the statistics across both years instead of separately by year could give better statistical power.

Indeed the multiple regressions would tell us whether the interaction term was significant or not, but since 2018 was an anomalous year in terms of weather conditions, we believe lumping the data from the two years will only give a spurious relationship.

Figure 1: I think it would be helpful to superimpose continuous measurements of temperature and water table (as lines) along with the dots showing values when fluxes were measured. This would allow those time points to be placed in the context of the

whole time series.

The time series in Figure 1 shows values for the environmental variables taken at the same time as the flux measurements, so the continuous measurements were added as an appendix mainly to contextualize the manual measurements. They do correlate though, if we look at the values on the same day between the manual and continuous measurements. We can add a graph to the appendix to show this if the editor and reviewers think it would be helpful.

Figures 3 and 4: I found these plots difficult to read with all the different colored dots. Connecting the dots with lines or plotting as bars rather than dots might make these figures easier to interpret.

We will try various ways of reporting the fluxes so the figures in the revised manuscript will be easier to interpret.

Figure 5: This figure should have separate panels for the two years (similar to the previous figures) or show one long time series. Plotting them on top of each other makes the plot difficult to read.

Agreed, as stated above. This will be done for the revised manuscript as well as including the average AR contributions so the variation is more prominent.

Table 2 and 3: The bold and italics notation for different years is difficult to read, especially since the order of years is not consistent. Also, there's no reason not to show all the data. These tables should just have a line for each year (two lines per environmental variable) and show all the values (whether statistically significant or not). And, ideally, include statistics over both years of combined data. Also, the regression parameters (slope(s) and intercept) should be included.

We will include the 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 won't add to the paper as these values won't be discussed in the text.