Replies to Reviewer Comments

Reply to Editor

Both reviews made a range of positive suggestions and I think you have proposed a range of important revisions. I am recommending major revisions in light of their comments. In particular, I would like you to further consider the point R1 makes about treating the years as a single dataset. If you consider them to be entirely different, I think you should demonstrate this via some statistical test and/or motivate the text better so the reader is clearer on this analysis. For me, while I have no issue in itself with analysing the years separately, it does raise questions about how general our interpretations can be as the entire dataset is only two years long and variability is marked. This needs to be carefully addressed in revision.

We thank the editor for their suggestions. With regards to treating the dataset as two separate years: 2018 was an anomalously hot year, and the resulting respiration fluxes should be considered as two separate populations. Combining the two years of data would have led to spurious relationships with the environmental variables. One can see this in the supplemental figures added, where for the shrubs especially, respiration was much higher in 2019 for the same temperature even though the slopes and R^2 *values were similar. This was true for relationships between respiration and both soil and air temperature. Although the relationships with temperature were more similar between the years for the sedges, the results would not have been comparable with the shrubs if the dataset came from a different timeframe. So, we decided to keep the analyses of the two years separate.*

The main revisions addressed the concerns of the reviewers regarding the statistical analyses, but we address each question / comment in turn below.

Reply to Reviewer 1

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.

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 included more detailed explanations of the statistical methods used along with citations of other studies that have used these methods (page 6).

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 have discussed in the revised manuscript the linear relationships of respiration fluxes with air temperature and soil temperature and have showed this by including the scatterplots made in the correlation analyses (added as supplemental material) as well as a table that includes the correlations in the appendix. Although there does not seem to be any relationship with WT depth when all the data points are considered, linear or otherwise, there are linear relationships over a certain range of data that coincide with what others have found in the literature, and which was discussed in the original manuscript (see supplemental figures).

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.

2018 was an anomalously hot year, and the resulting respiration fluxes should be considered as two separate populations. Combining the two years of data would have led to spurious relationships with the environmental variables. One can see this in the supplementary figures where for the shrubs especially, respiration was much higher in 2019 for the same temperature even though the slopes and R^2 *values were similar. This was true for relationships between respiration and both soil and air temperature. Although the relationships with temperature were more similar between the years for the sedges, the results would not have been comparable with the shrubs if the dataset came from a different timeframe. So, we decided to keep the analyses of the two years separate (as responded to the editor).*

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 added scatterplots a supplemental material to show the relationships with the environmental variables, along with a table of correlations in the appendix to show the strength of the relationships. We also added individual p-values in Tables 2 and 3 next to the R^2 values, including the non-significant results, as well as p-values, degrees of freedom, and F and t statistics in the text for the ANOVA and sample t-test results.

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 explained that there are three sources of CO_2 belowground for which we cannot discriminate: CO_2 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 discussed respiration more as an association of CO_2 with the structure of the peat. For example, with regards to the mosses, we have recycled C as CO_2 that is fixed by the mosses to be used in photosynthesis. We revised 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 changed the wording in the methods to make it clear that removal was done first then CO_2 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 made 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 made sure to be clear in which year and for which plant type 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 of the original manuscript whether the influences were positive or negative, we agree that there was no mention of the strength of these relationships. As stated above, we included scatterplots as supplementary material as well as a table of correlations to the appendix 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.

With regards to the coefficients of variation, we added that they are calculated by taking the standard deviation/mean, so they are not really associated with a significance level. However, Figure 5 was changed to include boxplots showing yearly averages to more clearly show the difference in the spread of AR contributions between the years and plant types.

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 referred 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 as well as additional scatterplots of respiration fluxes as a function of the environmental variables, adding additional scatter plots will not add much to the paper. We did include 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?

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 revised accordingly to make this clearer.

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 added a scatterplots as supplementary material, but the relationships were linear within a certain range of our observed data that coincided with relationships found in the literature. Otherwise, there was no relationship with WT depth when all of the data points were considered, as stated above.

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 no longer use 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 CO_2 that may diffuse through the mosses, while the mosses provide moisture in the water they retain during extended periods of drought. We think the data support this possibility, but we revised the manuscript to be clear that this is a speculation and that we are not making a conclusive statement.

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

We moved this part up to the results section and simply referred to the table in the text here.

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

We moved this part up to the results section and simply referred 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 temperature, we believe lumping the data from the two years would have only given a spurious relationship, as stated above.

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 decided not to add a graph to the Appendix though as we felt that this would not have added to the paper.

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 played around with the size, colour, etc. of the figures in the revised manuscript. Hopefully they are now 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.

Figure 5 was changed to include boxplots showing yearly averages to more clearly show the difference in the spread of AR contributions between the years and plant types.

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.

As stated above, we added the non-significant data as well as the individual p-values next to the R^2 *values in Tables 2 and 3 and separated the two years of data. We felt that including all of the regression parameters here though would have made the tables too busy. We opted instead to add scatterplots of some of the relationships with slope and intercepts as labels along with a table of the correlations in the appendix.*

Reply to Reviewer 2

This manuscript presents data from a field experiment where CO_2 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.

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 think that separating the environmental predictors of AR and HR into two sections was not a favourable option. We have re-worded some of the section headings though and moved up the last paragraph of section 4.1 to after the original line 254 as well as re-arranged some of the discussion paragraphs. Hopefully the revised manuscript flows 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 above, we provided a clearer definition of the statistical methods used, especially with regards to the regression trees. The authors thank the reviewer for the additional suggestions. We looked into conducting the stepwise AIC but since stepwise regressions are included in the creation of the regression trees, we felt that this would not add much to the manuscript.

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 CO_2 respired by surrounding plants instead of CO_2 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 discussed respiration more as an association of CO_2 with the structure of the peat. For example, regarding the mosses, we have recycled C as CO_2 that is fixed by the mosses to be used in photosynthesis. We revised the manuscript accordingly to clarify this. We also discussed that there are three sources of CO_2 belowground which we cannot discriminate: CO_2 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.

As stated above, we added the non-significant data as well as the individual p-values next to the R^2 values in Tables 2 and 3 and separated the two years of data. We added scatterplots of the relationships with slopes and intercepts as labels, along with a table of correlations in the appendix.

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 played around with the figures with regards to size and color. Hopefully they are clearer.

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.

Figure 5 was changed to include boxplots of yearly averages to show the difference more clearly in means and spread of AR contributions between the years and plant types. We also added error bars to the AR values in the appendix (Figure A2).

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.

As stated above, we added the non-significant data as well as the individual p-values next to the R^2 *values in Tables 2 and 3 and separated the two years of data. We added scatterplots of the relationships with slopes and intercepts as labels, along with a table of correlations in the appendix.*

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 (Lafleur et al. 2003), and incorporated mosses, shrubs, and sedges. We made sure to make this clear in the text.

Specific line comments:

Line 121: How much time elapsed between the removal of plant biomass and the installation of root exclosures and the first CO_2 flux measurements? Were vegetation removal treatments reapplied throughout the two years of measurements?

We explained 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 VWC 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 VWC are correlated though, and that WTD is thus a reasonable surrogate for changes in VWC, though it is different because of the hysteresis present. We added this explanation in the text of the revised manuscript.

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 added individual p-values in the tables next to the R^2 *values (to 3 decimal places) as well as individual p-values in the text for the ANOVA and sample t-test results (since one of the values went out to 4 decimal places, we did this for all p-values reported in the text).*

Line 216: There’s a small typo here, “mater” should probably be “water”.

We revised 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 also added individual p-values, degrees of freedom, and F and t statistics in the text for the ANOVA and sample t-test results. Our comments on the impact of sporadic rain events were speculative and we made 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 removed this paragraph.

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 removed these citations.

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?

We used the word “explanation” here instead of “importance”. We also describe how the statistical test is derived.

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 discussed in the revised manuscript the linear relationships of respiration fluxes with air temperature and soil temperature and showed this by including scatterplots made in the correlation analyses (added as supplementary material). Although there does not seem to be any relationship with WT depth when all of the data points are considered, linear or otherwise, there are linear relationships over a certain range of data that coincide with what others have found in the literature, and which was discussed in the original manuscript (see supplementary material).

Line 338: Other studies such as Rewcastle et al. 2020 (Pedosphere) use different methods of root exclosures that eliminate the possibility of CO_2 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 included it in the paper, although we specify that they are dealing with a forested bog rather than a shrub-dominated bog like Mer Bleue.

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 revised accordingly as stated above.

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).

We did not discuss evapotranspiration as a source of change in WT position as we did not measure this to confirm the possibility. However, the sentence here was simply mixed up by mistake with reference to the priming effect of C. We revised accordingly and both Robroek et al. (2016) and Zeh et al. (2020) found the same thing.

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 added a citation for a more recent study (Fenner and Freeman 2011) that was conducted in a bog and the authors thank the reviewer for the suggestion.