Responses to Reviewer 1:

The revised manuscript has substantially improved the explanation of the methods and results, and the updated figures represent significant improvements in terms of data presentation and readability. I thought the new Figures S1 and S2 were particularly useful for showing the relationships among fluxes and key environmental drivers. The changes to the text and figures also clarify the rationale for separating the analysis of the two years. Overall, I think the revisions to the manuscript have addressed the concerns I raised in my previous review. My main remaining suggestion would be to include Figures S1 and S2 in the main text rather than supplementary material. These figures show very clearly the relationships with carbon fluxes, temperatures, and water table that are a focus of the manuscript, and they provide useful clarity when paired with the time series plots in Figures 3 and 4. I feel that the supplemental figures represent an important component of the manuscript's main message and therefore fit better in the main text.

The authors thank the reviewer for their comments. We have changed Figures S1 and S2 to be Figures 6 and 7 as suggested and have included them in the text.

In Figure S1 and S2, I would also suggest showing which points were from high or low WT periods in all the panels, not just the WT Depth panels. Showing those differences in the plots of fluxes vs. temperature would help to visualize some of the interactions between temperature and water table and make it easier to see how the different ranges of water table conditions coincided with temperature conditions.

We have modified the figures which we hope assuages the concerns of the reviewer.

Other specific comments:

Line 163: "anomalously warm year in many places across the globe" – I would say whether it was an anomalously warm year in this site specifically since other places on the globe don't directly impact these measured fluxes.

Revised accordingly (line 174)

Line 329-330: This conclusion suggests that showing the shrub only plots in scatter plots as in Figure S1 might be a useful addition.

Revised accordingly to include a reference to Figures 6 and 7 (lines 340 - 341).

Responses to Reviewer 2:

This manuscript provides a valuable dataset and analyses that describe carbon fluxes from a bog, portioned into the contributions to ecosystem respiration by autotrophic (AR) and heterotrophic (HR) sources. This dataset itself is unique, and the analyses that link respiration to the influence of water table depth, temperature, and plant community dynamics make valuable contributions to the field. The manuscript, namely the statistical analyses and figures, are much improved from the initial submission. However, I have remaining concerns regarding the interpretation of the results and the discussion section especially regarding the interactions between mosses and vascular plants that affect heterotrophic respiration rates (mainly the last paragraph of section 4.2, starting line 364).

The authors thank the reviewer for their comments and will address each in turn.

After reviewing the original manuscript, both reviewers raised concerns with the authors' discussion of symbiotic relationships between mosses and vascular plants in relation to HR rates, particularly the ability of mosses to metabolize and respire labile carbon from vascular plant root exudates. First, it seems incorrect to discuss respiration from mosses in the context of HR, which by definition includes only respiration from the soil microbial community. Second, the ability of mosses to absorb, metabolize, and respire dissolved carbon from the soil solution is not widely accepted, and the authors give a single source from 1999 to support these speculations. The authors simply do not have the data to make claims regarding the source of the carbon respired by plants in the experimental plots studied here. I suggest instead drawing speculations regarding the effect of mosses on heterotrophic respiration rates from known relationships in the literature that connect the presence of mosses to water table depth/soil moisture and temperature, variables that the authors show significantly shape HR. It seems plausible that mosses could insulate peat from evaporative losses of water, perhaps even establishing cooler, anaerobic conditions beneath mosses that limit microbial activity and HR when moss is present. When moss is removed, the albedo of the bog surface changes so that the bog surface would become warmer, and evaporation would occur more rapidly without the insulative presence of mosses to allow for more rapid aerobic microbial activity. These types of relationships, between mosses and microbial activity and the environmental factors that shape microbial activity, warrant further discussion in the manuscript segments concerning HR and might be more appropriate and scientifically sound than discussing moss respiration from dissolved carbon sources.

We have provided a clearer explanation for the relationship between the mosses and the shrubs at Mer Bleue and provided citations of more recent research that show the possibility of the claims made in this study.

Other minor concerns remain as well, as noted below:

Line 9: "respiration microbial bacteria in soil, fungi, etc.": "respiration by the soil microbial community" would probably be a more concise and accurate description of HR

Revised accordingly (line 9)

Line 11: "and alters allocations of carbon to labile pools with different turnover rates": The relationship between respiration and carbon substrate complexity was not something that was examined in this study and should be removed here and elsewhere in this manuscript to avoid confusing readers regarding the factors analyzed in this relationship. This argument distracts from the more evidencebased conclusions you make regarding the influence of abiotic factors and plant functional type on ecosystem respiration.

Revised accordingly to remove statement (line 10)

Line 20: Here and elsewhere in the manuscript, you discuss different plant water acquisition strategies as an important driver of respiration rates in this system, but what you actually discuss is differences among plant rooting structures; these differences may or may not be actually tied to water acquisition. I think you should refer to these differences as rooting structure differences instead of water acquisition differences because that more accurately describes the arguments you make in your discussion and the literature that you cite as you do not measure water acquisition in this study.

Revised accordingly to describe the differences among rooting structures, but also stating that this indicates a difference in how the plants obtain water resources (line 20)

Line 31: "the dynamics of heterotrophic respiration... is not straightforward" should be "are not straightforward"

Revised accordingly (line 31)

Line 32: "substrate variables": Can you be more specific? I think this covers a very broad spectrum of biogeochemical variables... Substrate quality or complexity? Substrate quantity?

Revised accordingly to include examples (line 32)

Line 36: "...and CO2 that is supplied as a substrate by vascular plants": This sentence is misleading, and the intended point here is unclear. CO2 gas is not provided by plants as a substrate for microbial metabolism.

Revised accordingly to indicate "organic C supplied by plants" (line 37)

Line 44: "Ecosystem Respiration dynamics...": the R in respiration should not be capitalized.

Revised accordingly (line 44)

Line 51: "For example, a positive feedback in climate change...": Several typos and/or confusing word choice make this sentence hard to follow and the intended point unclear.

We rearranged the sentence structure for this paragraph (lines 51-54)

Line 53: "... turn over newly-photosynthesizing C..." should probably be "newlyphotosynthesized C"

Revised accordingly (line 53)

Lines 65-68: "This also indicates a problem in the conceptualization of ER: one cannot...": This sentence is very unclear and perhaps should be broken into several separate points. It's hard to understand what connection the authors are trying to make between CO2 released during HR and litter production as an intermediate contribution to HR... Carbon respirated by the microbial community is still considered HR regardless of what the ultimate fate of those CO2 molecules are.

We revised this paragraph to say there is a problem in our conceptualization of HR, not ER. While the typical definition of HR includes only respiration from the soil microbial community, as the reviewer *suggests above, we are claiming that HR is more complicated than that. Studies have suggested for a while now (citations provided in text) that HR is more related to vegetation dynamics and that root-soil interactions play a major role (lines 65-72).*

Line 83: Mean annual precipitation is listed as 943 mm, but this sentence states that annual snowfall is 223 cm. Are the units for snowfall accurate? I realize that there is some discrepancy for measured snowfall and realized water increments, but are those differences an order of magnitude apart?

These are the reported values on Environment and Climate Change Canada's website for the Canadian climate normal from 1981-2010. Snow is measured in cm with a ruler, then precipitation is reported as the water equivalent from all sources in mm.

Line 96: "...hence supporting a greater belowground biomass than sedges": This conclusion doesn't necessarily follow logically from the information you presented in this paragraph and at the very least, deserves its own reference. Why would the shallower root structure of shrubs necessarily equate to greater below ground biomass than the more deeply rooted sedges?

Revised accordingly to explain it is the relative BG to AG biomass that is greater in shrubs than in sedges, not the magnitude of the biomass (line 96)

Line 99: Live moss biomass still counts as aboveground biomass, so it seems illogical to consider stems buried by living moss to be considered belowground biomass.

As mentioned in line 97 and 100-101, it is the stems of the shrubs that are buried with the help of the mosses, not the live moss biomass.

Line 103: The conclusion made in the first sentence of this paragraph seems unsupported by the information provided. You state that sedges have a competitive advantage over shrubs yet start this sentence by stating that shrubs thrive in both dry and wet conditions. These statements seem to contradict one another.

Revised accordingly to include that while shrubs are adaptable to a changing climate, sedges have a competitive advantage as they can handle more extreme fluctuations in soil moisture (lines 105-106).

Line 110: Does the placement of the 9 collars represent 1 collar per plot? If so, how big are these plots within the shrub and sedge sections? As currently written, this seems to indicate pseudoreplication, where you measure respiration in 9 places with a single shrub and a single sedge plot.

We restructured this section of the methods to be clearer on the manual plot set up (lines 110 - 126)

Line 122: How big are each of the subplots with the vegetation manipulations?

We revised accordingly to make this clearer (line 112).

Line 127: Were the HR plots trenched to kill roots from plants that grow outside of the plots but close enough to produce roots that grow belowground into the plot area?

Revised accordingly to clarify the root exclosure set up (lines 128-129)

Line 139: "regression equations": linear models? Line 195: "Although it is important to acknowledge the hysteresis present...": I think you should describe why acknowledging hysteresis is important. It essentially means that WT depth is a better predictor of VWC in 2019 than in 2018.

Revised accordingly to include linear regressions (line 142). We made a variety of scatterplots (not shown) to make sure there were no non-linear relationships between CO_2 concentrations over time.

Section 3.2: Listing these results in the text is rather difficult to trudge through and derive any real sense of comparison between respiration rates in different years, with different plant manipulations, and between different plant types. Lit the results in a table instead, and use this section to describe comparisons between respiration rates among your treatments... i.e., Shrub respiration was x% greater in 2018 etc.

We do not want to change this and refer to figures that clearly show the trends. However, we did compile the results in a table (table 2) and refer to the table in these paragraphs.

Line 281: You mention "land-use" as a factor shaping respiration rates at many different points in this manuscript, but it is certainly not a factor that you explore in this experiment... I would advocate for removing reference to land-use from this manuscript not because it's not important, but because you don't investigate the influence of land use in your study, but you do investigate the influence of environmental and vegetative properties which should be your sole focus.

We've removed all mention of land-use from the text as we agree with the reviewer that this was not studied.

Line 320: Have you considered the fact that water table differences between your vegetation types alone might explain the vegetative differences in respiration rates? When the water table is higher, more deeply rooted plants will respire in totally saturated peat conditions, and a large portion of that respired CO2 will be dissolved in the soil solution instead of being released from the soil as gaseous CO2. This would mean that when the water table is higher, AR is automatically lower because at least a portion of deep root respiration won't be accounted for in your gaseous measurements.

This is a good point made by the reviewer, but the fact that the interaction between temperature and moisture explains a large portion of the variability in respiration rates, suggests that water table depth is not the only factor.

Line 332: "...which affects mainly the surface would influence the shrubs'....": I think it's important in this sentence to remind readers that surface changes affect shrubs and environmental changes deeper in the peat profile would affect sedges due to differences in the rooting depths of these two types of plants.

Revised accordingly (lines 340 – 341)

Line 350: Greater HR in drier periods may have more to do with the relationship between microbial activity and soil aeration/oxygen availability than the physiology of mosses and vascular plant/moss interactions.

Revised accordingly (lines 344 – 345)

Line 358: "...no difference between the two years in our study too though...": No difference in which variables?

Revised accordingly (line 365)

Line 364: "...why the respiration values...": Which respiration values?

Revised accordingly (lines 371 - 372)

Line 376: "...they were no longer able to benefit from this priming effect.": But microbial activity should also be stimulated by rhizodeposition/priming from vascular plants...

This sentence does not imply that no microbial activity is occurring; we suggest that the mosses are not benefiting from the change in vegetation cover.

Lines 385-386: "Especially with regards to unveiling the presence of the intermediate form of respiration we deemed plant-mediated HR..": Compared to the original version of this manuscript, all other mention of plant-mediated HR has been removed according to previous reviewer suggestions. Furthermore, I disagree completely that the authors have unveiled an intermediate contribution to ecosystem respiration. As stated above, the authors have no evidence to support the idea that mosses are essentially recycling CO_2 respired by microbes or by the surrounding vascular plants, and regardless, respiration stemming from mosses is not included in the category of heterotrophic respiration, which is entirely attributed to microbial activity.

Revised accordingly to refer to plant-associated HR rather than plant-mediated HR (line 396). And as stated above, we provided more evidence of the vegetation influence on HR in the text. We suggest this is plausible and that our conceptualization of HR needs to be changed (e.g., lines 386 – 389).

Lines 390-393: The reference to soil nutrient dynamics seems odd here, if not totally unrelated to the rest of the information discussed at length in this manuscript. This is the first time soil nutrients are mentioned as a driver of soil respiration and seem out of place in a conclusion meant to summarize this manuscript's findings.

We were suggesting something future studies can explore since it has been shown in many studies that moisture and nutrient dynamics are linked.