Dear Professor Wang,

Many thanks for considering our manuscript for publication in Biogeosciences. The review helped a lot to improve our first version, and we hope that this revised version of the manuscript now fulfils the demands for publication.

GENERAL COMMENTS & KEY CONCERNS This paper is generally well-written and the science competently executed, with clearly interpretable results. The work presented here is of broad interest to investigators concerned with the temperature & moisture responses of soil heterotrophic respiration, with wide applicability towards process-based modelling and understanding the C sequestration potential of different European soils. The factorial experimental design provides insight into the interactive or potential synergistic effects of temperature and moisture, allowing us to develop better mechanistic insights into soil respiration dynamics. I did, however, have a few key concerns or questions that arose upon reading the manuscript.
First, it would be useful if the authors were able to better illustrate in their Discussion what was novel or exciting about their work, as there are plenty of studies about heterotrophic soil respiration and land-use change. What was unique or particularly insightful about the findings that the authors presented here? How will this information help develop the state-of-the-art? In order for this paper to have more impact, and not simply appear to be incremental science, I would suggest that the authors develop the novelty of their work in the Discussion.

We thank the reviewer for pointing out that the novelty of our work was not illustrated clear enough. We corrected various sections throughout the manuscript as explained below:

Page 2, lines 8-9: We developed a new approach to calculate moisture sensitivity (MS) of CO_2 efflux. MS was calculated as the slope of a polynomial function of second degree.
Page 4, lines 4-5: Opposing results can be found in literature about the interdependencies between the factors temperature and moisture and their sensitivities of heterotrophic soil respiration.
Page 11, lines 15-26: Additionally to the Gaussian model equation we applied the Arrhenius function ($R(T) = R_0 * e^{aT^{-1}}$) to our results which showed similar trends but unrealistic Q_{10} values at temperatures below 8°C (Q_{10} ranging between 20 and 2000).

A lot of opposing literature exists on how temperature sensitivity relates to other factors like moisture content or land-use. Opposing results are often due to differing initial starting points, assumptions or interfering factors in field measurements (seasonality, autotrophic respiration, etc.). Kirschbaum (2006) and Lützow and Kögel-Knabner (2009) considered laboratory incubations to provide the best and least biased basis for estimating the temperature sensitivity of organic matter decomposition. In our laboratory incubation study we try to find an answer to this problem. We found that temperature *sensitivity converged towards 2 as temperature increased for all moisture contents at all sites investigated. We also found that…*

Page 13, line 18: We found a new approach to calculate moisture sensitivities. In our study moisture sensitivity…

Page 14, lines 10-12: However, we found moisture sensitivity to be positively correlated with temperature for both arable lands which both showed the highest bulk densities of all sites (> 1.00 g m^{-3}; Table 2).

Page 14, lines 20-22: This might be a reason for the positive relationship between moisture sensitivity and temperature we found at arable lands.

Second, are the authors concerned about possible hysteresis based on the methodology they used for adjusting the moisture contents of their soils (see page 6, lines 8-19)? The authors either added water or dried down their samples, depending on the desired target moisture content. However, it is possible that respiration may show hysteresis during dry-down or wet-up; a common phenomenon observed in arid/semi-arid systems or soils that experience wetting-drying cycles. One common technique to avoid the issue of hysteresis is to dry all soils down to a common moisture content, and then re-wet them to desired levels. This avoids the issue that biogeochemical process rates may differ on the dry-down or wet-up phases of the moisture cycle. Do the authors have any data or an explanation to defend their choice of methodology?

*We agree with the reviewer about the possibility that respiration may show hysteresis during dry-down or wet-up and therefore might differ on the dry-down and wet-up phases of the moisture cycle. However, due to the inclusion of peatland soil samples in our experiment the common technique mentioned above (*dry all soils down to a common moisture content, and then re-wet them to desired levels*) was not possible. Peatland soils have a different structure to other soils analysed here, this structure may be destroyed after harsh drought. In order to handle all soils equally and leave them as natural as possible we decided to take the fresh soils at ambient moisture content and either add water or dry down our samples to the starting point, even if we knew that we might miss any hysteresis effects. We now mention this weakness of our approach on page 6 lines 18-24.*

Third, did the authors consider using more sophisticated multiple regression, analysis of covariance, or mixed models to analyse their data? Given that the authors also had data on total C, total N, inorganic N, conductivity and pH, it would be useful to know what proportion of the variance in CO_2 fluxes was explained by these environmental variables.

We didn't consider the results of our analysis of covariance with the additionally measured soil characteristic data (on total C, total N, inorganic N, conductivity and pH; as shown in Table 2) as we did not measure soil characteristic data of each soil core that we measured for CO_2 (150 measured CO_2 fluxes per site, but only 3 replicates for each soil parameter). However, we tried to relate mean CO_2 fluxes over all temperatures and moisture contents to the 3 replicates per plot by means of analysis of covariance. These new results showed conductivity to be the only factor influencing CO_2 fluxes (57% proportion of the variance), which didn't reveal any new insights into our study results. Therefore, we found these results of covariance not to be meaningful enough for publication and stayed focused on temperature and moisture sensitivities.
Page 8, lines 14-16: Analysis of covariance was performed among soil characteristic data and mean CO_2 values over all temperatures and moisture contents did not reveal any significant correlations (data not shown).

Fourth, were the authors able to ascertain or control for soil texture/mineralogy? Soil texture/mineralogy can impact the amount of soil C storage and soil organic matter dynamics, and it would be useful to know if texture played a role in explaining C fluxes from the soils studied here.

We agree with the reviewer about the impact of soil texture/mineralogy on soil C storage and soil organic matter dynamics. We didn´t consider measuring soil texture/mineralogy due to the inclusion of peatland soils in our experiment. As peatland soils were purely organic and in order to stay consistent over all soil types, we did not include soil texture/mineralogy in our statistical analyses focused on temperature and moisture sensitivities.

Other specific comments are discussed in the section below.

SPECIFIC COMMENTS:
Page 1, lines 20-23: Revise sentence to read: "Intact soil cores were incubated in the laboratory in a two-way factorial design, with temperature (5, 10, 15, 20 and 25_C) and water filled pore space (5, 22 20, 40, 60 and 80 %; abbreviated WFPS) as the independent variables, while CO_2 flux was the response variable. The latter was measured with an automated laboratory incubation measurement system.

We corrected accordingly.

Page 6, lines 8-10: It would be useful for the authors to briefly describe (in 1 or 2 sentences) how they determined water-filled pore space from the gravimetric moisture measurements.

Page 6, lines 9-11: Water field pore space (WFPS) was determined by dividing volume percent water through porosity. Porosity was calculated with soil density and particle density.

Page 6, line 21: Spelling error; revise “ration” to “ratio.”

We corrected accordingly.

Page 6, line 23: Consider changing “conducting” to “conductivity,” as the convention is to refer to these instruments at “conductivity” meters.

We corrected accordingly.

Page 7, lines 19-24: Were data with non-normal distributions and/or unequal variances transformed for parametric analysis.

Yes, they were.
Page 8, lines 3-4: Data with non-normal distributions and/or unequal variances were transformed (LOG; square root) for parametric analysis.

Page 12, line 17: Revise the sentence to read: “due to the higher activation energy of recalcitrant substrates...” â˘A

We corrected accordingly.

Page 12, lines 19-20: Please revise the sentence: “Not the absolute amount of â˘A´lcarbon dioxide increases at this moisture range as NL-Spe...” The way this sentence is phrased is awkward and the transition from the previous sentence is inelegant.

We decided to skip the sentence after all as we realized that it rather accounted for confusion than for the comprehension of our results.

Page 13, line 20: Spelling error; “Therefore” and not “Therefor”.

We corrected accordingly.

Page 14, lines 5-14: This section needs to be re-worked slightly in order to acknowledge the speculative nature of the points made here; the potential wider implications for future environmental change are plausible, with the proviso that the response observed in this study holds true for both short-term changes (like those manipulations performed here) as well as for longer-term shifts. However, if the system begins to acclimate or adapt in the longer-term, then the changes proposed here may be more pronounced or damped from what was observed in this study.

We thank the reviewer for enlarging our conclusion section. We corrected accordingly.

Page 15, lines 3-6: The responses observed in this study hold true for both, short-term changes (like those manipulations performed here) as well as for longer-term shifts. However, investigations on acclimation or adaptation of ecosystem processes to climate change in the longer-term might lead to different responses and institutes room for future research.

Page 25, Figure 1: Please consider whether or not presenting these data with trendlines is the best means of representing the results. My concern is that the inclusion of trend lines implies that respiration responds to temperature or moisture in exactly the way suggested by the trend lines, whereas this level of certainty may not in fact exist. Alternatively, I would propose showing the data using boxplots without trend lines, as this would enable the reader to see the spread of the data for different treatments.

We thank the reviewer for the suggestion of using boxplots for showing our results. However, when doing so the possibility of showing all our measured CO_2 values at every temperature and moisture content investigated disappeared due to overlapping data points. As we conducted regression analysis with quadratic functions we found trend lines to be most representative for the outcome of our results, namely that the relationship of CO_2 and moisture content can be explained by a quadratic function. However, as we understand the point that the reviewer is trying to make we changed solid trend lines into dashed trend lines to avoid the implication the relationship of CO_2 and moisture content is exactly as the lines show. Additionally shown standard errors show the deviation of mean CO_2 fluxes. We hope this will lead to a satisfying representation of our results.
Figure 1 corrected accordingly.

Page 27, Figure 3: See point above about Figure 1; are the trend lines shown here appropriate?

See answer above.