

# ***Interactive comment on “Interdependencies between temperature and moisture sensitivities of CO<sub>2</sub> emissions in European land ecosystems” by C. Gritsch et al.***

## **Anonymous Referee #1**

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**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

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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.

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?

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<sub>2</sub> fluxes was explained by these environmental variables.

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.

Other specific comments are discussed in the section below.

SPECIFIC COMMENTS: Page 1, lines 20-23: Revise sentence to read: "Intact soil

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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<sub>2</sub> flux was the response variable. The latter was measured with an automated laboratory incubation measurement system.

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

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

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

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

Page 12, lines 19-20: Please revise the sentence: “Not the absolute amount of carbon 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.

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

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.

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Page 25, Figure 1: Please consider whether or not presenting these data with trend lines 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.

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

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