We thank associate editor Erika Buscardo and the three reviewers for their comments. Our responses are reported below in normal font, while the editor's and reviewers' comments are in italic.

Responses to the associate editor

I have read your point-by-point rebuttal letter in response to the comments of the four reviewers and I am happy for you to proceed and post the revised version. In your revision acknowledge and discuss all the limitations of your study that preclude the generalisation of the results (e.g. factors that can affect soil respiration after rewetting and were not considered in the study, absence of plants and consequent carbon limitation, soil drainage in laboratory vs field conditions, field vs laboratory soil rate respiration units).

Our response to the reviewers' comments are reported below, focusing on the actual changes made in the revised manuscript. We have left extended discussions in the individual responses published during the open review phase. In addition to addressing the reviewers' comments, we also edited the text elsewhere to improve clarity and consistency in the use of symbols and terms.

Responses to Reviewer #1

I enjoyed reading this paper, thank you. Comparing field vs lab soil rewetting responses is challenging, since the hierarchy of the drivers of the multiple processes involved is expected to change, as well as the scale at which they can affect these processes, not to mention the analysis snags that arise when aggregating different datasets. The study is timely, proposes a solid analysis, is concise and well written.

We thank you for this positive feedback!

My major concern is the absence of plants in the study, which echoes the importance of SOC in the results. Plant C inputs to the soil through rhizodeposition are a major driver of soil microbial activity, thereby of soil biogeochemical cycling (see section "Importance of plants" in Barnard et al. 2020). I expect C limitation (or even starvation) to strongly impact soil processes, and to push SOC forward in the hierarchy of drivers. As a consequence, the absence of plants strongly limits the generalization of the study. This tends to be forgotten as the discussion develops, and appears to be totally ignored in the conclusion for example. This is reinforced by the semantic nature of "field site". The field data originates from trenched plots, which the authors specify ensures only heterotrophic respiration (L100), yet this also ensures no labile C inputs to the soil, although it is not specified. There are few field sites, sensu natural condition sites, that are devoid of plant labile C input at some point in time (of course some periods preclude plant activity, eg extreme heat, drought or cold, which however bring many biological and biogeochemical processes to a halt or close to it), hence the term "field site", albeit correct here, can be somewhat misleading and contribute to the trend towards an over-generalization of the results that climaxes in the conclusion.

The reviewer is rightly pointing out that plants input can strongly impacts soil heterotrophic respiration after rewetting and therefore the terminology of "field site" may be misleading– we fully agree. Labile carbon inputs from plants to soils may depend on multiple drivers, and there is no general contribution percentage yet (Li et al., 2024; Wang et al., 2021). Without data, we prefer not to generalize our results to the field conditions with vegetation, but acknowledge that this work focuses only on the heterotrophic respirations.

Here we make this point more explicit in Methods by writing “Trenching separates the roots from the nearby vegetation, ensuring that only heterotrophic respiration is included in the measured rates. By halting the flow of carbon from the plants to the rhizosphere, trenching also reduces microbial respiration fuelled by root exudates, but it also increases the contribution of decaying roots to the respiration rate. The choice to only consider trenched plots limits the scope of our analysis to soils without live plants, but allows comparing field and laboratory data.” Please see lines 102 to 106.

In the Discussion we also acknowledged the role of plant by adding “It should be also noted that the SOC effects on heterotrophic respiration after rewetting in natural field conditions are related to labile carbon input from plants. In contrast, no labile carbon was added in the laboratory studies we considered. To allow comparing laboratory and field data, we used field respiration rates from trenched plots in the absence of plants. A more complete assessment of SOC effects on respiration rates should consider in both laboratory incubations and field studies the contribution of labile carbon from plants.” in lines 282 to 286.

In section 4.2 Uncertainties, we further acknowledged the limitation of this study in the absence of this study by adding “We did not include vegetation types in the data analysis, although plants affect microbial processes and thus the respiration pulses. However, laboratory incubations to study DRW responses are conducted in the absence of vegetation, which forced us to only use field data from trenched plots where roots were not connected to any living plant. Lacking data from incubations with plants, we can only acknowledge that the absence of vegetation limits the generality of our results.” Please see lines 408 to 411.

Regarding the terminology issue, we plan to retain the term “field site” and clarify that heterotrophic respiration rates after rewetting in field conditions in this study are in the absence of labile carbon input. We did this when we first introduced the concepts of field respiration by adding “(i.e., in trenched plots without roots) in line 41.

Soil drainage is commonly observed to differ greatly between lab and field soil experiments, and could contribute to explaining the results in the “Drier soils…” and “The effects of rewetting…” sections of the discussion. Lab conditions, even using undisturbed soil cores, result in preferential flow paths on the sides of the cores for example. Since the soil cores (or samples) in the lab are typically from the soil top layers, upon rewetting, water will more easily reach the bottom of the soil column and ultimately rewet the soil from both top and bottom, resulting in a more homogeneous and efficient rewetting that in the field. In the field, water tends to drain down the soil column and the rewetting effect is not concentrated in the top soil layers. Monitoring soil water content is sometimes not enough to take this effect into account, since it is often performed on a whole-sample weight basis in the lab (or even based in the amount of water added) as opposed to probes or soil sampling in the field, that cannot capture the soil water content of the entire column of soil.

We agree that rewetting is more homogeneous and efficient in the laboratory than in the field. Our rationale for using soil moisture data from the uppermost moisture sensors is that the majority of the CO_2 measured in the respiration chambers is probably from the top soil (higher SOC and microbial activity). While some preferential flow may occur so that soil wetting is not initially homogeneously distributed during a rain event, we speculate that over the 48-hour time window we considered to define the rewetting respiration pulse, soil moisture may be redistributed in the soils and thus wetting becomes more homogeneous. We have added this difference in soil drainage into Discussion by adding the content below

“In addition, soil rewetting is not always characterized by a uniform soil moisture increment—the wetting front propagates vertically downwards and from the macropores laterally into the bulk soil, resulting in heterogeneous conditions at least during the initial rewetting phase. As laboratory soils are more uniform than soils in natural conditions, laboratory rewetting can lead to homogenous moisture faster compared to rewetting in the field. This difference in water redistribution might be the main reason for the different effects of rewetting intensity on respiration in laboratory vs. field conditions. However, we argue that averaging respiration over 48 hours has helped to reduce this issue”. Please see line 346 to 351.

The rationale for the 48h timeframe choice for mean respiration rate is extensively justified (L82). Did the authors poke around the dataset using a different timeframe to see what the results look like, even on a subset of the data? This approach would be somewhat similar to a sensitivity analysis to gauge the effects of set parameters on the results, and could be useful to ascertain that the choices made are robust, in a similar way to what is presented for Dthetarewet (L182).

This is a very valid point, which we have discussed among the co-authors as well. A shorter time frame would also allow testing hypothesis on the delays of the respiration pulse under different soils or climates. We reduced the time frame in the field data sets thanks to the fact that measurements are at high frequency, and we ran a preliminary analysis to address the reviewer’s comment. The test results obtained using 24 hours are consistent with the current results obtained using 48 hours. Specifically, the median heterotrophic respiration rate after rewetting in the field (FR) was 674.7 μmol C m^{-2} h^{-1} for the 24 hour time frame, greater than the 593.1 μmol C m^{-2} h^{-1} for the 48 hour time frame (as expected since the respiration pulse often starts decreasing after a day). The random forest regressions explain a similar percentage (80% for 24 hours and 79% for 48 hours) of the variance of the log-transformed FR. The importance rankings using 24 hours are exactly the same as the current importance rankings, see Fig. R1 below. This evidence therefore confirms that our results are robust—at least for the field data. **We have also reported this figure in Appendix A as Fig A02**. Besides, we explained that this test was less applicable with laboratory data. In the revision, we elaborated on the above points in the section 4.2 Uncertainties by adding a paragraph in lines 382 to 392 as follows

“We would expect that the results obtained by averaging respiration over 48 hours may differ from those obtained using a shorter averaging time interval. This is because intense drying and rewetting events in the laboratory can cause a delay in the respiration pulse (Li et al., 2023a; Meisner et al., 2017), and sometimes the delay time can even exceed two days (Li et al., 2023b). To test if the results were sensitive to our choice of the respiration averaging time interval, we reduced the time interval in the field data sets, where measurements were at sufficiently high frequency. The ranking of the drivers of the mean respiration rates over 24 hours was the same as that for the mean respiration rates over 48 hours, suggesting that our choice of the time frame might have limited impacts on the results (Fig. A02). This test is not feasible for the laboratory datasets because only about half of the selected laboratory studies allow calculating respiration rates during the first 24 hours after rewetting. Using only this subset of studies to test the consistency of the results is not ideal because the distribution of the drivers of respiration for this subset differs from that of the whole data set. Therefore, we did not proceed further with the laboratory data”

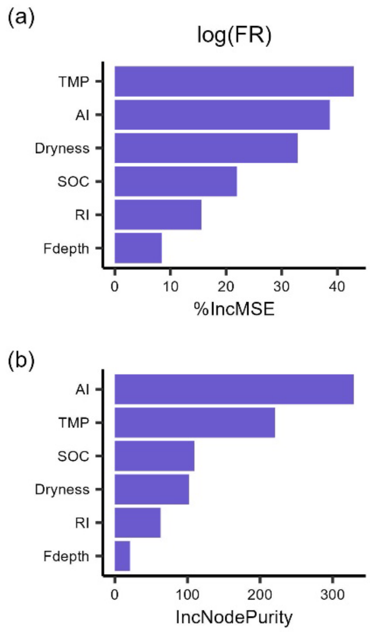

Figure R1: The importance ranking of predictors for mean respiration rates during 24 hours after rewetting, from field (FR) measurements, based on random forest models using %IncMSE (a) and IncNodePurity (b). Predictors include soil organic content (SOC), aridity index (AI), soil dryness, rewetting intensity (RI), soil temperature for FR (TMP), and soil moisture sensor depth for FR (Fdepth). This ranking is the same as we obtained when considering a 48 hour time frame.

Specific comments

L129 Could you please better explain why Dthetarewet and Dthetatolerance were set at 25% and 12.5% of soil moisture, respectively?

This is because the daily moisture variations of these two datasets can be as large as 12.5% of the moisture range. However, these fluctuations are not due to rainfall or rewetting events, but are actually fluctuations in the drying (no rewetting) period. It is possible that these fluctuations are driven by water redistribution by roots, or they could be spurious effects driven by temperature fluctuations. Based on this, we set 12.5% of the moisture range (see Fig. 1) as the largest variation in the drying period that can be tolerated, and the minimum soil moisture increment to be considered a candidate rewetting event as 25%.We clarify this issue in a revision by adding "This choice allows to reach a balance in these two datasets between the number of rewetting events retained in the analysis and their quality (i.e., how clearly they can be identified)" in line 153 to 154.

L298 Please replace "samples" by "cores" or "monoliths".

Yes, we have replaced "samples" with "cores" in the revised manuscript

Technical corrections

The list of lab studies is duplicated.

These references were thoroughly checked and the duplicated references were removed.

Responses to Reviewer #2

This article aims to validate whether the research findings on the drivers of respiration pulses after soil drying and rewetting (DRW) in the laboratory can be applied to field measurements. By integrating laboratory and field datasets, it compares the effects of multiple factors on soil respiration, which is of great scientific significance. The overall logic of the article is clear, the methods are reasonable, but there are still some areas that can be improved.

We thank you for this positive feedback!

Among them, the biggest problem / limitation of the MS is the small amount of data, which leads to a mismatch between the data of laboratory experiments and field experiments. If the amount of data is large, the uncertainty / error in this regard can be reduced. Therefore, the author needs to be cautious when drawing conclusions and should discuss and explain in the discussion.

We fully agree that the discrepancy between laboratory and field data could be an issue. This issue has been acknowledged in the current manuscript. For example, when we reported the results, we did not make a quantitative comparison of the insights obtained from laboratory data and the field data, but rather a qualitative comparison. We have added in Conclusion "Our comparison is based on nearly 40 laboratory studies, but only 6 field studies, so that the sample size of the data differs between laboratory and field datasets, and the overlap of the drivers we selected is not complete". Please see lines 441 to 443.

It is recommended to modify the title to: "Comparative Validation of Laboratory Insights on Soil Rewetting Respiration Pulse Drivers: Evidence from Field Measurements". Such a title emphasizes the process of comparative validation and highlights that the research results are based on evidence from field measurements, which can attract readers' attention to the core findings of the research more effectively.

Thank you! In the revision we have changed the title to "Validating laboratory predictions of rewetting respiration pulses using field data" to achieve a balance of conciseness and information.

At the same time, it is recommended to add some content about the significance of this research in the writing process, such as the importance of this research for the model simulation and future prediction of the DRW process; in terms of practical applications, the research results can provide a scientific basis for soil management and climate change adaptation strategies.

Yes, we have added the 4.3 Implication section in Discussion in response to this comment as below

The validation of laboratory findings on the drivers of the rewetting pulses with field measurements is necessary because laboratory data are often used for prediction of soil carbon stocks with mathematical models. Incorporating these results into model simulations could help improve the accuracy of global carbon predictions, especially for models that neglect rewetting pulses. In fact, most models of soil carbon cycling assume that respiration is a function of soil moisture (Bauer et al. 2008), but not of moisture changes. Therefore, such models describe how respiration varies when gradual variations of soil moisture occur, such as during drying, while neglecting the large respiration pulses occurring at rewetting. To model rewetting pulses, models need to include processes causing accumulation of bioavailable carbon during drying or release of labile substrates at rewetting (e.g., Brangari et al. 2020), but these processes are not easy to represent in a mechanistic way. One could argue that an empirical approach based on data such as those analyzed here could offer an alternative to roughly estimate the amount of carbon emitted at rewetting as a function of SOC, temperature, or other drivers.

When dealing with respiration rate and soil moisture data in different units, although the author believes that it does not affect the results, this treatment method may conceal some potential information or affect the accuracy of the model. Therefore, it is recommended that the author unify the units before analysis.

Having unified units for soil moisture and respiration rate and would be the ideal case for this work, we agree.

To convert the field respiration rate unit (μmol C m^{-2} h^{-1}) to the laboratory respiration rate unit (μg C $g^{-1}h^{-1}$), we would need to know the soil depth contributing to respiration in the field and soil bulk density information. In fact, the CO_2 efflux at the surface is equal to the CO_2 production rate per unit soil mass multiplied by contributing soil depth and by the soil bulk density. To assess if the unit conversion for respiration affects the results, we tried to convert the field measured respiration to units used in the laboratory incubations. This required some assumptions.

Because soil volume contributing to the measured respiration fluxes in the field is unknown (an issue also raised by reviewer 3) we assumed that the soil depth contributing to respiration is the same across six field sites (equal to 10 cm). In addition, we collected the bulk density information of these six sites in the literature (three values were estimated based on soil texture information), in order to make this unit conversion possible. The collected soil texture and bulk density information have been added in the Supplement 1.

The results showed that after unit conversion, the importance of temperature and climate background are still there, while SOC became a more important driver of field respiration rate (Fig. R2). This result indicates that, with our current assumption of contributing soil depth and bulk density information, different units may lead to slightly different conclusions. However, this assumption is uncertain and therefore the results obtained might not be robust. In fact, the contributing soil depth may differ between sites. This could further affect the comparison of drivers between laboratory and field data, as soil properties typically show vertical variation, and soil drying and rewetting occur at different rates along the vertical soil profile.

To assess the possible effects of our assumptions, we performed a sensitivity test by extracting contributing depth and bulk density from distributions centered around the estimated values at the six sites. The results showed that the importance of temperature and aridity index were still there (Fig. R3), which is consistent with our results and confirms the robustness of our results. **We have also reported this figure in Appendix A as Fig A03.**

To clarify the robustness of our results and in response to the reviewer's comment, besides reporting the sensitivity result in the Appendix A, we explained how we did the sensitivity test in the new section 2.3 Comparability of laboratory and field data, in lines 121 to 128:

“Respiration rates were normalized by dry soil mass in the laboratory studies and by ground area in the field studies. Respiration per unit soil mass can be converted to respiration per unit ground area by multiplying by the soil bulk density and the soil depth contributing to the measured respiration rate. Bulk density was not reported in some of the field studies, and the depth contributing to respiration is not known. Therefore, we performed a sensitivity analysis by extracting the contributing depth and bulk density from distributions centered around the estimated values at the six sites. This approach allows us to test the robustness of the driver importance ranking (Section 2.5 Data analysis). We conducted this analysis by randomly sampling the values of contributing depth from a uniform distribution ranging from 5 to 15 cm and those of bulk density from a uniform distribution ranging from 0.5 to 1.5 times the bulk density for each site”.

Besides, we also added a paragraph in the section 4.2 Uncertainties in line 392 to 399 as follows:

"To test whether the results were sensitive to the difference in respiration units between the laboratory and field data we performed a sensitivity analysis. The results showed that temperature and aridity index were still ranked as important drivers (Fig. A03), which confirms the robustness of our results. Also soil moisture units were different between laboratory and field studies, but the conversion of soil moisture units from percent water holding capacity to volumetric water content depends on the soil water holding capacity, which vary among sites. Without site-specific estimates of water holding capacity, harmonizing the soil moisture units could introduce more uncertainty, so we preferred not to perform any conversion. While we confirmed that harmonizing the respiration units does not change the ranking of the respiration drivers, we cannot exclude that some of the differences between laboratory and field respiration could change after expressing soil moisture with the same units".

The conversion of soil moisture units between percent water holding capacity and volumetric water content is not a universal constant, but it depends on the soil water content at field capacity, which in turn varies with texture and organic matter content. Sand content at four of the field sites varied from 26% to 98% (we do not have information for the remaining 2 sites), so we expect large variations in the water content at field capacity. Since the soils change markedly between studies, the conversion will change the values of soil dryness and rewetting intensity in a way that could affect the results. However, without site-specific field capacity data, we are afraid that this unit conversion would introduce even more uncertainty than the unit conversion for respiration. Because of this, we would prefer not to unify the soil moisture units. We acknowledge this limitation in the new section 2.3 Comparability of laboratory and field data:

"Soil moisture was reported as % of WHC in most laboratory studies and as % of volumetric soil moisture in the field studies. In loamy and fine-textured soils, the soil moisture values expressed as Note that % WHC values are approximately four times as larger than those expressed as % volumetric soil moisture values, because water holding capacity is at about half of the soil saturation, which in turn corresponds to a volumetric soil moisture around 50% (Clapp and Hornberger, 1978, Laio et al., 2001) (e.g., 50% WHC corresponds to a volumetric soil moisture of 12.5% if soil moisture at saturation is 50% and the WHC is at 50% of soil saturation). However, the conversion factor from % WHC and % volumetric soil moisture is not constant, and lacking specific soil property data we did not attempt to harmonize the units."

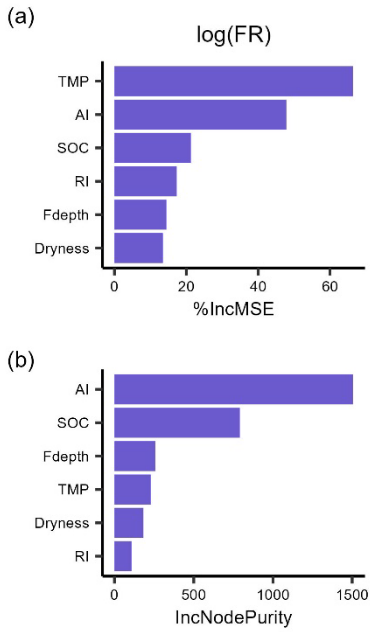

Figure R2: The importance ranking of predictors for mean respiration rates (μg C g $^{-1}$h^{-1}), during 48 hours after rewetting, from field (FR) measurements, based on random forest models using %IncMSE (a) and IncNodePurity (b). Predictors include soil organic content (SOC), aridity index (AI), soil dryness, rewetting intensity (RI), soil temperature for FR (TMP), and soil moisture sensor depth for FR (Fdepth). This ranking is slightly different from that obtained when considering a 48 hour time frame using original field respiration unit (μmol C m^{-2} h^{-1}).

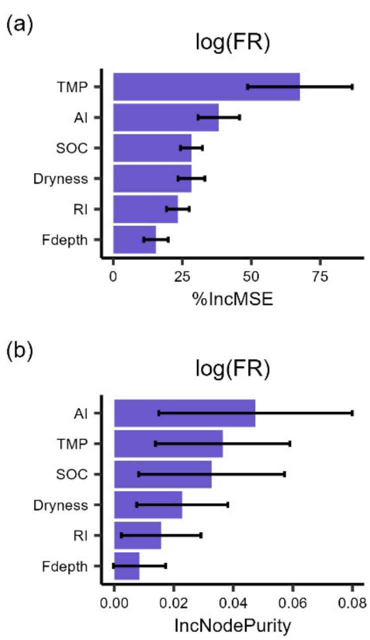

Figure R3: The importance ranking of predictors for mean respiration rates (μg C g $^{-1}$h^{-1}), during 48 hours after rewetting, from field (FR) measurements, based on random forest models using %IncMSE (a) and IncNodePurity (b). Predictors include soil organic content (SOC), aridity index (AI), soil dryness, rewetting intensity (RI), soil temperature for FR (TMP), and soil moisture sensor depth for FR (Fdepth). This ranking is averaged from 1000 importance rankings,

where each importance ranking was obtained by extracting contributing depth and bulk density from distributions centered around the estimated values at the six sites. Error bars are the standard deviations of each driver. This result is similar to that obtained when considering a 48 hour time frame using original field respiration unit (µmol C m^{-2} h^{-1}).

In addition, for some potentially important driving factors (such as drought duration, the number of DRW cycles, etc.), although it was found in the test that adding these factors did not increase the explained variance, since these factors are of great significance in field conditions, future research can further explore how to better incorporate these factors to improve the comprehensiveness of data analysis.

Yes, we could explore these drivers in our future research. Once an objective method for defining drought duration and the number of DRW cycles for the field data is identified, we will be able to conduct this research. We interpreted this as a general remark, so we do not plan to make changes to address it.

The discussion part has a relatively in-depth analysis of the results, discussing the differences in respiration responses under laboratory and field conditions and their possible reasons, such as changes in soil structure, microbial community changes, temperature and moisture changes, especially the plant carbon inputs. However, the discussion part can be further expanded to consider more factors that may affect soil respiration, such as soil texture, pH value, vegetation type, etc. In addition, a more in-depth discussion can also be carried out on the applicability and limitations of the research results in different ecosystems.

We acknowledge that soil properties like soil texture, soil pH are important drivers of respiration and help explain the variance in respiration responses after rewetting. However, whether or not the effects of soil texture and soil pH on respiration pulses are similar in the laboratory and in the field has not been tested. Testing this difference with our datasets is not feasible due to insufficient laboratory data on soil pH and soil texture. Therefore, for the sake of brevity, we prefer not to extend the discussion and also acknowledged that "Moreover, soil properties like soil texture and soil pH were not included due to lack of site-specific data. Therefore, whether or not the effects of soil texture and soil pH on respiration pulses are similar in the laboratory and in the field remains an open question" in section 4.2 Uncertainties line 405 to 408.

Regarding vegetation type, it is definitely an important driver of respiration pulse because it affects labile carbon inputs to soils (as also noted by reviewer 1), we have acknowledged the role of vegetation in section 4.2 Uncertainties by adding "We did not include vegetation types in the data analysis, although plants affect microbial processes and thus the respiration pulses. However, laboratory incubations to study DRW responses are conducted in the absence of vegetation, which forced us to only use field data from trenched plots where roots were not connected to any living plant. Lacking data from incubations with plants, we can only acknowledge that the absence of vegetation limits the generality of our results". Please see line 408 to 411.

Finally, it is recommended that the author add at least one field and laboratory DRW experiment at a sampling site, that is, to directly verify based on the same soil instead of being data-driven.

We agree that this could be a direct approach to validate laboratory results with field data. However, it may only allow us to validate the effects of "dryness" and "rewetting intensity" on the rewetting pulse (as these drivers can be manipulated in a given soil sample), but may be less applicable to other drivers that are site-specific (SOC, aridity index, soil temperature). For direct verification based on the same soils, laboratory experiments need to mimic different levels of DRW events at different incubation temperatures. Soils collected from six selected field sites (with different SOC content, climatic background) should all be used to conduct the above experiments. This would be a research project by itself and is outside the scope of this contribution. It remains a good idea for future research.

Overall, this is a valuable research paper. By comparing laboratory and field datasets, it provides important insights for validating the drivers of soil rewetting respiration pulses. Although there are some deficiencies, through appropriate improvements and further research, this research is expected to make greater contributions to the development of the soil carbon cycle field.

We thank you for this positive feedback!

Responses to Reviewer #3

Respiration is a central soil microbial activity and, thus, still an interesting and relevant research objective. This is particularly true for the comparison of field and laboratory measurements of soil respiration. The manuscript presented by Li et al. contains useful information, which can be published after minor revision.

We thank you for this positive feedback!

L26: … because microbial metabolic activity decreases and …

We have revised it as suggested. See in line 26

L34-35: Are 7 references necessary? Another possibility would be to separate the references on the different processes listed before.

These 7 citations cover different aspects of the rewetting pulse, so we would keep most of them. We have removed the citation to Manzoni et al. (2020), where previously published data were analyzed. We have cited these articles separately for the different experimental designs and data patterns, and revised as follows:

"For example, rewetting induces higher rates of respiration following exposure to more intense (lower soil moisture) (Fischer 2009; Lado-Monserrat et al., 2014; Li et al., 2023a), more extended (longer) (Miller et al., 2005; Tiemann and Billings, 2011; Meisner et al., 2017), and more pronounced (larger differences in water content between dry and moist samples) drought treatments (Fischer, 2009; Lado-Monserrat et al., 2014, Miller et al., 2005; Tiemann and Billings, 2011)."

L46: ... preparations, thus, the ...

L47: ..., resulting in …

L51: ... in sieved ...

L40-41, L58: …; Rousk and Brangari, 2022).

We have revised all above four language issues them as suggested.

L107, L131: Second-order subsections should be avoided.

We have removed those two second-order subsections.

L189: Two decimal numbers are not justified.

We have revised them as one decimal number.

L192, L297: "similar" not "comparable"

We have revised them as suggested.

L193-196: This statement belongs to the Materials and Methods section.

We agree. We have moved this statement when comparing laboratory and field data in lines 117 to 119.

Figures 2 and 3: Is it possible to add error indices?

Figure 2 shows the distributions of field data and laboratory data, so it already visualizes the data spread (this figure does not represent mean values or predictions that might require error estimates). For Figure 3, it is practically possible to add error indices. For example, using the bootstrapping method to run random forest for 1000 times, we would be able to calculate the mean and standard deviation of the importance values (i.e., %IncMSE) of each driver. However, we prefer not to add these error indices in the main figure and the importance ranking is fairly robust, as shown in our response to reviewer 2.

L252: I have doubts that downward leaching of DOC is a relevant process. I miss a statement on differences in gas flux conditions. Rewetting water may block pores. Also, the exact soil volume participating in the CO2 efflux is usually unknown.

We meant that in some field sites, DOC could be leached out, whereas in the laboratory, this does not necessarily happen because the samples are contained in a closed jar. We have clarified it in lines 277 to 278 by writing "Another reason may be that dissolved organic carbon can be lost via leaching in the field, but this does not necessarily happen in the laboratory, where samples are contained in closed jars."

The difference in gas flux conditions is now discussed a bit further down in relation to the effects of soil moisture increments on respiration pulse. Please see lines 343 to 346 as below

"Because soil moisture typically declines in the field immediately after it peaks, the period of limited oxygen availability coupled with high soil moisture may be shorter in the field than in the laboratory, so that the soil moisture increment in the field may not be as important a driver of carbon emissions in the field as it is in the laboratory".

We agree that having accurate soil volume information would help us to harmonize the units of the field respiration rate and laboratory respiration rate data. However, we do not have site-specific estimates for the soil depth contributing to respiration in field conditions. Assuming that the same depth contributes in all field sites and considering variations in bulk density, we can still convert the units, as shown in Figure R2 in the response letter for reviewer 2. In addition, we performed a sensitivity test by varying contribution soil depth and bulk density information, the results as shown in Figure R3 confirmed that our results are robust (please see the response to reviewer 2).

L291: No! It is difficult or even impossible to compact dry soil.

We have revised these sentences to read "shrinking dry soils can make substrates less accessible for microbial decomposition” in line 323.

L302: … inaccessible, thus, a …

We have revised it as suggested.

L310-312: Awkward statement! Rephrase!

We have revised it into “Because soil moisture typically declines in the field immediately after it peaks, the period of limited oxygen availability coupled with high soil moisture may be shorter in the field than in the laboratory, so that the soil moisture increment in the field may not be as important a driver of carbon emissions in the field as it is in the laboratory”. Please see lines 343 to 346.

L324: … 2015). This might …

We have revised it as suggested.

L569-720; The list of references contains numerous formatting mistakes and gives a sloppy impression.

We have thoroughly checked these references in revision.

L596: Volume number is missing!

We have added the Volume number “162”, together with any other missing information in the reference list.

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