Editorial board,

Biogeosciences

We thank the reviewer 1 for these comments. We respond to all comments below in plain text, while *comments by the reviewer are reported in italic.*

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. If we are offered a chance to revise this manuscript, we will make this point more explicit around Line 100, and revise the text as "…trenched plots (to ensure only heterotrophic respiration is included in the measured rates). The choice to only consider trenched plots limits the scope of our analysis to soils without plants, but allows comparison to patterns found in the laboratory studies."

To reduce possible confusion, we will explain in the Discussion that SOC effects on heterotrophic respiration after rewetting in natural field conditions (with plants) are strongly related to labile carbon input from plants. In contrast, there is no labile carbon input in the laboratory. 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 will do this when we first introduce the concepts of field respiration, in line 40.

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. If we have a chance to revise the manuscript, we will include this in the discussion. 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.

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 can reduce the time frame in the field data sets thanks to the fact that measurements are at high frequency, and we run 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 11.86 μmol C m^{-2} h^{-1} for the 24 hour time frame, greater than the 9.85 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.

This test is less applicable in the laboratory. The main reason for this is that only about half of the laboratory studies included in this paper allow us to obtain respiration rates after rewetting within 24 hours. Using a subset of studies to test the consistency of results is not ideal because the distribution of the independent drivers of these subsets differs from that of the whole data set. Therefore, in this case we did not proceed further.

Conceptually, the results obtained using 24 hours may differ from those obtained using 48 hours. 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). We can elaborate more on this point in the Discussion. Yet—despite these possible differences — the mean respiration rates over 24 hours in the field were predicted by the same drivers as the mean respiration rates over 48 hours, suggesting that our choice of the time frame might have limited impacts on the results

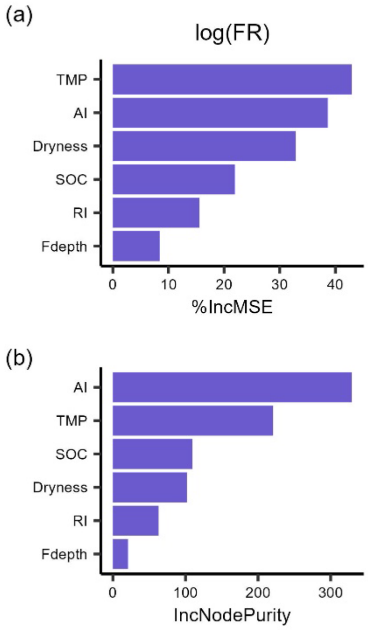

Figure R1: The importance ranking of predictors for mean respiration rates during 24 hours after rewetting, from field (FR) measurements, based on random forest models using %IncMSE (a) and IncNodePurity (b). Predictors include soil organic content (SOC), aridity index (AI), soil dryness, rewetting intensity (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 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%. This choice allows a balance between the number of rewetting events and the quality of the rewetting events from these two datasets. We can further clarify this issue in a revision.

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

Yes, we will replace "samples" with "cores"

Technical corrections

The list of lab studies is duplicated.

These references will be thoroughly checked if we have a chance to revise this manuscript.

References

Li, X., Leizeaga, A., Rousk, J., Hugelius, G., and Manzoni, S.: Drying intensity and acidity slow down microbial growth recovery after rewetting dry soils, Soil Biol. Biochem., 184, 109115, https://doi.org/10.1016/j.soilbio.2023.109115, 2023a.

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Meisner, A., Leizeaga, A., Rousk, J., and Bååth, E.: Partial drying accelerates bacterial growth recovery to rewetting, Soil Biol. Biochem., 112, 269–276, https://doi.org/10.1016/j.soilbio.2017.05.016, 2017.