Thank you very much for evaluating our manuscript positively. Followings are our response to your concerns. We hope our reply and suggested revisions will satisfy your concerns.

1) About different water contents among soils:

In our study, we consider that the soil water content at the soil sampling reflected the ability of soil to hold the water and thus the usual water contents in the field because the soil water content showed significantly positive correlations with WHC ($r = 0.87$, $p<0.01$). Nevertheless, none of the soil water content at the soil sampling and the water holding capacity (WHC) showed a significant relation with the increasing factor of CO_2 release by DWC (IF_{CO2}) not only as a linear correlation but also as a nonlinear relation, which was examined visually (Table 4; also see the figure below). These facts from the obtained data support us in stating that the variations in IFCO2 were significantly associated with soil metal-humus complexes and soil microbiology rather than different soil water content among soils (Tables 4 and 5, Fig 5). We will add the description of the positive correlation between the soil water content and WHC to the material and method section of the soil sampling (L80 to L88), especially to clarify the relation between the soil water content and the WHC.

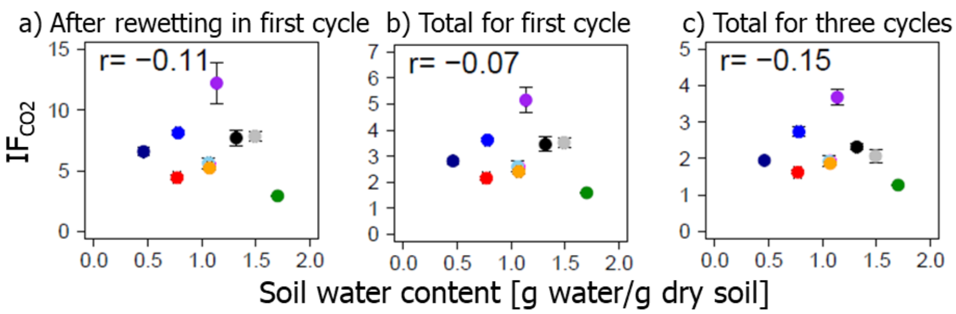

Figure. The relations between IF_{CO2} and soil water content at the soil sampling in the field. The relations against IF_{CO2} after rewetting in first cycle (a), as the total for first cycle (b), and as the total for three cycles (c). There was no significant correlation between the two variables.

2) About aerobic conditions for the incubation experiment:

The situation of the incubation experiment allows us to consider that the incubated soils have been aerobic even after the rewetting to increase the water content by twice the WHC. The primary evidence supporting this is that the CO2 concentrations in our experiment never overwhelmed 1%, thus the oxygen concentrations in the incubation jar have likely never decreased below 19% or lower. Also, a sufficiently large volume of our incubation jar (1.0L) against contained soil amounts (i.e., 5.31–10.63 g) and added water contents in the rewetting (i.e., ca. 6 to 7 mL) support the state of aerobic condition during the incubation. We will add this description to the material and method section of the incubation experiment (L110 to L131) to help the reader understand the aerobic conditions during the incubation.

3) About the mechanisms for large increases in CO_2 release after rewetting:

In our study, we suggested the two carbon sources that contributed to the increase in CO_2 release by DWC. One is the destruction of microbial cells by rewetting, which is more well-known than another factor, such as the release of carbon associated with the organo-metal complex. As you pointed out, microbial biomass carbon only accounts for a minimal fraction of SOC (i.e., ca. 1%). However, when considering the quantitative relationship between the amount of CO_2 increased by the dry-wet cycle (ca. 620 to 2,000 μg C/g dry soil) and the decreased amount of microbial biomass carbon (ca. 250 to 1,100 μg C/g dry soil), the microbial biomass decreases could contribute up to 64% of the CO_2 release increase during the 84-days incubation. In addition to the microbial biomass carbon, investigated soils contained 19,000 μg/g dry soil or more of pyrophosphate extractable-carbon (Cp; Table 3), which partially represented carbon associated with the organo-metal complex. Moreover, there were significantly positive correlations between IF_{CO2} and such organo-metal complex contents measured as pyrophosphate extractable aluminum (Alp) and iron (Fep) (Table 4). Especially, Alp showed consistent relations with IF_{CO2} in the present study (Fig. 5), suggesting Alp as the primary predicting factor for the IF_{CO2} variations. Thus, the carbon associated with the organo-metal complex, in addition to the destroyed microbial biomass, was suggested as the likely primary carbon sources contributing to the CO_2 release increase by DWC. Nevertheless, the strict mechanisms of these carbon sources to CO_2 release increase, including the persistence and timing of their contribution, still require further works (Schimel, 2018; Barnard et al., 2020), considering the significant contribution of more than two carbon pools to the CO_2 release increase (Slessarev and Schimel, 2020; Warren and Manzoni, 2023). To clarify these points, we will refine our sentences in the discussion section, especially from L201 to L236 and the conclusion (L238-L244), adding the references mentioned above (i.e., Schimel, 2018; Barnard et al., 2020; Slessarev and Schimel, 2020; Warren and Manzoni, 2023).

We will also add the identification number for each for the sub-panel in figures with more than one sub-panels (i.e., Fig 2-9).

References to be cited to the revised manuscript:

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- Slessarev, E.W., Schimel, J.P., 2020. Partitioning sources of CO_2 emission after soil wetting using high-resolution observations and minimal models. Soil Biology and Biochemistry 143, 107753. doi:10.1016/j.soilbio.2020.107753
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