GENERAL COMMENTS

The authors addressed many of my comments on the original manuscript, which improved the clarity of texts and figures. However, a few comments have not been adequately addressed. I would recommend withholding the acceptance of the manuscript until the following issues are sufficiently addressed.

We thank the Reviewer for allocating the time for reviewing the manuscript and providing numerous comments which helped us improve the quality of the manuscript. We have now addressed all the issues raised by the reviewer. The line numbers regarding the changes made, reported in this response letter, correspond to the manuscript version with tracked changes (Muhic_et_al_2023_revision2_markups)

SPECIFIC COMMENTS

Figure 1. Please include legends for open white squares and light gray squares in Figure 1a.

Response to the Reviewer:

The light gray and white squares are covered and open metal collars for soil methane flux measurement, as photographed by the drone, that remained in the soil after the previous experiments which are not directly related to this study.

They are not included in the legend as they are not relevant to this experiment and their inclusion could be somewhat misleading. It is also not possible to remove them as they are a part of the original/raw photograph, rather than symbols (although they look “too regular/rectangular” in Figure 1 (a) of the manuscript which might be causing the confusion). Both open and closed collars can be seen on the left side photograph below that was taken during the post experiment bulk soil sampling, and open collar is shown or the right side photograph, taken during the experiment.

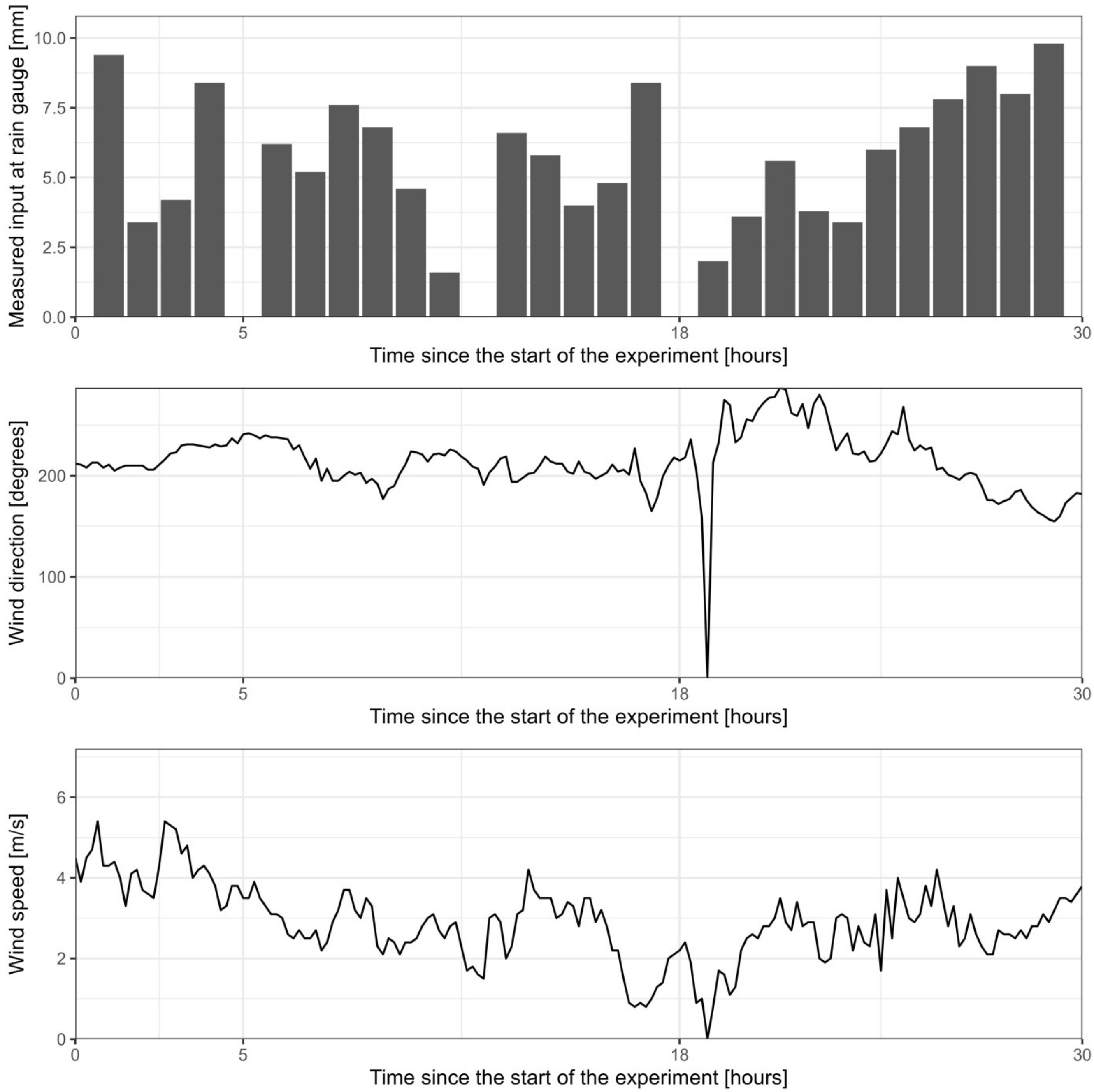
Changes made to the manuscript text:

Clarification was added to the Figure 1 caption at lines 88 – 91: “*Aerial photograph by Bastian Steinhoff–Knopp (Leibniz University Hannover, September 2018). The grey and white rectangles seen along the boardwalk are soil methane flux measurement collars that were installed prior to our experiment, remained at the study site, but did not have a function during the experiment”*

Line 133. This was Line 120 in the original manuscript concerning the total amount of irrigation (163.6 mm) recorded by the tipping bucket rain gauge. According to the authors’ explanation, the irrigated area is 3-3.5 m by 10-21 m, which translates to 30-73.5 m2. On this area, 20 times 1000 L (= 20 m3) of water was applied, meaning that the possible range of irrigation amount is 272 to 625 mm. This is only possible if: (1) the measurement by the tipping bucket was inaccurate, or (2) the actual area of irrigation was much greater than the authors estimated. For a controlled experiment like this, the quantitative assessment of input rate is important. Please re-examine the data and revise the description.

Response to the Reviewer:

The “3-3.5 m” width reported in the previous version of the manuscript was an editing typo that was corrected accordingly in the revised manuscript. According to [1] and [2] The actual irrigated area was approximately 118 m^2, with length of 10-21 m and width of 3 – 5.5 m. Furthermore, as outlined in Chapter 4.4 and Appendix D of [1], the spatial distribution variability was only measured during notably windy weather or if natural precipitation event coincided with the irrigation, while the irrigated water was seemingly evenly distributed over the plot during the calm weather. As shown in the figure below, both wind speed and wind direction were relatively stable during the entire 30-hour irrigation period, and the irrigation rate was not influenced by the wind speed or direction. The only drastic change of wind direction and simultaneous reduction of wind speed occurred around the 19th hour of the irrigation period, almost coinciding with the break in irrigation that was made in order to conduct the bulk soil sample collection.



Additionally, considering that 20 m^3 of water was transported to the site and spread over ~ 115 (5.5 m x 21 m) to 118 m^2, the total irrigation amount should ideally be in the range of 169.5 - 174 mm. As the pump that was used to move water from the 1000 L water tanks to the sprinkler system had to be submerged in water at all times, some small amount of water always had to remain at the bottom of the tank, thus slightly reducing the amount of water that was supplied to the sprinkler system. With this in mind, the measured amount of 163.6 mm seems rather reasonable and representative of the actual conditions during the experiment. 6 - 10 mm of difference (between the 169.5 – 174 range and 163.6 mm) would correspond to some 35 - 50 L of water per tank that were lost due to the pump submergence condition.

Changes made to the manuscript text:

Lines 124 – 127: "*The sprinkler setup was installed by Määttä (2020) and maintained by Korkiakoski et al. (2022), and sprinklers were positioned so that irrigation water can be distributed evenly within the EP, covering the area of 3-5.5 m width and 10-21 m length, with a total area of approximately 118 m2 in calm weather*."

Lines 133 – 139:" *A total amount of 163.6 mm of irrigation water was recorded by the tipping bucket precipitation gauge, which can be considered representative of the actual conditions as it is only slightly lower than the calculated amount of 169.5 – 174 mm (considering 20 m^3 of water and an area of 115 - 118 m^2). The 6 - 10 mm of total difference between the measured value and the calculated range roughly corresponds to 35 - 50 L of water loss per tank, which is reasonable considering that water pump which moved the water from the tank to the sprinklers had to remain submerged at all times, meaning that not all water could be extracted from the tanks. Furthermore, the irrigation rate was not influenced by wind speed or direction (Fig. S7).*"

Additionally, the figure shown above, with wind speed and direction and irrigation rate during the experiment, was added to the Supplement as Figure S7.

Lines 258-260. This was Line 186 in the original manuscript. The authors simply moved the sentences to Section 3.1 without revising them. This does not address my comment on the original manuscript. Please make a meaningful revision to address the comment.

Short recap:

The original comment from the previous revision was: "*Line 186. This sentence describes the response of 35-cm depth. However, I see that the 60-cm sensor responded before the water table started to rise, but this sampler was located far above the water table. This seems contrary to the sentence. Please explain. Overall, this paragraph could use a clearer writing that is consisted with the data presented in figures*."

The sentence in question (lines 234 – 236 of the revised manuscript) is: "*The isotope ratio of the water sampled from the pan lysimeters (blue dots and lines in Fig. 2c) in both soil profiles responded to irrigation only after the groundwater level went up to their installation depth (35 cm)*."

The new response where reviewer concern is taken account and explained more carefully:

The fact that the fastest isotopic response was observed at 60 cm depth is mentioned in the manuscript (line 218 of the revised manuscript: "*The fastest isotopic response in the EP was observed at a 60 cm depth (dashed black line in Fig. 2c) after only 5 hours*." It is also mentioned in lines 435 – 437: "*The isotopic enrichment observed at 60 cm depth (Fig. 2b) during the irrigation, that occurred before the enrichment in upper soil layers, further shows that preferential flow pathways were active even in the early stages of the experiment*.")

The idea behind the sentence in question, regarding the pan lysimeters, was to indicate that pan lysimeters did not seem to respond at all to the "downwards" infiltration, both during the experiment (Figure 2) and during the whole observation period (Figure 4), meaning that the water originates from the groundwater rise and/or lateral fluxes. In fact, there wasn't even any water to be collected prior to the groundwater rise. This was also previously commented on later in the manuscript (lines 437 – 439 of the previous version of revised manuscript, now in lines 458 - 460): " *On the other hand, the abrupt appearance and disappearance of large quantities of water in pan lysimeters, observed both during the irrigation experiment and the snowmelt, can be used to infer lateral flows and the near saturation of the macropore network*.") The sentence dedicated to pan lysimeter results in section 3.1 was expanded into a small section and modified to indicate the abovementioned reasoning more clearly.

Changes made to the manuscript text:

The revised paragraph (lines 243 - 252) reads as follows: "*The pan lysimeters were empty at the beginning of the experiment, indicating that no freely draining water had reached the pan lysimeter collecting bottles in the period prior to the experiment. Furthermore, no water was found in the pan lysimeter collecting bottles during the first 20 hours of the irrigation, although approximately 100 mm of irrigation water was applied to the plot by that moment. The samples only appeared after groundwater level exceeded the installation depth of the pan lysimeters (35 cm) and the collecting bottles got "overtopped" by the rising groundwater. From that point onwards, large quantities of water were evacuated from the collecting bottles at each hour and δ^2H value of pan lysimeter water rose sharply, from -52 ‰ to 2.5 ‰, reaching the most enriched values at the end of the irrigation (at 30 hours). The isotopic signal got more depleted immediately afterwards, thus displaying similar dynamics to the groundwater isotopic signal.*"

Line 384-392. This was Line 313 in the original manuscript. I am not convinced by the authors' explanation. Please present a more clear and less speculative explanation for a major shift in isotopic composition resulting from a small change in water content.

Short recap:

The original comment from the last revision was: "*Line 313. (Smaller pores in the soil matrix) is filled first. Figure 2d indicates only 5% increase in water content at 5-cm depth. This seems inconsistent with a major shift in isotopic composition depicted in Fig. 3. Does it make sense in terms of mass balance consideration? Please explain*"

The answer was: "*During the first 5 hours of the experiment, the soil moisture content in first 10 cm of depth increased by less than 10 %, while the bulk soil d$_2$H values increased by some 90 ‰ (from -77 to +11 ‰). Such d$_2$H increase could not be caused by simple mixing of the antecedent soil water with the infiltrating water, as the amount of antecedent water (~35 %) was much higher than the soil moisture increase. This indicates that the process of soil water displacement in the upper soil layers was initiated in the early stages of the experiment, as the enriched water started entering the soil matrix and altering the isotopic signal of the soil water. It should be noted that the observed soil water enrichment could not only be caused by mixing and displacement processes, as the bulk soil water also contains a certain fraction of very mobile infiltrating water, which can further skew the isotopic values towards the enriched values.*"

In the new response, the Reviewer's concern is taken account and the section was modified as follows to improve clarity. Changes to the manuscript text (lines 373 - 389):

"During the first 5 hours of the irrigation, the average soil moisture content in top 10 cm increased by 7 % points (from 35.5 to 42.5 %), while the average bulk soil δ^2H value in top 10 cm increased by 88 ‰, from -77 to +11 ‰. During the same period, 36.2 mm of irrigation water with δ^2H value of 76.9 ‰ was applied to the plot. Assuming that 1) all additional water in the topsoil could be represented through the soil moisture content increase and 2) bulk soil δ^2H signal changes only due to simple mixing of the antecedent and newly infiltrated water; these additional 7 % points of soil moisture would have to have a δ^2H signal in the range of +460 ‰ to shift the bulk soil water δ^2H signal from -77 to +11 ‰. Alternatively, if the δ^2H value of irrigation water in the observed 5-hour period (76.9 ‰) was considered as the endmember of the newly infiltrated water, a soil moisture increase of some 41 % percentage points would be required to result in a bulk soil water enrichment of +88 ‰. Following this, it is clear that assumptions of either well-mixed conditions or piston flow are both simplifying the processes at play and that there are some limitations to using soil moisture content as a sole indicator of bulk soil δ^2H signal variability. Namely, soil moisture content cannot accurately represent soil waters of all mobilities, especially in the case of fast draining and macropore water, and furthermore cannot indicate soil water displacement process. The only way to actualize such a strong shift in the bulk soil δ^2H signal is through a combination of two processes: 1) partial soil matrix water displacement and 2) mixing with highly mobile infiltrating or macropore water. Furthermore, neither of these two processes can be clearly visible in the suction lysimeters' δ^2H values, as they do not sample the waters of very low mobility (soil matrix water contained in the smaller pores) and generally do not show an immediate response to more mobile or macropore waters.

Line 443-460. This was Line 288 in the original manuscript. I asked the authors to present a more logical explanation about the connection between the rise of the water table and the lack of response of soil moisture at 60cm. The authors attempt to address this comment by simply removing the 60cm data from the graph. It is not acceptable to remove the essential data just because they do not fit with the interpretation. Please leave the data in the figure and come up with a revised interpretation that is consistent with the data.

Response to the Reviewer:

The authors regret the misunderstanding caused by the removal of data that shows the dynamics of soil moisture at 60 cm depth in Profile 2. The idea behind the removal of data was to be particularly strict with the inputs rather than to remove them out of convenience. The data has been now reincluded into the graphs, their effect on the main findings (soil matrix refilling/homogenization) of the study has been clearly addressed and added to the section 4.3.

Throughout the whole observation period, lasting one whole year, the soil moisture values at the Profile 2 60 cm sensor were rather high (~ 45 %) and stable, despite strong groundwater dynamics at the plot, changes observed in the other soil moisture sensor installed at 60 cm depth and isotopic changes observed in both bulk soil and soil lysimeter samples at 60 cm depth.

The observed soil moisture dynamics could have arguably been caused by sensor malfunction but could also showcase the existence of soil sections that are largely isolated from the surrounding soil matrix or macropore network. Such decoupling can occur due to a localized soil compaction that is either a natural facet of soil heterogeneity or artificially created during the sensor installation. While the readings of this sensor should be interpreted with caution, they indicate that some patches of the soil might be decoupled from the surrounding soil at all times. However, the lack of soil moisture change does not undoubtedly prove that there are no soil water fluxes and isotopic signal changes at this location. It is entirely possible that antecedent soil water gradually gets displaced by the infiltrating water, but this effect gets obscured by the near saturation soil moisture values. The isotopic composition dynamics of soil samples collected in the vicinity of the sensor location does indicate that water mixing and displacement principally occurs at this depth. Still, the isotopic signal of samples collected at one spot is not necessarily representative of all the surrounding soil, especially considering the high spatial heterogeneity typically observed in till soils. While the full extent of potential soil decoupling cannot be quantified, it is highly unlikely that few isolated soil patches can significantly affect the conclusion of the study, i.e. a general soil isotopic homogenization that occurs as the aftermath of snowmelt.

Changes made to the manuscript text (lines 531 - 539):

"Contrary to the observed isotopic dynamics, some hydrographic observations made both during and after the experiment, namely the soil moisture measured by 60 cm deep sensor in Profile 2, indicate that the irrigation and snowmelt events did not produce intense changes in soil moisture. The measured soil moisture value at this location was always about 45 %, regardless of hydrological conditions (see full black line in Fig. 2d and Fig 4c). While such soil moisture dynamic could have arguably been caused by sensor malfunction, it could also showcase the existence of soil sections that are largely isolated from the surrounding soil matrix or macropore network due to a localized soil compaction that is either a natural facet of soil heterogeneity or artificially created during the sensor installation. While the full extent of this potential decoupling between certain portions of soil matrix cannot be quantified, it is highly unlikely that few such isolated soil patches can significantly affect the conclusion of the study, i.e. a general soil isotopic homogenization that occurs as the aftermath of snowmelt."

References:

[1] Tiia Määttä, 2020, METHANE FLUX CHANGES DURING IRRIGATION EXPERIMENT IN BOREAL UPLAND FOREST SOIL, Master's thesis, UNIVERSITY OF HELSINKI, FACULTY OF SCIENCE, DEPARTMENT OF GEOSCIENCES AND GEOGRAPHY, DIVISION OF GEOGRAPHY, https://helda.helsinki.fi/items/daec65cb-4f05-46ea-9474-00a1ff775f71

[2] Korkiakoski, M., Määttä, T., Peltoniemi, K., Penttilä, T., and Lohila, A.: Excess soil moisture and fresh carbon input are prerequisites for methane production in podzolic soil, Biogeosciences, 19, 2025–2041, https://doi.org/10.5194/bg-19-2025-2022, 2022.