

Referee #2:

Comment 1: As such, the dataset is interesting, but the current challenge of the manuscript is the large overlap with earlier paper from the same roof (Konopka et al. 2021) reporting the first 5-years of the same 9-year measurement period.

Dear Referee #2,

we thank you for your comment and the review of our manuscript. We are pleased to hear that you find our dataset interesting.

We agree that there is a temporal overlap with a previous study from the same roof, although the Konopka et al. (2021) study was using a different temporal approach (i.e. the meteorological year) to evaluate the NEE budget. The meteorological year lasted from September of each year to the next August. However, for reasons of comparison, we decided in the current manuscript to use the calendar year for assessment of NEE, which is more common in energy balance studies.

To our knowledge this study is the only work which presents multiple-year, long-term ecosystem fluxes from an extensive green roof ecosystem. Hence, we argue that it is important to study the entire 9-year period in the present manuscript to analyse whether processes and findings from the first 5 study years (< 2019) were consistent with the second phase of the study period (> 2019) or certain differences/trends arise. As it turned out, the second phase was not a continuation of the results of the first phase, but carbon dynamics significantly changed, e.g.

- Section 3.4.1, where we point out, based on the median diurnal cycle of each year, that the period 2020-2023 showed higher respiration during the night compared to the period of 2015-2019.
- Section 3.4.2 highlights the change in NEE in the years 2022 and 2023, which we then link to an increase in total organic carbon in the substrate (section 3.4.3).

Hence, our findings distinguish significantly from the previous study and there is a clear knowledge gain on the carbon dynamics of extensive green roofs. Additionally, we quantify the ratio of yearly ET to yearly precipitation, which is indicative of retention and an important aspect for green roof ecosystems, and has not been analysed in the earlier study (see response to comment 8).

Comment 2: Already there, it was shown how the integral turbulence statistics look good as well as it included a detailed footprint evaluation of the EC system.

Our manuscript provides a more detailed analysis of data quality by providing the integral turbulence characteristics over the full 9 years and adds the cospectra of sensible heat, H₂O and CO₂. Also, we use a different, well-established footprint model (Kljun et al., 2015) compared to the Kormann and Meixner model (2001) used in Konopka et al. 2021, which is known to overestimate the flux source area (Auvinen et al., 2017; de Boer et al., 2013; Heidbach et al., 2017; Hellsten et al., 2015). We believe that a thorough analysis of data quality is essential and is the basis for any further steps of data analysis. The two figures on the data quality are presented in the appendix, and only their explanations are presented in the manuscript. We are open to making this section even more compact in a revised version of the manuscript but think that it should be given enough room in the manuscript, also because the readers need to be able to trust the data from this complex flux measurement site.

Comment 3: The same paper also examined in detail the seasonal and year-to-year variability of carbon fluxes and evapotranspiration (and Bowen ratio), and the dependency of the carbon fluxes on environmental conditions.; Comment 5: ... Furthermore, many of the reported results were self-

evident such that the fluxes have seasonal and diurnal variability, or that they depend on environmental variables.

While you state that the seasonal and diurnal variability is “self-evident”, we think that it must be quantified for the reader to be able to understand the patterns and magnitudes of the carbon (and water) flux during different times of the day and year as a function of different environmental drivers. We argue, that it is in the nature of (especially long-term) surface-atmosphere exchange studies from different ecosystems such as forests, wetlands, agricultural fields and urban areas (e.g. FLUXNET, ICOS) that authors report on seasonal and diurnal variation of fluxes in their research papers to introduce patterns and processes. Otherwise, further analysis of specific processes would lack important context.

We chose to include the seasonal and interannual variability of carbon fluxes as well as the dependency on environmental conditions to see if the patterns and relationships from the Konopka et al. 2021 paper can still be observed during the additional 4 years of observations. Hence, we can provide more robust relationships given the elevated variability of environmental conditions during the 9-year study period.

Comment 4: It appears that the only new outcome of the present paper is that in 2022-2023 the green roof acted as a source for carbon due to higher amount of soil organic carbon (SOC). This was explained in a lengthy way in the manuscript as it is kind of self-evident that such high respiration rates can only relate to increased amount of SOC in the soils. What is more interesting is that the green roof carbon fluxes are so sensitive to additional carbon in the system making them very sensitive for any management practice.

As you have mentioned, one important outcome of the study is that in 2022-2023, the green roof switched to a source for carbon, that we were able to relate to a higher amount of soil organic carbon (SOC). This underscores the high additional value of the extra 4 years of data and highlights the importance of long-term monitoring.

It is true that it appears intuitive to link net carbon respiration during 2022-2023 to an increased amount of SOC, however, other reasons are possible and need to be clarified, such as a declining trend in assimilation, e.g. from degrading vegetation or significant weather conditions. Generally, the influence of respiration on NEE dynamics is a vital research topic, as highlighted by recent research on the global respiration-induced weakening of land carbon sink (Dang et al. 2026). However, we agree, that the topic of the green roof system recently switching to a carbon source and the sensitivity of the green roof to additional soil organic carbon in the system should be clearly stressed (i.e. research question) in a revised version of the paper.

Comment 6: The language of the manuscript would need to be improved as past/present tenses were used and the authors talked eg of nocturnal respiration whereas there is no such thing but rather vegetation and soil respire also in daytime but this is masked by the photosynthesis.

By using this term we wanted to highlight the relative importance of the respiration flux during night-time hours. However, we agree that the term could be misleading. Hence, we will revise this term, along with adjusting past/present tenses where necessary.

Comment 7: Finally, the authors do not compare the calculated energy, water and carbon fluxes to other urban green areas types such as lawns, parks, forests and trees so that there would be understanding how the green roof compares with other urban green area types in cooling and uptaking carbon.

In the current version of the manuscript, we compared our findings on the carbon sequestration potential only to natural ecosystems such as forests and grasslands. We agree, that this is indeed an interesting topic for the reader, and will include a brief comparison with other types of urban green infrastructure (e.g. lawns, parks) in a revised version of our manuscript, to analyse the potentials of green roofs as an urban nature-based solution.

Comment 8: For the manuscript to be published in Biogeosciences, the authors would need to clearly bring new information on the coupled fluxes of water, heat and carbon, something that the earlier work from the same roof would not have reported. Until this, the manuscript appears as a simple report without new knowledge to the scientific community.

In addition to the previously mentioned new outcomes of our manuscript (e.g., long-term development of the green roof, changes in carbon dynamics, and sensitivity of carbon flux to SOC), this manuscript also contributes significantly to the scientific community's knowledge base by, e.g. providing direct long-term measurements of evapotranspiration (ET) over 9 years.

No other studies quantified ET from green roofs via direct measurements over a large flux source area such as those provided by eddy-covariance over a period of several years. ET was often modelled or calculated as the residual of the water balance, with the disadvantage of error accumulation. The data in our manuscript and its analysis aid in a better understanding of the water fluxes of green roofs and are valuable for the scientific community, e.g. for hydrological modelling. Furthermore, direct measurements of ET allow us to estimate retention over multi-annual periods, unlike values reported from other studies that use runoff measurements over shorter periods.

We acknowledge your constructive criticism on the current version of our paper. It will be helpful to improve the quality of our manuscript in a revised version of the manuscript.

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