Dear Paul Stoy and reviewers,

We sincerely thank you for your constructive comments on our manuscript. Your feedback has helped us improve the quality of our submission, and we appreciate the careful and critical evaluation of our work.

In the following response, we address each comment and concern raised by the reviewers using the format described below:

- Each reviewer comment or question is reproduced exactly as it appeared in the original review and appears underlined.
- Our responses are provided in regular black text.
- Revisions made to the manuscript are highlighted in blue text.
- All line numbers refer to the revised manuscript with tracked changes enabled.

Along with the changes mentioned in this document, we made some minor adjustments and corrections which can be seen in the tracked changes version of the revised manuscript.

Referee #1:

Comment 1: This manuscript about the ability of green roof to act as carbon sink is very important topic. There are available such eddy or chamber investigations from different fields (forests, peatlands) but in some reason there is a lack of such researches about green roofs, which are great opportunity in city environment to improve air quality. Present research is also unique investigation because of its very long period and have therefore clear importance for readers. The text is clear to understand and all findings are correctly explained. All necessary details about measurements and results are given. All my questions are already answered in the article. I am very satisfied with this manuscript. Therefore generally, in my opinion, the present form is acceptable.

Dear Referee #1,

we thank you for your comments and the review of our manuscript, in which you were highlighting the importance of our manuscript for the scientific community and the uniqueness of the long-term observations.

Comment 2: When water availability is very low during hot spring and summer, there is lower carbon assimilation by green roof plants. Are there any solutions to a situation where the temperature is ideal but there is no water? One possibility is irrigation but on such a large roof it is complicated. It could be a future topic to investigate whether irrigation helps maintain carbon sequestration efficiency.

As we have mentioned in our manuscript, water availability plays an essential role in carbon assimilation. During phases with low water availability, irrigation is a possible solution, but like you mentioned, water consumption would be high given the size of the green roof. In our manuscript we already discussed a sustainable irrigation strategy, i.e. automated and rainwater-based, that can be used to optimise ecosystem services. For the given roof we did look into the effects of green roof irrigation with regard to the reduction of urban excess heat/sensible heat flux (Heusinger et al., 2018). However, as you mentioned, this is still a topic for future investigations.

Comment 3: As the authors write, there are now solar panels on the roof. Are there any plans to study the interaction between a green roof and panels? The effect of shading from panels on plants?

The effect of the implementation of solar panels on the green roof and its plant community is an interesting research question that could be the subject of a future publication. We have submitted a research proposal to study PV green roof interactions in more detail.

Comment 4: If you could do a similar large roof survey, what would you do differently? For example, use other plant species besides Sedums.

Carbon assimilation varies across plant species and the composition of the plant community on the green roof. Hence, as we have mentioned in our manuscript, it would be interesting to investigate whether our findings can be transferred to other extensive green roofs with different plant communities. At the moment we conduct eddy-covariance observations on an even larger green roof in southern Germany, to analyse transferability of findings and processes.

Comment 5: The formatting and language use throughout the article are correct. No technical errors were found. Also, all references which are used in the text are also showed in the part References.

Thank you.

References for comment to Referee #1:

Heusinger, J., Sailor, D.J., Weber, S., 2018. Modeling the reduction of urban excess heat by green roofs with respect to different irrigation scenarios. Building and Environment ,131, 174–183. https://doi.org/10.1016/j.buildenv.2018.01.003

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, H_2O and CO_2. 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.

As explained above, this section already is concise (10 sentences, 349 words), and we believe that the explanations made in the main body of the manuscript are necessary for this complex measurement site. Hence, we decided not to make this section even more compact, because important information on the data quality would be missing.

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.

We have implemented the changes in the revised version of the manuscript. The introduction has been rewritten and now introduces the potential significance of additional carbon in the GR system for the carbon exchange and the long-term stability of the carbon sink intensity (lines 63-67):

"Additionally, the long-term stability of the carbon sink intensity of extensive GR remains understudied. Atmospheric uptake of carbon via photosynthesis and decomposition of plant litter will increase the amount of carbon in the GR system. This may be complemented by additional carbon

from external inputs such as maintenance or management practises (e.g. fertiliser application, mowing, weeding), which subsequently influence substrate respiration and surface-atmosphere carbon exchange of the GR. Consequently, this determines variation of the sink intensity with time. Better insight into these interactions, however, is essential for evaluating GR carbon sequestration potential, resilience, and contribution to urban climate adaptation and mitigation."

Also, an additional research question was introduced (lines 71-72):

"We hypothesise that the extensive green roof is a sink for carbon and investigate how the carbon sink intensity evolves with green roof age."

… and lines 12-13 in the abstract:

"We investigate, 1) whether the GR is a net annual sink for carbon, 2) if the sink intensity remains stable over the whole study period, and …"

Additionally, we made a separate heading in the results section for our results on the substrate organic carbon. Because the reviewer noted that the chapter was a bit lengthy, we made it more concise (lines 303-316):

"3.4.3 Sensitivity of NEE to additional carbon in the GR substrate

The shift of the annual NEE to a net source of carbon in 2022 and 2023 (Fig. 8, Fig. 9) was predominantly driven by increased respiration rather than decreased photosynthetic uptake (Fig. 6, Fig. 10). Hence, we examined substrate TOC measurements, which were sampled at the site from 2020 to 2023 (cf. Sect. 2.4), for possible changes such as additional external inputs of carbon between the years 2021 and 2022. We found an abrupt increase in substrate TOC from 2021 to 2022 (Fig. 11a), which coincides with the observed changes in carbon flux measured via EC.

We used the initial substrate TOC and added the annually observed NEE value (red area in Fig. 11b). In contrast, the substrate TOC as quantified from substrate samples was linearly interpolated for the period from 2012 to 2020 and displayed for different assumptions of the dry bulk density and initial TOC (cf. section 2.4). It is evident that the increase in substrate TOC is not entirely due to atmospheric carbon sequestration but due to an additional source of carbon, as indicated by the two estimates separating after 2021 (Fig. 11b)."

Furthermore, we stressed the sensitivity to additional soil organic carbon in the discussion and summary (lines 396-398):

"Hence, carbon fluxes are sensitive to additional input of carbon into the system, pointing to the potential impact of management practices (e.g. fertiliser application) on carbon sequestration potentials of GR ecosystems."

… and lines 442-444:

"Given the moderate net ecosystem exchange of extensive green roofs in comparison to other vegetated ecosystems, this highlights the sensitivity of the carbon sink intensity to additional carbon in the system, e.g. by management practises such as fertiliser application."

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.

In the revised version of the manuscript, we do not use the term "nocturnal respiration" anymore in order to avoid confusion. There were 3 instances, where this term was used, and now they read (lines 260-262):

"During night-time the carbon exchange between the GR and atmosphere showed net positive fluxes, representing net carbon release (Fig. 6) with the magnitude between +0.85 µmol m^{-2} s^{-1} and +1.63 µmol m^{-2} s^{-1}."

… and only *"respiration"* in lines 383 and 394.

Also, there were many instances, where we replaced the term "assimilation" with "uptake", because while the term "assimilation" is often used for leaf-level observations, the term "uptake" is more suitable for observations of ecosystems. Hence, we have also updated Figure 11 and Table A1, such that now they read "Net uptake rate" and "Net release rate" instead of "Net assimilation rate" and "Net respiration rate".

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.

We added a paragraph in the discussion which compares the carbon sequestration potential of the studied green roof to other urban green infrastructures (lawns, parks, urban forests, urban trees) (lines 348-357):

"The same applies for the comparison to other types of urban green infrastructures (which are discussed as urban NBS) or natural ecosystems. Thölix et al. (2025) used different ecosystem models (JSBACH, LPJ-GUESS, SUEWS) combined with eddy-covariance measurements in Helsinki, Finland, to estimate the annual NEE of different urban vegetation types. The mean NEE for forested sites was approximately -92 and -143 g C m^{-2} for the period 2006–2021, and -76 and -123 g C m^{-2} for an urban park, whereas the mean NEE for lawns was approximately -30 and -7 g C m^{-2}, for JSBACH and LPJ-GUESS, respectively. The entire vegetation sector, comprising different urban vegetation types, sequestered −40 (±34), −44 (±19), and −52 (±55) g C m^{-2} as quantified from the JSBACH, LPJ-GUESS, and SUEWS model output, respectively. Nowak et al. (2013) quantified carbon sequestration by urban trees in the United States using urban tree field data from 28 cities and 6 states and applying biomass and growth equations. The annual net carbon sequestration varied between -81 and -401 g C m^{-2} of tree cover and averaged to -205 g C m^{-2} of tree cover."

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.

References for comment to Referee #2:

Auvinen, M., Järvi, L., Hellsten, A., Rannik, U., & Vesala, T. (2017). Numerical framework for the computation of urban flux footprints employing large-eddy simulation and Lagrangian stochastic modeling. *Geoscientific Model Development*, 10, 4187–4205. https://doi.org/10.5194/gmd-2016-302

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Heidbach, K., Schmid, H. P., & Mauder, M. (2017). Experimental evaluation of flux footprint models. *Agricultural and Forest Meteorology*, 246, 142–153. https://doi.org/10.1016/j.agrformet.2017.06.008

Hellsten, A., Luukkonen, S.-M., Steinfeld, G., Kanani-Sühring, F., Markkanen, T., Järvi, L., et al. (2015). Footprint evaluation for flux and concentration measurements for an urban-like canopy with coupled Lagrangian stochastic and large-eddy simulation models. *Boundary-Layer Meteorology*, 157, 191–217. https://doi.org/10.1007/s10546-015-0062-4

Kljun, N., Calanca, P., Rotach, M. W., and Schmid, H. P. (2015): A simple two-dimensional parameterisation for Flux Footprint Prediction (FFP), *Geosci. Model Dev*., 8, 3695–3713, https://doi.org/10.5194/gmd-8-3695-2015

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