Response to reviewer #3.

We thank the reviewer for the positive assessment of our manuscript and for the constructive comments and suggestions, especially we clarified the description of our analysis regarding the travel time and the limits of applying the comparison between 12 and 35 m to the previous change in inlet height of 6 to 12 m. We have addressed all comments raised by the reviewer, and our detailed responses to each comment are provided below, with the reviewer comments shown in black and the author responses shown in blue.

The article “Atmospheric CO2 and CH4 measurements at Schauinsland station, Germany: A comparative study of diurnal and seasonal variations at 12 and 35 m intake height” by Großmann et al. investigates the comparability and representativeness of two inlet locations at the Schauinsland monitoring station with respect to atmospheric CO_2 and CH_4 measurements. As Schauinsland has one of the longest continuous CO_2 records worldwide, maintaining an uninterrupted and consistent time series is essential for understanding long-term greenhouse gas trends and variability.

The manuscript is well structured and provides a comprehensive analysis of the observations. Its findings provide valuable guidance for assessing data consistency and maintaining the quality of long-term greenhouse gas records, also for other monitoring stations facing similar challenges related to inlet changes and data continuity.

Overall, the manuscript presents a relevant and useful contribution to the field of atmospheric greenhouse gas monitoring. I recommend publication after minor revisions outlined below.

Specific comments:

Page 1, line 28 ff: It is unclear why only continental greenhouse gas monitoring stations are cited as constraints for estimating both land and ocean carbon fluxes. Given the importance of marine observations for constraining ocean carbon fluxes, please add marine and ocean-based stations or clarify why they are not mentioned.

Authors response: We agree on the importance of marine monitoring stations and added “marine” to the respective sentence.

Page 2, line 35 ff: Could you please provide references to the observational networks used? It would also be helpful to briefly discuss how comparable and compatible the measurements from the different networks are.

Authors response: We added the references of the observational networks and changed the sentence to: “the Monitoring and Evaluation Programme (EMEP, Tørseth et al., 2012) and the Global Atmosphere Watch Programme (GAW, WMO, 2025a) and joined the Integrated Carbon Observatory System (ICOS, Heiskanen et al., 2023) network in May 2022.”

Page 2, line 41: Please refer to the “WMO/GAW” recommendations, as these compatibility targets are defined within the Global Atmosphere Watch (GAW) programme rather than by “WMO” alone.

Authors response: Thank you for clarification, we added “WMO/GAW” in the manuscript.

Page 2, line 42 ff: The term “precision” may be misleading, as the ICOS targets cited above refer to network compatibility rather than measurement precision. Consider replacing “precision” with “compatibility” or “measurement consistency” to avoid confusion.

Authors response: Thank you for clarifying the formulation, we replaced “precision” with “measurement consistency”.

Page 2, line 62 ff: Please consider using “dry mole fraction” instead of “mole fraction” throughout the manuscript, unless humidity effects are explicitly included.

Authors response: Humidity effects are indeed not included, in order to be more clear, we replaced “mole fraction” with “dry air mole fraction” and added “hereafter referred to as mole fraction”

Page 3, line 83 ff, Fig. 1: For the further discussion, it would be helpful if the authors could include in Figure 1c the nearby potential local sources mentioned in the text, such as the city of Freiburg, Hofsgrund, and settlements in the Upper Rhine Plain.

Authors response: Figure 1c is primarily intended to illustrate the location of the pastures and does not provide sufficient coverage to show nearby settlements such as Freiburg, Hofsgrund, or other locations in the Upper Rhine Plain. To improve the spatial context, we have added a scale bar to the figure.

Page 4, line 93 ff: Please clarify whether condensation in the sampling line, upstream as well as downstream of the 2 °C pre-drying unit in winter could occur. If so, please describe how this potential issue is addressed at this site.

Authors response: Thank you for raising this point. Potential condensation effects are assessed through line and shelter tests conducted twice a year, according to ICOS guidelines. These tests showed no detectable influence on the measured covering different seasons, CO_2 mole fractions.

Page 4, line 95 ff: While the CRDS instrument measures H_2O in general, this sentence reads as if ambient H_2O is being measured directly. As I understand it, ICOS does not perform calibrated ambient H_2O observations, and a pre-drying unit is installed upstream of the instrument. Please rephrase the sentence for clarity and adjust Table 1 accordingly.

Authors response: Thank you for clarifying, we added the following sentence:
“The measured H_2O mole fraction is uncalibrated and is used solely as an ancillary quality-control parameter.”

Page 6, line 124 ff: It is unclear what is meant by “quality control” in this context. Please briefly clarify which specific procedures are performed by the station manager after ICOS data processing.

Authors response: We added more information on the manual QC. The sentence now reads:
“After the ICOS data processing, the station manager performs an additional manual quality control by reviewing the data in conjunction with the station logbook and flagging periods affected by maintenance, contamination, or other documented technical issues (Hazan et al., 2016, Yver-Kwok et al., 2021).”

Page 7, line 126 ff: It is not clear whether the concept of “target measurements” (STT and LTT) is familiar to all readers. It may be helpful to briefly explain these terms and include a reference.

Authors response: We agree and have clarified that the STT and LTT cylinders are measured as independent, unknown samples. Their measured mole fractions are compared with the nominal values assigned by the ICOS Calibration Laboratory (ICOS CAL) (Yver-Kwok et al., 2021). We added:

“These target cylinders are measured as independent, unknown samples and are not used for instrument calibration. Their measured mole fractions are compared with the nominal values assigned by the ICOS Calibration Laboratory (ICOS CAL). The differences should remain well within the ICOS network comparability goals and show neither significant temporal drifts nor step changes, particularly after changes to the calibration cylinders or measurement setup (Yver-Kwok et al., 2021).”

Page 7, line 130: The averages agree well. However, a few periods fall outside the WMO/GAW compatibility goals. Could the authors please comment on these deviations? Are there specific time periods (e.g. autumn 2023) associated with higher uncertainties or systematic effects? In addition, please clarify how these uncertainties are accounted for in the subsequent analysis.

Authors response: Thank you for your comment. The STT measurements are well within the compatibility goals of 0.1 ppm. However, the error bars in Fig. 3 are misleading and were removed in an updated version of the plot.

Page 7, line 135: The time series shows a data gap in 2022. For clarity, it would be helpful to mention this explicitly in the text.

Authors response: The gap in the ambient air is due to the change from instrument 529 (in function until June 19 2022) to instrument 881 (starting June 21 2022) and a gap in the data of instrument 1072 (17 to 20 June 2022).

Page 7, line 143 ff: The mean seasonal cycle is derived by averaging data from three years. Since atmospheric CO_2 and CH_4 concentrations exhibit a long-term increase, it may be helpful to briefly comment on whether this trend could influence the averaged seasonal cycle. While the long-term growth is expected to cancel out for the analysis of the intake differences, it may be relevant when considering the absolute concentrations.

Authors response: We thank the reviewer for this helpful comment. We have clarified in Section 2.5:
“As no significant difference in the long-term trends between the 12 m and 35 m intake heights is expected, the long-term trend is expected to largely cancel out when calculating the difference between the two intake heights. Since the subsequent analysis is based on these intake-height differences rather than on absolute mole fractions, the long-term trend is not expected to significantly affect the results.”

Page 7, line 146: Please explicitly clarify whether the term “LT” includes or excludes daylight saving time (summer time) in your analysis.

Authors response: To improve clarification, we changed the time periods to UTC.

Page 8, line 148 ff: Please clarify how this sampling criterion of minimum of three daily means affects the representativeness of the monthly means used in the analysis.

Authors response: Thank you for the comment. It does in fact not influence the monthly means as the minimal number of daily means per month is only two times less than 20 (June 2022 with 10 and December 2022 with 13). The mean number of daily means per month is 26.3 ± 4.6. The criterion was removed.

Page 8, line 163 ff: The range between minimum and maximum dry mole fractions is provided. It may also be helpful to include the mean and, in particular, the variability (e. g. standard deviation) to better characterise the distribution.

Authors response: In this paragraph it was chosen to mention the range instead of the respective means and standard deviations as the range shows that the 12 m time series experiences larger variability in CO_2 and CH_4, while the means only show small differences compared to the standard deviation. The respective means and standard deviations are:

CO_2 at 12 m: 422.7 ± 7.2 ppm
CO_2 at 35 m: 422.6 ± 7.2 ppm
CH_4 at 12 m: 2017.9 ± 29.0 ppb
CH_4 at 35 m: 2027.4 ± 29.0 ppb

Page 8, line 165 ff: The potential travel time of the air masses between the two intake locations, as well as the residence time in the sampling lines, is not discussed. It is therefore unclear how the measurements are synchronised to ensure that the same air mass is compared for both instruments. Please clarify how this is accounted for in the analysis and whether it may influence the results.

Authors response: Thank you for raising this point. In fact, the residence time of the air masses in the tubing from the top of the tower to the instrument is approximately 1 minute. The two time series are synchronized to correct for this effect. The difference in travel time of air masses is evaluated via pollution peaks arriving at the two intakes and found not to be relevant for the comparison on an hourly time scale.We added in section 2.5:

"To account for the difference in residence time between the two intake lines, which is approximately 1 min, the minutely data from the two intake systems were first synchronized in time. The synchronized data were then aggregated to hourly means."

Page 9, line 171, Fig 4d: While the variability in summer is clearly higher for ΔCO_2, it is difficult to discern similar seasonal differences for CH_4 (Fig. 4d). Including a measure of variability (e.g. error bars) may improve the interpretability of the figure.

Authors response: The error bars (standard error) on the monthly mean mole fraction difference in CH_4 is too small to see in this figure (at the order of 0.1 to 0.2 ppb), yet the spread in hourly mean mole fraction differences serves as measure of the variability. The seasonality is indeed weak, which is analysed in detail in Fig. 7c. We increased the line width for better visibility.

Page 10, line 191, Fig. 5: To which averaging time does the WMO/GAW compatibility goal refer? As noted before, please also clarify how the travel time of air masses between the two intake lines is accounted for and whether it may affect the results, particularly in the minute-resolution data analysis.

Authors response: The WMO/GAW compatibility goal does not refer to a specific averaging interval. The difference in travel time is considered by temporal synchronization of the minutely data and not relevant for means calculated from hourly data.

Page 11, line 198 ff: It is unclear whether the reported values represent the minimum and maximum observations. If so, these may be influenced by individual events and not fully represent the variability during the respective period. Please consider whether a statistical measure such as the standard deviation would provide a more representative characterisation of the atmospheric variability.

Authors response: In this section, we only aim to provide a measure of the observed variability based on the minimum and maximum values and to show that the observed range is larger for the 12 m intake than for the 35 m intake. A more detailed analysis is subsequently conducted using the diurnal and seasonal mean and standard deviation of the differences between the two intake heights.

Page 14, line 274 ff: The explanation of the CH_4 seasonality is somewhat unclear. The dominant atmospheric CH_4 sink is oxidation by OH, which exhibits a pronounced seasonal cycle on large spatial scales. However, the current discussion appears to mix large-scale processes with regional transport and local source influences. For example, the statement that the sink is more important in summer when CH_4-enriched air masses are transported to the station is not entirely clear and would benefit from further clarification. In addition, it would be helpful to explicitly state which CH_4 sources and sinks are considered and how they are related to solar radiation. In particular, the link between anthropogenic CH_4 sources and solar radiation is not clear.

Authors response: Thank you for addressing this, we changed the respective paragraph to: "The seasonality with increased mole fractions in winter and decreased mole fractions in summer reflects the seasonally changing solar irradiation driving the atmospheric CH_4 sink. Superimposed on this seasonality are patterns related to regional transport: From April to November, CH_4 enriched air masses are transported to the station more frequently leading to elevated mole fractions, especially in August and September.

Page 18, line 344, Fig. 10: The figures also include the baseline extracted using the REBS method (purple line, b= 0), which is stated to overestimate the concentrations (p. 17, line 335 ff). However, even for the selected parameter settings (b=8 and 3), the deviations from the 35 m reference measurements remain substantial (Fig. 10 (d): up to approximately 40 ppb and 20 ppb, respectively). It is therefore not entirely clear why the baseline (b = 0) was not considered in the study and how it would perform in comparison to the reference measurements. Could the authors comment on the performance and limitations of the REBS baseline more explicitly and whether it could be considered as an alternative approach in the analysis?

Authors response: We agree that the description of the REBS algorithm was not entirely clear. Implementing $\beta = 0$ would imply using the one-hour moving mean over the mole fraction as threshold. This would introduce a strong bias into the time series as only data below this curve would be accepted, without considering the natural variability of the time series.

To improve clarity in the description of the REBS algorithm, a paragraph on the estimate of the γ parameter and a sensitivity study was added to the Supplements:
"The γ value is estimated as the standard deviation of all data below the fitted baseline curve. While in Cristofanelli et al (2023) this was done using a rolling window of the previous 24 h for

each data point, in this study γ was estimated from the complete period to which the algorithm is applied to (May to November of the years 2022 to 2024). Figure S8 shows the application of the algorithm to an example time series using different approaches for estimating γ, including rolling windows ranging from 1 h to two weeks as well as γ estimates derived from the 10 day example period and the complete May to November Time series. At the Schauinsland station the spikes occur very frequently during short periods, whereas other periods are largely unaffected. Therefore, estimating γ for shorter sections of the time series can lead to strongly enhanced γ values (grey line), reducing the performance of the spike detection algorithm. Hence, it was chosen use a constant γ value of 1.65, calculated from the months May to November of the three years of data."

Page 22, line 411 ff: As the historical measurements have changed from a 6 m to a 12 m inlet height, could this study be used to assess how the long-term time series may be affected by the different sampling positions? A brief discussion of this aspect in the manuscript would be useful for the future interpretation of the long-term record at Schauinsland.

Authors response: Thank you for addressing this. We agree that implications of the comparison between 12 and 35 m intake height to this previous change from 6 to 12 m are important. We are currently re-evaluating the historic data set and are investigating potential changes in mole fractions related to this change in measurement height. A future study will address a comprehensive assessment of the historical record, including recommendations for the continuity with the change in inlet height from 6 to 12 m.

Page 22, line 426 ff: It is mentioned that low-frequency eddy covariance measurements could help to further quantify the influence of the surrounding biosphere. Could the authors briefly explain why conventional eddy covariance measurements are not considered?

Authors response: We agree that conventional eddy covariance measurements would be very helpful, yet they require the installation of new measurement equipment. In contrast, low-frequency eddy covariance measurements only require replacing the 2D wind sensor with a 3D wind sensor. We added "at low effort" in the respective sentence.

Page 23, line 436 ff: It is mentioned that the historical CH_4 record measured at 12 m may be influenced by local pollution, potentially introducing biases in model simulations. Presumably, this issue could also affect the earlier measurements conducted at 6 m. Could the authors comment on the implications for the long-term record and how this issue may be addressed in future studies? It would be helpful if the authors could either provide an estimate of the potential effect or offer recommendations for the interpretation and use of the historical CH_4 record.

Authors response: Thank you for this comment. We agree that the impact of local pollution on the historic 6 m intake needs careful investigation. The historical CH_4 record at Schauinsland station Schauinsland includes GC measurements until 2015 with a lower temporal resolution of approximately 5 minutes. Since the application of the REBS method requires high-resolution data, such as provided by CRDS, the application of the algorithm described in this study to the historic data is limited. We are currently investigating strategies to evaluate potential pollution in the period pre-CRDS and especially at 6 m intake height and address them in a future study. We added to the manuscript:
"Therefore, for the CRDS-based CH_4 record, the application of a spike detection algorithm to identify local contamination events in the 12 m measurements should be considered. For earlier measurements with lower temporal resolution, further investigation is required to develop appropriate quality-control strategies."

Technical corrections:

Page 1, line 23: I suggest to change “Long-term, high precision measurements of atmospheric CO2 and CH4 are essential for monitoring and understanding of global biogeochemical cycles […]” to “[…]are essential for monitoring and understanding global biogeochemical cycles […]”

done

Page 2, line 24: Change “inter-annual” to “interannual”

done

Page 2, line 52 ff: Consider clarifying this sentence by explicitly stating that short-term perturbations from local emissions represent an additional challenge for obtaining “representative” long-term atmospheric “background” observations, e.g. “…an additional challenge for representative long-term atmospheric background observations.”

Authors response: Thank you for clarification, we changed the sentence to “pose an additional challenge for the representativeness of long-term atmospheric background observations.”

Page 9, line 171, Fig 4: The distinction between the colours and line styles is not clearly visible. Please consider using alternative styles to improve readability.

Authors response: To improve readability of Fig. 4, the colour of the monthly means at 12 and 35 m were changed to different colours from the okabe ito set and the line strength was increased.

Page 10, line 191, Fig. 5: The distinction between the blue and orange lines is not always clear, as they overlap during certain periods. Using different line widths (e.g. a thicker line for the one in the background) could improve readability.

Authors response: Unfortunately, using thicker lines lead to less resolution of the short-term variations, which we intend to show with this plot.