

Review of “Evaluating low-cost NDIR CO₂ for atmospheric observation in rural settings”

I read with great enthusiasm the work presented by the authors, who compare low-cost NDIR CO₂ sensors against a market-grade reference device for atmospheric carbon dioxide mixing ratio measurements in rural settings. The authors provide discussed improving sensor readings and identifying the root causes of deviations. However, I have a major concern regarding the overarching aim of the study, namely, the deployment of low-cost sensors (LCS) in rural environments and their operational reliability. In its current form, the manuscript is not yet suitable for publication.

General Comments

1. The results suggest that the tested LCS are not yet sufficiently reliable for deployment in rural environments, whereas they appear to perform more effectively in urban settings. The general average trend may be comparable but signal-to-signal observations between LCS and reference analyser is not well in the rural setting.
2. The manuscript requires some re-structuring required. The quality and presentation of the figures require improvement to enhance clarity and interpretability.

Specific Comments

- **L28–29:** The authors recommend using the sensors as a reliable measurement source, but this assertion contradicts the findings presented in Figure 10, which demonstrate poor sensor performance in rural conditions. This discrepancy should be addressed.
- **L64–67:** The cited reference does not adequately support the statement made here. The authors should rephrase this section to more accurately reflect the use of low-cost sensors in the context of the referenced work. Specifically, it is not about the eddy covariance methods make expensive but the use of expensive analysers.
- **L360:** This content appears to belong in the Materials and Methods section and should be relocated accordingly.
- **L401:** Section 3.4 (“Suitability of LCS CO₂ in rural applications”) appears to be the core of the rural evaluation. However, this section lacks statistical analysis. Furthermore, Figure 8 is not clearly explained and does not effectively support the narrative presented here.
- **L467:** This important finding, central to the study’s stated purpose of rural deployment, is not mentioned in either the abstract or the conclusion. It should be included in both.
- **L484–488:** These lines are not informative and add little value to the discussion. The authors should either condense this section into a more concise statement or remove it entirely.

Figures

The authors appear to use full-scale measurement ranges for CO₂ mixing ratios and environmental parameters throughout the main text. It would be beneficial to include zoom-in panels in relevant figures to highlight finer signal details. Additionally, the manuscript contains an excessive number of figures; several could be merged—for example, by combining figures for environmental parameters and mixing ratios for both urban and rural settings—and the remainder moved to the Supplementary Information.

Several figures suffer from poor readability due to overlapping lines (e.g., Fig. 3), excessively thin line weights (e.g., Fig. 5), illegible numerical labels, or overly lengthy captions (e.g., Fig. 9). These issues should be addressed to improve overall presentation.

Editorial Corrections

- **L16 and throughout:** The term “mixing ratio” should be used in place of “concentration” for consistency with atmospheric measurement terminology.
- **L16** NDIR is not spelled-out.