

Response to
<https://doi.org/10.5194/egusphere-2026-144>
from Referee #4

This study presents a fast-response, low-cost carbon dioxide sensor for flux measurements. It is based on approximately five months of field observations and includes comparisons with a standard eddy covariance system. The sensor's performance is thoroughly evaluated, and it appears to offer a cost-effective alternative for field CO₂ flux measurements. However, a few major issues should be addressed before the manuscript can be considered for publication in AMT.

We are grateful for useful comments and recommendation. Our responses to comments and corrections are found below, along with proposed changes to the manuscript.

Major Comments:

1. Clarification of technical details: As this is primarily a technical paper intended for users of CO₂ flux measurements, I think some methodological aspects need to be explained more clearly in the manuscript itself. In particular, it is not sufficiently clear what improvements were introduced with the software update and how these changes affected the sensor performance. A more explicit description would be very helpful. Similarly, the autocalibration procedure should be described in more detail. At present, it is difficult to understand how this procedure is implemented in practice and how it affects the long-term behaviour of the sensor. Related to this, it would also be useful if the authors could comment on the temporal evolution of sensor drift. For example, does the drift increase approximately linearly with time, or does it follow a different pattern?

The prototype development involved series of minor software updates trying to improve the instrument's performance and characteristics. However, the software update on 21 July 2022 improved the sensor performance significantly, eliminating the signal jumps after each autocalibration.

Prior to this software upgrade, the auto-calibration was performed according to the following algorithm. The signals were measured with 5 Hz until temperature of the cell changed certain threshold (around 0.2° C). Then autocalibration was applied by measurement a reference signal at 1 Hz for 1 sec; after such calibration the CO₂ reading was adjusted to the reference signal. Then measurements continued again with 5 Hz frequency.

The drift was primarily a function of temperature and time. However, we could not establish well-behaving functional relationship on temperature and therefore performed linear correction between the calibrations in the earlier phases of the prototype development.

With the software update on 21 July 2022, the signal jumps occurring with reference autocalibrations disappeared. The autocalibration procedure was modified to perform step-wise predictions for the drift between the calibrations against the reference signal using internal signals from instruments components. Continuous corrections to the signal based on such drift predictions virtually removed discontinuities in signal after each autocalibration.

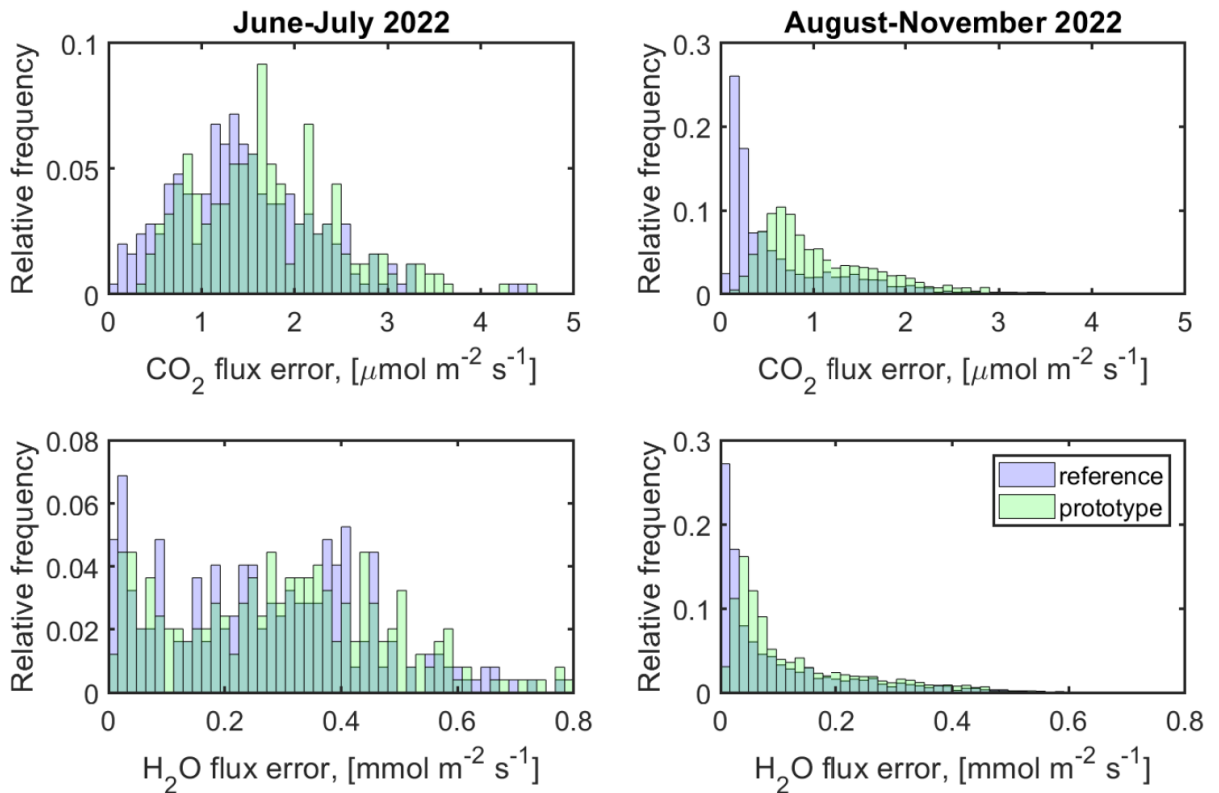
Due to built-in autocalibration against the reference signal the long-term signal drifts were avoided. At the end of the field experiments the prototype's calibration was tested in Vasilala's lab and no drifting was detected.

2. Response time and wind conditions: The approach used to estimate the sensor response time would benefit from some further clarification. As I understand it, the estimate is derived from data collected under unstable atmospheric conditions and within a relatively narrow wind speed range of 2.5–3.5 m s⁻¹. This raises the question of whether the derived response time is partly influenced by the prevailing wind or turbulence conditions. If so, the authors should discuss to what extent the estimated response time may depend on wind speed or atmospheric state more generally. Would the same response time be obtained under calmer conditions, or under stronger turbulence? Some discussion of the robustness of this estimate would strengthen the paper.

Low-pass filtering of the system is mainly due to frequency response of the analyser (+tube damping in case of H₂O), and thus the property of the instrument and not turbulence as such. The unstable conditions were chosen to use the measurements with higher flux magnitude in order to obtain better results. This is a common practice in response time estimation (e.g. Aubinet et al. 2008). In addition, the limited wind speed interval was used to use observations from similar conditions as the spectra are usually normalized to non-dimensional frequency (the non-dimensional frequency is proportional to height and inversely proportional to wind speed, i.e. spectra are shifted in frequency space in proportion to wind speed). In summary, the conditions were selected to form an ensemble of similar observations.

3. Uncertainty in weak flux measurements: I also feel that the manuscript should discuss more carefully the uncertainty associated with very weak CO₂ fluxes. For example, in Figure 6 the H₂O fluxes show a relatively smooth decline from July to September. The August H₂O fluxes are weaker than in July, but they are still clearly visible and remain higher than those in September. In contrast, the CO₂ fluxes over the same period remain close to zero. This makes me wonder how reliably the sensor can quantify very small CO₂ fluxes. A similar concern arises from Figure 7. If one focuses on the August data only, excluding the period from 21 July 2022 to the end of July 2022, the agreement between prototype and reference appears rather weak for small CO₂ fluxes, especially in the range of about -5 to 5 μmol m⁻² s⁻¹. This suggests that the uncertainty may be substantial in the near-zero range. I would therefore encourage the authors to discuss more explicitly the reliability and uncertainty of the sensor under weak-flux conditions.

The random noise of the instrument was provided in Fig. 5 and how it translates to flux uncertainty in Sect. 3.1. However, total flux random uncertainty is larger than the uncertainty originating from the instrumental noise; it is contributed also by stochastic nature of turbulence. In general, the random flux uncertainty is proportional to flux magnitude, however, in near-zero exchange conditions the relative flux uncertainty (i.e. the random error divided by the flux magnitude) becomes very large. We calculated the flux random uncertainties of both systems, see Fig. below. The histograms of the total random flux uncertainty for June-July period, when higher absolute flux values prevailed, did not differ essentially for the prototype and the reference systems. However, during August-November period, the distributions for the reference system peaked at smaller values. This effect can be attributed to the impact of the higher signal noise of the prototype system. We have estimated the random uncertainty of the CO₂ flux approx. 0.24 μmol m⁻² s⁻¹. This corresponded to the noise std of 1 ppm. However, during earlier phases of the field testing the noise level was roughly higher by a factor of 2. We agree with the reviewer that the noise level at initial phases of the field measurements introduced significant flux random uncertainty that limits accurate detection of small fluxes. However, the noise level obtained towards the end of the field experiments (after the software update on 20 September 2022) is a good compromise in our opinion. Also, the random uncertainty does not impact accurate measurement of averages over longer periods. We will discuss the topic of random uncertainties of fluxes in more detail in the revised MS.



Other Comments:

In Figure 4, sensible heat flux is used as an indicator of atmospheric stability. I wonder whether momentum flux or friction velocity (u^*) might be more appropriate for this purpose. At least, the choice of sensible heat flux should be justified more clearly.

In general, intention was to select unstable conditions as under these conditions co-spectra are known not to depend on stability parameter in the ASL. The criterion for heat flux $> 25 \text{ W m}^{-2}$ was chosen to avoid near-zero sensible heat fluxes because the co-spectral estimates become uncertain under such conditions and transfer function is obtained by dividing the CO_2 co-spectrum with the temperature co-spectrum. Thus, the threshold was chosen to avoid division by near-zero values, which would lead to higher uncertainty in calculated results.

It would be useful to include some practical recommendations for long-term operation and maintenance. Based on the results shown, the system appears to require relatively frequent updates, and some guidance for future users would add value to the paper.

The latest software updates improved the analyzer performance and after development completion no frequent updates would have been needed. The intention was to complete development with fully production-ready product which would satisfy the criteria set for eddy covariance measurements, including robustness of maintenance.

The prototype sensor operates at 5 Hz, whereas the reference eddy-covariance system typically operates at 10 Hz. The manuscript should discuss how this difference in sampling frequency may affect the flux calculation and the comparison between the two systems. In particular, could the lower sampling rate lead to a bias or loss of information?

The lower sampling rate is less important than the frequency response of the analyzer. The frequency of signal sampling as such is related to the impact of disjunct sampling on calculated fluxes. If the measurements are instant values of the true signal, but sampling is done in intervals determined by sampling frequency, then calculated fluxes are not affected by systematic bias.

It is shown that sampling rate has no influence on the expected value of an eddy-covariance flux estimate. The uncertainty in the flux estimate, however, increases with decreasing sampling rate. This sampling induced uncertainty is comparable with the natural random uncertainty in the flux estimate due to the stochastic nature of turbulence. It is shown that the sampling induced uncertainty becomes important when the sampling interval is larger than the integral time scale of the flux time series (see, Bosveld and Beljaars, 2001; also Rinne et. al., 2000; Turnispeed et al., 2009). In terms of sampling interval influence on calculated fluxes, the reduction of sampling rate from 10Hz to 5Hz has essentially no impact. Therefore, we concentrated on the analyses of the sensor frequency response. The frequency response of the analyser was determined to be 0.2 seconds. We will include discussion on the impact of the sampling rate on the fluxes in the revised MS.

Technical Corrections:

Line 16: I would suggest avoiding the word “promising”, which sounds slightly informal in this context.

Yes, we will re-phrase.

Line 137: Please clarify which lag time was used for the CO₂ flux calculation.

We used fixed lag time of 0.6 sec, will be added to the revised MS.

Line 147: Delete the extra word “for”.

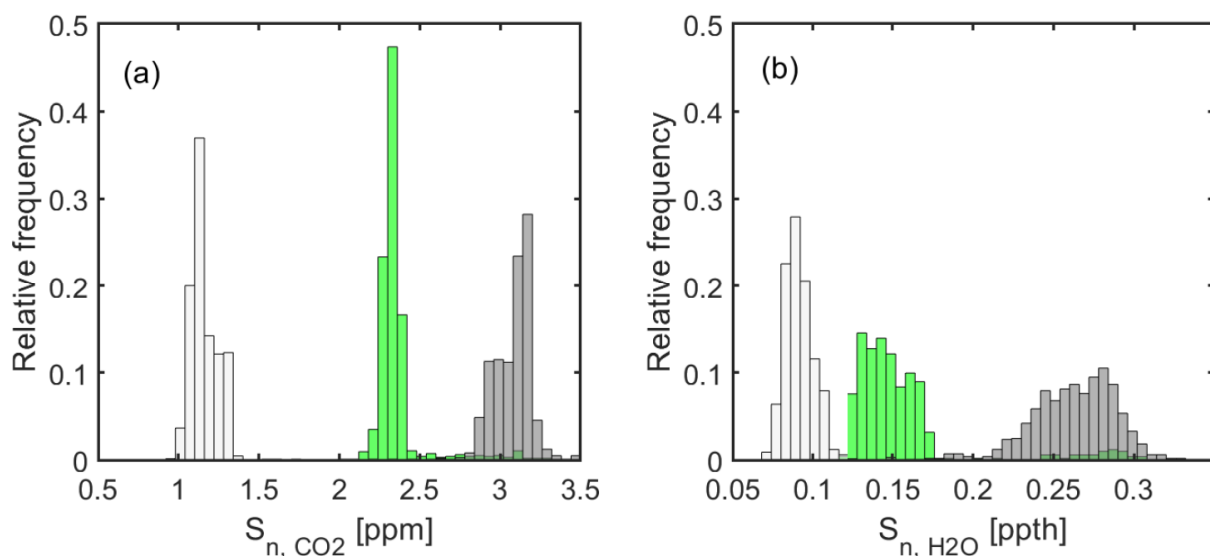
Will be deleted, thank you.

Line 213: Please clarify whether this refers to r or R^2 , and use the appropriate notation consistently.

Fig. 7 shows the correlation coefficients i.e. R . We have used mistakenly in text the R values and saying those were R^2 . We will use R^2 values in the revised MS consistently.

Figure 5: It would help readability to use slightly more distinct colours or symbols for the different periods.

Thank you for the suggestion. We will denote different periods with different colors in the updated figure of the revised MS. The figure below shows observations from the beginning of measurements till the software update on July 21 2022 with green, from then on till Sept. 20 with dark grey and from then till the end of campaign with light grey.



Citation: <https://doi.org/10.5194/egusphere-2026-144-RC1>

Referencies

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