

Response to reviewer comments on Enders et al., 2025: „Towards routine shipborne measurements of columnar CO₂, CH₄, CO, and NO₂: a case study for tracking regional-scale emission patterns“

Reply to reviewer 2:

The questions of the reviewer are shown in italics. The list of references can be found in the main manuscript.

Enders et al. present an approach for distinguishing emission sources using the ratios of CO₂, CH₄ and CO dry-air mole fractions measured by a shipborne FTIR (EM27/Sun) instrument together with NO_x derived from NO₂ vertical column densities measured by a direct-sun DOAS instrument.

The instrumentation and methods are briefly explained in this manuscript and have been discussed in previous studies.

The general feasibility of the approach is demonstrated through a case study based on measurements conducted in Mikawa Bay, Japan, in 2023. The derived emissions from this case study are compared with the EDGAR and REAS inventory data sets and show good spatial agreement with the location of the emitters.

The paper is well written, the analyses are thorough, and the approach shows potential for validating satellite data sets in coastal regions.

We thank the reviewer for the appreciation of our work and the helpful comments. Please find our point-by-point reply below.

My two main concerns relate to the case study itself and its application to satellite validation:

1. The presented case study does not convincingly demonstrate that the identified emitters are responsible for the observed measurements. Although you mention that self-emissions are negligible, some of the recorded enhancements (particularly the strong signals after leaving Mikawa Bay) could be influenced by the vessel's own emissions. In addition, only wind direction is shown, while wind speed is missing. What was the vessel's cruising speed? Furthermore, why is only a single time series example presented, despite several months of available data?

To strengthen this analysis, I recommend including a map of wind trajectories for the selected case study, for instance using HYSPLIT or FLEXPART simulations.

The deployment on the Nichiyu Maru was a short-term opportunity within the SOOP program (section 3). The ship, however, is a suboptimal carrier for our purposes since it mostly travels during the night, while during the day, it is moored in the harbor for loading operations. This, together with other requirements such as good weather conditions, prevented us from collecting a more comprehensive dataset on emission episodes. Nonetheless, a more in-depth analysis of data from the whole campaign (including another deployment on the same ship in 2025) is currently ongoing. The goal of the present paper is to document the technical setup and the COCCON-compatible

performance and to showcase the usefulness for emission studies. Follow-up studies will refer to the present study for the technical details and put more focus on the use cases.

Fig. A1 shows the wind field at a pressure level of 950 hPa for our case study at Nagoya Bay. The wind field (taken from ERA5) was rather constant in direction and speed throughout the region. In addition, the wind field was constant with altitude, as confirmed by analyzing the wind fields for the surface wind and the wind at 800 hPa. While we agree that the quantitative usage of a Lagrangian trajectory model (such as FLEXPART, STILT, HySPLIT) would be beneficial, we refrained from setting up such a system due to the technical complexity and the limited expected gain, as the wind fields were simple during the analyzed period, allowing for qualitative, straightforward interpretation anyway. In particular, we expect that for quantitative interpretation, the resolution of publicly available wind fields (ERA5 ca. 0.25°) would not be sufficient for the local scales of our study, which would imply running a regional weather model first to generate the input for the trajectory model.

Regarding the self-emissions, we can point towards the ship's cruising speed, which was around 14 knots (approx. 7.2 m/s) after the ship left the port (see Fig. A2). Having had such a high cruising speed makes self-contamination virtually impossible if a wind with a constant speed of around 11 m/s blows from a perpendicular direction. This is the case during the time when the two main plumes were detected. We agree with the reviewer that self-contamination cannot be fully ruled out after the ship turned at the tip of Mikawa Bay and traveled in a direction parallel to the main wind direction. In that case, however, clear plume signatures should be visible, especially for NO_x . We find some larger variation in the NO_x data after 14:30 JST (see Fig. 8). Since, however, clouds started to become present (being the reason for the datapoints getting more scarce than before 14:30), they cannot clearly be interpreted as plume signatures, but might also be caused by variability due to scattering. In addition, for such a nearby source, a much stronger enhancement would be expected.

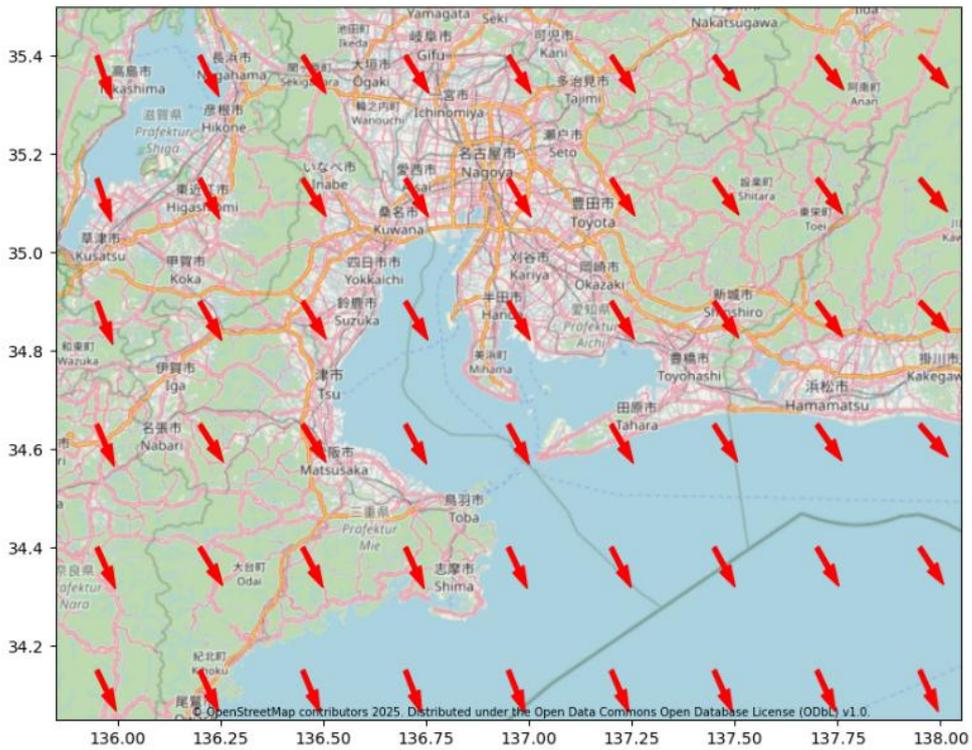


Figure A1: Wind vectors taken from the ERA5 reanalysis (Hersbach et al., 2020) for 14:00 JST on 13 November 2023. The length of the vectors is proportional to the wind speed. The wind field is for a pressure level of 950 hPa.

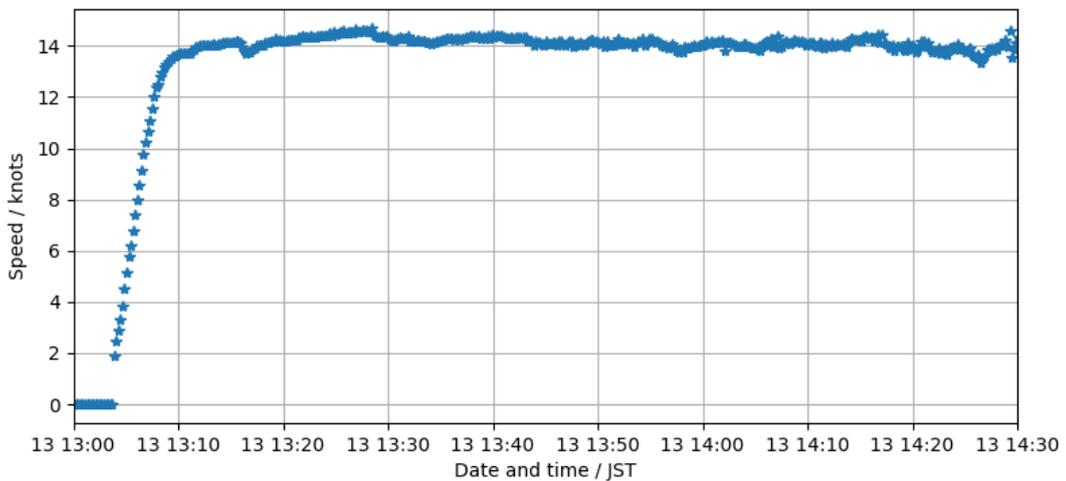


Figure A2: Speed of the vessel during the period depicted in the case study. Speed was measured with the GPS sensor onboard our instrument.

To make these points clearer, we have added the following to the manuscript:

Line 347ff.: “In addition, the chosen period is ideal for this task, since an almost constant, unidirectional wind field with a wind speed around 11 m/s (taken from the ERA5 reanalysis (Hersbach et al., 2020)) makes a simple interpretation of the dataset possible.”

Line 353ff.: “The ship Nichiyu Maru left Toyohashi port (Aichi prefecture) at 13:05 Japan Standard Time (JST), traveled with a speed of around 14 knots (7.2 m/s) westward along the coast of the Atsumi peninsula through Mikawa Bay, [...]”

Line 357ff.: “Since the ship traveled in a direction perpendicular to the direction of the wind at a large speed, self-contamination by the plume of the Nichiyu Maru can be ruled out at least until 14:30 JST. Even afterward, no clear signatures of self-contamination can be found in the data (see Fig. 8).”

Line 464ff.: “The current study is limited by the approximate use of wind directions from the ERA-5 reanalysis (Hersbach et al., 2020), which is possible here because of the closely located sources and a very uniform wind field. These conditions are, of course, not warranted during all days of the deployment, and especially scenarios where the ship is further away from emission sources are of large scientific interest. To study such sources, the usage of wind trajectory models such as FLEXPART (Pisso et al., 2019) is needed. An optimized analysis framework including such trajectories is currently under development.”

2. While the main motivation for this work is to support the validation of satellite measurements, the manuscript does not explain how the proposed emitter estimation approach could be applied in that context. Please include a short discussion clarifying how this method could contribute to satellite data validation.

The authors would like to thank the reviewer for pointing out that this section was not clear. Following also suggestions from reviewer 1, we have restructured our “Conclusion and outlook” section. The main point is that the proposed emitter estimation approach in this paper is for studying local emitters and not for satellite validation. Satellite validation is just another use case for the instrument, which was already demonstrated in our previous studies (Klappenbach et al., 2025; Knapp et al., 2021; Butz et al., 2022). The reason why satellite validation is mentioned in the outlook is that the new functions of the instrument (remote operation, added VIS-spectrometer) make it much easier to use the instrument for this use case, or make a validation of a broader selection of satellites possible. We hope that the modified version of the final section makes this clearer:

“[...] This shows that our shipborne measurements of CO₂, CH₄, and CO are compatible with the standards of the COCCON and, in consequence, they can be adjusted to the TCCON scale through a multiplicative adjustment. Thus, our setup is suitable for several potential use cases.

Here, we have demonstrated, in a case study, the detection of plume enhancements of ΔCO_2 , ΔCO , and ΔNO_2 , carrying the outflow from the heavily populated Nagoya region. [...] Building on the demonstrated potential of the instrument, long-term shipborne installations will enable systematic emission monitoring along coastal hotspots.

A second use case, satellite validation over the open ocean, where currently almost no data exists (Müller et al., 2021), was already demonstrated for the ship-borne FTS, still requiring on-board personnel, in our previous publications (Klappenbach et al., 2015; Knapp et al., 2021). Having now an

upgraded instrument that can operate fully remotely for several weeks and that is compatible with COCCON standards, as demonstrated within this study, makes future routine deployments for satellite validation over the open ocean possible. In addition, the added capability to simultaneously measure NO₂ makes our instrument an ideal platform for the validation of the newest generation of satellites measuring this air pollutant along with the greenhouse gases, if an appropriate route for the ship is chosen.”

L4: Please change to: "direct-sun DOAS"

Changed.

L12: I recommend replacing "concentration" with "volume mixing ratio" throughout the manuscript, as only VMR (and VCD) values are presented in the figures.

We thank the reviewer for pointing out the ambiguity in the use of the term “concentration.” Since “concentration” was used in several sentences of the manuscript to refer to both VCD and VMR, we decided to replace it with the general term “trace gas abundance” or “abundance” to avoid any ambiguity. The following changes have been made:

Line 12: “concentration” -> “trace gas abundance”

Line 19: “concentrations” -> “abundances”

Line 20: “concentration” not replaced, since it is used in terms of “concentration gradients” needed for top-down estimates.

Line 21: “concentration measurements” -> “column measurements”

Line 26: “concentrations” -> “abundances”

Line 43ff: Sentence removed.

Line 45: “concentration” -> “vertical column densities (VCD)”

Line 56: “concentration” -> “trace gas abundance”

Line 255: “Concentrations” was not exchanged, since it is the correct wording here for this general discussion of Langley’s method.

Line 365: “background concentrations” -> “background column”

Line 389: “concentrations” -> “abundances”

Line 407: “concentration enhancements” -> “column enhancements”

L14: You mention emission monitoring but do not provide emission estimated for the case study. It should be possible to calculate emissions using the VCD data together with wind/vessel speed vectors.

The authors thank the reviewer for this suggestion. However, we would like to point out that this is not as easy as it seems at first glance. The authors think that the reviewer here points at the mass balance method, which was, e.g., also used with a similar setup by our group in Luther et al. (2019). This method is, however, only applicable for isolated sources. The latter is not true for the two

plumes in the case study, which we have identified as a superposition of two main and several minor sources. It is therefore not possible to give a simple emission estimate within the scope of this study. Modelling of several plumes with an inversion framework (e.g., based on Lagrangian footprint calculations) would be needed for that, but we deem it outside the scope of the present study.

We have nevertheless tried the mass balance approach (as outlined in Luther et al. (2019)) and calculated emissions of around 6 Mt/yr of CO₂, 6.7 kt/yr of NO₂, and 370 kt/yr of CO for the whole double plume structure. If the emissions of all cells in the Nagoya harbor area are summed up, EDGAR v6.1 gives us emissions of approximately 3.5 Mt/yr of CO₂, 4.8 kt/yr of NO₂, and 200 kt/yr of CO. While it is a good indicator for the overall performance of our instrument that the emissions are in the same order of magnitude, a direct comparison is not possible, since it is not clear which grid cells of EDGAR contributed to the actual plume and which sources are only included in the background which was subtracted. In addition, as discussed above, these calculations do not help us with respect to source attributions, which is the main goal of the case study. We therefore decided not to include them in the paper.

L90: Which part of the light beam is used for the DOAS measurements? Please elaborate on this point.

The following has been added to the manuscript, also following suggestions by reviewer 1: “The DOAS instrument uses part of the light beam from the solar tracker, which is not needed for the FTS. This is possible since the beam from the solar tracker has a diameter approximately twice as large as the aperture of the FTS. The DOAS itself needs less than 0.15% of the total beam diameter. The aperture of both instruments is sufficiently far away from the fringes of the beam of the tracker mirror. The part of the solar beam needed for the DOAS is coupled into an optical fiber[...].”

Section 2.2: Since stable temperature is crucial for DOAS performance, what was the average detector temperature variation during the case study? How might any temperature variations have influenced your results?

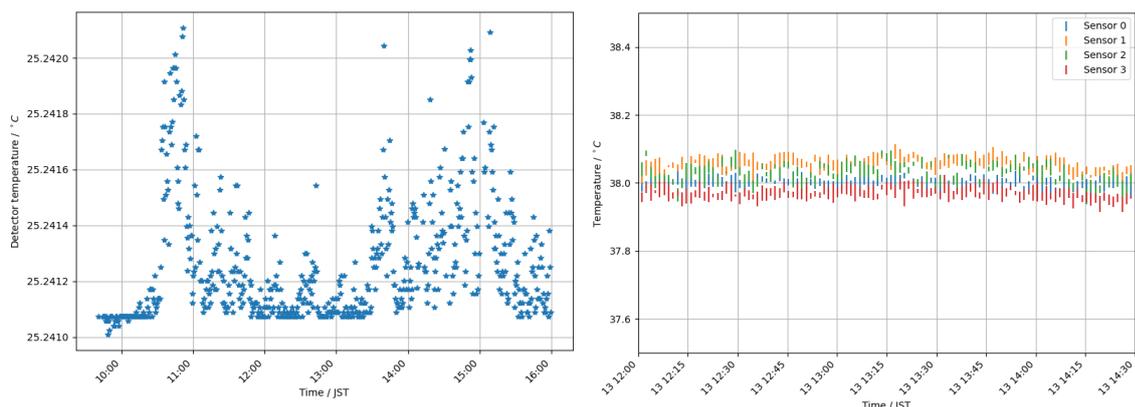


Figure A3: Temperature of the DOAS detector (left) and temperature of the DOAS box measured with four different sensors (right) on November 13 during the period of the case study.

During the case study, the detector temperature of the DOAS varied by less than 0.0015 K (see Fig. A3). The temperature inside the DOAS box (i.e., between the first and the second cooling stage) varied by less than 0.2 K (see Fig. A3). Similar variations were found during most other days. All measurements where temperature variations exceeded 0.5 K were manually flagged and excluded

from further analysis. Due to the low detector temperature variations, we do not expect any influence on our results.

L146: If ancillary in situ data are available, please specify what kind of data these are and explain why they were not included in the manuscript.

The in situ data collected onboard the Nichiyu Maru within the framework of SOOP consists of meteorological data and trace gas data. The meteorological data were partly used within this manuscript, since one of the in situ pressure and temperature sensors of our instrument failed during the deployment. Therefore, the “in situ ground-based pressure and temperature measurements” mentioned in lines 190f. used for the EM27/SUN retrieval are taken from the SOOP dataset. In principle, the in situ sensors onboard the Nichiyu Maru also collect wind data, which is a capability our instrument does not have. Unfortunately, due to some issues with sensor location, the in situ sensors did not correctly compensate for the motion of the ship when measuring the wind direction. We therefore refrained from using the in situ wind data and used the ERA5 dataset instead.

The sensors onboard the Nichiyu Maru also measure in situ trace gas abundances of, e.g., CO₂, CH₄, CO, and NO_x using standard gas analyzers. Since the in situ instruments have, in general, a different sampling footprint than the remote sensing spectrometers, a simple comparison is, however, not possible within the scope of this publication. A second manuscript focusing on a comparison between the in situ and remote sensing data, including a discussion of caveats and reasons for differences between the instruments, is currently under preparation.

We have added the words “meteorological and chemical in situ data” in line 146 of the manuscript to make clear what type of data is collected.

L153: It seems unlikely that no ship plumes were detected during several months of measurements. The wind and vessel velocity vectors in the first figure even suggest possible self-emissions. Please elaborate on how you ensured that signals from the vessel's own emissions were excluded.

The Nichiyu Maru usually travels at a high speed compared to other ships of around 14 knots (approx. 7.2 m/s). Unless a strong wind comes from the opposite direction of the ship's travel direction, self-contamination is not possible since the chimneys are around 100 m behind our instrument. In order to also rule out self-contamination during unfavorable wind conditions, all recorded days were manually checked for suspicious plume signatures, such as plumes with large amplitude, but small width, or repeating patterns of such plumes. No such suspicious signatures were found while the ship was moving. However, as also mentioned in the manuscript, suspicious signatures were found while the ship was docked in harbors. An example is shown in Fig. A4.

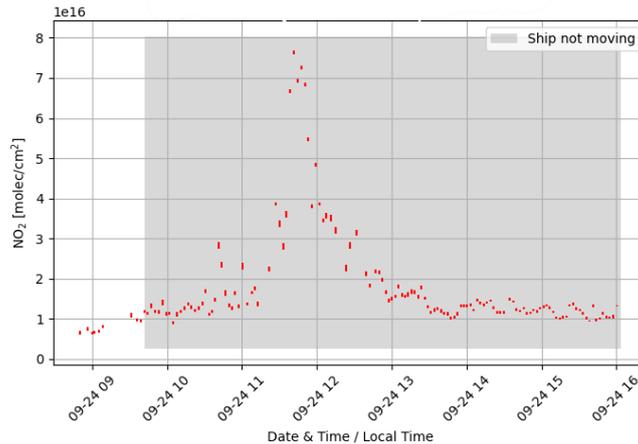


Fig. A4: NO₂ VCD recorded on 24 September 2023, where the ship was in the harbor most of the day. Since the wind direction was changing during this day, several plumes swept through the field of view of the spectrometer. These plumes can either be interpreted as self-contamination from the ship or as the signature of nearby sources.

It should be noted that contamination by other ships, e.g., travelling in the opposite direction, cannot be ruled out. However, such scenarios would lead to plume signatures with very small widths. Such signatures were found in the data by manual analysis and excluded from further interpretation.

L162: How many days of daytime measurements are available, and how many of these contained useful data?

In total, 112 days of daytime measurements are available. On 57 days, the weather conditions were favorable enough to have useful data produced at least during part of the day.

Following also suggestions from reviewer 1, we added after line 163: “In total, daytime measurements are available for 112 days of the deployment. On 57 of these days, the weather conditions were favorable enough to have useful data produced at least during part of the day (see Sect. 4 for information on quality filters included in the retrieval).”

Table 2: Which windows were used? Why do you show all windows here?

The RemoTeC algorithm uses all the retrieval windows listed in Table 2 simultaneously. If multiple windows contain a certain molecule as a target absorber, its abundance is fitted simultaneously across all the windows. For example, CO₂ is retrieved from two windows simultaneously. Splitting the spectrum into several windows is useful to exclude uninformative spectral regions in between and to avoid overly complex parameterizations of the background radiance polynomials.

L204: What percentage of your measurements was filtered based on the pressure comparison? What is the maximum deviation between surface and measured pressure values?

Since the percentage depends heavily on the weather conditions, we did not calculate a number for the whole campaign. On the day of our case study (a day with good weather conditions), for example, 1.6% of our measurements got flagged by the pressure filter, and 2.7% got flagged by the DC filter. The mean deviation between the surface and spectroscopic pressure is 29.3 mbar on the

day of the case study. The maximum deviation is 34.1 mbar, leading to a mean offset-subtracted maximum deviation of 4.8 mbar.

L218: Is this correction also applied to the NO₂ VCDs? Typically, such effects are accounted for using air mass factors derived from radiative transfer models. Have you considered using model-based AMFs, and how would you expect the results to differ from those obtained with the geometric AMF, including the applied correction? The geometric AMF does not account for aerosols, which were likely present. Please discuss how aerosol effects may influence your results and the implications for the AMF approach.

The airmass-dependent bias in EM27/SUN measurements is not the same as the bias that is corrected for using model-based AMFs in a DOAS retrieval. The airmass-dependent bias is most likely related to instrumental artifacts, from which all direct-sun viewing FTS suffer (see Wunch et al., 2011).

Our DOAS spectrometer collects direct sunlight with a narrow field-of-view (1.15°). Direct sunlight is very bright, and single-scattered light that enters the telescope has almost the same lightpath as direct sunlight. Thus, it requires light that has at least been scattered twice to induce significant lightpath changes. Given that all our data have reasonably large sun elevations (SZA < 70°) and that the sun tracking requires clear-sky conditions anyway, we consider the contamination by second-order and higher-order scattering negligible.

In general, scattering by aerosols leads to a higher DC-variability in the EM27/SUN data. Since we perform a filtering for this as detailed in Sect. 4.1, it is not expected that our filtered EM27/SUN dataset still contains spectra with artifacts caused by scattering at aerosols. A similar filter is included in the processing of the DOAS data, which filters for large detector saturation variations (i.e., brightness fluctuations), as detailed in Sect. 4.2. We, therefore, also do not expect that spectra perturbed by aerosol scattering influence our final, filtered dataset.

Table 3: Why did you include IO in your fit? Did you detect any tropospheric IO above the instrument's detection limit?

Figure A5 shows the retrieved IO dSCDs for the day of the case study. It can be seen that IO dSCDs around the detection limit have been found. In order to confirm or exclude IO detection, we would need to set up a retrieval dedicated to IO (not NO₂), e.g., for the optimal spectral range. Running a retrieval specifically targeting IO is out of scope for this publication. We have run sensitivity studies with various DOAS configurations, including with and without IO as an interfering absorber. These sensitivity studies show that retrieved NO₂ dSCDs are robust against changes in the DOAS configuration, as can be seen in Fig. A6.

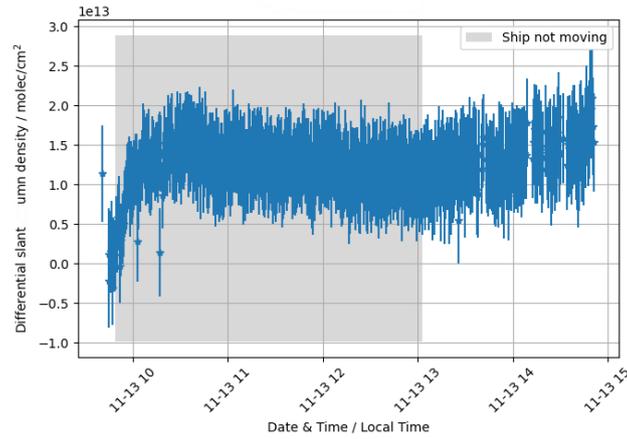
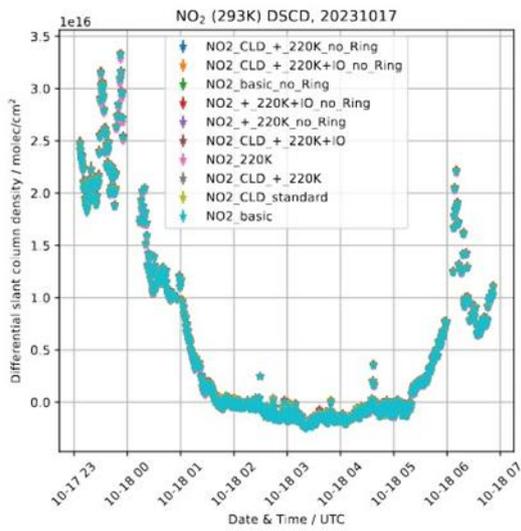
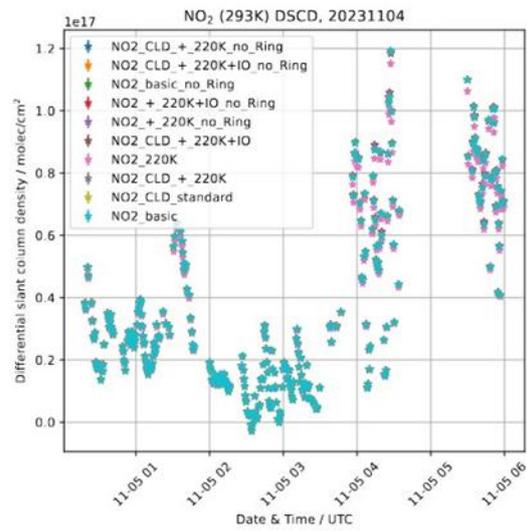


Figure A5: dSCD for IO measured on 13 November 2023. The error bars correspond to the fit error.



(a) October 17, 2023.



(b) November 4, 2023.

Figure A6: Final NO₂ dSCDs retrieved with all retrieval scenarios included in the sensitivity study. While the left panel shows results for a day with good weather conditions, the right panel shows results for a day with large brightness fluctuations caused by high-altitude clouds. Absorber cross-sections were used as defined in Table 4. Within the case studies, the additional absorbers “Ring”, “CLD”, “IO”, and “NO₂ at 220K”, which are used in the retrieval scenario outlined in Table 4, were partly switched off as detailed in Table A1. Within the main manuscript, the scenario “NO₂_CLD+_220K+IO” is used.

Name	NO ₂ at 220K	Ring effect	CLD	IO
NO2_CLD_standard	No	Yes	Yes	No
NO2_basic_no_Ring	No	No	No	No
NO2_CLD+_220K+IO	Yes	Yes	Yes	Yes
NO2_CLD+_220K_no_Ring	Yes	No	Yes	No
NO2_220K	Yes	Yes	No	No
NO2_basic	No	Yes	No	No
NO2_CLD+_220K+IO_no_Ring	Yes	No	Yes	Yes
NO2_CLD+_220K	Yes	Yes	Yes	No
NO2+_220K_no_Ring	Yes	No	No	No
NO2+_220K+IO_no_Ring	Yes	No	No	Yes

Table A1: Definition of the retrieval scenarios included in the sensitivity study shown in Fig. A6. The scenario used in the main publication is “NO2_CLD+_220K+IO”.

L246: Did you test alternative noon spectra, and if so, how did this affect your reference SCD?

The reference SCD for NO₂ is determined according to the Langley method described in section 4.2 and illustrated in Fig. 5. We chose our reference noon spectrum such that the reference SCD is small, i.e., such that the dataset (e.g., used in Fig. 5) has few negative NO₂ dSCDs. For determining the reference SCD, however, the fit uses all the 20% lowest dSCDs (orange in Fig. 5). Choosing a different reference spectrum would simply shift the zero crossing of the Langley fit and thus, yield the same total SCDs.

L314: Please provide an explanation for the higher mean error (ME).

Between the SRF measurement in February and May 2024, the instrument was moved with a crane twice to be set up and taken down from the ship. During one of these moves, the crane heavily bumped the instrument against parts of the ship. We assume that this led to a change in detector alignment, which is the reason for the change in ME. It should be noted that only the SRF from channel 1 was used in the final retrieval setup, since it is considered more reliable in general.

We have added the following sentence to the manuscript: “It is assumed that this change is caused by an incident where the crane bumped the spectrometer against parts of the ship when it was lifted to the upper deck in February 2024, leading to a slight misalignment of the second detector channel. Since we consider the SRF measurement based on channel 1 more reliable, we have used an SRF from that channel for the final retrieval described in Sect. 4.1.”

L330: It is unclear to me why an additional scaling is necessary given that a correction was already applied earlier. Could you please include an example (perhaps in the appendix) illustrating how your corrections and scaling steps affect the final results?

It is not clear to the authors what the reviewer refers to when asking about additional scaling. As outlined in lines 216ff., all measurements are referenced to TCCON (and by that to WMO standards) by a single scaling factor. This procedure is standard, and, e.g., is also applied to all data of the COCCON network. In line 216, it is referenced to Sect. 4.3 regarding a detailed description of how these scaling factors are calculated for our instrument. The procedure is then discussed in lines 321ff. in detail. Lines 330ff. only state that the factors, whose calculation is outlined here, are used for the scaling as described in Sect. 4.1.

Figure 7: Please include wind speed information. If the wind speed remained relatively constant, state this in the figure caption. If not, update the figure to show the wind speed variations. Additionally, include the vessel's velocity in the caption and discuss the potential contamination from the vessel's own emissions. Could you estimate a characteristic gas signature of the vessel's plume?

Please refer to our reply to general comment 1.

L364: Were any CH₄ enhancements observed in the measurements? Please also explain the decreasing trend shown in Figure 8.

On some days of the deployment, enhancements of up to 20 ppb were observed. The case day presented in the manuscript is therefore not ideal to discuss methane enhancements. Because of that and because methane is not needed for the proposed analysis approach, we have omitted a detailed discussion in the publication.

It can also only be speculated about the cause of the downward trend of around 6 ppb shown in Fig. 8. There are several LNG terminals in the harbor of Nagoya and, in addition, Nagoya has a large city gas network (https://www.tohogas.co.jp/lang/en/corporate/document/annual/pdf/2023toho_tougou_en_print.pdf, page 71). Urban plumes caused by fugitive emissions from natural gas infrastructure are known from other studies (e.g., Jones et al., 2021). Such a scenario would also fit the observation that the methane column decreases while the ship moves away from Nagoya. In order to confirm this interpretation, measurements at a larger distance from Nagoya would, however, be needed to check whether the trend continues.

Figure 10: Adjust the color scale so that weaker emitters are visible. It would also be helpful to include a comparable figure illustrating CO₂ emissions.

The authors would like to thank the reviewer for this suggestion. We have changed the color scale and also set all grid cells with CO emissions below 1000 t/yr to grey in order to make weaker emitters more visible. The updated figure is shown below:

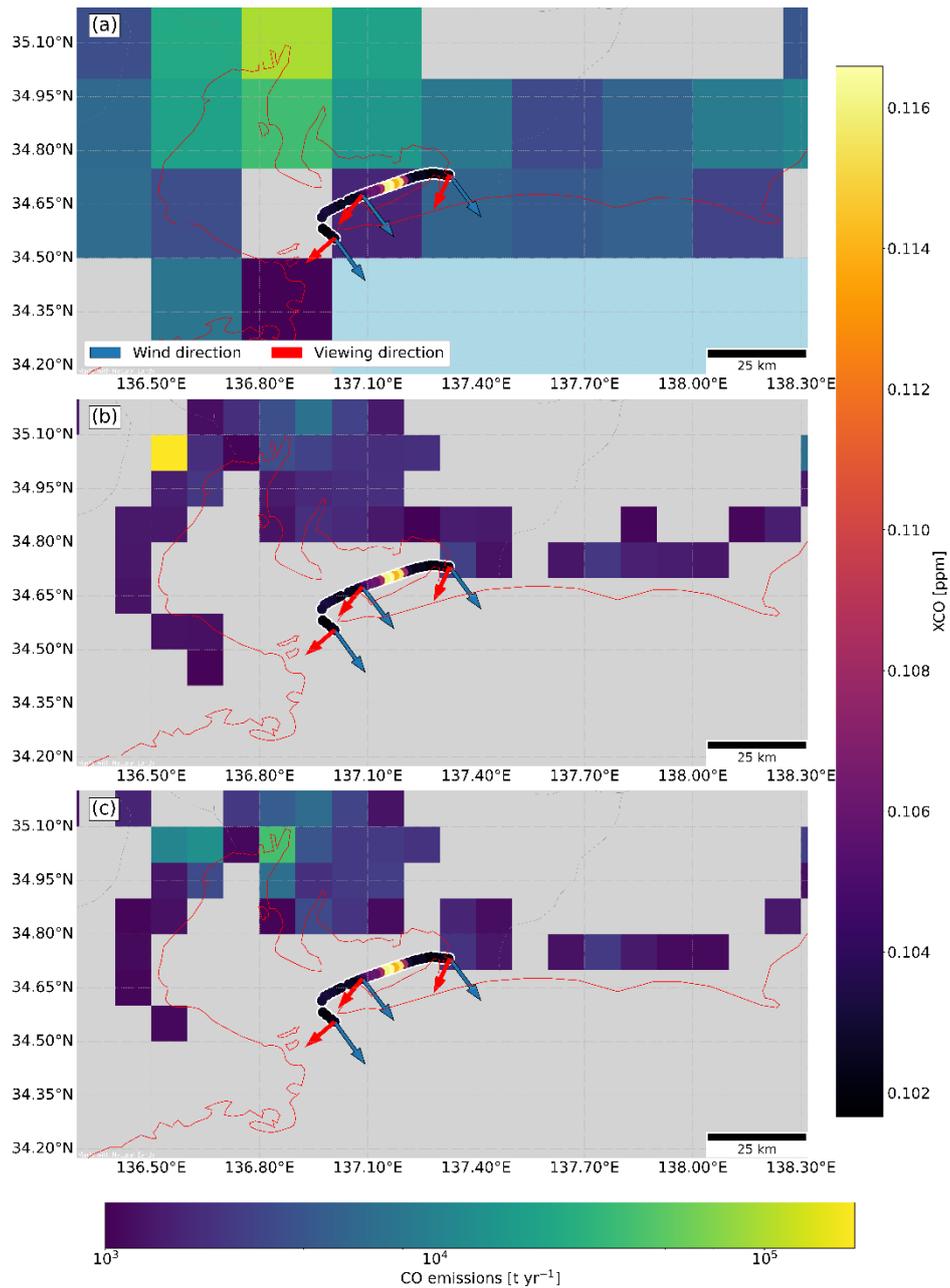


Figure A7: Updated version of Fig. 10 from the manuscript.

The following has been added to the caption: “Cells with emissions below 1000 t yr⁻¹ according to the respective inventory are shown in grey.”

Figure A8 shows the same plot for CO₂ as suggested by the reviewer. However, as stated in the manuscript, only one source with strong CO but not that strong of CO₂ emissions has been misplaced. Therefore, there are no major differences between the two versions of EDGAR visible. Since all main sources in the Nagoya area are point sources, which are already visualized in Fig. 7, we do not see any added value in adding Fig. A8 to the manuscript.

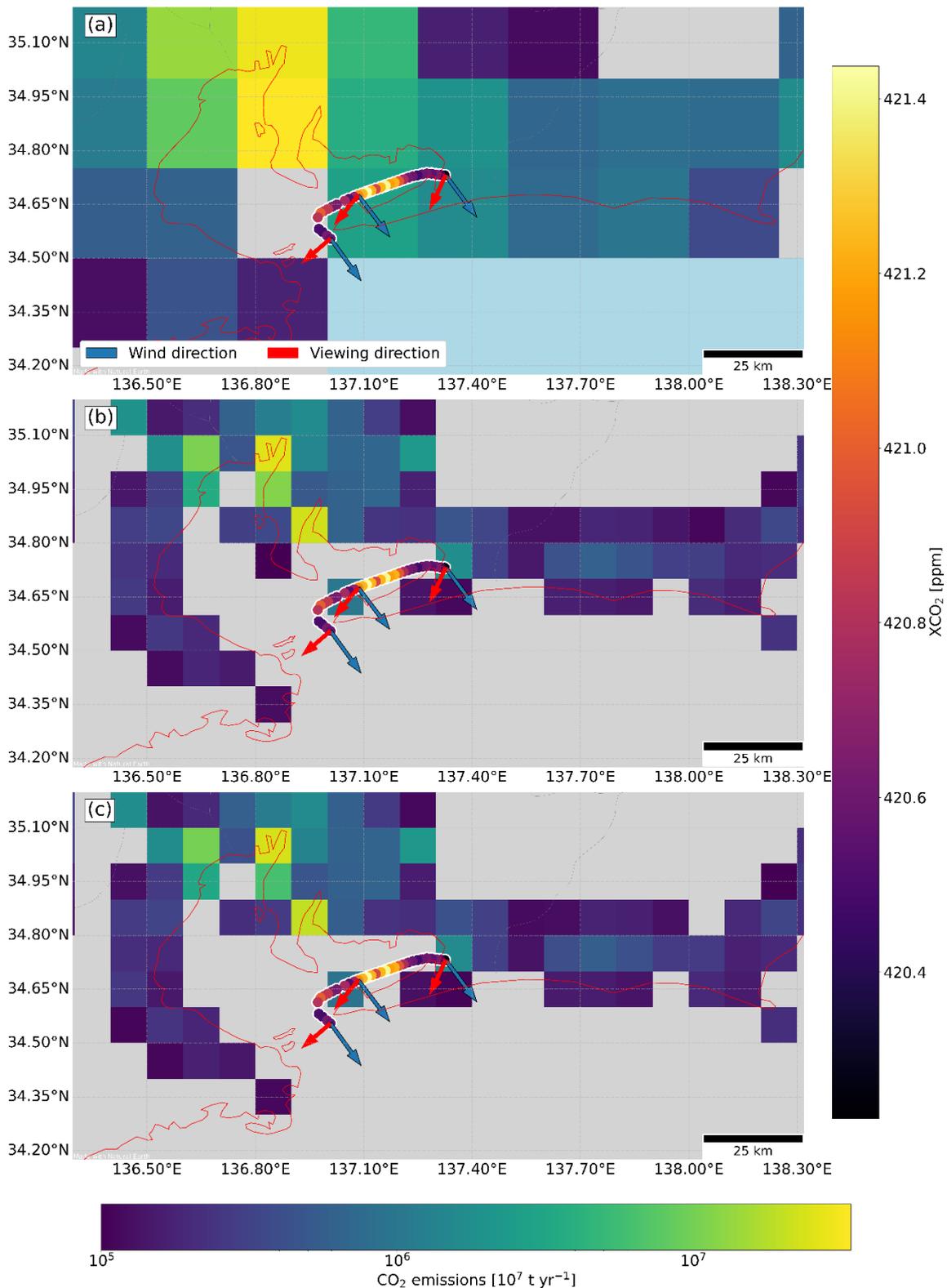


Figure A8: Comparison of measured XCO₂ enhancements to CO₂ emission patterns from different inventories: (a) REAS inventory for 2015 (Kurokawa et al., 2020); (b) EDGAR v6.1 for 2018 (Crippa et al., 2018); (c) EDGAR v8.1 for 2022 (Crippa et al., 2023). Cells with emissions below 10⁵ t yr⁻¹ according to the respective inventory are shown in grey. Wind directions (blue arrows), interpolated to the location and time of the measurement, are taken from ERA-5 (Hersbach et al., 2020). Viewing direction is shown by the red arrows. Note that the REAS inventory (a) only includes land grid cells.

L396: From Table 7 it seems that you refer to oil rather than lignite because the NO_x/CO₂ ratio is larger for oil.

We thank the reviewer for this comment. We have revised the wording to avoid implying a direct causal relationship between fuel composition and the observed NO_x/CO₂ ratios. The key point here is that lignite contains much more oxygen and much less carbon than crude oil. The excess oxygen (above the optimum ratio) decreases the combustion efficiency and, by that, reduces the combustion temperature. Because of the temperature-dependence of the Zeldovich reaction, this in the end reduces the NO_x production. This leads to a lower NO_x/CO₂-ratio for lignite. It should be noted that all these mechanisms are non-linear.

In order to avoid this ambiguity, we have rephrased the sentence in the following way: “Concerning fuel type, for example, lignite coal contains, on average, much less carbon, much more oxygen, and about the same amount of nitrogen as crude oil (Klemm and Hoppe, 1980), leading to a higher NO_x/CO₂-ratio for oil-fired compared to coal-fired power plants (Guevara et al., 2024) through non-linear reaction mechanisms.” Note that we also changed the wording from “more nitrogen” to “about the same amount of nitrogen”, since the difference in nitrogen content is almost negligible compared to the difference in oxygen and carbon content.

L414: meared --> measured

Changed.

L463: Please add wind trajectories to the discussion.

Please refer to our reply to “general comment 1”, where changes to the manuscript are also listed.