Before addressing the specific comments provided by each reviewer, we provide a general statement outlining the main revisions implemented in the manuscript.

The reviewers highlighted two key aspects that require improvements of the study:

1. **A more in-depth comparison between in situ observations and model results, including a discussion of the processes explaining their differences;**
2. **A revision of the section previously referred to as "Estimated uncertainties in FCO_2", particularly regarding terminology and its actual scope.**

The primary objective of this study was to investigate the variability of surface pCO_2 in the Southern Adriatic sea using a recently validated time series (Dentico et al., 2026). A key aspect of the analysis was to provide observationally constrained estimates of CO_2 flux in a region where direct measurements remained scarce and temporally discontinuous. In this context, the model was used as a complementary tool to partially compensate for temporal gaps in the dataset to support the interpretation of the results rather than a validation target. Consistent with this scope, the revision has focused on strengthening the interpretation of the model-data comparison rather than introducing extensive new analyses.

We have instead prioritised a substantial revision of the section on CO_2 flux uncertainty estimation, where additional analyses have been implemented. In the revised manuscript, we tested:

1. **Different wind inputs (in situ observations and ERA5 reanalysis)**
2. **Different temporal resolutions of the in situ wind data (hourly, 6-hourly, and daily averages)**
3. **Alternative parameterisations of the gas transfer velocity *k*, including the quadratic formulation of Wanninkhof et al. (2014), the cubic formulation of Wanninkhof and McGillis (1999), and the hybrid formulation of Nightingale et al. (2000).**

These analyses result in a revised set of CO_2 flux estimates, presented as annual averages and seasonal values (winter/convection period and summer/post-convection period).

General comments

This paper presents a surface ocean pCO_2 time series, along with other parameters, measured in the Southern Adriatic Sea (SAd), from April 2015 to May 2024. This time series had already been presented in Dentico et al., 2026 and this is the continuity of that work. In this more recent paper, a valuable addition was added: the CO_2 flux. The CO_2 flux estimated from these in situ observations was compared to the CO_2 flux estimated using an ocean reanalysis (referred to as model data) and the estimated uncertainties from different methods were discussed. Additionally, the pCO_{2sw} was separated into its thermal and non-thermal components, and the source-sink dynamics for summer-winter, in the SAd were discussed. Two seasonal phases were identified but unfortunately gaps in the dataset prevented further investigation on the role of certain processes, such as the impact of bloom phases in summer on the pCO_{2sw}. Nonetheless, this analysis offers valuable insight on the processes governing air-sea CO_2 exchanges in that region.

Although the authors gave a detailed comparison of their study to surface ocean pCO_2 variability in the surrounding regions of the Mediterranean Sea, a more quantitative comparison would have been useful. Some values are mentioned for dense water formation sites, for instance the flux in the northern Adriatic Sea and the carbon sequestration rate in the Gulf of Lion, but this comparison could have been deepened.

Once some minor modifications are made, this works merits publication. Indeed, this study provides useful, recent, CO_2 flux estimates, discusses systematic uncertainties and enables a model (reanalysis and forecast) sea surface pCO_2 validation in that region. While I have no doubt that these results should be published, I still have some questions and I have suggested minor modifications, listed below.

We thank the reviewer for the careful reading of the manuscript and for the constructive comments provided.
In agreement with the reviewer's observations, and consistently with comments raised by the other reviewers, we acknowledge that the comparison between the Southern Adriatic sea and other Mediterranean regions can be strengthened. In the revised manuscript, we will expand this discussion by including a more quantitative comparison of surface *p*CO_2 variability and air–sea CO_2 flux with other Mediterranean regions. We also

agree on the importance of better clarifying the relationship between the present study and Dentico et al. (2026). In the revised manuscript, we will more explicitly state in both the Introduction and the Data and Methods section that this work represents a continuation of the previously published ESSD paper.

Minor comments and modifications

Section 1: Introduction

Lines 41-48: In the introduction, there is a detailed description on ocean acidification, and while it might be useful to give the general context, there is no further mention on ocean acidification later, this part could be condensed to one or two sentences.

We agree with the reviewer and, consistently with a similar comment raised by another reviewer, we will substantially reduce the discussion of ocean acidification in the Introduction. In the revised manuscript, this topic will be briefly mentioned only to provide the general context, while the focus of the Introduction will be shifted towards the peculiarities of the Mediterranean Sea carbonate system and the processes more directly related to the objectives of the present study.

The references should be updated, SOCATv2022 is cited instead of the latest version.

We will change this reference and we will cite the latest version of SOCAT.

Similarly, a more recent version should be cited: *Bakker, D. C., Alin, S. R., Bates, N. R., Becker, M., Feely, R. A., and Gritzalis, T. (2023). SOCAT version 2023- An alarming decline in the ocean CO2 observing capacity.*

We will update this reference.

Lines 67-70: "*The latest estimates from Roobaert et al. (2024) quantified the global coastal CO2 flux equal to -2.2 GtCO2 yr-1 (-0.6 PgC yr-1) which represent around 21% of the total current CO2 flux from the atmosphere to the ocean (10.5 GtCO2; Friedlingstein et al., 2025).*" Is 2024 really the latest estimate?

In the original version of the manuscript, the term "*latest*" was intended to refer to the most recent estimate reported by the cited authors. In the revised manuscript, we will reformulate this sentence more carefully and update the discussion by including more recent estimates of coastal air–sea CO_2 flux when present.

Lines 92-101: *"The paper is organized as follows. Section 2 describes the study area, Section 3 describes the data used and the thermodynamic calculation performed to estimate carbon flux. Results are presented and discussed in section 4. Conclusions are provided in Section 5."* Is that necessary? I suggest removing this.

We will consider this suggestion in the revised version of the manuscript.

Section 2 - Study area: Some paragraphs on the study area are taken from Dentico et al., 2026, section 2.2, it should be cited appropriately.

Although there is a mention of Dentico et al., 2025 (to be corrected to 2026) in section 3, it should be clear from the start that this study is the continuity of a previous, already published, work. The abstract is misleading (lines 18-20): *"In this study, a newly validated, decade-long (2015-2024) high-resolution time series of CO2 … has been analysed."*

We agree with the reviewer. In the revised manuscript, we will clarify from the beginning that the present study represents a continuation of the previously published ESSD paper, where the observational dataset and quality-control procedures were described in detail. Accordingly, in the revised Abstract, the expression "*a newly validated*" time series will be replaced with "*a recently validated*" time series.

Legend of figure 1: I don't see the red lines.

The legend of the figure will be corrected in the revised manuscript.

Legend of figure 2: Correct to Dentico et al., 2026 (not 2025). Same comment for the rest of the paper.

All the references related to Dentico et al. (2025) will be corrected in the manuscript with Dentico et al. (2026).

Figure 3a: There are a lot of gaps in FCO_2 estimated from in situ observations, and this $FCO_{2EMSO-E2M3A}$ doesn't seem to compare well with the reanalysis FCO_{2RD}.

Considering this, can it be concluded with certainty that this region acted as a mean sink for the 2015-2024 period?

We thank the reviewer for raising this important point. We agree that the temporal gaps in the observational dataset and the differences between observation-based and reanalysis-based CO_2 flux estimates require a cautious interpretation of the mean sink/source behaviour of the region. As described in the general statement to the reviewers, the revised manuscript will include additional analyses assessing the effect on the CO_2 flux estimates of different wind products, wind temporal resolutions, and gas transfer velocity parameterisations. These analyses will help to better constrain the robustness of the estimated annual flux. In addition, we will include a map of the annual surface CO_2 flux from CMEMS products to better contextualise the E2M3A observatory within the regional-scale Southern Adriatic carbon flux patterns.

Line 409: *"March data were available only in 2024*" – on figure 3b should the mean be shown? If there was data available only in March, showing the "mean" here is misleading.

Which method does figure 3b use, M1 or M2?

In the revised manuscript, we will clarify that the annual average in the figure (black line) was calculated using M2. In both M1 and M2 approaches, March 2024 was included in the calculation because the month satisfied the minimum data coverage criterion adopted, i.e., at least 14 observations available within the month.

Line 415-416: *"Further, these results confirm the role of dense water formation sites as primary drivers of carbon sequestration."* The results show a negative CO_2 flux at the surface, but this doesn't necessarily indicate carbon export. This only indicates exchanges at the surface, not storage at depth. I suggest rephrasing that sentence.

We thank for highlighting this important distinction. In the revised manuscript, we will therefore rephrase this sentence to avoid overinterpreting the results. In the original version, the statement was intended in a more general sense, referring to the potential role of deep water formation regions, such as the Southern Adriatic Sea, in contributing to carbon transfer towards deeper layers through convection processes.

432-434: “*The use of 6-hourly wind speed averages is widely adopted as it smooths extreme values, …*” Add a reference.

We will add a reference. For instance, Wanninkhof, 2014 was already mentioning it.

Lines 482-485: “*As shown in Dentico et al., (2025) the measured values by Pro-Oceanus membrane sensor can report higher (>10 µatm) differences with pCO2sw calculated from discrete seawater samples. Here, it was not possible to compute a statistically robust measure of this uncertainty.*” Was there no drift of the Pro-Oceanus CO_2 sensor?

As described in Dentico et al. (2026), no additional drift correction was applied to the Pro-Oceanus *p*CO_2 sensor data, as no significant sensor drift was identified during the quality-control assessment of the time series.

Line 506: “*Finally, in autumn and spring the mean differences were smaller (+5.13 µatm and -16.12 µatm, respectively).* » This sentence should be moved to the end of the paragraph.

We will move this sentence.

Section 4.4, lines 500-509: Why was pCO_{2sw} from the probe higher than the reanalysis data in 2022, was the reanalysis product not able to simulate the Marine Heatwave?

We will detail the discussion on the *p*CO_{2sw} difference between the probe and the model. In the manuscript we will highlight that the strong difference is likely driven by the combined effects of warming, enhanced stratification, and biogeochemical processes. Such interactions could be difficult to represent in current biogeochemical models, which may explain the model inability to reproduce the observed *p*CO_{2sw} difference.

Figure 5: Why use AF, a forecast, if RD, a reanalysis is already available?

We chose to include both the AF and RD products because, despite being derived from a similar modelling framework, they integrate different types of input data and assimilation approaches. Our objective was therefore to investigate whether the AF product could reproduce variability patterns that are less evident in the RD product.

Lines 533-541: Again, on figure 6, there seems to be a large discrepancy between FCO_2 estimated from measurements and FCO_2 RD. I'm not convinced by this last paragraph, given those discrepancies, there is no guarantee that the model reanalysis bias would be the same as the observation's bias.

We agree with the reviewer that the discrepancies between observation-based and reanalysis-based CO_2 flux estimates require caution in the interpretation of the long-term mean fluxes. In the original version of the manuscript, this comparison was intended mainly as a qualitative assessment, considering that the model was able to reproduce some of the main observed seasonal patterns. We also explicitly stated that these estimates should be interpreted with caution. We will rephrase this paragraph to further clarify this aspect.

Line 538-539: "*-4.9 mmol m^{-2} day^{-1} and -4.5 mmol m^{-2} day^{-1}, respectively for the two M1 and M2 methods*" Is this for the whole period? If so, what would be the mean flux (estimated from observations) obtained using only days with observations, without B?

What is the value of B and what is the mean flux using FCO_2 RD?

What is the difference with (lines 559-560): *"These calculations resulted in a mean sink flux of -1.15 mmol m^{-2} day^{-1} (M1) and - 0.76 mmol m^{-2} day^{-1} (M2) in the period 2015-2024*"? This is not very clear.

In the revised manuscript, we will explicitly report the value of B and clarify how it is used in the flux calculations. We will also include the mean FCO_2 derived from the RD product. We will also clarify the different flux estimates (observation-based fluxes, RD-derived fluxes, and estimates obtained using gap-filling or correction procedures) in the updated version of the manuscript. The values reported in lines 559–560 refer only to fluxes calculated from the in situ observations using the M1 and M2 approaches.

I understand that to be robust M1 and M2 are compared, but I feel that this might have been better in supplementary. Why not choose one method? Otherwise, it can be confusing, the reader must go back again, to check which method is M1 and which one is M2.

We agree with the reviewer and in the revised manuscript we decided to keep only M2 and move the description of M1 in the Supplementary Material.

In general, there are a lot of acronyms, which are described only once, and it can be difficult to follow at times.

We agree with the reviewer and, in the revised manuscript, we will simplify the text by reducing descriptions that are not directly relevant to the scope and interpretation of the study. Consequently, the number of acronyms used will be substantially reduced.

The highest uncertainty on FCO_2 due to U (section 4.3) is around 3 mmol m^{-2} day^{-1}, which is considerable considering the mean values. Considering these uncertainties, I'm not convinced on the affirmation that this region was a net sink on the 2015-2024 period.

Similarly to what was pointed out before, the new analyses will help to clarify the role of the southern Adriatic as a weak-to-moderate sink in the period 2015-2024.