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 comment:

In this manuscript authors describe and interpret new pCO2 data in the Adriatic in 2015-2024. These data were recently presented in detail and published (Dentico et al, 2026). Here authors used the pCO2 data first to separate the drivers (only thermal and non-thermal terms are quantified); the data are then used to calculate CO2 fluxes. As the data were not continuously acquired, two approaches (M1 and M2) are presented to derive CO2 fluxes (synthetized in Table 2) and authors also addressed the uncertainty of the fluxes depending the wind (synthetized in Figure 4). They found that the region is a CO2 sink in October-April and a CO2 source in May-September. The seasonality of pCO2 is mainly control by temperature as observed in previous work in the western Mediterranean Sea (e.g. Bégovic and Copin-Montégut 2002; Hood and Merlivat, 2001; Wimart-Rousseau et al, 2023; Tsiaras et al 2024). The discussion on drivers of pCO2 appears limited to the temperature effect although biological processes and winter mixing are also likely driving the pCO2 temporal variations. Interestingly authors also compare the pCO2 data and derived fluxes with models results (Cossarini et al 2021, available at CMEMS) suggesting that models reproduce the pCO2 time-series excepted in winter (simulated pCO2 lower than observations). The comparison also suggests that simulations are not able to reproduce large anomalies such as in summer 2022. Consequently the large CO2 source observed in summer 2022 (up to 100 mmolC/m2/d) is not simulated and the annual flux from the model probably not correct.

These suggestions have been very helpful in improving the clarity of the introduction especially in revising some concepts that are not detailed discussed and the scientific interpretation on the drivers of *p*CO_2. Without providing additional analyses, we also strengthened the analysis of the difference between in-situ observations and model results.

These are very interesting results but the processes that explain the differences between data and model are not fully discussed. Would that be linked to the mixed-layer/stratification not well simulated? I think the analysis should extend the discussion to inform what should be corrected in the model. For example, it would be interesting to present and discuss the DIC and TALK profiles from the model.

We thank the reviewer for this suggestion. As mentioned in the general statements for the reviewers, in our study, the model is primarily used as a complementary tool to support the interpretation of the in situ observations. Accordingly, instead of focusing on additional analyses we discussed better the interpretation of the differences between the model outputs and the observations. An analysis of the possible correction of the model could be indeed

the topic of a following study. Nevertheless, we agree with the reviewer as part of the differences can be explained by mixing/stratification processes not well simulated by the CMEMS reanalysis. In particular, the discrepancies observed in winter (late December - late February), may be related to vertical resolution and parametrizations of the model that may smooth or underestimate the vertical transport of DIC to the surface associated with convection events.

On the other hand, in the title the term "Decadal" suggests that the trend of the CO2 flux would be explored. Is the CO2 sink increase or decrease in the Adriatic ? This is not addressed in the paper although I think authors have in hand the data (E2M3A mooring and models) to explore this topic. In the southern Adriatic Cossarini et al (2021) estimate an increase of the CO2 sink of around 0.1 mmolC/m2/d/yr over 1999–2019. How this is compared with the new results obtained in 2015-2024. This could be also compared with reconstruct pCO2 and air-sea CO2 fluxes also available at CMEMS (Chau et al 2024).

We thank the reviewer for the comment. The use of "*over the last decade*" in the title refers specifically to the 2015–2024 period and was deliberately chosen instead of "decadal" to avoid potential misinterpretation. The manuscript does not address trends, while this would indeed be an interesting point for further investigation, it falls outside the scope of the present study. Regarding specific comments on the title, we agree to add the specific period.

Finally, in the introduction authors mentioned the carbon sequestration and ocean acidification. However, this is not discussed although authors have in hand pCO2 data, some discrete TALK data, as well as pH data for 2015-2016 to investigate this topic and to compare with the model. Authors might try to calculate DIC and pH using TALK and pCO2, the time-series of TALK being deduced from TALK/Salinity relation. This could be compared with pH trend deduced from a Neural Network method (pH trend over 2014-2024 = -0.023/decade at the mooring location, Chau et al, 2024).

We thank the reviewer for this valuable suggestion. We agree that the investigation of ocean acidification and long-term changes in the carbonate system represents an important research direction in this region. In the original manuscript, these processes were introduced to provide a broader context; however, we acknowledge that they were not explicitly addressed in the analysis. Following the reviewer's comment (and similar remarks from other reviewers), we have revised the introduction to better align it with the actual scope and results of the study.

The type of analysis suggested by the reviewer, such as reconstructing DIC and pH from pCO_2 and TAlk, and evaluating long-term pH trends, requires a dedicated methodological framework. In particular, deriving a continuous TAlk time series from salinity would introduce uncertainties related to its seasonal variability and to regional processes affecting salinity (e.g., circulation patterns like the BIOS) considering the limited temporal coverage of discrete carbonate system observations in the region (both in terms of number of samples and seasonal distribution). Such investigations are currently being developed in a separate study.

In addition, to complement the mooring data (recently described by Dentico et al, 2026), the authors may add data from SOCAT (in 2014-2020) near the location of the E2M3A mooring. I also strongly recommend send their new pCO2 data to SOCAT.

Following the reviewer's suggestion, we will modify Figure 2 in the revised version of the manuscript. In particular the figure will display the time series of daily a) pCO_2 measured by the sensors, where the points represent the pCO_2 derived from discrete samples and the coloured line the pCO_2 from SOCAT; b) Thermal and non-thermal component of pCO_2; c) Sea surface temperature; d) Dissolved oxygen and apparent oxygen utilization; and e) Sea surface salinity.

The mixed layer depth and chlorophyll-a time series will be added to Figure 5.
References in this manuscript are adapted and I suggest add few others.

The manuscript is suitable for publication after revision. Specific and minor comments are listed below.

;;;;;;;; Specific comments

We thank the reviewer for the careful reading of the manuscript and for the constructive suggestions provided. In several comments (C07, C13, C17, C21, C22, C32, C33, C34 and C36), the reviewer proposed additional analyses aimed at a more detailed evaluation of the CMEMS reanalysis and its comparison with observations and/or other products. We fully agree that these analyses are scientifically relevant and potentially very useful to better investigate the processes controlling carbonate system variability in the Southern Adriatic Sea (e.g., vertical mixing, biological processes). However, such analyses would require a more dedicated and systematic framework than the one adopted in the present study. For this reason, some of the reviewer's suggestions have not been implemented in the present

revision maintaining the main focus on the interpretation of the observational time series..

C-01: Title: “Air–sea CO2 exchange in the Southern Adriatic Sea: assessing its role as a moderate carbon sink over the last decade”. I suggest specify the period 2015-2024. “Last decade” is somehow general and maybe not appropriate for future readers in 2030, 2035, 2040… Suggestion: “Air–sea CO2 exchange in the Southern Adriatic Sea: assessing its role as a moderate carbon sink in 2015-2024” or “Air–sea CO2 exchange in the Southern Adriatic Sea: an analysis over 2015-2024.”

We thank the review for the suggestion and we will change the title of the revised manuscript specifying the studied period. The new title will now be ‘*Air–sea CO_2 exchange in the Southern Adriatic Sea: assessing its role as a moderate carbon sink over the last decade (2015-2024)*’.

C-02 Line 20: “The results showed that seasonal temperature variability and winter convection were the main processes driving the dynamics of surface partial pressure of CO2 (pCO2sw).” In winter is it convection or mixing in top layers ? Biological processes are not driving pCO2 in this region ?

We thank the reviewer for the comment. We agree that the term “*convection*” may be too general. In the South Adriatic, winter convection can range from shallow mixing in the upper layers (< 400 m as in 2015, 2016, 2019 and 2020; Figure 2) to deep convective events (> 600 m or down to 800 m in 2018, 2022 and 2023; Figure 2), depending on the intensity of air–sea heat loss and preconditioning of the water column (e.g., Le Meur et al., 2025). In our dataset, the observed increases in pCO_{2sw} during winter, supported by positive AOU anomalies, are consistent with vertical mixing processes that episodically involve deeper layers. Regarding the role of biological processes, we agree that they are likely an important component of the pCO_{2sw} seasonal cycle. Although the limited temporal coverage during the spring bloom period prevents a fully quantitative assessment, several indicators (e.g., negative AOU values and increased oxygen concentrations) suggest that biological CO_2 uptake contributes to lowering pCO_{2sw} following winter mixing.

Accordingly, in the abstract (lines 21 - 23) we will change the sentence as follows: “*The results showed that seasonal temperature variability and winter vertical mixing processes were the dominant drivers of pCO_{2sw} variability, with biological processes contributing particularly during the post-convective productive period*.”

Secondly, we will revise the description of the results in Section 4.1 (lines 314 - 319): *'The increase in surface pCO_{2sw} during winter convection was particularly evident between late December and late February of 2016–2017, 2017–2018, 2020–2021, and 2023–2024 (Fig. 2a) and was associated with the upward transport of DIC-rich intermediate and deep waters (Touratier et al., 2016; Ingrosso et al., 2017). Depending on atmospheric forcing and water column preconditioning, the depth of vertical mixing ranged from relatively shallow mixing in the intermediate layers (< 400 m depth) to deeper convective events (> 800 m depth).'*

C-03: Line 32: Gulev et al., (2021), not in references

The reference will be added.

C-04: Line 38-48: Authors introduced the ocean acidification but this is not discussed in the results (see also general comment).

According to our answer in the general comments section, we will modify the introduction reducing the discussion on ocean acidification.

C-05: Line 53: Authors write: "in more than 30 million observations in the Surface Ocean CO2 Atlas (SOCAT) V2022 release." In the most recent version V2025 there are now more than 40 million observations. Please add reference to Bakker et al (2016). Note that version V2026 should be soon available (June 2026).

This sentence will be changed in the revised version of the manuscript and the suggested reference will be added. If the preprint won't be published before June 2026 we will update this part with the newest version of SOCAT V2026.

C-06: Line 55: "….and the amount of new observations per year in the open ocean has decreased since 2017 (Bakker et al., 2023)." This is correct but for this paper focused in the Adriatic is it really useful to recall that in the introduction? Maybe specify that few fCO2 data in SOCAT are available in the Adriatic (and only since 2014).

We thank the reviewer for the suggestion. In the revised version of the manuscript we will specify the scarcity of fCO2 data in the Adriatic sea.

C-07: Line 58: "However, generating a coupled physical-biogeochemical reanalysis remains a complex challenge (Park et al., 2018), due to uncertainties in the representation of interactions between physical and biogeochemical processes,…". I guess the comparison of pCO2 presented in figure 5 offers very interesting results to highlight processes not well represented in the model (e.g. winter mixing ?).

Yes, we agree with the reviewer and we will deepen the discussion on the difference between observations and the model output.

C-08: Line 89: Authors write: "..hereafter EMSO-E2M3A, is presented". I guess you can refer to Dentico et al (2026).

The reference Dentico et al., (2026) will be added at the end of the sentence.

C-09: The introduction is somehow long and, regarding the analysis, might be focused on uncertainty of CO2 flux in the marginal seas and recall Mediterranean Sea specificities such as high salinity and low Revelle factor (see a review in Álvarez et al 2023). For the CO2 data, in addition to SOCAT, you can also refer to the CARIMED data-base (Álvarez et al 2025).

We thank the reviewer for this valuable comment. We will adjust the introduction accordingly, especially considering the major changes and additional analyses in the CO_2 flux uncertainty section.

C-10 Line 115: Figure 1 Caption: "...are also displayed and highlighted by the two red lines." There is no red line in this figure.

The caption of the figure will be corrected.

C-11 Line 157: "The secondary mooring line is composed of an instrument chain with sensors at different depths for physical and chemical measurements from the seafloor to the intermediate layer (Cardin et al., 2025a)." Here we learn that sensors recorded observations at depth. Why not using these data to get a view of the variability of the MLD and subsurface data to better interpret the surface pCO2 changes ?

We thank the reviewer for the suggestion. We decided to use only CMEMS reanalysis for two main reasons. First, computing the MLD depth using the secondary mooring data we would need to do an interpolation of the data as the measures are made only in 7 layers with average distance between them of around 150/200 m. Moreover, the use of the model allowed us to have a regional consistency of the MLD in the region instead of using only the mooring measurements (single point). Indeed, in the methods we explained that the model time series are the results of a spatial mean computed over a grid around E2M3A.

C-12 Line 174: "A detailed description of the dataset used here and the quality control procedures applied to the data are available in Dentico et al. (2025)." Dentico et al (2025) not in reference. I suspect this is Dentico et al (2026). Same on Line 213.

Yes, all the time Dentico et al. (2025) is cited in the text, it will be changed in Dentico et al. (2026) and the correct reference will be added in the manuscript.

C-13 Line 226: “The EU Copernicus Marine Service (CMEMS) provides regular and systematic information on the ocean physics and biogeochemistry for the global ocean”. The CMEMS also provides reconstructed fields of pCO2, AT, CT, pH and air-sea CO2 fluxes (Chau et al, 2024) including in the region investigated here. It might be interesting to use this product available for 1985-2024 to compare with the data and the simulations and discuss the differences.

We thank the reviewer for the suggestion. In line with what explained in the general statement for the reviewers, we have focused the revision on strengthening the analysis of the uncertainty of CO_2 flux estimates. A more detailed and quantitative evaluation of model performance will require additional dedicated analyses that we will consider for a future development of the present study.

C-14 Line 228: “the European regional seas (Le Treon et al. 2019).” Correct Name Le Traon et al.

The citation will be corrected with the Le Traon et al. as suggested.

C-15 Line 295: “where, mean refers to yearly mean values of sea surface temperature (SST, °C),” What not using the mean over the full period ?

We thank the reviewer for this comment. We followed the formulation proposed by Takahashi et al. (2002), which is based on the use of an annual mean sea surface temperature. This choice is consistent with the original methodology described in that study (see Section 4.1.1 of Takahashi et al., 2002), and has therefore been used in our study.

C-16 Line 308: “However, during winter convection, vertical mixing increased surface pCO2sw, as shown by the pCO2_NT component (Fig. 2a).” Is it also seen in the model ?

In the revised manuscript, we will clarify that this behaviour is not fully captured by the model. A discussion of this process was already included in the previous version, and it will be strengthened to better highlight the limitations of the model in representing it. We will indeed move the actual Figure 2d and and Figure

2e together with Figure 5 in order to better describe qualitatively the differences between the *p*CO_2 data from observations and from the model.

C-17 Line 314: "The post-convection periods (usually from March to May-June) were characterised by a phytoplankton bloom (Fig. 2e), but its direct effect on pCO2sw was less clear as few data were available in these periods." Again, would the model help to interpret the impact of the bloom on pCO2 ?

We thank the reviewer for the comment. We agree that the model could provide additional insight into the relationship between phytoplankton dynamics and surface pCO_2 variability during the post-convection period. In the revised manuscript, we have expanded the discussion to better highlight the possible role of biological activity in modulating pCO_2 during spring and early summer. Nevertheless, as stated at the beginning, a more detailed investigation of the coupled physical–biogeochemical processes controlling these dynamics is not provided in the revised version of the manuscript.

C-18 Line 322: "Independent measurements from an Unmanned Surface Vehicle (USV) operating in the area in July 2019 during the ATL2MED experiment (Martellucci et al., 2025) further confirmed this pattern, showing coherent short-term variations with the EMSO-E2M3A time series." It would be interesting to add the data from this USV when it was close to the mooring. This could be added in Figure 2 for July 2019 to complete the time-series.

We thank the reviewer for the suggestion. We will modify Figure 2a in the revised manuscript so it will include the *p*CO_2 data measured by the probe, the *p*CO_2 from SOCAT and the *p*CO_2 measured during the ATL2MED experiment.

C-19 Line 324: "Interestingly, higher pCO2sw values were recorded in summer 2022, with a peak in July 2022, coinciding with the occurrence of a Mediterranean Marine Heatwave (MHW)." This is in line with results in the western Mediterranean Sea (Metzl et al, 2025).

We will revise the manuscript to include a reference to Metzl et al. (2025) and to better highlight the consistency of our observations with recent findings in the western Mediterranean Sea. In particular, we will emphasize the role of marine heatwaves in thermodynamic processes, but also stratification and biological activity, thereby influencing *p*CO_2 variability and air–sea CO_2 fluxes.

C-20 Line 327: “Thus, the elevated pCO2sw observed in this period might be linked to the MHW, although further analyses, for instance in relation to wind change patterns (e.g., Pecci et al., 2024) would be required to confirm this hypothesis.” Pecci et al not yet published ?

We will update the reference Pecci et al. (2024) in the revised manuscript with the recent publication Pecci et al. (2026).

Pecci, M., Sferlazzo, D., Anello, F., Becagli, S., Colella, S., De Silvestri, L., Di Iorio, T., Iaccarino, A., Meloni, D., Monteleone, F., Piacentino, S., Principato, E., and Di Sarra, A.: First Derivations of Air-Sea CO_2 Fluxes in the Central Mediterranean and Possible Impact of the 2022–2023 Marine Heatwave, JGR Oceans, 131, e2025JC023007, https://doi.org/10.1029/2025JC023007, 2026.

C-21 Line 333: “...promoting an efficient exchange between bottom and surface pCO2sw.” Would be interesting to show vertical profiles of DIC in the SAd near the mooring (see for example data available in CARIMED, Álvarez et al, 2025). The DIC profiles are also available in the CMEMS model you used.

We thank the reviewer for the suggestion. Regarding the comparison with the CARIMED dataset, we note that the available DIC profiles refer to periods preceding the high-frequency surface pCO2 time series analysed in this study. In addition, the currently available profiles (after 2014) rarely cover the winter convective period, except for a very limited number of cases. Since winter mixing represents one of the key processes discussed in this work, the limited temporal overlap and seasonal coverage would make a consistent interpretation of the relationship between vertical carbonate system structure and the observed surface pCO_2 dynamics particularly difficult. A more robust comparison between historical DIC profiles and surface observations would likely require a modelling framework able to consistently reproduce the temporal evolution of the carbonate system over longer periods. In that case, the combination of modelled vertical dynamics and historical DIC observations could help to better investigate the role of convection-driven upward transport of carbon-rich deep and intermediate waters.

C-22 Line 338: “In the SAd the effect of biological activity was less clear because of data gaps.” But the CMEMS model could help to investigate this process.

We thank the reviewer for the comment. For this comment, please refer to the general statement provided at the beginning.

C-23 Line 341: "... as highlighted in previous studies (Socal et al., 2012; Cerino et al., 2012)." Socal et al not in reference.

This reference will be updated in the final revised manuscript with the correct one:
Turchetto, M., Boldrin, A., Langone, L., and Miserocchi, S.: Physical and biogeochemical processes controlling particle fluxes variability and carbon export in the Southern Adriatic, Continental Shelf Research, 44, 72–82, https://doi.org/10.1016/j.csr.2011.05.005, 2012.

C-24 Line 367: Figure 2 caption "Modified from Dentico et al. (2025)." Correct Dentico et al. (2026). Note that Dentico et al. (2026) also show the wind speed.

As mentioned before, all the citations Dentico et al. (2025) will be changed with Dentico et al. (2026). Indeed, we used "*modified from*" because in that figure different parameters are displayed but others are similar (i.e., pCO_2 without thermal and non thermal components, temperature, salinity and dissolved oxygen without AOU).

C-25 Table 2: Would be nice to present a figure of the flux from table 2

We thank the reviewer for the comment. In the final revised version of the manuscript that table and description of M1 will be moved to the Supplementary Material. M2 will be kept as the main method used to compute all the averages.

C-26: To compare with other studies, what is the integrated annual flux (sum of monthly fluxes in molC/m2/yr) ?

We thank the reviewer for the comment. We already provided a comparison with other regions in the manuscript (lines 445-450). However, we will deepen this comparison as explained in C-35.

C-27 Line 415: "Further, these results confirm the role of dense water formation sites as primary drivers of carbon sequestration". Maybe I missed a point but I don't think the description of the air-sea CO2 fluxes in this section support this conclusion.

We thank the reviewer for the comment. We agree that the sentence, as originally written, could overstate the implications of the results presented in this section. In the revised manuscript, this statement will therefore be rephrased. Our intention was to refer more generally to the potential role of deep convection sites, such as the Southern Adriatic Sea, in contributing to carbon transfer towards deeper layers.

C-28: Section “4.3 Estimated uncertainties in FCO2”. Please introduce the aim of this section. Is this section 4.3 really important ? I would suggest move this sensitivity analysis in Supplementary Material.

As we explained in the general statement, this section will be modified in the revised manuscript.

C-29 Figure 4 is not very clear: only a table would be useful to list the results of this sensitivity analysis.

In the revised manuscript this table will be removed and changed with a table showing the results of the new analysis.

C-30 Line 481: “Sensor based pCO2sw not always met the manufacturer and the ICOS or SOCAT accuracies (Steinhoff et al., 2025).” Steinhoff et al (2025) not in reference.

The reference will be added in the revised manuscript.

C-31 Line 483: “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.” Recall that discrete samples were dedicated to pH and TALK measurements ?

It is correct: the pCO_{2sw} from discrete samples was estimated using the pair TAlk and pH.

C-32: Section “4.4. pCO2sw and FCO2: in situ data and model comparison”. To explain the pCO2 differences between data and models authors show that this is not linked to SST and salinity. As there are systematic low pCO2 in the models in winter would that be linked to MLD (less import of DIC-rich from subsurface in the models) ?

We thank the reviewer for the comment. We kindly ask the reviewer to refer to the answer given in the general comments.

C-33: Are the MHW discussed on lines 323-325 identified in the models ?

We thank the reviewer for the comment. In the present manuscript, the correspondence between the elevated surface pCO_2 values observed during summer 2022 and the occurrence of a MHW is presented as a possible interpretation rather than a demonstrated causal relationship. Including SST products from satellites or reanalyses could help to investigate whether the

marine heatwave signal is also captured by the model. In this regard, Pirro et al. (2024) already identified the occurrence of this event in the Southern Adriatic Sea using Argo float data. Such an analysis would shift the focus toward the validation of additional model variables, particularly temperature fields, rather than surface pCO_2.

C-34: I think maps of the annual or seasonal CO2 flux from the model in the Adriatic are missing (see for example Cossarini et al 2021, Figure 11). This would help identifying the distribution of the fluxes regarding the location of the mooring.

We thank the reviewer for the helpful suggestion. In the revised manuscript, we will add a surface map of the annual CO_2 flux in the region in the revised manuscript. This addition will help to place the EMSO-E2M3A observatory within its regional context and to illustrate that E2M3A is representative of the broader CO_2 dynamics in the southern Adriatic Sea.

C-35 Line 540: "Even if these estimations should be considered with caution, they further confirm that the southern Adriatic acted as a moderate carbon sink over the last decade." I think authors should conclude this section with results of annual fluxes (sum of monthly values in molC/m2/yr) to compare with other studies.

We thank the reviewer for the comment. In the revised manuscript we have strengthened the comparison between annual estimates for the southern Adriatic and those reported for other regions. The discussion has been expanded and clarified to provide a more quantitative and consistent comparison with previous studies, including the northern Adriatic Sea and the Gulf of Lion.

C-36 Line 551: "In winter, average pCO2sw were around 380 µatm, with marked increase especially during wintertime convection periods when CO2-rich intermediate and deep water were upwelled to the surface." This is probably correct (import of CO2-rich water), but not quantified. It might be useful to look at the MLD from the models to quantify this process. Again, I think vertical profiles of properties such as DIC and TALK from the model would help to interpret the observed pCO2 seasonality.

We thank the reviewer for the comment. Please, refer to the general statement we provided at the beginning. The answer to this comment has also been addressed in C-21 and C-32.

C-37 Line 558: "One of the key results of this study was to demonstrate that over the past decade the SAd acted as a moderate carbon sink." This is a conclusion for the annual flux and it would be useful to list the annual value (in molC/m2/yr) and indicate if the sink was always moderate or if there any trend (increasing sink ?) as observed in other regions.

In the revised manuscript, we will include the integrated annual CO2 flux expressed in mol C m^{-2} yr^{-1} to facilitate comparison with previous studies.
Due to the temporal gaps in the observational dataset, however, a robust assessment of long-term trends cannot be derived from the in situ estimates alone. We acknowledge that model-based estimates could potentially be used to explore temporal trends. However, given the discrepancies identified between the reanalyses and the observations, a validation would be required before performing any reliable trend estimations.
Regarding the use of the term "*moderate*", we will clarify that this refers to a relative comparison with other Mediterranean regions, such as the northern Adriatic Sea and the Gulf of Lion, which are characterised by stronger CO_2 uptake. We will also revise the wording to more cautiously describe the Southern Adriatic as a likely weak-to-moderate CO_2 sink, in light of the new analysis that will be provided in the manuscript.

C-38 Line 589: Cardin et al, 2025: 2025 a or 2025b or both ?

The reference here will be corrected with Cardin et al. (2025b) as this is the dataset containing the surface data. Cardin et al. (2025a), instead, refers to the dataset containing the deep mooring data.

C-39 Line 607: Acknowledgements: Should you add CMEMS ?

Here we cited CMEMS with the following sentence: This study has been conducted using E.U. Copernicus Marine Service Information.

C-40: Figure S6: Caption: correct color code (data and model in black and blue, difference in grey).

The figure caption will be corrected in the manuscript.

;;;;;;;;;; Reference added in this review not listed in the manuscript

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;;;;;;;;; end review

Citation: https://doi.org/10.5194/egusphere-2026-1360-RC1

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 manuscript presents an interesting work about the surface partial pressure of CO2 dynamics and associated CO2 air-sea flux in the South Adriatic area from a ten-year time-series recognized by EMSO and ICOS, two relevant European Research Infrastructures.

Although the work is interesting and relevant for the ocean CO2 observing community it needs some corrections and more discussion. In my humble opinion it has been written quite fast, without much reflection. There are several mistakes describing the data and methods, and the discussion is mainly focused on the comparison with products from CEMS and the impact of wind products in the estimates of air-sea fluxes.

Another general comment is the use and abuse of acronyms, several of them are not defined, others as just used once. So please read carefully and avoid using them if not widely used in the manuscript.

The EMSO- E2M3A pCO2 and ancillary data is not properly presented. The authors have published a paper just about this data set in ESSD https://essd.copernicus.org/articles/18/2119/2026/essd-18-2119-2026.html and it is not even properly cited (Dentico et al 2025 is not provided in the References). Although surely the pCO2 data is widely described in the ESSD paper. In the Biogeosciences manuscript there should be short description of the pCO2 data with evidences on the precision and accuracy of the data, the declared accuracy of the instrument manufacturer (5 uatm) is too optimistic, in fact, it might be provided as a % not a bulk absolute value, it is probably pCO2 dependent. In addition, the impact of this uncertainty should be considered in the air-sea fluxes uncertainty estimation.

Regarding the uncertainties in the air-sea CO2 flux estimates, section 4.3, I suggest a mayor revision on this section. As written now it is not an uncertainty assessment, it is a bias assessment. An uncertainty assessment should be citing the GUM guide, https://www.bipm.org/documents/20126/2071204/JCGM_GUM_6_2020.pdf , where systematic and random uncertainties are considered in the calculation of a quantity. Performing a proper uncertainty analysis is a quite difficult issue. Consequently I suggest a rewording of the section: bias assessment?.

Section 2 about the study site: the general oceanography in the Adriatic basin and particularly in the South Adriatic is rather long focusing on aspects that are not considered in the discussion of the data, particularly the BIOS. In the opposite, this section from line 146 onwards it presents the EMSO E2M3A observatory. I suggest these paragraphs should go in the Data and methods section, maybe within a subsection named “EMSO E2M3A observatory”.

The discussion should be enlarged regarding two main issues
- Surface pCO2 dynamics in the SAd compared to other Adriatic and Mediterranean Sea published works.
- Impact of winds on the air-sea CO2 flux calculations, there are several works evaluating this issue as for example

o https://bg.copernicus.org/articles/10/2993/2013/
o https://b.tellusjournals.se/articles/10.3402/tellusb.v57i2.16777

We thank the reviewer for the careful reading of the manuscript and for the constructive suggestions provided. These comments were very helpful in improving both the clarity and the scientific robustness of the study.
Following the reviewer's suggestions, we will strengthen the discussion and the comparison of surface *p*CO_2 dynamics and carbon flux in the southern Adriatic sea in respect to other Mediterranean regions. Changes will be presented in Section 4.1 in the revised manuscript.
We will also reduce the description of oceanographic features (modification will be presented in Section 2) that are not directly discussed in the interpretation of the results. Consequently, the use of acronyms will be substantially reduced throughout the text to improve readability.
At the same time, more emphasis will be given to the description of the E2M3A observatory (modification will be presented in Section 3.1). In this regard, the connection with the previously published ESSD paper (Dentico et al., 2026) will be clarified. Moreover, all incorrect citations and references to this paper will be corrected.
Regarding the *p*CO_2 measurements, we will correct the manufacturer-declared accuracy as a percentage. We agree with the reviewer that this source of error should be acknowledged in the uncertainty analysis of the CO_2 flux estimates but we won't include this estimate in the revised manuscript.
Following the reviewer's comments on Section 4.3, we will substantially revise both the terminology and the scope of the previous "*uncertainty analysis*". As correctly pointed out, the analyses presented here do not represent an uncertainty assessment in the sense defined by the GUM framework. Accordingly, the terminology has been revised throughout the manuscript to avoid this ambiguity. In the revised manuscript, we will instead focus on an assessment of the CO_2 flux calculations, with particular attention to the impact of different wind products, wind temporal resolutions, and gas transfer velocity parameterisations.

SPECIFIC COMMENTS

Abstract
I would also make clear the scarcity of surface pCO2 measurements in the MedSea. In line 19 I would highlight that you are presenting surface pCO2 data (define pCO2), EMSO needs to be explained.

In the revised manuscript, we will strengthen the Introduction to better highlight the scarcity of surface *p*CO_2/*f*CO_2 observations in the southern Adriatic sea as reflected by the limited coverage available in SOCAT. We will also specify in the Abstract that the study is based on a recently validated high-frequency surface *p*CO_2 time series, referring to the associated ESSD publication where the dataset and quality-control procedures are described in detail.

Regarding the use of the acronym EMSO, we chose to keep the observatory name EMSO-E2M3A in the Abstract, as its full explanation is provided in the main text.

In line 25 please check the use of “uncertainty” once the corresponding section has been reviewed.

We thank the reviewer for this comment. As discussed in the general statement provided to the reviewers, the previous section on the “*uncertainty analysis*” has been substantially revised. The use of 'uncertainty' will be modified consequently in the text.

Line 28, define EU. The abstract might contain the main findings of the work relative to surface pCO2 drivers and also about the comparison with CMEMS products

In the revised Abstract, we will replace the acronym ‘EU’ with ‘European’ for clarity. We will also update the Abstract to better summarise the main findings of the study, including the results of the additional analyses on CO_2 flux estimates using different wind products, wind temporal resolutions, and gas transfer velocity parameterisations.

1 Introduction

In lines 38-40, the speciation of CO2 in seawater is not used in the manuscript, I suggest to avoid formulations and just use the text.

We will remove the speciation of CO_2 keeping just the text in the revised manuscript.

Line 43 .. references should be at the end of the phrase.

We will add a reference to the sentence.

Line 49 … estimates of the global CO2 air-sea flux

We will modify the sentence according to the reviewer's suggestion.

Line 53 … SOCAT is relevant now, but it was also the LDEO work by Prof. Takahashi.

We thank the reviewer for this remark. In the revised manuscript, we will acknowledge the historical contribution of the LDEO surface ocean CO_2 observations initiated by Prof. Takahashi, together with the current relevance of the SOCAT synthesis product.

Line 55. … before starting the “Ocean reanalysis …” section add a break point.

If the reviewer refers to this sentence: "*Ocean reanalyses have demonstrated significant potential in bridging observational gaps by providing comprehensive, three-dimensional, basin-wide datasets that enable the investigation of spatial and temporal variability across multiple scales (Cossarini et al., 2021)*." there is already a break point before it.

Line 74-77… I would suggest another citation .. maybe from GOOS https://goosocean.org/what-wedo/2030-strategy/.

We will add this reference in the revised manuscript.

The book by Schroeder and Chiggiato should be cited in the next paragraph and please correct the citation.. Schroeder, K., & Chiggiato, J. (Eds.). (2023). Oceanography of the Mediterranean Sea: An Introductory Guide. Elsevier. ISBN 978-0-12-823692-5.

We will correct the citation and we will add Schroeder et al. (2023) in the next paragraph as suggested.

Line 89 .. pCO2 should have been defined previously

We first mention pCO_2 here, where we clearly stated that it is the partial pressure of CO_2, and after we defined it as pCO_2 in brackets.

Line 92-93 --- maybe the authors are right .. but other works have been published about the pCO2 dynamics in the Adriatic Sea … Check Table 2 in https://essd.copernicus.org/articles/18/2119/2026/essd18-2119-2026.html. If the authors think their work is unique please it should be based on something else than the region.

We thank the reviewer for this comment. In the first version of the manuscript, the term "*region*" was intended to specifically refer to the southern Adriatic sea. We agree, however, that this wording could be misleading and interpreted more broadly as referring to the entire Adriatic basin, where other studies on pCO_2 dynamics are available. In the revised manuscript, we will modify the Introduction to better specify the novelty of this time series in the southern part of the basin where observation remained sparse.

2 Study Site

Please see my general comments. There are too many acronyms that are not further used in the manuscript. Although I love the oceanography of the Adriatic Sea, only those processes that would be needed to explain the surface pCO2 variability would be described . Is the BIOS mechanism related with the surface pCO2? Do you detect any signature in surface pCO2 regarding the river input?

We thank the reviewer for the comment. In the revised manuscript, we will reduce the description of the Adriatic Sea oceanography and processes that are not directly relevant to the interpretation of the surface *p*CO_2 variability. Accordingly, the use of acronyms throughout the section will also be reduced. We will focus on the physical processes most closely related to the observed *p*CO_2 and hydrographic, for instance convection and salinity changes associated with the BIOS mechanism.
Concerning river inputs, no clear signature was identified in the surface *p*CO_2 time series at the study site, likely due to the offshore location of the observatory.

Line 151-152.. please check that the EMSO, ICOS and ERIC acronyms need to be defined here or even earlier in the manuscript

Here EMSO and ICOS acronyms were defined. The missing acronym for ERIC will be added.

Line 157. The references Cardin et al. 2025 a and b are equal. Please check.

Cardin et al. 2025a,b refers to two different dataset. The correct reference for Cardin et al. 2025a will be added.

3.1 In situ measurements

Line 164 .. Cardin et al. (2025b)? correct

Cardin et al. (2025b) is correct.

Line 167 Dissolved oxygen is briefly used in the work, avoid using DO.

We will follow the reviewer suggestion avoiding the use of DO if not further discussed in the text.

Line 174 Dentico et al. (2025) reference missing.

Dentico et al. (2025) references in the manuscript will be corrected as Dentico et al. (2026). Similarly, the reference section Dentico et al. (2026) will be added.

Line 175 define ENEA

The definition of ENEA will be added in the manuscript.

Please clearly separate the presentation of surface seawater and atmospheric pCO2.

We thank the reviewer for this comment. In the revised manuscript, we will reorganise the Data and Methods section to more clearly distinguish between the

description of surface seawater $p CO_2$ and atmospheric $p CO_2$ datasets, including their respective sources, processing, and use in the CO_2 flux calculations.

Line 201… too many digits for uatm values.... I think they should be rounded to integer values. Please check in the rest of the sections.

We will adjust the digits accordingly.

In equation (3) .. u is wind speed while in Eq 4 is something different .. please check

We will modify the equation in the revised manuscript. In particular, we will keep u as wind speed in Eq. 3, while for Eq. 4 we will change u and v in x and y respectively.

3.2 Model data

CMEMS might be defined before and it means EU Copernicus Marine Service for the Mediterranean Sea.

Line 228 .. delete Mediterranean .. it is contained in CMEMS

CMEMS is the acronym for the Copernicus Marine Service as reported in following link: (https://help.marine.copernicus.eu/en/articles/4444611-how-to-cite-copernicus-marine-products-and-services). Line 228 is correct as it is.

Line 233 uatm ..not uamt

It will be corrected in the manuscript.

Line 234 .. for the surface chlorophyll .. not "of"

It will be corrected in the manuscript.

Eq. (4) is it really necessary? .. it might be described and then you avoid the misunderstanding with the previous equation.

During the revision process, we will reconsider the necessity of Eq. (4) and evaluate whether a textual description could improve the readability of the methodology. Nevertheless, we will revise the symbols accordingly to ensure a distinction between the variables used in the different equations.

3.3 Thermodynamics calculations

There are not thermodynamics calculations in this section, it refers to air-sea CO2 fluxes calculations. Please check and change.

We agree. We will change the title of this section to "*Air-sea CO_2 flux calculation*".

Eq (6) what is the "I" there?

It is number 1. Probably in the formatting of the uploaded version of the manuscript when converting from doc to pdf. We will take care of checking that in the final version of the manuscript the formula won't create any misunderstanding.

Line 280 .. delete the acronym for CMPP

We thank the reviewer for this comment. In the revised manuscript, we chose to retain the CMPP acronym in this section, as it is directly associated with the terminology adopted in Wanninkhof (2014) and helps to clearly identify the specific parameterisation discussed. Nevertheless, following the reviewer's general comment regarding the excessive use of acronyms, we will substantially reduce acronyms in other sections of the manuscript.

3.4 Thermal and non thermal

Please write correctly the equations.

We will review and correct the equations.

Line 289 .. delete "DIC" it is not necessary

DIC will be removed.

4 Results and discussion

Please avoid acronyms USV, MHW.. etc

We agree with the referee that the text had too many acronyms. Some of them, if not necessary, will be removed.

I wonder if there is any relationship between pCO2 and AOU, or even between the thermal and non thermal pCO2 and AOU.

Our results show a co-variability between AOU and the non-thermal component of *p*CO_2, particularly during winter, when vertical mixing brings oxygen-depleted and DIC-rich subsurface waters to the surface. In contrast, no direct relationship is observed between AOU and the thermal component.

In the revised manuscript, we will expand the discussion of this aspect to better clarify the relationship between AOU, vertical mixing processes, and the non-thermal contribution to surface pCO_2 changes.

Please check the significant numbers for the CO2 fluxes, two decimals is too much. Correct Table 2 accordingly.

As mentioned above for the pCO_2 values, we will revise the significant digits used throughout the manuscript.

In line 423 .. please use “gradient” not the formula Within the model and data comparison, is there any hypothesis to explain the differences?

Following the substantial revision of Section 4.3, as discussed in the general statement to the reviewers, the specific formulation mentioned at line 423 will be removed. Regarding the differences between model and observational data, in the revised manuscript we will further discuss the possible mechanisms underlying these discrepancies, particularly in relation to vertical mixing. However, consistently with the scope of the present study, we will limit this discussion to an interpretative perspective without introducing additional analyses.

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 $FCO_{2EMSO\text{-}E2M3A}$ 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 pCO_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 pCO_{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 pCO_{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.