

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 (synthesized in Table 2) and authors also addressed the uncertainty of the fluxes depending the wind (synthesized 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 pCO₂. 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 $p\text{CO}_2$ 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 $p\text{CO}_2$ 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) $p\text{CO}_2$ measured by the sensors, where the points represent the $p\text{CO}_2$ derived from discrete samples and the coloured line the $p\text{CO}_2$ from SOCAT; b) Thermal and non-thermal component of $p\text{CO}_2$; 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 CO₂ 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 ($p\text{CO}_{2\text{sw}}$)."
In winter is it convection or mixing in top layers ? Biological processes are not driving $p\text{CO}_2$ 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 $p\text{CO}_{2\text{sw}}$ 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 $p\text{CO}_{2\text{sw}}$ 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 CO₂ uptake contributes to lowering $p\text{CO}_{2\text{sw}}$ 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 $p\text{CO}_{2\text{sw}}$ 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 $p\text{CO}_{2\text{sw}}$ 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 CO₂ 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 CO₂ 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 pCO_{2sw}, 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 $p\text{CO}_2$ 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 $p\text{CO}_2\text{sw}$ was less clear as few data were available in these periods." Again, would the model help to interpret the impact of the bloom on $p\text{CO}_2$?

We thank the reviewer for the comment. We agree that the model could provide additional insight into the relationship between phytoplankton dynamics and surface $p\text{CO}_2$ 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 $p\text{CO}_2$ 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 $p\text{CO}_2$ data measured by the probe, the $p\text{CO}_2$ from SOCAT and the $p\text{CO}_2$ measured during the ATL2MED experiment.

C-19 Line 324: "Interestingly, higher $p\text{CO}_2\text{sw}$ 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 $p\text{CO}_2$ variability and air-sea CO_2 fluxes.

C-20 Line 327: "Thus, the elevated pCO_{2sw} 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 CO₂ 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 pCO_{2sw}." 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 pCO₂ 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., $p\text{CO}_2$ 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 pCO_{2sw} 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 pCO_{2sw} 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. pCO_{2sw} 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 pCO₂ 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 pCO₂.

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 CO₂ 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 CO₂ 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 CO₂ uptake. We will also revise the wording to more cautiously describe the Southern Adriatic as a likely weak-to-moderate CO₂ 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

Álvarez, M., et al (2023). Chapter 11 - Mediterranean Sea general biogeochemistry, Editor(s): Katrin Schroeder, Jacopo Chiggiato, Oceanography of the Mediterranean Sea, Elsevier, Pages 387-451, <https://doi.org/10.1016/B978-0-12-823692-5.00004-2>.

Álvarez, M., et al, CARIMED (CARbon, tracers, and ancillary data In the MEDiterranean Sea): A ship-based data synthesis product – overview and quality control procedures, *Earth Syst. Sci. Data Discuss.* [preprint], <https://doi.org/10.5194/essd-2025-759>, in review, 2025.

Bakker, D. C. E., et al: A multi-decade record of high-quality fCO₂ data in version 3 of the Surface Ocean CO₂ Atlas (SOCAT), *Earth Syst. Sci. Data*, 8, 383-413, doi:10.5194/essd-8-383-2016, 2016

Bégovic, M. and C. Copin-Montégut: Processes controlling annual variations in the partial pressure of CO₂ in surface waters of the central northwestern Mediterranean Sea (Dyfamed site), *Deep Sea Res. II*, 49, 2031–2047, 2002

Chau, T.-T.-T., et al.,: CMEMS-LSCE: a global, 0.25°, monthly reconstruction of the surface ocean carbonate system, *Earth Syst. Sci. Data*, 16, 121–160, <https://doi.org/10.5194/essd-16-121-2024>, 2024.

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