

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 CO<sub>2</sub> dynamics and associated CO<sub>2</sub> 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 CO<sub>2</sub> 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 pCO<sub>2</sub> 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 pCO<sub>2</sub> data is widely described in the ESSD paper. In the Biogeosciences manuscript there should be short description of the pCO<sub>2</sub> 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 pCO<sub>2</sub> 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 CO<sub>2</sub> 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](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 pCO<sub>2</sub> dynamics in the SAd compared to other Adriatic and Mediterranean Sea published works.
- Impact of winds on the air-sea CO<sub>2</sub> 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  $p\text{CO}_2$  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  $p\text{CO}_2$  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  $\text{CO}_2$  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  $\text{CO}_2$  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  $p\text{CO}_2$  measurements in the MedSea. In line 19 I would highlight that you are presenting surface  $p\text{CO}_2$  data (define  $p\text{CO}_2$ ), EMSO needs to be explained.

In the revised manuscript, we will strengthen the Introduction to better highlight the scarcity of surface  $p\text{CO}_2/f\text{CO}_2$  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  $p\text{CO}_2$  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 pCO<sub>2</sub> 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<sub>2</sub> flux estimates using different wind products, wind temporal resolutions, and gas transfer velocity parameterisations.**

## **1 Introduction**

In lines 38-40, the speciation of CO<sub>2</sub> 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<sub>2</sub> 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 CO<sub>2</sub> 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<sub>2</sub> 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://goosocan.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 .. pCO<sub>2</sub> should have been defined previously

**We first mention pCO<sub>2</sub> here, where we clearly stated that it is the partial pressure of CO<sub>2</sub>, and after we defined it as pCO<sub>2</sub> in brackets.**

Line 92-93 --- maybe the authors are right .. but other works have been published about the pCO<sub>2</sub> 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<sub>2</sub> 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 pCO<sub>2</sub> variability would be described . Is the BIOS mechanism related with the surface pCO<sub>2</sub>? Do you detect any signature in surface pCO<sub>2</sub> 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  $p\text{CO}_2$  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  $p\text{CO}_2$  and hydrographic, for instance convection and salinity changes associated with the BIOS mechanism.**

**Concerning river inputs, no clear signature was identified in the surface  $p\text{CO}_2$  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  $p\text{CO}_2$ .

**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  $p\text{CO}_2$  and atmospheric  $p\text{CO}_2$  datasets, including their respective sources, processing, and use in the  $\text{CO}_2$  flux calculations.**

Line 201... too many digits for  $u_{\text{atm}}$  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  $u_{\text{atm}}$  ..not  $u_{\text{amt}}$

**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 CO<sub>2</sub> fluxes calculations. Please check and change.

**We agree. We will change the title of this section to “Air-sea CO<sub>2</sub> flux calculation”.**

Eq (6) what is the “l” 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 pCO<sub>2</sub> and AOU, or even between the thermal and non thermal pCO<sub>2</sub> and AOU.

**Our results show a co-variability between AOU and the non-thermal component of pCO<sub>2</sub>, 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  $p\text{CO}_2$  changes.**

Please check the significant numbers for the  $\text{CO}_2$  fluxes, two decimals is too much. Correct Table 2 accordingly.

**As mentioned above for the  $p\text{CO}_2$  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.**