

## REVIEW OF

egusphere-2026-1360 | Journal relation: BG Submitted on 13 Mar 2026

**Air–sea CO<sub>2</sub> exchange in the Southern Adriatic Sea: assessing its role as a moderate carbon sink over the last decade by Carlotta Dentico, Gianpiero Cossarini, Giuseppe Civitarese, Michele Giani, Angelo Rubino, and Vanessa Cardin**

### 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  $CO_2$  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>

## SPECIFIC COMMENTS

### Abstract

I would also make clear the scarcity of surface  $pCO_2$  measurements in the MedSea. In line 19 I would highlight that you are presenting surface  $pCO_2$  data (define  $pCO_2$ ), EMSO needs to be explained. In line 25 please check the use of “uncertainty” once the corresponding section has been reviewed. Line 28, define EU. The abstract might contain the main findings of the work relative to surface  $pCO_2$  drivers and also about the comparison with CMEMS products

### 1 Introduction

In lines 38-40, the speciation of  $CO_2$  in seawater is not used in the manuscript, I suggest to avoid formulations and just use the text.

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

Line 49 ... estimates of the global  $CO_2$  air-sea flux

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

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

Line 74-77... I would suggest another citation .. maybe from GOOS <https://goosocean.org/what-we-do/2030-strategy/>. 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.

Line 89 ..  $pCO_2$  should have been defined previously

Line 92-93 --- maybe the authors are right .. but other works have been published about the  $pCO_2$  dynamics in the Adriatic Sea ... Check Table 2 in <https://essd.copernicus.org/articles/18/2119/2026/essd-18-2119-2026.html>. If the authors think their work is unique please it should be based on something else than the region.

### 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  $pCO_2$  variability would be described . Is the BIOS mechanism related with the surface  $pCO_2$ ? Do you detect any signature in surface  $pCO_2$  regarding the river input?

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

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

### **3.1 In situ measurements**

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

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

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

Line 175 define ENEA

Please clearly separate the presentation of surface seawater and atmospheric pCO<sub>2</sub>.

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.

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

### **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

Line 233 uatm ..not uamt

Line 234 .. for the surface chlorophyll .. not “of”

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

### **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.

Eq (6) what is the “l” there?

Line 280 .. delete the acronym for CMPP

### **3.4 Thermal and non thermal**

Please write correctly the equations.

Line 289 .. delete “DIC” it is not necessary

#### 4 Results and discussion

Please avoid acronyms USV, MHW.. etc

I wonder if there is any relationship between  $p\text{CO}_2$  and AOU, or even between the thermal and non thermal  $p\text{CO}_2$  and AOU.

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

In line 423 .. please use "gradient" not the formula

Within the model and data comparison, is there any hypothesis to explain the differences?

Best regards