

## Answer reviewer 1.

**General comments:** This is a timely and useful dataset from which the interannual (and seasonal) fluxes of CO<sub>2</sub>, between the atmosphere and sea, can be calculated without the use of further datasets or model output. It represents a 10-year time series of carbonate chemistry, physical and atmospheric data in the Mediterranean Sea and is unique for the area. The data are easily accessible from the link provided in the text. The paper describes the quality control that has been applied to the data, and how the data may be used in future to provide a flux product.

*We thank the reviewer for the valuable comments and suggestions, which helped us to improve the clarity and precision of the manuscript. Below, we provide a response to each comment. For each answer, we also indicated the changes that have been implemented in the final revised version of the text.*

**Specific comments:** The CO<sub>2</sub> data accuracy is stated as +/- 5 $\mu$ atm (please confirm that this is the manufacturers assessment, ~line 190). Clearly the sensor has larger offsets (Table 1). Do the offsets change between pre and post deployment (due to the biofouling introducing a drift)? Or if this is a constant offset that could be corrected for (as was the case in the Saildrone experiment)?

*We confirm that the  $\pm 5 \mu\text{mol/kg}$  in line 190 refers to the manufacturer (Pro-Oceanus) stated accuracy. Offset changes between pre- and post-deployment calibrations could be evaluated only in two cases:*

- *12/09/2014 (pre-deployment) – 20/10/2015 (post-deployment, buoy recovery): in this case, the offset was lower at the end of the deployment.*
- *29/07/2017 (pre-deployment) – 08/10/2018 (post-deployment, buoy recovery): here, the offset increased over the deployment period, which we attribute to biofouling, as the sensor was deployed during summer.*

*For all other periods, pre- and post-deployment comparisons were not available due to technical issues. Since only two years could be corrected, and to ensure consistency across the entire time series, we decided not to apply additional corrections to the dataset.*

I would like to see a reference for line 46 (describing the area as a climate change hotspot).

*References have been added in the revised manuscript. In particular, we included Zittis et al. (2019), Urdiales-Flores et al. (2023), and Lazoglou et al. (2024), which investigate regional climate projections using modelling approaches, potential drivers, and the identification of the most vulnerable subareas of the Mediterranean Sea respectively.*

*Zittis, G., Hadjinicolaou, P., Klangidou, M., Proestos, Y., and Lelieveld, J.: A multi-model, multi-scenario, and multi-domain analysis of regional climate projections for the Mediterranean, Reg Environ Change, 19, 2621–2635, <https://doi.org/10.1007/s10113-019-01565-w>, 2019.*

*Urdiales-Flores, D., Zittis, G., Hadjinicolaou, P., Osipov, S., Klingmüller, K., Mihalopoulos, N., Kanakidou, M., Economou, T., and Lelieveld, J.: Drivers of accelerated warming in Mediterranean climate-type regions, npj Clim Atmos Sci, 6, 97, <https://doi.org/10.1038/s41612-023-00423-1>, 2023.*

*Lazoglou, G., Papadopoulos-Zachos, A., Georgiades, P., Zittis, G., Velikou, K., Manios, E. M., and Anagnostopoulou, C.: Identification of climate change hotspots in the Mediterranean, Sci Rep, 14, 29817, <https://doi.org/10.1038/s41598-024-80139-1>, 2024.*

Line 72 suggests the station is an ICOS site, but it is not clear whether a subset of the data is available from the ICOS carbon portal?

*The EMSO-E2M3A station is part of the ICOS network and is currently in the process of being certified as a Class 2 site. However, its status as an open-ocean station, located approximately 60 nautical miles from the coast, poses significant logistical challenges for increasing in-situ calibration campaigns, which are a mandatory requirement for class upgrading. As a result, E2M3A data are not yet available through the ICOS portal.*

Where accuracy was quoted eg: line 132 please clarify if this is a manufacturer assessment

*This will be changed in the final version of the manuscript. In particular, line 131, 132, 134 - 135, 146 and 194 where the term accuracy was referred to the manufacturer stated accuracy.*

Line 138 suggests that flags are applied to the data (and no corrections are made) – however line 260 references ‘corrected’ data, which may just include the de-spiking so this should be clarified – I get the impression that offsets have not been applied to the data but it would be good to know what difference the offsets would make to the final flux calculation please

*Yes, in the case of wind, only flags have been assigned to the data. In line 260, ‘corrected data’ refers to CTD data that are the only one that have been actually corrected as explained in section 2.2.2. (despiking, linear interpolation of data with gaps greater than 6 hours, application of offset, and instrument drift correction). We clarify the use of ‘corrected’ in the text, specifying that only CTD have been corrected. The CO<sub>2</sub> fluxes will be calculated using temperature and salinity data to which offsets were applied. For wind data, it was not possible to apply offset as we didn’t have other independent reference measurements to compare E2M3A with. Similarly, and as explained in the first comment, it was not possible to apply a constant offset and correct this data.*

line 158 is unclear – what is the ‘which t’ referring to?

*It’s a typing mistake: the t after which will be removed in the final version of the manuscript.*

The dataset refers to hourly data (though some data – such as CO<sub>2</sub> are available 4 hourly)

*Yes, it is correct.*

Line 194 suggests that pre and post calibrations are made at OGS – please quantify the frequency of visits to the site, is it an annual visit?

*As stated in the manuscript, these should be considered as checks rather than calibrations, as at OGS there is no facility for CO<sub>2</sub> probes calibration (thus it can only be performed by the manufacturer). In general, we performed a check of the instruments several months before deployment in the Calibration and Metrology Center (CTMO) facility. This is explained in line 198-199. In case of sensor malfunctioning, this ensures sufficient time to send the instrument to the manufacturer and to perform a quick test*

*just before deploying the instrument. We specified the frequency of the visits to the EMSO-E2M3A observatory in the final manuscript (line 201-202).*

Line 267 suggests that there are only physical impacts on CO<sub>2</sub> – but are there responses due to productivity too (as you report for the oxygen)? Can you explain the double peak in dissolved oxygen or is this the biological impact that you mention?

*We believe that physical processes play a dominant role in surface pCO<sub>2</sub> dynamics in the southern Adriatic, although the influence of biological processes should be further investigated. Even though the southern Adriatic is an oligotrophic region, with relatively low primary production, previous studies have described the effect of biological processes on the carbon cycle (e.g., Socal et al., 2012; Cerino et al., 2012). Nevertheless, the role of the biological carbon pump should be carefully evaluated and a net distinction between sequestration and turnover of carbon pools (Weiss et al., 2025) should be quantified.*

*The observed double peak in dissolved oxygen reflects biological activity: the first peak corresponds to the post-convection bloom, while the second is associated with a smaller bloom occurring in the region during late summer/autumn.*

Line 272 could list the years with higher temperatures in summer (as was done for salinity in the following paragraph) – a little more detail about the anomalies would be useful

*A discussion of the seasonal anomalies of surface temperatures has been included in the manuscript. Particularly, lines 276 - 282 have been changed as follows.*

*Temperature reflects the seasonal alternation between winter mixing and summer stratification, with values ranging, on average, from 14.87 °C in winter to 25.39 °C in summer (Figure 2b). Particularly high temperatures were recorded in 2015, when summer maxima reached 29.6 °C in July. In the following years, summer temperatures remained close to the seasonal average. Interestingly, winter minima showed a gradual increase over time, from values near the seasonal average in the period 2016 - 2019 to an average of around 15.04 °C starting from 2021. Overall, surface temperatures appear slightly higher than those reported for the northwestern Mediterranean, especially during the summer months.*

Technical corrections: line 76 should read 'represents an important resource'

*It will be changed in the final version of the manuscript.*

likewise, line 95 should be plural for 'components'

*It will be changed in the final version of the manuscript.*

The mooring diagram in figure 1 could be clearer as I was curious to know what depth the biogeochemical measurements extended to

*We will add the depths of the sensors in the figure. However, if the reviewer is referring to the instruments on the mooring, the precise depths have changed toward the years, so we will put 'nominal' depths. Regarding the sensors at the surface, it is specified in the text that sensors were deployed at 2 m depth.*

Line 201 should refer to 'seven data points' (rather than just seven data)

*It will be changed in the final version of the manuscript.*

The Bensi reference is in capital letters (this may be convention for a thesis though?)

*It's not a convention and this will be changed in the final version of the manuscript.*

The Cardin et al reference (line 402) has 'et al' listed when all authors should have been listed. Likewise, Menna et al., Line 494

*It will be changed in the final version of the manuscript.*

## **Answer reviewer 2.**

Review of "Surface pCO<sub>2</sub> and hydrography in the dense water formation area of the southern Adriatic" by Carlotta Dentico et al.

### **General comment**

The manuscript describes a dataset gathering atmospheric data as well as hydrological and biogeochemical ocean data collected in the South Adriatic Pit, a region of water mass formation through intermediate to deep convection in the Mediterranean Sea. The authors present the quality-control and correction methodologies, the corrected data, and discuss their reliability and potential use for air-sea CO<sub>2</sub> flux estimates.

This dataset represents a very valuable contribution to studies on carbon and oxygen cycles in a region that plays a crucial role in the physical dynamics and biogeochemical functioning of the Mediterranean Sea. In particular, it will be highly useful for estimating air-sea CO<sub>2</sub> fluxes, assessing model outputs, and evaluating uncertainties associated with observational data.

The manuscript is generally well written and well organized, and the dataset is accessible and clearly presented. However, I have several concerns that should be addressed before I can recommend publication.

*We thank the reviewer for the constructive comments and suggestions, which contributed to improving the manuscript. Below, we addressed each comment and we indicated the corresponding modifications made in the final version of the manuscript.*

### **Main comments**

My first comment concerns the Introduction. Since the manuscript focuses on air-sea CO<sub>2</sub> fluxes, I suggest adding a paragraph reviewing previous studies on air-sea CO<sub>2</sub> fluxes in the Adriatic Sea. In particular, the authors could further develop the role of this region in the uptake and storage of atmospheric CO<sub>2</sub>, based on existing studies in the Adriatic Sea and/or other Mediterranean deep- or intermediate-water formation regions.

*In the introduction we included a discussion of the main results from the paper of Ingrosso et al. (2017). This study highlighted for the first time the role of the Adriatic Sea dense water formation in sequestering and storing anthropogenic carbon dioxide. The*

*following has been added in the text: 'Previous studies have demonstrated that NAdDW enriched in CO<sub>2</sub> water mass cascades in the southern Adriatic deepest layers and mixes with ambient waters, leading to substantial modifications of the CO<sub>2</sub> content of AdDW (Cantoni et al., 2015; Inghrosso et al., 2017). Additionally, Inghrosso et al. (2017) provided the first observational evidence of the role of southern Adriatic dense water formation in the sequestration of anthropogenic CO<sub>2</sub>, capturing conditions indicative of vertical convection and CO<sub>2</sub> accumulation at intermediate depths. These findings highlight the importance of sustained observations for understanding carbon dynamics in this dense water formation region. In this context, the physical and biogeochemical properties of the southern Adriatic Sea (SAd) are monitored through coordinated open-ocean observations, including research vessels, moorings, and autonomous platforms such as ocean gliders and Argo floats.'*

*Regarding the discussion in other Mediterranean deep/dense water formation regions, as the reviewer suggested this also further in the manuscript, we decided to include it in Sect. 3, where, a comparison of pCO<sub>2</sub>, hydrography and wind between southern Adriatic and other sites in the Mediterranean sea has been made.*

References:

*Inghrosso, G., Bensi, M., Cardin, V., and Giani, M.: Anthropogenic CO<sub>2</sub> in a dense water formation area of the 465 Mediterranean Sea, Deep Sea Research Part I: Oceanographic Research Papers, 123, 118–128, 466 <https://doi.org/10.1016/j.dsr.2017.04.004>, 2017.*

*Cantoni, C., Luchetta, A., Chiggiato, J., Cozzi, S., Schroeder, K., and Langone, L.: Dense water flow and carbonate system in the southern Adriatic: A focus on the 2012 event, Marine Geology, 375, 15–27, <https://doi.org/10.1016/j.margeo.2015.08.013>, 2016.*

My second main comment concerns the pCO<sub>2</sub> data. I understand that sea water pCO<sub>2</sub> is provided in the file EMSO-E2M3A-B CARBON\_pCO<sub>2</sub>.txt. However, from Section 2.2.3 and L. 333-336 of the conclusion it is unclear whether atmospheric pCO<sub>2</sub> from the year 2023—data that would be extremely valuable for air–sea CO<sub>2</sub> flux estimates—is also included in the dataset. If not currently provided, please consider adding these atmospheric pCO<sub>2</sub> data to it?

*It is correct that for the moment the atmospheric pCO<sub>2</sub> is not included in the dataset. We decided to first collect a longer time series before including the already available 4 months in the dataset presented here.*

My third set of main comments relates to the description and analysis of the seasonal cycles of parameters in Section 3 (“Robustness and seasonal consistency”).

a) Please consider adding to Section 3: (i) a figure and description of the pH time series for 2015–2016, since the analysis and corrections of these data are described in a dedicated subsection of Section 2 and since ocean acidification is highlighted in Abstract and Introduction ; (ii) atmospheric pCO<sub>2</sub> time series (if available) given the strong focus on air–sea CO<sub>2</sub> fluxes and the discussion in Section 4.

*We decided not to include a dedicated discussion on pH because the available time series is short and temporally discontinuous. Although ocean acidification is mentioned in the Abstract and Introduction, the current amount of data does not allow for robust conclusions. Nevertheless, pH is included in the dataset because, unlike atmospheric pCO<sub>2</sub>, its time series is longer (two years) and seawater samples are available for comparison with the autonomous probe measurements. Similarly, atmospheric pCO<sub>2</sub> is not included at this stage, as the available time series is still too short (approximately four months) to support a meaningful analysis of the data.*

b) I recommend adding explanations for data gaps, in particular: (i) the three main gaps in 2019–2020, 2021, and 2022–2024; (ii) the gaps in the pCO<sub>2</sub> time series.

*Explanations for data gaps are given in the caption of Figure 2, particularly we wrote that ‘Periods of missing data were due to maintenance operations, malfunctioning of the sensors and/or the removal of measurements that failed the quality-control procedures described in Sect. 2.2.’. Nevertheless we integrated a sentence in the text to make it more clear.*

*In particular, we wrote ‘Data gaps were due to maintenance operations, malfunctioning of the sensors and/or the removal of measurements that failed the quality-control procedures described in previous sections.’*

c) The analysis of the seasonal cycles of pCO<sub>2</sub> and dissolved oxygen should be clarified and further developed. I suggest emphasizing the specificity of seasonal variability in this open-convection region by comparing it with previous studies conducted in this region or other water-mass formation areas. Additional detailed comments on that point are provided below in the following specific comment section.

*Specific modifications were introduced in the manuscript, following the reviewer suggestions, and will be presented below.*

Specific and technical comments

L. 12: I suggest “uptake by” or “into the ocean”

*Modified in the final version of the manuscript.*

L. 16: I suggest replacing “like” with “such as”

*Modified in the final version of the manuscript.*

L. 25: Repetition of and → “quality check, correction, and post-processing”.

*Modified in the final version of the manuscript.*

L. 34–35: Unit → GtC yr<sup>-1</sup> (minus sign not visible).

*Modified in the final version of the manuscript.*

L. 47: I suggest removing “water” or replacing with “waters”

*Modified in the final version of the manuscript.*

L. 51: I suggest citing the final published version of Kapsenberg et al. (2017).

*Modified in the final version of the manuscript (this was a typing error).*

L. 59: “E–NE winds”.

*We decided to leave E-NE wind, as it refers to the Bora wind direction, with Bora being a specific wind type.*

L. 61–63: Please refer to previous observational or modelling studies documenting enhanced CO<sub>2</sub> dissolution.

*As this sentence specifically refers to the northern Adriatic and to the formation of NAdDW and associated CO<sub>2</sub> dissolution processes, we chose to cite the recent work by Cantoni et al. (2024), which provides a comprehensive observational review of carbon dynamics in the Adriatic Sea.*

L. 70–72: The sentence “The observatory ... networks” appears redundant; please consider removing it or merging it with the previous sentence.

*Modified in the final version of the manuscript as follows: ‘Here, meteorological, physical and biogeochemical data collected by the EMSO ERIC and ICOS ERIC South Adriatic observatory named EMSO-E2M3A, operated by the Italian National Institute of Oceanography and Applied Geophysics (OGS) will be presented. EMSO-E2M3A is part of the European Multidisciplinary Seafloor and Water Column (EMSO) - South Adriatic Regional Facility - (EMSO ERIC) and of the Integrated Carbon Observation System (ICOS) networks. It has been in operation since 2006 representing the longest open-ocean time series in the whole Adriatic [..].’*

L. 73: I suggest replacing “being” with “making it”.

*Modified in the final version of the manuscript.*

L. 76: Remove the comma.

*Modified in the final version of the manuscript.*

L. 79: Please clarify what “These results” refers to.

*Modified in the final version of the manuscript as follows: ‘Ultimately, this dataset and its potential applications will contribute to assess the role of the southern Adriatic in regulating CO<sub>2</sub> exchange and to quantify the carbon stored in the deep layers.’*

L. 83: I suggest defining the abbreviations SAd and SAP at first occurrence.

*Modified in the final version of the manuscript. However, here, SAP is in its first occurrence.*

L. 89: “the Ionian Surface Water”.

*Modified in the final version of the manuscript.*

L. 90: Add a comma after “Strait”.

*Modified in the final version of the manuscript.*

L. 91–93: I suggest following the recommendation of Schroeder et al. (2024) in naming intermediate water masses in the southern Adriatic Eastern Intermediate Water (EIW) since they can be the result of a mixing of LIW and Cretan Intermediate Water.

*Katrin Schroeder, Sana Ben Ismail, Manuel Bensi, Anthony Bosse, Jacopo Chiggiato, et al.. A consensus-based, revised and comprehensive catalogue for Mediterranean water masses acronyms. Mediterranean Marine Science, 2024, 25 (3), pp.783-791.*

*Modified in the final version of the manuscript as follows: 'Eastern Intermediate Water (EIW) which is not a water mass per se, but the combination of Levantine Intermediate Water (LIW) and Cretan Intermediate Water (CIW; Schroeder et al., 2024);'*

L. 92: Remove "it".

*Modified in the final version of the manuscript.*

L. 100: "The reversal of the NIG leads to the entrance of Atlantic Water (anticyclonic NIG), which decreases ..."

*Modified in the final version of the manuscript.*

L. 101–102: The end of the sentence "and the Levantin Surface Water (cyclonic NIG)" and the reference to "The latter" are unclear and should be clarified.

*Modified in the final version of the manuscript as follows: 'The anticyclonic phase of the NIG leads to the entrance of the Atlantic Water, which decreases the salinity and the density of the AdDW. The cyclonic phase brings warm and salty EIW and Levantine Surface Water in the basin increasing the salinity (and density) of the outflowing AdDW into the IS that gradually impairs the cyclonic NIG, eventually reversing it to an anticyclone.'*

L. 107: I suggest "allowing real-time transmission of meteorological, and ocean surface hydrological and biogeochemical data".

*Modified in the final version of the manuscript.*

L. 108–110: Are the deepest data from the second mooring line accessible in another repository? If so, please provide the link.

*Modified in the final version of the manuscript. The link to the repository has been added, specifically Cardin et al. (2025a).*

*Reference:*

*Cardin, V., Le Meur, J., Ursella, L., Dentico, C., Siena, G., Mansutti, P., Brunetti, F., and Partescano, E.: EMSO-E2M3A-Water-Column-time-series-South-Adriatic [dataset], <https://doi.org/10.13120/ZE9Q-3E51>, 2025a.*

L. 121: Add a comma after “data”.

*Modified in the final version of the manuscript.*

L. 121: “The data [...] require quality control”.

*Modified in the final version of the manuscript.*

L. 126: “These data include”.

*Modified in the final version of the manuscript.*

L. 140: Remove the abbreviations “Temp” and “(Sal)”, which I think are not used later.

*Modified in the final version of the manuscript. However, the abbreviation ‘Sal’ is used in Figure 2.*

L. 147: I suggest “a modified salinity threshold compared to Cardin et al. (2014)”. Please specify the new threshold value.

*We added the new threshold value of 39.5 instead of 39 that was used in Cardin et al., 2014.*

L. 153: "the second quality-control procedure *focused on*".

*Modified in the final version of the manuscript.*

L. 158: Remove the "t".

*Modified in the final version of the manuscript.*

L. 173: Please specify the maximum difference obtained between discrete-sample DO and probe DO.

*We specified this in the text: specifically we obtained a maximum difference of - 10.35  $\mu\text{mol/kg}$  (-0.232 mL/L).*

L. 180: I suggest "*alternating mode*".

*Modified in the final version of the manuscript.*

L. 182: " $\mu\text{atm}$ ".

*Modified in the final version of the manuscript.*

L. 207: Close the parenthesis in Eq. (2).

*Modified in the final version of the manuscript.*

L. 264 and following: I suggest avoiding reference to Pecci et al. (2024) if it remains a preprint. Comparison with Ligurian Sea time series (Merlivat et al., 2018; Coppola et al., 2020) would be valuable.

*Coppola, L., Boutin, J., Gattuso, J.-P., Lefèvre, D., & Metz, N. (2020). The Carbonate System in the Ligurian Sea. In C. Migon, P. Nival & A. Sciandra (Eds.), *The Mediterranean Sea in the Era of Global Change* (vol. 1). ISTE / Wiley. DOI : 10.1002/9781119706960.ch4*

Merlivat, L., Boutin, J., Antoine, D., Beaumont, L., Golbol, M., and Vellucci, V.: Increase of dissolved inorganic carbon and decrease in pH in near-surface waters in the Mediterranean Sea during the past two decades, *Biogeosciences*, 15, 5653–5662, <https://doi.org/10.5194/bg-15-5653-2018>, 2018.

We checked for Pecci et al., (2024) and it is still a preprint, thus in the final manuscript it will remain cited in this way. We included a comparison with the Ligurian Sea, answering also to the previous comment of the reviewer, inserting a summary of the information reported in Merlivat et al. (2018) in Table 2.

L. 266–267: I suggest rephrasing this sentence to remove the confusing link between vertical mixing and the enhancement of CO<sub>2</sub> solubility. Do you mean the decrease of temperature due to vertical mixing enhances CO<sub>2</sub> solubility? Vertical mixing of surface waters with high-TCO<sub>2</sub> waters can induce an increase in surface pCO<sub>2</sub> at constant temperature and salinity (Copin-Montégut and Bégovic, 2002; Copin-Montégut et al., 2004; Touratier et al., 2016;). It may be the case for instance at the end of 2017 and beginning of 2021 in the present dataset.

Copin-Montégut, C., and Bégovic, M.: Distributions of carbonate properties and oxygen along the water column (0–2000 m) in the central part of the NW Mediterranean Sea (Dyfamed site): influence of winter vertical mixing on air–sea CO<sub>2</sub> and O<sub>2</sub> exchanges, *Deep-Sea Res. Pt. II*, 49, 2049–2066, [https://doi.org/10.1016/S0967-0645\(02\)00027-9](https://doi.org/10.1016/S0967-0645(02)00027-9), 2002.

Copin-Montégut, C., Bégovic, M., and Merlivat, L.: Variability of the partial pressure of CO<sub>2</sub> on diel to annual time scales in the Northwestern Mediterranean Sea, *Mar. Chem.*, 85, 3–4, <https://doi.org/10.1016/j.marchem.2003.10.005>, 2004.

Touratier, F., Goyet, C., Houpert, L., Durrieu de Madron, X., Lefèvre, D., Stabholz, M., and Guglielmi, V.: Role of deep convection on anthropogenic CO<sub>2</sub> sequestration in the Gulf of Lions (northwestern Mediterranean Sea), *Deep Sea Res. Pt. I*, 113, 33–48, <https://doi.org/10.1016/j.dsr.2016.04.003>, 2016.

Yes, the sentence was not clear. In the final version of the manuscript we revised it as follows: ‘Lower values are observed in winter, with pCO<sub>2sw</sub> concentrations ranging between 350 and 400 μatm, mainly due to the cooling of the sea surface layer. However, during periods of winter convection and enhanced vertical mixing, pCO<sub>2sw</sub> increases, as observed between December and February in the 2017–2018, 2020–2021, and 2023–2024 periods.’

L. 267: I suggest replacing “occasionally” with “in 2022”.

*Modified in the final version of the manuscript.*

L. 272: Please clarify whether this sentence refers to Garcia-Ibañez et al. (2024).

*Modified in the final version of the manuscript.*

L. 275: If retaining Pecci et al. (2024), please be more specific about the measurement location.

*Modified in the final version of the manuscript as follows: 'These salinity values are typically higher than those observed in the central Mediterranean (e.g., Lampedusa site, Pecci et al., 2024)'.*

L. 276–277: Please add the influence of Cretan waters in Sect. 2.1 to match this sentence.

*Modified in the final version of the manuscript as we introduced the EIW (see comments before) when we described the different water masses present in the southern Adriatic. In this way, it is more clear the effect of Levantine or Cretan water in the salinity values measured in the southern Adriatic compared to other Mediterranean Sea regions.*

L. 278–279: The explanation of the seasonal DO cycle appears unclear to me. Mixing can decrease surface DO when the mixed layer reaches deep waters that are poorer in oxygen (Ulses et al., 2021). It may be the case in the present data, in December/January of each year, and explain the double peak shape of the curve. Second, could you please explain the meaning of “productive season”? I find the association of “productive season” with “biological activity consumes oxygen” awkward. Third, please consider specifying the influence of phytoplankton blooms: are they responsible for the peak values? I recommend rephrasing the sentence.

*Ulses, C., Estournel, C., Fourrier, M., Coppola, L., Kessouri, F., Lefèvre, D., and Marsaleix, P.: Oxygen budget of the north-western Mediterranean deep- convection region, Biogeosciences, 18, 937–960, <https://doi.org/10.5194/bg-18-937-2021>, 2021.*

*Modified in the final version of the manuscript as follows: 'Oxygen concentrations follow the expected seasonal dynamics (Figure 2d): lower values are shown during winter mixing, when ventilation dominates, followed by an increase (first peak in the time series)*

that corresponds to the post-convection bloom. In summer, oxygen concentration decreases again due to respiration processes (e.g., Martellucci et al., 2024b). Finally, DO increases again at the end of summer (second peak in the time series) due to a smaller bloom occurring in the region during late summer/autumn.

L. 279–280: Please provide more detail on the regional wind climatology.

*Modified in the final version of the manuscript as follows: 'Wind speed data (Figure 2e) are also consistent with the regional mean values (e.g., Turk et al., 2010; Pecci et al., 2024) and underline the role of atmospheric forcing in sustaining vertical exchanges, particularly in winter. Mean wind speed value was 5.12 m/s but being frequently higher than 10 m/s. Nevertheless, here an assessment of the main wind regimes in the southern Adriatic was not performed as it was beyond the scope of this manuscript.'*

L. 290: I suggest "in recent years" or "in the last two decades", Turk et al. (2010) showing 2007-2008 observations.

*Modified in the final version of the manuscript.*

L. 321–322: Please consider adding "in the southern Adriatic Sea", if it is the first time in this area.

*Modified in the final version of the manuscript.*

L. 323–324: Clarify what "This latter" refers to.

*Modified in the final version of the manuscript as follows: 'In addition, the methods used to perform quality control (QC) of the data and possible improvements are discussed. QC is particularly important for  $p\text{CO}_{2\text{sw}}$  data as many challenges (such as strict ship time windows) were faced in the region as discussed in Sect. 2.2.'*

L. 324: "as discussed in Sect. 2.2"

*Modified in the final version of the manuscript.*

L. 325–326: This sentence appears redundant with L.319–321.

*This sentence has been removed in the final version of the manuscript.*

L. 331: Please add "at the surface" after "carbon flux"

*Modified in the final version of the manuscript.*

L. 332: Please specify “the considerations discussed in Sect. 4”.

*We agree with the reviewer that this sentence was a bit general and repetitive. Indeed in Sect. 4 we discussed the importance of estimating all the uncertainties related to the input variables of FCO<sub>2</sub> calculation. Thus, in the final version of the manuscript the sentence has been modified as follows: ‘This dataset could be also used to characterize carbon flux at the surface in the SAP using only in situ observations, explicitly estimating uncertainties associated with the input variables for CO<sub>2</sub> flux calculations (Sect. 4).’*

L. 333-336: Please clarify what “it” L. 333 refers to. Is atmospheric pCO<sub>2</sub> included in the dataset?

*No, it refers to uncertainty estimation and the sentence has been changed in the final version of the manuscript as follows: ‘In particular, uncertainty related to key parameters such as atmospheric pCO<sub>2</sub> and wind speed, which are typically derived from other observatories or retrieved from models.’*

L. 334: Please provide references or specify the types of studies mentioned.

*We included the study of Ulses et al. (2023) which aimed to investigate the deep-convection process and its impact on biogeochemical fluxes based on observational platforms and numerical models. In the study, the authors performed various sensitivity tests to estimate the uncertainties of the modeled air-sea CO<sub>2</sub> flux. The most relevant for this manuscript are:*

- *The first set of tests that was based on the parametrization of the gas transfer coefficient  $k$ .*
- *The second set of sensitivity tests, were they considered the atmospheric mole fraction by adding and subtracting an associated uncertainty of 3 ppm due to spatial variabilities (Keraghel et al., 2020).*

*Reference:*

*Ulses, C., Estournel, C., Marsaleix, P., Soetaert, K., Fourier, M., Coppola, L., Lefèvre, D., Touratier, F., Goyet, C., Guglielmi, V., Kessouri, F., Testor, P., and Durrieu De Madron, X.: Seasonal dynamics and annual budget of dissolved inorganic carbon in the northwestern*

*Mediterranean deep-convection region*, *Biogeosciences*, 20, 4683–4710, <https://doi.org/10.5194/bg-20-4683-2023>, 2023.

Table 2: Consider citing Ligurian Sea and Gulf of Lion datasets (with DYFAMED being another open-convection region). References of recent published articles and dataset:

*Merlivat Lilliane, Boutin Jacqueline (2020). Mediterranean Sea surface CO2 partial pressure and temperature data. SEANOE. <https://doi.org/10.17882/56709>*

*Wimart-Rousseau C, Wagener T, Bosse A, Raimbault P, Coppola L, Fourier M, Ulses C and Lefèvre D (2023) Assessing seasonal and interannual changes in carbonate chemistry across two time-series sites in the North Western Mediterranean Sea. Front. Mar. Sci. 10:1281003. doi: 10.3389/fmars.2023.1281003*

*Coppola, L., Boutin, J., Gattuso, J.-P., Lefèvre, D., & Metzl, N. (2020). The Carbonate System in the Ligurian Sea. In C. Migon, P. Nival & A. Sciandra (Eds.), *The Mediterranean Sea in the Era of Global Change* (vol. 1). ISTE / Wiley. DOI : 10.1002/9781119706960.ch4*

*Merlivat, L., Boutin, J., Antoine, D., Beaumont, L., Golbol, M., and Vellucci, V.: Increase of dissolved inorganic carbon and decrease in pH in near-surface waters in the Mediterranean Sea during the past two decades, *Biogeosciences*, 15, 5653–5662, <https://doi.org/10.5194/bg-15-5653-2018>, 2018.*

*Kapsenberg, L., Alliouane, S., Gazeau, F., Mousseau, L., and Gattuso, J.-P.: Coastal ocean acidification and increasing total alkalinity in the northwestern Mediterranean Sea, *Ocean Sci.*, 13, 411–426, <https://doi.org/10.5194/os-13-411-2017>, 2017.*

*As mentioned, references from the Ligurian Sea have been added in Table 2. However, we chose to include only the study by Merlivat et al. (2018), as it specifically focused on the surface layer. In contrast, the results of Wimart-Rousseau et al. (2023) mainly pertain to the mixed layer, while Kapsenberg et al. (2017) addressed surface changes in total carbon, total alkalinity, and pH, without providing data on pCO<sub>2</sub>. Although these studies are highly valuable and provide important insights, we aimed to maintain the focus of the manuscript on pCO<sub>2</sub>, since the available seawater samples (e.g., for total alkalinity) and autonomous measurements (e.g., pH) cover only short periods and are therefore insufficient for a robust comparison with Ligurian Sea time series.*

Figure 1: Please improve Fig. 1a by adding region and strait names (SAP, Middle Adriatic, Otranto Strait, Adriatic and Ionian Seas); please increase the resolution of Fig. 1b which is difficult to read.

*The figure has been updated according to the reviewer's suggestion in the final version of the manuscript.*

Figure 2: In panels d (and possibly c), data points appear connected by a line at the end of 2015, which may be misleading. Please check.

*Similarly, this figure has been updated and corrected according to the suggestions given here.*