

RESPONSE TO REVIEWERS

Jeanne Dombret, Hugo Bellenger, Xavier Perrot, Laëtitia Parc, Lester Kwiatkowski, Frédéric Chevallier, Laurent Bopp, Marion Gehlen, Roland Séférian, Sarah Berthet, and James C. Orr: Data-based estimates of ocean carbon uptake biased high from neglect of submonthly atmospheric pressure variability, Submitted to Biogeosciences.

Review #1

We want to thank the two reviewers for their feedback and suggestions on our manuscript. After a careful read, we have tried to clarify any ambiguities, and we have modified the manuscript accordingly. Please find below the detailed responses to the reviewers' comments and the associated modification of the text. The reviewers' comments are in blue, our responses in red and extracts of the manuscript in black with changes highlighted.

REVIEWER 1

This study uses environmental variables at different temporal resolutions to estimate data product-based air-sea CO₂ fluxes and demonstrates that conventional flux calculations based on monthly mean variables overestimate ocean CO₂ uptake by 0.12 Pg C yr⁻¹. This bias is substantial, representing approximately 5% of the total ocean CO₂ uptake and accounting for about 25% of the discrepancy between model-based and data-based flux estimates. The experiments are well designed, the results are clearly presented, and the manuscript is well written. I therefore recommend publication after the authors address the following minor comments.

Line 34: For a non-expert of models, the reader might don't know that the model has a very high resolution for all the variables. Probably first state that this resolution issue does not affect the model-based flux estimates.

In response, we added the highlighted text below to line 29:

Here we investigate how this gap might change by more fully accounting for submonthly variability in observation-based estimates, **a step closer to the roughly hourly frequencies used in models.**

Line 60: check if this is a typo, "fco2 atm" I think. fixed

Line 61/141: CO₂ fixed

Line 62: maybe add “often”, ...coefficient is often parameterized... Done

Line 64: “Thus” should be “The” ? “Thus” has been removed, now line 65.

Line 70-71, 80-82: I can understand what the authors want to argue here, but I am not sure if I agree on the statements themselves. I agree that the model can simulate the feedback process, but it does not mean the data-based flux estimate does not account for this process. Any feedback occurs in the ocean side should have already been reflected in the observed $f\text{CO}_2\text{w}$ values. I agree that the model is less sensitive to the K , but the model is sensitive to the vertical transport, which is uncertain in the model if I am right. If you want to stress the uncertainties associated the data-based and the model-based flux estimates, I guess a fairer way is to be honest to both methods. Similar comment for the next paragraph. I would suggest including some uncertainty analysis of the models, which does not harmful to any of the results presented in this study.

Line 70-71: For more balance in our presentation of the limitations of data-based and model-based estimates, we added the following sentence on line 73-74:

While models have their own problems, such as biases in simulated circulation fields, they are insensitive to the magnitude of the gas transfer velocity.

Line 80-82: Of course, the observed $f\text{CO}_2\text{w}$ is affected by the interaction between atmosphere and ocean, and thus the feedback between $f\text{CO}_2\text{w}$ and air-sea flux. But there is no compensation in the data-based approach when there are biases in estimates of the gas transfer velocity. This lack of coupling in the data-based approach makes it more sensitive to errors in the gas transfer velocity than in the real ocean and in GOBMs, where the coupling is inherent. This is more precisely stated in the new version of the manuscript. Lines 81-87 now read:

Using observed rather than simulated $f\text{CO}_2\text{sea}$ is an advantage since it reflects the real ocean's air-sea interaction. But it comes with a disadvantage: the data-based approach cannot benefit from the previously mentioned feedback between the flux and $f\text{CO}_2\text{sea}$. Thus compared to the GOBMs, data-based estimates of air-sea CO_2 fluxes are more sensitive to errors in the bulk flux parameterization, with associated uncertainties scaling proportionally to the imposed gas transfer velocity (Gloege et al. 2025, Jersild and Landschützer 2024).

Paragraph starting line 87: We now briefly discuss uncertainties sources of the GOBMs to better balance the strengths and limitations of the two approaches. Lines 92-101 now reads:

Friedlingstein et al. (2025) suggest a possible 10%-20% underestimate from the GOBMs for the ocean uptake of anthropogenic carbon owing to several potential problems: an overestimated Revelle factor, salinity biases in the Southern Ocean, an underestimated

penetration of carbon into the ocean interior from weak vertical mixing and transport, and a delayed beginning for the anthropogenic perturbation, after the actual start of the industrial era (see Bronselear et al., 2017, Terhaar et al. 2022, Terhaar et al. 2025).

Other potential sources of model-data discrepancy come from the data-based approach. First, there is the $0.65 \pm 0.15 \text{ Pg C yr}^{-1}$ that must be added to the data-based estimates to back calculate the anthropogenic carbon flux from the total flux by removing the global effect of the net preindustrial outgassing driven by the natural imbalance between riverine input and sediment loss of carbon (Aumont et al., 2001; Regnier et al., 2022), as discussed by DeVries et al. (2023), Perez et al. (2024) and Planchat et al. (2025). Another important source of uncertainty is the sparsity of SOCAT measurements in some key regions such as the Southern Ocean (Hauck et al., 2020, 2023). Finally, discrepancies may also stem from the very different temporal resolutions of the two approaches.

Line 112/117: missed the right part of the bracket. Fixed

L138/143: The K0 fixed

L140: while -> and fixed

L141: CO2 -> CO2 fixed

L143: a is referred to as the coefficient of gas transfer in W14. OK, now line 148 (to avoid confusion, we now refer to a as the empirical scaling constant)

L153: New paragraph for “For simplicity...” Done

Line 173: just a question, looks like the monthly Xco2 is used. If a higher resolution Xco2 is available? Do you think the Xco2 also response to the cyclone? I guess there will be a vertical profile of Xco2, do you think the cyclone will increase the vertical mixing of the atmosphere and results in change of Xco2?

Line 194: ok, this partially answers my question for line 173, but I still curious about your opinions on my question.

We confirm that we use monthly X_{CO_2} to compute all fluxes. To make this point clearer, the sentence previously on line 194 appears now sooner in the text on lines 179-180. We also added Table 1 which summarizes all datasets that we have used to compute fluxes.

The weekly surface X_{CO_2} dataset does exist but only as zonal means so it cannot be used to resolve the anomalies associated with localized meteorological events. As the reviewer suggests, there is a vertical gradient in X_{CO_2} . Thus, vertical transport associated with deep convection can affect surface X_{CO_2} . For instance, using aircraft measurements, Li et al (2010) evaluated the MJO modulation of surface X_{CO_2} caused by vertical motion in the

lower troposphere, finding a peak-to-peak amplitude of ~ 1.0 ppmv. In cyclones, there may well be similar variations in X_{CO_2} . But whether changes in surface $f\text{CO}_2$ are nearly entirely dominated by the associated drop in surface atmospheric pressure would require a full dedicated study. Although interesting, we must leave this question for future work.

Li, K. F., B. Tian, D. E. Waliser, and Y. L. Yung, 2010: Tropical mid-tropospheric CO₂ variability driven by the Madden-Julian oscillation, Proc. Nat. Acad. Sci., 107, 45, 19171-19175, www.pnas.org/cgi/doi/10.1073/pnas.1008222107

Line 171: Since indicated in line 187 that the ERA5 data is used, the highest resolution of the SST product available (in ERA5) is hourly I believe.

The ERA5 dataset indeed contains an hourly skin sea surface temperature (skt) representing the interfacial temperature with (1) the effect of diurnal warming occurring within the first meters to first tenth of meters of the ocean and (2) the cool skin effect that is a temperature gradient within the diffusive microlayer of less than a millimetre-depth. Recent studies have shown the potential effect of these upper ocean vertical gradients on the global ocean carbon sink (Watson et al. 2020, Dong et al. 2022, Bellenger et al. 2023 already cited in the manuscript). However, to date, all GOBMs and all but one data-based products are using a bulk sea surface temperature (sst) equivalent to the daily sst from ERA5. This sst represents the temperature of the inter-diurnal mixed layer or the temperature of a few meters-depth. Therefore, to be straightforwardly comparable to current data-based products and GOBMs estimates we choose to use the daily sst. Not including the hourly variations of the skin temperature (skt) allows us to avoid confusing the reader with effects that have already been addressed in previous studies (Watson et al. 2020, Dong et al. 2022, Bellenger et al. 2023). This is now stated in lines 180-184 :

We also use the bulk daily sea surface temperature rather than the hourly sea surface skin temperature to be consistent with current GCB data-based estimates. All but one of those estimates use bulk sea surface temperature. Thus we do not include effects of diurnal warming and cool skin that have been addressed in previous studies (Watson et al. 2020, Dong et al. 2022, Bellenger et al. 2023).

Line 224 (now 251): can you add a sentence why they are different in the end years?

The difference between our estimate and GCB 2024 in the final years can be traced to the changes with time in the GCB data-based product estimates. In GCB 2021 (Friedlingstein et al. 2022), the slowdown of oceanic sink in the last years is captured by GOBMs but not by data-based products. This slowdown becomes clear in data-based estimates only from GCB 2023 (Friedlingstein et al. 2023). In this study, we use the latest OceanSODA-ETHZ DIC and Alk gridded dataset that was produced in 2021 (Gregor et al. 2021) and that

therefore does not capture this slowdown. We added the following discussion (now lines 252-256):

Our estimate S_{ref} is comparable in magnitude to the average from the GCB data products although it does not capture the precise interannual evolution, in particular after 2016 (Figure 1a). This difference is certainly due to the fact that the latest version of the DIC and Alk OceanSODA-ETHZ dataset (v2021, Gregor et al. 2021) does not capture the change in Alk, DIC and fCO_{2sea} associated with the slowdown in ocean sink after 2016 (e.g. Friedlingstein et al. 2022). This slowdown indeed only emerged in GCB data-based products after GCB 2023 (e.g. Friedlingstein et al. 2025).

Fig. 1a: the GCB results represent the average of latitude from -60 to 60, right? Probably indicate this information in the caption. Moreover, in the Key points and abstract, look like the conclusion are relevant for the global ocean, but it is in fact for 60oS-60oN, right? I know the averaged flux beyond this range may not significant relative to the global average, but in somewhere of the manuscript, this should be discussed, I think. Fig 14 from Takahashi et al. (2009) could be an example.

Thank you for pointing out this imprecision. We state in the caption that our evaluation of the global carbon sink is indeed limited to 60S-60N, and we have added in line 197-199:

Regions poleward of 60° contribute only marginally to the global sink (Takahashi et al., 2009).

Line 268: Two suggestions the authors can consider if they want to include in the discussion: 1) do you think the 14C inventory-constrained K has already include this pressure effect? 2) What do you suggest for the future GCB? Looks like this pressure effect is relatively constant in different years. Do you think we can still use the monthly pressure for the data product (to reduce the calculation), but add 0.12 Pg C yr-1 to the final estimated flux? Or for GCB, they can directly subtract 0.12 Pg c yr-1 from their data-based CO2 sink estimates?

Thank you for these suggestions.

1) We concur that scaling the empirical constant a in the formulation for the gas transfer velocity based on the bomb ^{14}C inventory in the real ocean does inherently include the effect of atmospheric pressure. A sentence has been added in the discussion lines 157:

Since it is based on the real ocean inventory of bomb ^{14}C , this scaling approach should inherently account for the effects of submonthly variability of all variables that affect the gas transfer velocity; however there is a 20% uncertainty associated with the bomb ^{14}C inventory and thus k .

2) Our correction is indeed constant in time, yet its value should depend on the method employed to derive the data-based estimate. In addition, taking into account submonthly variation in pressure is straightforward and should not heavily impact the calculation as it only affects the atmospheric fugacity of CO₂ term. Fortunately, it does not affect the seawater fugacity term, which requires advanced statistical or sparse treatment of ocean observations with large gaps. We specify this suggestion in the discussion lines 323-326:

The exact correction will depend on the calculation details and thus will differ between data-based products. For future GCB exercises, we suggest that each group compute independently both directions of the flux $k_{660}(u)A(T,S)fCO_{2atm}$ and $k_{660}(u)A(T,S)fCO_{2sea}$ at maximum temporal resolution before computing monthly averages and taking the difference to obtain the flux.

Line 296: Please check if Rustogi et al. (2025) is a model-based study or a data product-based estimate. The recent study by Dong et al. (2025, Nature Communications) highlighted that the bubble-induced asymmetric transfer increases data-based CO₂ uptake by 0.3-0.4 Pg C yr⁻¹.

Rustogi et al. (2025) is a model-based estimate. We have now included the results from Dong et al. (2025), and compared them to Rustogi et al. (2025), lines 329-335:

The gap between data-based products and GOBMs also depends on model resolution and their representation of coastal regions (e.g. Resplandy et al. 2024) as well as on processes not yet commonly included in the flux parameterization, such as the ocean cool skin (+0.3 Pg C yr⁻¹, Dong et al. 2022), rain (+0.14-0.19 Pg C yr⁻¹, Parc et al. 2024) and wave breaking-induced bubbles. While these bubbles may increase modelled CO₂ uptake by 0.07 Pg C yr⁻¹ (Rustogi et al., 2025), they could increase data-based estimates by up to 0.3-0.4 Pg C yr⁻¹ (Dong et al., 2025).

REVIEWER 2

The study by Dombrey et al. investigates the role of unaccounted-for variability in the computation of monthly fluxes of fCO₂ data-products used in the GCB. The study finds that the “missing variability” contributes to the mismatch between fCO₂ data-products and GOBMs also used in the GCB. The study is thus providing an answer that partially explains a question that is puzzling the ocean CO₂ community. The manuscript is to-the-point, relatively well written with few grammatical errors. Thus, I don't have any major concerns and my review is short. However, I feel there are sections in which the authors could guide the reader a little more in terms of their methodology. In my review, I first list some overarching concerns (though not major). And then I list smaller, line-by-line comments.

In the methodology section, the data sources are described late on in the section. It may be better to add these data sources as a table which is then placed earlier in the manuscript (e.g., somewhere between Eq 2 and Eq 3).

Because of this good advice, we have now moved the dataset description between Eq (2) and Eq (3) as suggested (now lines 175-199), and added the following table:

Table 1. Sources of all datasets used in this study.

Variable	Description	Time resolution	Source
P_{atm}	Surface pressure	Hourly	ERA5
u	5m wind speed	Hourly	ERA5
Sea ice	Sea ice fraction	Hourly	ERA5
T	Sea surface temperature	Daily	ERA5
S	Sea surface salinity	8-days	Droghei et al. (2018)
DIC	Dissolved inorganic carbon	Monthly	OceanSODA-ETHZ
Alk	Alkalinity	Monthly	OceanSODA-ETHZ
X_{CO_2}	CO2 dry air mole fraction	Monthly	NOAA MBL
[P]	Phosphate concentration	Monthly climatology	Broullón et al. (2019)
[Si]	Silicate concentration	Monthly climatology	Broullón et al. (2019)

When it comes to the definition of flux (Eq 1), I recommend sticking to the notations used by Wanninkhof (2014; W14), eq. 2. This then becomes $F = k K_o (fCO_2 atm - fCO_2 sea)$. Then describe k as defined in W14, $k = aU^2 (Sc/660)^{-0.5}$. To simplify things later (eq 5), one could then use the following assignment of $A = K_o a (Sc/660)^{-0.5}$. However, this recommendation is not a strong opinion since it is so slight, but it might just make it easier for the fCO2 data-product community, who tend to use the Wanninkhof notation.

Thank you for this remark. Although the notation from Wanninkhof (2014) is more conventional for the data-product community, we think that our notation is similar while highlighting the near perfect covariance with temperature between the solubility and Schmidt number correction term, which we regroup together as “A”. We also prefer not to add new equations to avoid duplication. From the description of terms given after equation (1), we think all readers familiar with the subject of gas exchange will be able to follow.

Equation 5 seems to have two missing terms. Though, I’m not 100% certain about this, so I’d like to have the authors’ input on this. The first may be implicitly incorporated into the bulk term (I think). And the other is not mentioned. I’ve broken down the decomposition into its basic parts first, and then shown them as shown in the paper. Note that I’ve factored aA out, and I’m showing the equation in terms of F_h and not $\overline{F_h}$ for simplification. If these terms are somehow incorporated, please make this clear in the text or supplementary and explain why they are not included.

The reviewer's decomposition is correct. However, the two additional terms average to zero when considering the monthly mean $\overline{F_h}$ that is shown in equation (5) (now equation (6)). That is, by definition $\overline{\Delta'} = 0$ and $\overline{u'} = 0$, so $\overline{aA\overline{u}^2\Delta'} + \overline{2aA\overline{u}u'} = aA\overline{u}^2\overline{\Delta'} + 2aA\overline{u}\overline{u'} = 0$. In the revised manuscript, we have now made this step more explicit, by including the decomposition for F_h , then its average $\overline{F_h}$ (now lines 232-234):

To investigate the non-linearities between high-frequency variations of the variables that are responsible for differences between F_{ref} and $\overline{F_h}$, a Reynolds decomposition is used with respect to u and $\Delta = fCO_{2atm} - fCO_{2sea}$ for each month from 2009 to 2018, assuming that the solubility and Schmidt number variations cancel out to give $A = \overline{A}$ (Etcheto et Merlivat 1988), an assumption good to within 5% locally (Wanninkhof, 2014):

$$F_h = aA\overline{u}^2\overline{\Delta} + aA(u')^2\overline{\Delta} + 2aA\overline{u}u'\Delta' + aA(u')^2\Delta' + aA\overline{u}^2\Delta' + 2aA\overline{u}u'\Delta' \quad (5)$$

Which results in the monthly mean:

$$\overline{F_h} = \underbrace{aA\overline{u}^2\overline{\Delta}}_{\text{meanbulk}} + \underbrace{aA(u')^2\overline{\Delta}}_{\text{variance}} + \underbrace{2aA\overline{u}u'\Delta'}_{\text{covariance}} + \underbrace{aA(u')^2\Delta'}_{\text{3rdordermoment}} \quad (6)$$

bulkterm *correctionterms*

It would also be helpful to indicate in this equation that bulk formulation is \approx GCB flux.

We added the highlighted sentence (now line 241):

“The combination of the first term (mean bulk) and 2nd term (variance) is referred to as the bulk term ($aA\overline{u}^2\overline{\Delta} = aA\overline{u}^2\overline{\Delta} + aA(u')^2\overline{\Delta}$). Thus by definition it includes the enhancement from wind speed variance. The bulk term is very close to the reference flux F_{ref} . This agreement along with the assumption that $A = \overline{A}$ is illustrated by the mean difference between F_{ref} and the bulk term ($aA\overline{u}^2\overline{\Delta}$), which ranges from 0 to - 0.2 g C m⁻² yr⁻¹ (Figure S1), an order of magnitude smaller than the mean differences between $\overline{F_h}$ and F_{ref} (Figure 1c). **This bulk term (mean bulk and variance in (6)) is the same as the GCB flux definition.**”

There is a recent study by Ford et al. (2024) that does an excellent job of investigating uncertainties of air-sea CO₂ fluxes. This should be included in the discussion and reflected on how your study relates to theirs. See their Fig 4 specifically.

We have now included Ford (2024) as a reference in the introduction :

Line 88:

It is estimated that the ensemble mean 1- σ uncertainty for the 10 models is 0.5 Pg C yr⁻¹ while that for the 8 data products is 0.6-0.7 Pg C yr⁻¹ considering both random and systematic uncertainties (Friedlingstein et al., 2025; Ford, 2024).

Line 104-106:

Another important source of uncertainty is the sparsity of SOCAT measurements in some key regions such as the Southern Ocean (Hauck et al., 2020, 2023; Ford et al., 2024).

L83: I would take this one step further and link this to wind specifically to connect the dots for the reader.

We refer to the complete gas transfer velocity parameterization whatever the form used. In our case, uncertainties come from the empirical scaling constant a and from the wind speed. But other possible parameterizations can take other forms and take into account other variables or parameters (e.g. Fairall et al. 2011, Harrison et al, 2012 or Dong et al. 2025 now cited in the manuscript). However, we acknowledge that there is now a possible confusion between the coefficient of gas transfer and the gas transfer velocity. We changed “gas transfer coefficient” to “gas transfer velocity where applicable, for instance, now in line 84-87:

Thus compared to the GOBMS, the data-based estimates of air-sea CO₂ flux are more sensitive to errors in the bulk flux parameterization with associated uncertainties scaling proportionally to the imposed gas transfer velocity (Gloege et al. 2025, Jersild and Landschützer 2024).

Fairall et al. 2011: Implementation of the Coupled Ocean-Atmosphere Response Experiment flux algorithm with CO₂, dimethyl sulfide, and O₃, J. Geophys. Res., Vol. 116, C00F09, doi:10.1029/2010JC006884

Harrison, E. L. et al. 2012: Nonlinear interaction between rain- and wind-induced air-water gas exchange. J. Geophys. Res. Oceans 117, C03034.

L244: Indicate what you mean by synoptic scales: On synoptic scales (< 10 days?)

The sentence now reads (Line 276):

“On synoptic timescales (2-10 days),...”