We thank one anonymous referee and Dr. Raymond Najjar for their valuable comments and efforts. We also thank Dr. Andrew Kowalski for engaging with our manuscript in a community comment. We provide an updated manuscript, including updated figures, that incorporates changes in response to reviewer feedback. In the following, the comments of the reviewers are presented in **blue**. Our responses are in **black**. Changes in the text based on the tracked version are in **green**. Line numbers listed are based on the **tracked changes version**.

The most substantial revision involves the flux calculation itself. Following the first major comment from Dr. Raymond Najjar, we now include a sea level pressure correction and a cool skin temperature correction in the O_2 saturation concentration, we document the treatment of sub-monthly wind variability that was included in the submitted product but not described, and we quantify the sub-monthly covariance between the gas transfer velocity and the DO anomaly using daily output of a CESM2 simulation. We also fixed a bug in our implementation of the S09 scheme, which has only minor effects on the results. Together these changes improve the simulated meridional APO gradient in the Southern Hemisphere, while leaving the other metrics we analyzed largely unchanged. We note that these changes reduced the raw global annual mean outgassing from 1029 to 368 Tmol yr^{-1}, but because we interpret only the adjusted annual mean flux, which is held unchanged by the observational constraint, all main conclusions hold. All fluxes, uncertainties, figures, and tables have been regenerated with the updated calculation.

Beyond the calculation, we have expanded the manuscript in response to the remaining comments. We added the rationale and sensitivity tests for using mixed layer averaged DO, a discussion of estuarine net heterotrophy within the coastal data gap, and a brief treatment of net ocean metabolism in the Introduction.

In response to the community comment, we make two points. First, we maintain that our treatment of water vapor is physically correct. The O_2 partial pressure governing dissolution is set in the thin layer of air in molecular contact with the sea surface, which is saturated at the skin temperature, so the vapor pressure is evaluated at that interface, rather than at the ambient humidity of the overlying air. Second, using a constant dry-air O_2 mole fraction is also correct. The O_2 partial pressure is the product of the dry-air mole fraction and the dry-air pressure, which is the total pressure minus the vapor pressure, so the humidity dilution raised in the comment enters our calculation through the pressure term rather than through the mole fraction. The dry-air mole fraction itself is observed to be uniform in the atmosphere to within about 0.01%, which is why it can be held constant.

Finally, we redesigned the figures following the detailed suggestions of Referee 2, with larger panels and fonts, print-safe colors, a map layout that keeps the South Atlantic contiguous, and uncertainty maps moved to the supplement.

RC1

Minor comments:

1. L69: “reduces both the upwelling of nutrients”: doesn’t that reduce photosynthesis and thus reduce O2 outgassing?

We have revised this paragraph to provide a clearer line of thought.

L69-78: This net outgassing is driven by ocean heat uptake, which reduces oxygen solubility, and by warming-induced stratification, which reduces the downward transport of oxygen-rich surface waters into the ocean interior (Breitburg et al., 2018; Keeling et al., 2010; Keeling and Garcia, 2002; Schmidtko et al., 2017). Net ocean metabolism contributes an additional term, with oxidation of riverine organic carbon driving a small ingassing, and burial or anaerobic oxidation of marine organic matter driving outgassing (Najjar and Keeling, 2000; Regnier et al., 2022). These metabolic terms are expected to be small relative to the solubility and ventilation terms. Accurately quantifying this annual flux remains challenging because its magnitude is small compared to both the seasonal cycle and interannual variations.

2. L105: “with high accuracy”: According to which metric? A brief information would be useful

The metrics for high accuracy are presented in Section 2.1. GOBAI-O_2 v2.3 has a global mean offset of 0.0 µmol kg^{-1}, and global mean RMSD of 8.3 µmol kg^{-1}. Here we refer to that section.

L112-113: This product successfully generates monthly 3D maps of DO to 2000 meters depth from 2004 onwards with high accuracy, as quantified in Section 2.1.

3. L130: replace “data” by “fields” to avoid confusion with point measurement data

Revised as suggested.

4. L138: “gridded values”: how are these interpolated in space and time? A brief information would be useful

We have added a sentence to clarify the method.

L150-153: The Roemmich and Gilson (2009) fields are produced by mapping individual Argo profiles onto a 1° by 1° monthly grid by optimal interpolation, combining a locally weighted least-squares estimate of the mean field with objective mapping of profile anomalies using spatial covariance functions.

5. L300: “bi-monthly”: every 2 months or twice per month? I think the term is used ambiguously

“bi-monthly” is referred to as every 2 months. We have revised the phrase.

6. L357: replace “SCA” by “seasonal cycle” as you do not mean amplitude here

Revised as suggested.

7. L542: “double-peak structure”: unclear because I do not actually see a double-peak structure in the 60N-80N panel of Fig 6.

This is actually a summer plateau. We have revised the sentence for clarity.

L627-629: In the northern mid-to-high latitudes (60°–80°N), our simulations successfully capture the prolonged outgassing period and the broad, plateau-like summer maximum observed in atmospheric data (Fig. 6), and agree with the observed SCA (Table 2).

8. L548: “in the flux product”: would be good to announce the later discussion on model errors, otherwise the reader keeps wondering

We have revised the following sentence to acknowledge the possible bias coming from the transport model used.

L637-639: The mechanisms driving these phase lags remain unclear and require further investigation, and we cannot rule out a contribution from transport model error given that our simulations use a single transport model (Jin et al., 2025).

9. L831-832: “We note that uncertainty due to DO may essentially stem from the annual mean flux adjustment that corrects DO concentrations for each ensemble member.”: not sure I fully understand this sentence

We have revised this sentence to improve readability.

L968-972: We note that, in the adjusted product, part of the variance attributed to DO originates in the annual mean flux adjustment itself. The adjustment is applied as an offset to DO that is drawn independently for each ensemble member, so that member-to-member spread is absorbed into the DO term of the variance decomposition (Fig. 5) rather than appearing as a separate source.

10. L849-850: "resolving flux interannual variability": but didn't you say only the IAV of the seasonal cycle is resolved?

We have revised the sentence.

L990-991: This dataset represents a significant advance over existing air-sea O_2 flux products by resolving interannual variability in the seasonal cycle of the flux.

11. L167: "Mixed"

Fixed.

RC2 Raymond Najjar

Major comments

Surface oxygen corrections

Skin temperature and atmospheric pressure corrections are noted as important in the introduction but are not considered in study. The omission of the atmospheric pressure correction seems particularly relevant because it has a strong latitudinal variation that might help explain why the simulated APO differs so much from observed APO (Figure 8). Specifically, APO is simulated to be too low in the high southern latitudes and too high almost everywhere else. Najjar and Keeling (2000) found that the pressure correction could be as large as –45 µmol/kg in at 75°S and about 4 µmol/kg at 50°N. The skin correction estimated by Najjar and Keeling (2000) was modest, but perhaps better formulations exist now and should be revisited.

Other corrections that are not included (and I think not even mentioned) are those related to processes occurring at sub-monthly time scales. The simplest and most common of these corrections is the one related to the non-linear dependence of the gas transfer velocity to wind speed—often parabolic. In this example, the gas transfer velocity computed from the monthly mean wind speed is lower than the monthly average of the gas transfer velocity computed from, say, daily or hourly winds. Because this is a large effect, about 25% (as pointed out in Wanninkhof's well-cited 1992 gas-transfer-velocity synthesis), I assume that the authors have already included it, but they say nothing about it, as far as I can tell. But there are also other nonlinearities, such as the co-variation between the gas transfer velocity and the oxygen supersaturation, which were discussed in Keeling et al. (1998).

Another correction that should be at least noted is related to the net metabolism of the ocean, as noted in the minor comments (it's probably small). The bottom line is that there are numerous additional corrections to the flux that have been ignored. I think the atmospheric pressure correction is fairly straightforward and should be included; I would not be surprised if it has been included but was simply not mentioned. Certainly, the regional corrections from ocean inverse modeling might capture the corrections that are missing, but this point should be fleshed out.

We thank the reviewer for this thorough comment. We have added the suggested corrections to the flux calculation and documented several that were already included but not described. We note that the annual flux adjustment (Sect. 2.4) absorbs most of the effect of these changes, since it constrains the global and regional annual mean fluxes independently of the gas exchange formulation, so the adjusted product and the main conclusions are largely unchanged. We also fixed a bug in our implementation of the S09 gas exchange scheme, which only has a minor effect on our final results. We address each point in turn and list all manuscript changes. We then summarize how the results changed at the end.

1. Sea level pressure correction. Now we use monthly mean sea level pressure from ERA5 to scale the O_2 saturation concentration by (P_a - p_H2O)/(P_0 - p_H2O), and the same pressure replaces the fixed one atmosphere in the small bubble terms of the L13 and S09 schemes (new Eq. 6 and revised Appendix A). Over 2004-2024, the pressure scaling ranges from 0.951 to 1.020 and changes the saturation concentration between -17.3 and +7.2 umol/kg. Its zonal annual mean reaches -10.4 umol/kg at 64.5°S, the southern edge of our domain, and stays within 1.5 umol/kg between 40°S and 40°N. This latitudinal structure is fully consistent with the reviewer's expectation from Najjar and Keeling (2000), whose -45 umol/kg value at 75°S lies poleward of our domain. The reviewer's intuition about Figure 8 was also partly correct. In the revised simulations the Southern Hemisphere meridional APO gradient decreases from 0.20 to 0.13 per meg per degree latitude, closer to the observed 0.04, although this change bundles the pressure correction with the other revisions listed below, and the residual equatorial enhancement inherited from the inversion constraints remains.

2. Skin temperature correction. Now we compute the cool skin offset as dT_skin = -0.14 - 0.30 exp(-U10/3.7) K following Donlon et al. (2002), and evaluate the O_2 saturation concentration and the Schmidt number at the interface temperature. The offset ranges from -0.14 to -0.43 K with a global mean of -0.22 K and raises the saturation concentration by 0.5 to 2.8 umol/kg, driving additional O_2 ingassing everywhere. As the reviewer anticipated, the effect is modest but not negligible.

Changes made regarding #1 and #2:

L231-262: The anomaly is calculated as the deviation of DO from GOBAI ($[O_2]$) plus an annual mean adjustment term (δ) against O_2 saturation ($[O_2sat]$), following

$$\Delta[O_2] = [O_2] + \delta - [O_2^{sat}]. \quad (5)$$

This adjustment term constrains the regional and global annual mean O_2 fluxes based on independent observation-based estimates as described in Section 2.4 and Appendix B. Throughout this study, the raw flux refers to the flux calculated without the adjustment δ, while the adjusted flux uses the optimized δ described in Section 2.4 and Appendix B.

$[O_2sat]$ is the O_2 saturation concentration at the air-sea interface, and it therefore requires a skin temperature correction and a sea level pressure correction (Najjar and Keeling, 2000). The interface is cooler than the water immediately below it, and O_2 solubility increases as temperature decreases, so evaluating $[O_2sat]$ at the bulk temperature underestimates it. The equilibrium concentration is also set by the partial pressure of O_2 in the overlying air, which

varies with the weight of the atmospheric column above the sea surface, so referencing [O_2sat] to a fixed one atmosphere misrepresents it wherever sea level pressure departs from 101,325 Pa (Najjar and Keeling, 2000). We therefore calculate

$$[O_2^{sat}] = C_{sat}(T_s + \Delta T_{skin}, S_s) \times \frac{P_a - P_{H_2O}}{P_0 - P_{H_2O}}, \quad (6)$$

where C_{sat} is the O_2 solubility of Garcia and Gordon (1992) at a total pressure of one atmosphere, T_s and S_s are the temperature and salinity of the shallowest GOBAI-O_2 level (2.5 m), P_a is the monthly mean sea level pressure from ERA5 (Hersbach et al., 2020), P_0 is the reference sea level pressure of 101,325, and P_{H2O} is the equilibrium water vapor pressure at Ts calculated following Bolton (1980). This vapor pressure describes the uppermost 1 mm thin layer of air in molecular contact with the sea surface, which is saturated at the water temperature and sets the O_2 partial pressure governing dissolution, rather than the ambient humidity of the overlying air (Weiss, 1970). ΔT_{skin} is the cool skin temperature offset, calculated from the 10-m wind speed (U_{10}), following Donlon et al. (2002)

$$\Delta T_{skin} = -0.14 - 0.30\, e^{-U_{10}/3.7}. \quad (7)$$

Here we use U_{10} from the same wind product that provides the gas exchange velocity in each of the flux models. F_p in Eq. 4 is calculated as the product of the gas exchange coefficient across the large bubble interface and $\Delta[O_2]$. F_c is calculated as the product of a mass transfer coefficient (k_c) and a constant O_2 atmospheric dry-air mole fraction (X_{O_2} = 0.2094). Full descriptions of the three gas exchange schemes are presented in Appendix A, following Emerson and Bushinsky (2016).

3. Sub-monthly wind nonlinearity. We actually included this effect in the submitted product. All submitted fluxes were computed from the monthly mean squared wind speed, $\langle U^2_{10} \rangle = U^2_{10}(\text{mean}) + \sigma^2(U_{10})$ from higher-resolution (hourly to daily) winds, following the practice of the SeaFlux CO_2 product (Fay et al., 2021). This captures the effect the reviewer cites from Wanninkhof (1992) exactly for terms that scale with the square of the wind speed. We simply forgot to say so, and the revised Sect. 2.3 now states it.

L217-222: Wind speeds enter the calculation as monthly fields computed from higher resolution wind speeds, ranging from hourly (ERA5) to 6-hourly (JRA-3Q), and daily (CCMP). To retain the contribution of sub-monthly variability to gas exchange, we combine the monthly mean speed (U_{10}^{mon}) with the intra-month variance ($\sigma^2(U_{10})$) of these winds (Fay et al., 2021), following

$$U_{10} = \sqrt{(U_{10}^{mon})^2 + \sigma^2(U_{10})}, \quad (2)$$

4. Covariance of gas transfer velocity and O_2 anomaly. We now quantify with the diagnostics the reviewer suggested. We used daily output of the CESM2 simulation from Sect. 4.1 to compare fluxes computed at daily resolution with fluxes reconstructed from monthly means of the same simulation. Over 2004-2020 the monthly reconstruction underestimates the global annual mean outgassing by 34±4 Tmol yr^{-1} and overestimates the flux seasonal cycle amplitude by 4 to 10 % in the subpolar regions. Both numbers are now given in the revised discussion as an estimate of what our monthly product cannot resolve. For details, see response to minor comment 25.

5. Net metabolism. We agree it is worth stating and likely small. The revised introduction notes that net ocean metabolism contributes a small flux, with oxidation of riverine organic carbon driving ingassing and burial or anaerobic oxidation of organic matter driving outgassing (Najjar and Keeling, 2000). We cannot apply an explicit correction for this term, for two reasons. First, our fluxes are diagnosed from observed DO fields rather than from a biogeochemical budget, so any metabolic influence on open ocean surface DO is already implicitly contained in the calculated flux. Second, the metabolic component supported by riverine organic carbon is concentrated in nearshore waters that lie outside our domain (Sect. 4.1), and its magnitude and spatial distribution remain too poorly constrained to support a defensible open ocean correction.

L75-78: Net ocean metabolism contributes an additional term, with oxidation of riverine organic carbon driving a small ingassing, and burial or anaerobic oxidation of marine organic matter driving outgassing (Najjar and Keeling, 2000; Regnier et al., 2022). These metabolic terms are expected to be small relative to the solubility and ventilation terms.

6. Annual flux adjustment.

The global and regional annual mean fluxes of the adjusted product are constrained to the heat based global estimate and the Resplandy et al. (2016) inversions regardless of the gas exchange formulation, so any correction whose main expression is in the annual mean and its latitudinal structure is largely absorbed by the adjustment. Nevertheless, adding the pressure and skin corrections together with a correction to the S09 small bubble term (discussed below) changes the raw global annual mean flux from 1029 to 368 Tmol/yr, while the adjusted product changes far less. The adjusted global annual mean remains 61 Tmol/yr by construction, the simulated APO trend from the adjusted flux is unchanged at 1.5±0.8 per meg/yr, and the adjusted seasonal amplitudes change by only a few percent in most regions. The corrections instead matter most for the raw flux, for the seasonal cycle at high latitudes, and for reducing the burden placed on the adjustment. The revised discussion now states this explicitly.

L276-281: We optimize the constant adjustments (δ), and add them to each DO ensemble member (Eq. 5). This constant DO adjustment term varies for each of the six ocean regions, each year, and each flux ensemble member. To avoid an abrupt step in the adjusted flux at the region boundaries, the regional adjustments are smoothed across those boundaries with a 3° Gaussian weighting of the region masks, while each regional annual mean flux still matches its target (Appendix B).

L1146-1154: Applying a constant adjustment inside each region and a different constant in the neighbouring region produces an abrupt step in the adjusted flux boundary. We therefore smooth the regional adjustment across boundaries. For each region, we smooth its regional mask with a Gaussian kernel of 3° standard deviation truncated at 9°, so that the adjustment varies gradually across each boundary and stays constant in the interior of each region. Because each regional adjustment also acts on the neighbouring regions, the regional adjustments are solved jointly, which keeps the annual mean flux integrated over each original region equal to its inversion target. This smoothing slightly shifts the adjusted hemispheric fluxes from 75 and -14 to 71 and -13 Tmol yr^{-1} for the Northern and Southern Hemispheres respectively, based on v2.3.

7. Additional fix. While implementing these corrections we found and corrected an inconsistency in our implementation of the S09 scheme. The completely dissolving bubble term carried the atmospheric O_2 mole fraction (X_{O_2}) a second time, inconsistent with Eq. 3 of Stanley et al. (2009), which resulted in an underestimate of that term. This error has been fixed. After this correction, flux computed using S09 shows the largest SCA over high latitudes.

8. Main changes in results.

8.1 Raw global annual mean O_2 outgassing decreases from 1029 to 368 Tmol yr^{-1} (all corrections combined). The adjusted global annual mean is unchanged at 61 Tmol yr^{-1} by construction.

8.2 Hemispheric O_2 flux SCA increases from 17.7 (NH) and 17.9 (SH) to 18.1 (NH) and 19.9 (SH) mol m^{-2} yr^{-1}. Regional SCA changes by a few percent, largest in the Arabian Sea and Bay of Bengal.

8.3 The SH meridional APO gradient decreases from 0.20 to 0.13 per meg per degree latitude.

8.4 The S09 scheme now yields the largest seasonal amplitudes instead of W97, which changes the scheme ordering statements in Sect. 3.1.1 and 3.2.1.

Figure quality

I know great care has been taken in choosing and making these figures and I am generally impressed with them. However, two issues remain, both having to do with readability in print: (1) font sizes are sometimes too small and (2) colors do not render well. Let's take Figure 2 as an example. This is an extremely important figure, as it captures the regional variability in the annual cycle. While I like the overall design, it is hard to read in print. The axis labels are very small—I estimate them at about 5 point. The labels of the regions on the map are even smaller, about 4 point. Though ESSD has no standard that I could fine, I think 8 point is a reasonable minimum, which is about what the panel titles are. The color scheme is clever and looks fine on the screen, but when you print the figure out on paper, some of the panels, particularly F, K, and R, almost disappear (and note that I am using a very good quality printer at my workplace). For the font size issue, maybe the solution is to (1) widen the overall figure, (2) increase font size on the axis labels, (3) change Jan, Mar, etc. to J, M, etc. or 1, 3, etc., (4) increase the font size on the map labels, and (5) on the map labels only retain the panel letters (A, B, etc.) and not include the codes (NP-SPSS, NP-STSS, etc.). For the color issue, maybe just make the panels black (or at least everything but the curves). The current 6 x 5 panel grid could be played with, too (maybe 7 x 4 would work better and place the figure title above the grid instead of within it). I am pointing out Figure 2, but most of these comments apply to the other figures with a similar format: Figures 4, 5, S3, S4, and S5.

On the same theme of figure readability, I would increase the size of the maps throughout the paper. Take Figure 1 as an example. Panels A, C, E, and G are real gems and I could spend a lot of time poring over them. But they are so small, with a lot of detail lost on the printout. I suggest doubling the size of these maps (but keep font sizes the same—they are fine). While I appreciate the attention to the error analysis by showing panels B, D, F, and H, I am not sure I need to see these panels, at least in the main text (could be move to supplementary). I think the errors are conveyed well enough in Figure 2 and Figures 3C and F. Same goes for Figure 3—panels A and

C are rich and wonderful but too small to be appreciated. The colors in Figure 3A also do not render well on a printout—the whole map looks dark blue expect for a few yellow spots.

One last change I would make in the maps is to line up the western edge with the southern tip of Africa (about 20 degrees East) so that the South Atlantic Ocean is shown as contiguous. For example, there are lovely coastal upwelling features along southwest Africa that get a little lost in the current presentation in Figures 1 and 3.

Otherwise, the remaining figures are notably clear and effective.

We thank the reviewer for these detailed and constructive suggestions. We agree with all of them and have revised the figures accordingly.

For Fig. 2 and the figures sharing its format (Figs. 4, 5, S3, S4, and S5), we have reorganized the panel grid to enlarge each panel, placed the figure title above the grid in its own strip, increased the axis title and axis text sizes to at least 8 pt at print width, shortened the month labels to single letters, enlarged the map labels, and retained only the panel letters on the map while removing the region codes. We have also darkened the colors of the panels that faded in print (previously panels F, K, and R), so that all panels remain legible on paper.

For Fig. 1, we have enlarged the four seasonal mean flux maps and moved the four uncertainty maps to the supplement, as suggested.

For Fig. 3, we have enlarged the SCA and annual mean maps, revised the color scale of the SCA map so that its spatial structure renders well in print, and moved the uncertainty panels to the supplement.

Finally, we have shifted the western edge of all maps to 20 degrees East, so that the South Atlantic Ocean, including the coastal upwelling features along southwest Africa, is shown as contiguous throughout the paper.

All of these changes are visible in the updated figures included with the revision.

Minor comments

1. Lines 50–52: "Air-sea O_2 fluxes are driven by surface ocean DO concentration changes resulting from physical and biogeochemical processes." I suggest leaving out "surface ocean DO concentration changes resulting from" because, at least in the case of thermally induced fluxes, it is the fluxes that cause the changes in concentration, not the other way around.

We agree with this suggestion and similar suggestions in the third minor comment below. We have deleted the second and third sentences in the first paragraph. We combine the first sentence with the second paragraph.

L48-57: Accurate estimates of air-sea O_2 fluxes have the potential to place critical constraints on ocean productivity, CO_2 uptake, and heat flux (e.g., Keeling et al., 1993, 2010; Keeling and Manning, 2014), but large biases and uncertainties exist in current estimates (Jin et al., 2023). Air-sea O_2 fluxes are driven by physical and biogeochemical processes, and they exhibit pronounced seasonal variability associated with seasonal changes in solubility, biological activity, and ocean mixing (Redfield, 1948).

2. Line 51. Define the first appearance of "DO" in the main text. Better yet, do not use the acronym because O_2 works fine.

We have added dissolved oxygen (DO) to define it.

3. Lines 52–54: "Because oxygen has low solubility in seawater, changes in surface ocean DO create large disequilibria with the atmosphere, producing strong gas exchange as the ocean atmosphere system seeks equilibrium (Garcia and Gordon, 1992)." There are two problems with this sentence. First, I would not say that there are large disequilibria in surface DO. To first order, open-ocean surface DO is very close to the saturation concentration (typically within a few percent) unlike, say, CO2 . Second, the degree of disequilibrium for a dissolved gas in surface waters is related to two ratios: (1) the oxygen change from a process divided by the saturation concentration (which is equal to the solubility times the atmospheric partial pressure) and (2) the ratio of the time scale of the process to the gas exchange equilibration time scale (mixed layer depth divided by the transfer velocity). My inclination would be to delete the sentence.

We agree with the suggestion. For details, see the response to the first comment.

4. Lines 57. "takes" should be "take"

Corrected.

5. Lines 63-64. “seasonal air-sea heat fluxes directly affect O_2 solubility” isn’t quite right. Temperature is the direct effect, not heat flux. I would say “seasonal temperature changes.”

Revised as suggested.

6. Lines 67–71. This sentence is about the present-day, globally integrated air-sea oxygen flux, saying it should be positive as a result of the impacts of global warming. There is no mention of net metabolism of the ocean here. Najjar and Keeling (2000) considered the oxidation of riverine organic carbon and argued that it would lead to a small ingassing. But there is also outgassing associated with autochthonous organic matter that is buried or oxidized anaerobically. My guess is that these metabolism effects are still small, but they should at least be mentioned here and maybe updated with more recent estimates of the ocean’s organic carbon budget (e.g., Regnier et al., 2022, https://doi.org/10.1038/s41586-021-04339-9) and maybe denitrification and sulfate reduction. Same sentence. I read the sentence as saying that the ocean is a source of oxygen to the atmosphere in part because of a reduction in the upwelling of nutrients. This doesn’t make sense to me, because fewer nutrients would reduce primary production and hence outgassing.

We appreciate these detailed thoughts. We have revised the paragraph to read as follows:

L69-78: This net outgassing is driven by ocean heat uptake, which reduces oxygen solubility, and by warming-induced stratification, which reduces the downward transport of oxygen-rich surface waters into the ocean interior (Breitburg et al., 2018; Keeling et al., 2010; Keeling and Garcia, 2002; Schmidtko et al., 2017). Net ocean metabolism contributes an additional term, with oxidation of riverine organic carbon driving a small ingassing, and burial or anaerobic oxidation of marine organic matter driving outgassing (Najjar and Keeling, 2000; Regnier et al., 2022). These metabolic terms are expected to be small relative to the solubility and ventilation terms.

7. Line 102. Please spell out the GOBAI acronym on its first appearance.

We have defined GOBAI-O_2 as “Gridded Ocean Biogeochemistry from Artificial Intelligence - Oxygen”.

8. Line 132. The horizontal resolution of GOBAI-O2 is given, but the vertical resolution should be provided as well because data throughout the mixed layer is used.

We have added the corresponding information.

L139-142: The dataset covers about 86% of the global ocean (by surface area) from -179.5° to 179.5° longitude and -64.5° to 79.5° latitude on a 1°×1° grid, and extends from 2.5 m to 2000-m depth on 58 pressure levels. The vertical resolution is 10 m over the upper 150 m, coarsening with depth to 100 m below 1500 m.

9. Lines 166–167. Along the same lines, why is the mixed-layer average used instead of the surface value? I suggest some discussion of this point and a comparison of the two values and implications for the computed flux. I also wonder if the results would change if a different mixed-layer depth criteria were used because the MLD is sensitive to such criteria, as shown by De Boyer Montegut et al. (2004).

We use the mixed layer average because our fluxes are monthly. The mixed layer overturns on timescales short compared to a month, so the O_2 exchanged over a month is drawn from the entire mixed layer reservoir rather than from a fixed shallow level, and monthly mean DO at a single level additionally inherits transient stratification and reconstruction noise. We have added this rationale to Section 2.2 and a new comparison to the Discussion, which also quantifies the sensitivity to the MLD criterion suggested by the reviewer.

L180-183: We average DO over the MLD because the monthly integrated flux exchanges O_2 with the entire mixed layer reservoir, which overturns on timescales short compared to a month. This averaging also reduces the influence of transient near-surface stratification and reconstruction noise in the DO fields (Section 4.2).

L857-869: We compute fluxes from MLD-averaged DO because the flux integrated over a month exchanges with the entire mixed layer, which overturns many times within a month, whereas monthly mean DO at a fixed shallow surface layer inherits transient near-surface stratification and reconstruction noise that do not represent the reservoir engaged in gas exchange. In a single year test (2015) in which fluxes are instead computed from DO at the shallowest GOBAI-O_2 level (2.5 m), the hemispheric flux SCA decreases by roughly 20% and the strong wintertime O_2 uptake at high latitudes weakens substantially, for example, from 63 to 31 mol m^{-2} yr^{-1} in the North Atlantic subpolar region in January. These differences lead to a worse agreement with airborne observations shown in Section 3.2. The constrained global annual mean flux is insensitive to this choice. We also tested the sensitivity to the MLD criterion by

recomputing fluxes with the alternative temperature criterion of De Boyer Montégut et al. (2004), which increases the hemispheric SCA by 4% (NH) and 7% (SH) and changes the regional SCA by a median magnitude of 6%, indicating that our results are robust to the MLD criterion choice.

10. Line 170. Define EN4.

We note that EN4 has no official expansion. It is version 4 of the Met Office Hadley Centre EN series of quality-controlled subsurface ocean temperature and salinity profiles and objective analysis, and the EN4 reference paper (Good et al., 2013) never expands the acronym. We have therefore retained the acronym, which is widely used in the community, but we have added the reference here, which was previously missing.

11. Lines 201–203. The authors separate the oxygen flux into a diffusive component and a bubble component, but both are actually diffusive. I suggest using "surface" or "surface skin" instead of "diffusive."

We now use "surface skin" throughout the paper.

12. Lines 209–210. I am puzzled by the wording in these lines. Shouldn't it simply say "The anomaly is calculated as the deviation of DO from saturation, plus an annual mean adjustment term (d), following: …" I am wondering, what is [O2]adj ? Something seems off.

This sentence has been revised, with additional information about the skin temperature and sea level pressure corrections. For details, see the response to the first major comment. $[O_2]$adj was used to refer to the DO after the annual mean correction, which is what is used for the adjusted flux calculation and our main result. Now we use $\Delta[O_2]$ instead. Sorry for the confusion.

We added this sentence to make it clear:

L236-237: Throughout this study, the raw flux refers to the flux calculated without the adjustment δ, while the adjusted flux uses the optimized δ described in Section 2.4 and Appendix B.

13. Lines 230–232. Sentence is awkwardly worded. Maybe "… to match those randomly selected from one of the five …" is clearer. Also, the authors might consider smoothing the associated correction, because you can see abrupt changes at the boundaries of the five regions in, for example, Figure 1.

We have revised the sentence accordingly. We have also applied a smoothing function near the boundary of each defined region to avoid abrupt changes. For details, please refer to the #6 point in our response to major comment 1 above.

14. Line 247. Define RECCAP2.

We added the corresponding definition as REgional Carbon Cycle Assessment and Processes (RECCAP2).

15. Lines 263–267. It is surprising to me that TM3 is still being used for simulations of atmospheric oxygen. The model is state of the art for the 1990s. Can the authors defend the choice of this model given the expectation that atmospheric transport modeling has improved substantially over the last few decades?

We agree that TM3 is an older-generation transport model, but our evaluation strategy was specifically designed to be insensitive to the choice of transport model. Our primary evaluation compares simulations against tropospheric-average (1000 to 400 hPa) APO from airborne surveys rather than against surface station records. The APO model intercomparison of Jin et al. GMD, 2025, which included TM3 alongside newer-generation transport models, showed that simulated tropospheric-average airborne APO seasonal cycles are highly consistent across transport models, while simulations sampled at surface stations diverge substantially, because column averaging removes most of the sensitivity to how each model represents vertical mixing. Those newer models do not show clear improvements compared to the older model TM3. This reasoning was discussed in Section 3.2.1, where we also attribute the residual station-level biases at the coastal sites LJO and CBA (Fig. S11) to known TM3 deficiencies reported in Jin et al., GMD, 2025. In addition, in the station-based trend evaluation (Fig. 7), the background corrections are derived from flux estimates transported by four different transport models (Jin et al., GMD, 2025), so transport model spread is propagated into the observational uncertainty of that comparison.

16. Line 290. Use hPa instead of mbar.

Revised as suggested.

17. Line 315. I think this should be S2, not S1.

Revised to S3, based on the new figure order.

18. Line 318, Table 1, and elsewhere. Line 318 is the first mention of seasonal cycle amplitude (SCA), which is an important metric analyzed throughout the paper. There are two points I would like to make. First, amplitude is a well-defined term for sinusoids and is equal to half the range, not the range itself, which is used here (maximum minus the minimum). Further, here we have two sinusoids, so a single amplitude seems inappropriate for describing the magnitude of the seasonality. I like the metric that the authors use, but suggest calling it seasonal cycle range, so SCR. The second point regards the presentation of SCA in Tmol/yr (column 3 of Table 1), which I was initially puzzled about because it is similar to the term Seasonal Net Outgassing (SNO), which was first used by Keeling and Shertz (1992). My suggestions here are to (1) clearly define in the Table 1 caption that column 3 is simply column 2 multiplied by the region area and (2) note in the text the relationship between the quantity in column 3 and SNO. My sense is that SCA in Tmol/yr should be about twice SNO.

We prefer to retain the term SCA, which in the atmospheric trace gas literature conventionally denotes the peak-to-trough difference of the fitted seasonal cycle. This usage is well established in studies that this work directly builds on and compares against, including the airborne APO analyses of Jin et al., GBC, 2023 and Jin et al., GMD, 2025, whose SCA definition and our Table 2 ratios match exactly. Renaming the metric would break terminological continuity with these studies.

To remove any ambiguity, we now define SCA explicitly at first use as the difference between the seasonal maximum and minimum of the fitted cycle. Regarding the second suggestion, we decided not to introduce SNO in the paper, since it is not used elsewhere in our analysis and reporting two closely related seasonal metrics could cause confusion. For the reviewer's information, computing SNO from our fitted hemispheric seasonal cycles following Keeling and Shertz (1992), by removing the annual mean and integrating the flux over the months when it is upward, gives 363 Tmol in the Northern Hemisphere and 584 Tmol in the Southern Hemisphere. We added text comparing these values with prior studies. We also note that the SCA column is the peak-to-trough range of the flux rather than a time-integrated quantity, so for a sinusoidal cycle the expected relationship is SNO equal to the area-integrated SCA divided by 2π rather than by 2. Our values give ratios of 6.3 and 6.4, close to 2π.

L369-371: Fig. 3 shows flux seasonal cycle amplitude (SCA), defined as the difference between the maximum and minimum of the fitted seasonal cycle, and annual mean values, presented as both gridded fields and zonal averages with associated uncertainties.

L389-394: The hemispheric seasonal net outgassing (SNO), defined as the area under the positive portion of the two-harmonic fitted seasonal cycle after removing the annual mean flux, is 363±36 Tmol in the Northern Hemisphere and 584±55 Tmol in the Southern Hemisphere. These values are only slightly larger than the earlier estimates of 322 and 534 Tmol from Najjar and Keeling (2000) and 312 and 549 Tmol from Garcia and Keeling (2001), both of which were based on the hydrographic observations archived by 1994 and 1998, respectively.

L619-621: SCA and annual mean fluxes in Tmol yr^{-1} are the corresponding per-area values in mol m^{-2} yr^{-1} multiplied by the area of each region. Region acronyms are defined in Fig. 2.

19. Line 323. Section 3.1.1. Flux seasonal variation. Some brief comparisons to Najjar and Keeling (2000) would be helpful. For example, the SCA in Najjar and Keeling (2000) for the Northern and Southern Hemispheres is about 15 and 18 mol/m2 /yr, respectively, quite similar to what is found in the current study (17.7 and 17.9 mol/m2 /yr, Table 1).

We agree that a comparison to Najjar and Keeling (2000) is helpful and have added it to Section 3.1.1. Note that the hemispheric SCA values in the revised product are 18.1 and 19.9 mol m^{-2} yr^{-1} for the Northern and Southern Hemispheres, so our amplitudes are now larger than those of Najjar and Keeling (2000) in both hemispheres, although the two studies consistently find a larger per-area SCA in the Southern Hemisphere. We have revised the section.

L380-389: The Northern Hemisphere exhibits prolonged summer outgassing from mid-April to early November, with a peak in June followed by a broad outgassing plateau through September. In contrast, the Southern Hemisphere shows only a slightly longer outgassing period than its uptake period, without a clear peak and plateau structure. The SCA in the Southern Hemisphere of 19.9±1.86 mol m^{-2} yr^{-1} is larger than that in the Northern Hemisphere of 18.1±1.69 mol m^{-2} yr^{-1} (Table 1). Our results suggest a similar seasonal flux structure but larger flux SCA in both hemispheres compared to the earlier study of Najjar and Keeling (2000), which reported approximately 15 and 18 mol m^{-2} yr^{-1} for the Northern and Southern Hemispheres, respectively.

20. Line 329. Two distinct peaks in the Northern Hemisphere are noted. There is a nice description later about the cause being related to the subsurface oxygen maximum in the Northern Hemisphere (lines 352–356) and lack thereof in SH (lines 368–370), but I also think that the summer monsoon in the Indian Ocean must play a role. Also, it might be worth noting that the double peak in the mean annual cycle of the NH (and lack thereof in the SH) is present in the analysis of Najjar and Keeling (2000).

We have revised the text to mention the impact from monsoon, and acknowledge the similarity compared to Najjar and Keeling (2000).

L386-389: Our results suggest a similar seasonal flux structure but larger flux SCA in both hemispheres compared to the earlier study of Najjar and Keeling (2000), which reported approximately 15 and 18 mol m^{-2} yr^{-1} for the Northern and Southern Hemispheres, respectively.

L420-423: At the hemispheric scale, the reduced outgassing after the June peak also partly reflects strong monsoon-driven O_2 uptake over the Arabian Sea and Bay of Bengal (Figs. 2L and M). Excluding these two from the Northern Hemisphere average removes most of the reduction in summer outgassing.

21. Line 336. Referring to large SCA at mid-to-high latitudes. Should be referencing Fig 3A and C instead of Fig 2A–C?

Fixed.

22. Line 348. There is a claim that the timing of the peak shifts earlier as you go south. I don't see that for NA-STPS, which has a peak in September.

We have revised the sentence to only refer to the onset of net outgassing, rather than both the onset and peak time.

L411-413: The onset of net O_2 outgassing shifts earlier from high to low latitudes, likely due to earlier bloom onset and thermal changes at lower latitudes.

23. Lines 375 to 377. I think it's odd to note that maximum uptake in the Arabian Sea is consistent with the SH flux cycle. The processes driving maximum uptake in the two regions are very different, with the former being due to monsoon-driven upwelling and the latter due to mixedlayer deepening and cooling. The double peak was noted in oxygen supersaturation by Najjar and Keeling (1997) and air–sea oxygen flux by Najjar and Keeling (2000).

We have revised the sentence to acknowledge the connection between the northern Indian Ocean flux pattern and monsoon dynamics.

L443-446: In the Arabian Sea (Fig. 2L), the seasonal flux pattern is dominated by strong O_2 uptake associated with monsoon-driven upwelling in summer, while the Bay of Bengal (Fig. 2M) remains a net O_2 source throughout the year with reduced outgassing during the monsoon season.

24. Lines 692–697. This is the paragraph about missing coastal data. I think it's worth highlighting estuaries here. Most estuaries are net heterotrophic, which means that they are more likely to be sinks rather than sources of atmospheric oxygen. There are a few syntheses of estuarine net heterotrophy that could be cited, including Herrman et al. (2015) https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2013GB004736.

We agree and have added a statement on estuarine net heterotrophy to this paragraph, citing the synthesis of Herrmann et al. (2015). This addition also reinforces our point that the sign of the missing coastal flux is uncertain, and it is consistent with the discussion of net metabolism added in response to an earlier comment.

L790-795: In contrast, most estuaries are net heterotrophic, oxidizing organic matter delivered from land in excess of their local production, and are therefore more likely to act as O_2 sinks (Herrmann et al., 2015). Without O_2 flux estimates over these regions, we potentially miss summer-time O_2 outgassing events associated with high biological productivity and net O_2 uptake by heterotrophic estuarine and other coastal systems.

25. Line 698. Use of CESM2 to explore data gaps is a nice addition to the discussion, but please give the resolution of the ocean model. Also note if the model includes sources of nutrients and organic carbon from rivers—this is important for capturing the metabolic poise of nearshore waters. If daily output are available for this model, then one could assess the covariation between the oxygen anomaly and the transfer velocity, which was noted in the major comments. Line 918. The number 2.27 should have units attached to it.

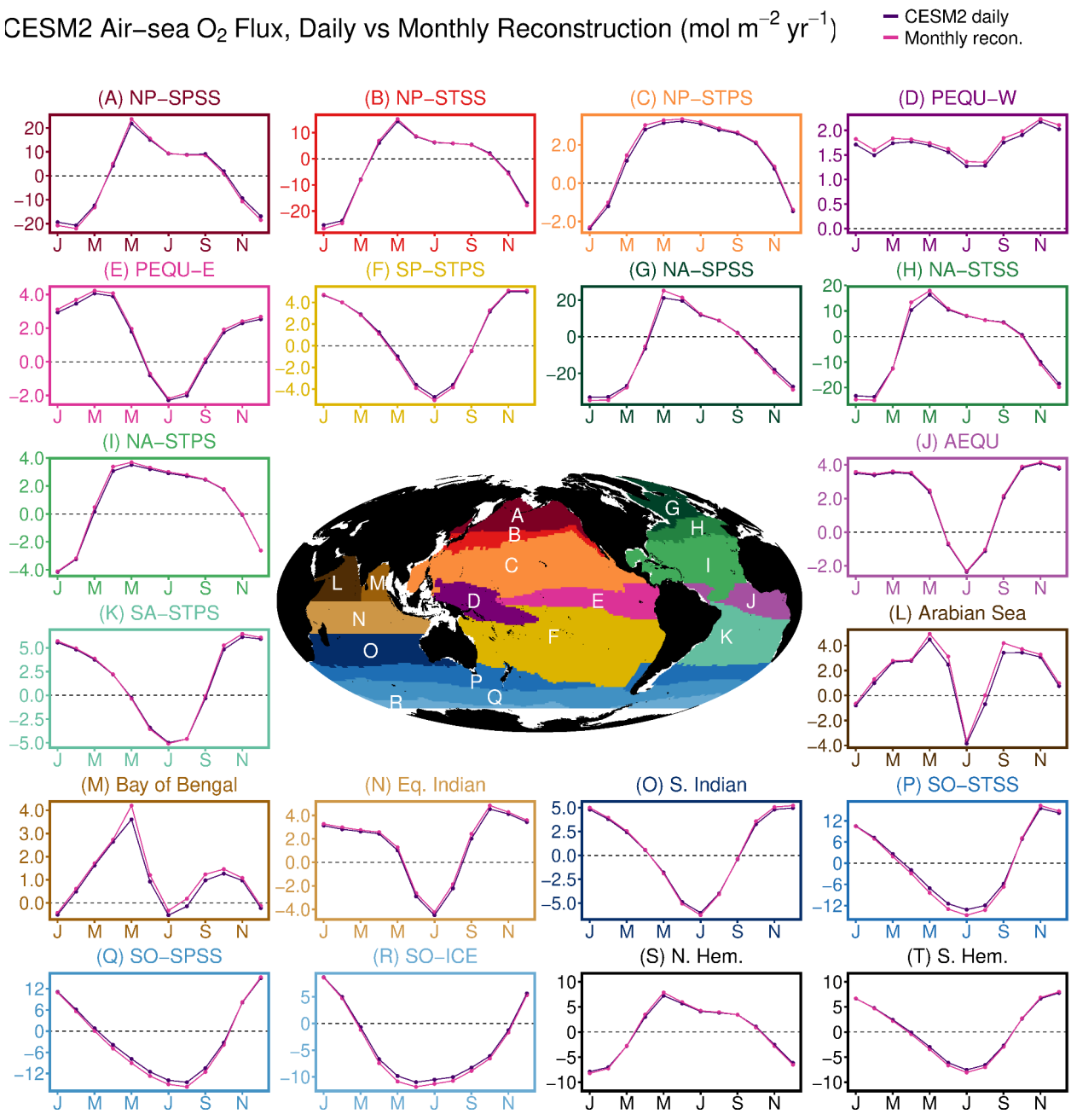
We have added the ocean model information to Section 4.1. The CESM2 FOSI simulation uses the POP2 ocean model at nominal 1 degree horizontal resolution with 60 vertical levels (Yeager et al., 2022), and its ocean biogeochemistry component includes riverine inputs of nutrients, dissolved inorganic carbon, alkalinity, and dissolved organic matter (Danabasoglu et al., 2020), although nearshore processes remain coarsely resolved at this resolution. Regarding the

609 covariance suggestion, daily output is available for this simulation and we have carried out the
610 suggested assessment using daily fields of air-sea O_2 flux, gas transfer velocity, oxygen anomaly,
611 and ice fraction. The results are now reported in Section 4.1 and two new SI figures.
612 Reconstructing the flux from monthly mean fields omits a covariance term equivalent to about
613 34 Tmol yr^{-1} of global outgassing, overestimates the seasonal cycle amplitude by up to 9% in
614 subpolar regions and 5 to 6% at the hemispheric scale with unchanged phase, and slightly
615 increases the annual mean meridional gradient. In our product the annual mean effect is absorbed
616 by the observational constraint on the global annual mean flux, and the seasonal effects are
617 smaller than the reported ensemble spread. Since this covariance effect is not sampled by our
618 ensemble, which spans DO, wind product, and gas exchange scheme choices, we apply no
619 model-based correction and instead document it as a known limitation of monthly flux products.
620 Units of m s^{-1} have been added to the value 2.27.

621 **L796-802:** Here we explore the contribution of these data gaps to area-integrated flux seasonal
622 cycles in the Northern Hemisphere and Southern Hemisphere using a Community Earth System
623 Model version 2 (CESM2) Forced Ocean-Sea-Ice (FOSI) simulation (Yeager et al., 2022), which
624 uses the POP2 ocean model at nominal 1 degree horizontal resolution with 60 vertical levels and
625 includes riverine inputs of nutrients, dissolved inorganic carbon, alkalinity, and dissolved organic
626 matter (Danabasoglu et al., 2020), although nearshore processes remain coarsely resolved at this
627 resolution.

628 **L817-837:** The same simulation allows us to assess the effect of computing fluxes from monthly
629 mean DO fields, which neglects the sub-monthly covariance between the gas transfer velocity
630 and the DO anomaly. We note that the averaging of wind speeds to monthly is treated separately
631 in our flux calculation, which uses the monthly mean of the squared wind speed, computed from
632 high-resolution winds as the squared monthly mean plus the intra-month variance (Section 2.3).
633 Here we reconstruct the CESM2 flux from monthly mean transfer velocity, DO anomaly, and ice
634 fraction and compare it to the flux accumulated from daily model output over 2004 to 2020
635 (Figs. S14-15). Because gas exchange in CESM2 is quadratic in wind speed, the monthly mean
636 of the daily transfer velocity is identical to one computed from the monthly mean of the squared
637 wind speed, so the reconstruction contains no wind speed averaging bias and its difference from
638 the daily accumulated flux arises only from the covariance term. The monthly reconstruction
639 omits a positive covariance term equivalent to 34±4 Tmol yr^{-1} of additional global outgassing. It
640 also overestimates the flux SCA by up to 9% in subpolar regions and 10% in the Bay of Bengal,
641 and by 6% (NH) and 4.5% (SH) at the hemispheric scale, with peak months unchanged (Fig.

S14). The monthly flux slightly overestimates the meridional gradient of annual mean flux by deepening the 45°S to 65°S sink and overestimates the equatorial outgassing peak by about 7% (Fig. S15). In our product, the global annual mean is constrained to observation-based values, so the annual mean covariance effect is effectively absorbed by the annual adjustment, while the seasonal effects implied by CESM2 are smaller than our reported ensemble spread (Table 1). We do not apply model-based correction and instead document this effect as a known limitation of monthly flux products.



649

650 Fig. S14: Monthly climatology (2004-2020) of air-sea O_2 flux in the CESM2 FOSI simulation
651 for 18 RECCAP2 regions plus Northern and Southern Hemisphere averages, within the domain
652 of our flux product. The flux accumulated from daily model output (dark purple) is compared
653 with the flux reconstructed from monthly mean transfer velocity, DO anomaly, and ice fraction
654 (pink). The difference between the two curves isolates the effect of neglecting sub-monthly
655 covariance between the gas transfer velocity and the DO anomaly (Section 4.1). The center map
656 shows the region boundaries as in Fig. 2. Positive values indicate outgassing.

657

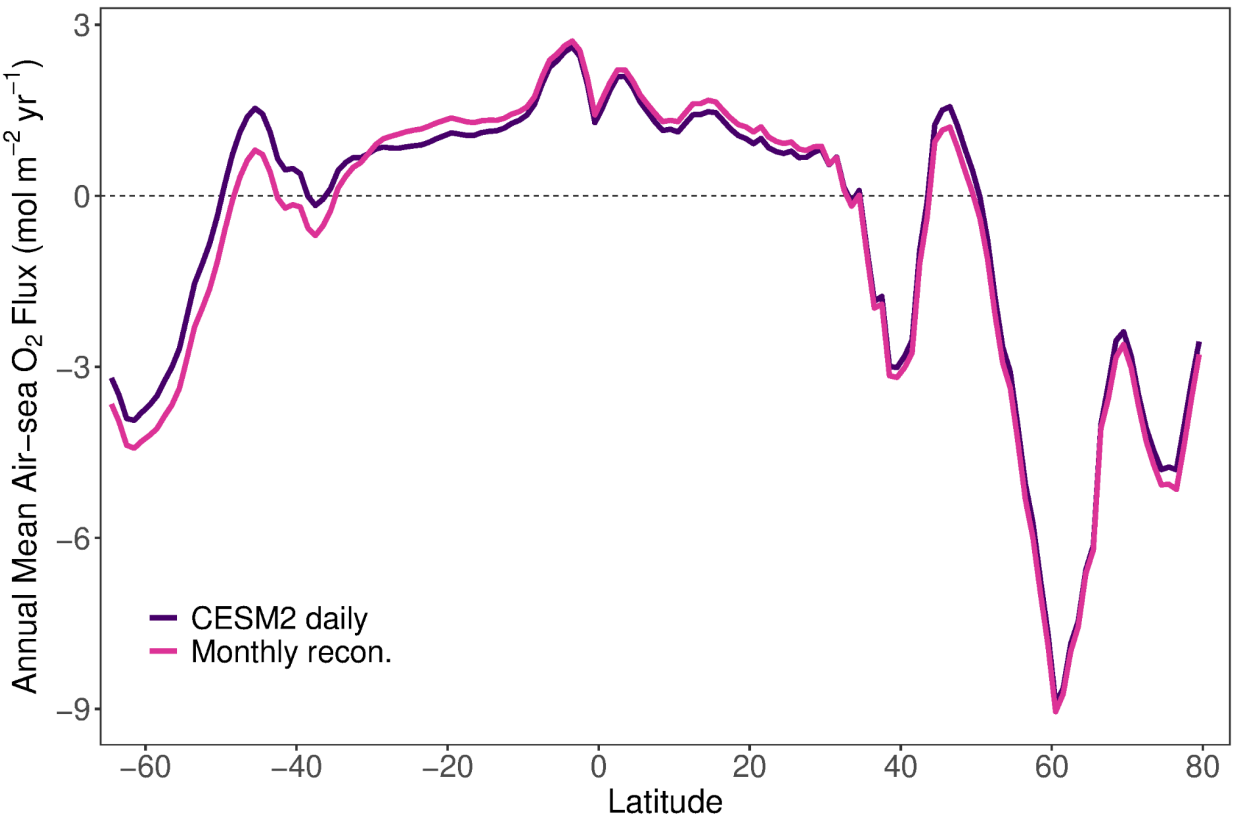


Fig. S15: Zonal annual mean air-sea O_2 flux (2004-2020) in the CESM2 FOSI simulation, computed from daily model output (dark purple) and from the monthly mean reconstruction (pink), following the same procedure as Fig. S14. Neglecting sub-monthly covariance overestimates the ingassing between 45°S and 65°S and overestimates the outgassing near the Equator, increasing the meridional gradient of the annual mean flux.

26. Fig. B3. This is not cited in the appendix, only in the main text. I suggest either citing it in the appendix or moving it into the supplementary section.

We have added the following sentence in Appendix B to cite Fig. B3.

L1171-1172: The adjustments have only minimal impacts on the flux seasonal cycle (Fig. B3).

27. Fig. C1. I think there is overlap in some of the colored curves. I should see 8 colored curves (2 products for each of the 4 types of fluxes) on each panel, but it looks like there are fewer, presumably because of similarity within a flux type. I think the caption just needs to state this. Also, define ensemble in the first line—average of four transport models?

The two products for the air-sea N_2 flux essentially overlap, and the two products for the fossil fuel O_2 and CO_2 fluxes largely overlap, because these flux fields are consistent between products. We have revised the C1 caption to state this. We have also clarified in the caption that the ensemble mean refers to the mean across our air-sea O_2 flux ensemble rather than an average of transport models.

L1324-1337: Fig. C1: Monthly climatology (2004–2024) of hemispheric mean air-sea O_2 flux from this study (black), averaged across the flux ensemble (Section 2.5), compared to non-oceanic flux correction components (2004–2020 average) used to correct APO observations. Each flux type for correction is shown for the two flux products described in Appendix C, with both products of a given type sharing the same color, so eight colored curves are plotted in each panel. We note that the two air-sea N_2 flux products essentially overlap, and the two fossil fuel O_2 and the two fossil fuel CO_2 flux products largely overlap, reflecting the consistency of these flux fields between products. Each flux component (Tmol day^{-1}) creates an atmospheric tracer concentration in ppm units, which can be further converted to per meg units through scaling (Eq. C1). Here, we scale each flux component to directly show its contribution to observed APO in the per meg-equivalent unit. Note that air-sea N_2 flux is sign-reversed according to Eq. C1. The comparison demonstrates that the non-oceanic O_2 flux components have much smaller seasonal variations compared to the dominant air-sea O_2 flux that drives the observed APO seasonal cycle.

28. Line 1375. It's R. G. Najjar. There is a typo on the paper.

Corrected.

29. Lines 1393–1394. Seems to be missing some information, like maybe a URL or a publisher.

This is a technical report. We have revised.

Keeling, R. F., Walker, S. J., and Paplawsky, W.: Span Sensitivity of the Scripps Interferometric Oxygen Analyzer. SIO Reference Series. https://escholarship.org/uc/item/7tt993fj, Scripps Institution of Oceanography, UC San Diego, 2020.

Community comment from Andrew Kowalski

As in many previous studies of atmospheric oxygen, this manuscript overlooks the fundamental and controlling influence of water vapor on oxygen availability (Kowalski et al., 2025). The repeated assumption of a constant atmospheric oxygen mole fraction of 0.2094 is inappropriate for many terrestrial surface environments and introduces a systematic bias into the air–sea flux calculations in equations (2) through (4). By neglecting humidity, the authors implicitly assume that the amount of oxygen in air is invariant, whereas it is directly constrained by the presence of water vapor.

The effect is readily illustrated by contrasting typical polar and tropical surface air compositions. In very dry polar air, the water vapor mole fraction may be as low as ~0.0004, leaving dry air to comprise ~99.96% of the total. Under such conditions, representative mole fractions for nitrogen, oxygen, and argon are approximately 0.7808, 0.2095, and 0.0093, respectively. In contrast, tropical surface air commonly exhibits water vapor mole fractions exceeding 0.03, reducing dry air to ≤97% of the total. The corresponding mole fractions of nitrogen, oxygen, and argon are then reduced to less than 0.7577, 0.2033, and 0.0090, respectively—representing a decrease of more than 6000 ppm for oxygen. Assuming a constant oxygen mole fraction therefore leads to a substantial overestimation of the oxygen available for dissolution in tropical surface waters.

This issue is more clearly framed in terms of oxygen's true thermodynamic control: according to Henry's law, oxygen solubility depends on its partial pressure. By Dalton's law, total atmospheric pressure is the sum of the partial pressures of all constituents. Elevated water vapor partial pressure in tropical air does not generally coincide with elevated surface pressure; in fact, mean sea-level pressure near the equator is typically below the global average of 1013.25 mb. Consequently, increased humidity necessarily depresses the partial pressure of dry air and, proportionally, that of oxygen. Ignoring this effect systematically skews estimates of oxygen exchange at the air–sea interface.

The manuscript overlooks this humidity dependence in several key locations, including (i) the second paragraph of the Introduction, (ii) Section 2.3 as a whole, and (iii) Section 2.7, which notes that water vapor is indeed measured but its effect on oxygen availability is not taken into account. Given that humidity is already observed, the omission is both unnecessary and consequential.

The authors should therefore recompute oxygen mole fractions and partial pressures in a manner consistent with atmospheric humidity. Doing so is essential for physically correct estimates of air–sea oxygen fluxes.

Reference

Kowalski, A. S., Janssens, I. A., and Pérez-Priego, O., 2025, Water vapour dynamics as a key determinant of atmospheric composition and transport mechanisms, Biogeosciences, 22, 8005–8012, https://doi.org/10.5194/bg-22-8005-2025

During discussions in another forum, it has been argued that the oxygen community correctly applies solubility equations via an assumption that air directly at the water surface is essentially at 100% relative humidity, such that only the water temperature and not the atmospheric humidity is required to apply Henry's law. I would like to preclude such an argument here because it reflects a fundamental misunderstanding of air–sea gas exchange processes.

By definition, 100% relative humidity occurs when the partial pressure of water vapor (e) equals the saturation vapor pressure (e_s; Petty, 2008). Under such conditions, the system is in thermodynamic equilibrium with respect to phase changes of water, meaning that evaporation and condensation occur at equal rates, resulting in no net flux of water vapor. Such an equilibrium state is rarely realized at the sea surface and is typically limited to conditions such as fog. More commonly, net evaporation prevails, with evaporation exceeding condensation in proportion to the difference between e and e_s (Brutsaert, 1982).

Because it is e rather than e_s that enters Dalton's law and constrains the partial pressure of dry air—and thereby that of oxygen—it follows that ambient humidity necessarily influences oxygen dissolution. The assumption that it does not is untenable.

References

Brutsaert, W., Evaporation into the Atmosphere: Theory, History, and Applications, Springer Science+Business Media, Dordrecht, the Netherlands, ISBN 978-90-481-8365-4, 1982

Petty, G. W.: A first course in atmospheric thermodynamics, Sundog Publishing, Madison, USA, ISBN-10 0-9729033-2-1, 2008

We thank Dr. Andrew Kowalski for engaging with our manuscript. We disagree with the central claim. The influence of water vapor on the oxygen partial pressure that drives air-sea exchange is correctly included in our calculation, which uses the vapor pressure at the air-water interface rather than the humidity of the ambient air. We explain point by point.

1. According to Henry's law, the partial pressure that controls dissolution is set at the air-water interface, where gaseous O_2 in the molecular sublayer is saturated at the water temperature and equilibrates with dissolved O_2. The water does not exchange gas directly with the bulk atmosphere, so no correction for ambient humidity is necessary. This is the standard formulation of gas exchange and solubility used throughout oceanography (Benson and Krause, 1984; Garcia and Gordon, 1992; Weiss, 1970). The partial pressure of O_2 in the air in contact with the water

surface follows $P_{O_2} = X_{O_2} \cdot (P - e_s(T_s))$, where $e_s(T_s)$ is the saturation vapor pressure at the water temperature.

2. A saturated air-sea interface could coexist with net evaporation. The supplementary comment argues that a saturated surface layer would imply zero net evaporation. This confuses local equilibrium at the interface with equilibrium of the whole air column. In the standard theory of evaporation, presented in the reference the comment cites (Brutsaert, 1982), the air in molecular contact with the water is saturated at the surface temperature, and evaporation is the diffusive and turbulent transport of vapor from that saturated interface toward the drier air above, at a rate proportional to $e_s(T_s) - e$, where e is the actual water vapor partial pressure in the bulk air above.

3. There is no missing vapor correction in the method section. The vapor correction the comment requests is already applied, explicitly, in the definition of the solubility we use. Our $[O_2sat]$ from Garcia and Gordon (1992) is a fit to the standard atmospheric concentrations of Benson and Krause (1984), which are constructed from vapor free Henry's Law coefficient measurements by applying an explicit water vapor term, with the O_2 partial pressure defined as 0.20946 (1 atm - P_{wv}) and P_{wv} the saturation vapor pressure over seawater (their Eqs. 20 and 22). In the revised manuscript, prepared in response to Referee 2, we additionally correct for departures of sea level pressure from one atmosphere and for the cool skin temperature offset, evaluating $[O_2sat]$ scaled by $(P_a - p_{H2O}) / (P_0 - p_{H2O})$ with $[O_2sat]$ as a function of surface ocean temperature (with a cool skin temperature correction) and salinity. p_{H2O} is the equilibrium vapor pressure at the interface temperature and P_a the monthly ERA5 sea level pressure. Ambient H_2O concentration has no relevance for these calculations.

We have added the corresponding text:

L238-256: $[O_2sat]$ is the O_2 saturation concentration at the air-sea interface, and it therefore requires a skin temperature correction and a sea level pressure correction. The interface is cooler than the water immediately below it, and O_2 solubility increases as temperature decreases, so evaluating $[O_2sat]$ at the bulk temperature underestimates it. The equilibrium concentration is also set by the partial pressure of O_2 in the overlying air, which varies with the weight of the atmospheric column above the sea surface, so referencing $[O_2sat]$ to a fixed one atmosphere misrepresents it wherever sea level pressure departs from 101,325 Pa (Najjar and Keeling, 2000). We therefore calculate

$$[O_2^{sat}] = C_{sat}(T_s + \Delta T_{skin}, S_s) \times \frac{P_a - P_{H_2O}}{P_0 - P_{H_2O}}, \quad (6)$$

where C_{sat} is the O_2 solubility of Garcia and Gordon (1992) at a total pressure of one atmosphere, T_s and S_s are the temperature and salinity of the shallowest GOBAI-O_2 level (2.5 m), P_a is the monthly mean sea level pressure from ERA5 (Hersbach et al., 2020), P_0 is the reference sea level pressure of 101,325 Pa, and P_{H2O} is the equilibrium water vapor pressure at T_s calculated following Bolton (1980). This vapor pressure describes the uppermost 1 mm thin layer of air in molecular contact with the sea surface, which is saturated at the water temperature and sets the O_2 partial pressure governing dissolution, rather than the ambient humidity of the overlying air (Weiss, 1970). ΔT_{skin} is the cool skin temperature offset, calculated from the 10-m wind speed (U_{10}), following Donlon et al. (2002)

$$\Delta T_{skin} = -0.14 - 0.30\, e^{-U_{10}/3.7} \quad (7)$$

4. The polar versus tropical contrast of X_{O_2} in the comment computes the wrong quantity. The quoted dilution of more than 6000 ppm is the change in the moist-air mole fraction of O_2 in the ambient atmosphere. That number is real but it is not the quantity that controls dissolution (see points 1 and 3). Over warm tropical water the interface vapor pressure is about 4% of the total pressure, and this suppression is fully included in our saturation concentrations. The difference between the comment's proposal and our treatment is not whether the vapor effect is included, but whether the vapor term is evaluated at the interface (the correct choice) or in the ambient air.

5. The one pathway where ambient humidity does enter the flux, the bubble injection terms, is negligible for three independent reasons. For completeness we note that the air entrained by breaking waves in the bubble mediated flux terms is ambient near-surface air rather than the saturated interfacial layer, so the oxygen content of a given volume of entrained air does depend on ambient humidity. Below we list the three considerations that lead us not to correct for this.

First, the pathway is geographically self-limiting. Bubble injection requires strong winds and is concentrated at high latitudes, where the cold marine air holds a water vapor mole fraction below about one percent even near saturation, so ambient humidity changes the oxygen content of the injected air by less than one percent. Over warm tropical waters, where the vapor fraction reaches three to four percent, wind speeds are low and the bubble flux itself is negligible.

Second, the magnitudes of the injection terms are calibrated empirical coefficients rather than first principles quantities. The S09 coefficient was determined by fitting an upper ocean model to a multiyear time series of five dissolved noble gases at BATS, so it reproduces the gas quantities that were naturally entrained in bubbles, with whatever humidity they contained, and it carries a quoted one sigma uncertainty of 29 percent on the total air injection flux (Stanley et al., 2009). The L13 term derives from a bubble resolving simulation whose entrained air volumes are constrained by observed void fractions and bubble penetration depths (Liang et al., 2013). A

humidity distinction of at most a few percent in the composition of the injected air is therefore well within the uncertainty of the coefficients themselves, an uncertainty our ensemble already propagates through the spread among gas exchange schemes.

Third, although we disagree that the moist air oxygen mole fraction correction is needed, we nevertheless recompute the fluxes with X_{O_2} (1 - x_{H2O}) in the bubble injection terms of the L13 and S09 schemes, using surface humidity from MERRA-2, to determine the size of the effect directly. In the regions where bubble mediated uptake is strongest, the subpolar North Atlantic, the subpolar Southern Ocean, and the seasonally ice covered zone, with annual mean uptake of 10 to 17 mol m^{-2} yr^{-1}, the change in the annual mean flux is 0.006 to 0.023 mol m^{-2} yr^{-1}, no more than 0.25%.

6. A constant O_2 dry-air mole fraction (X_{O_2}) is not an assumption that O_2 is invariant. The dry-air mole fraction of O_2 is observed to be uniform across latitude, altitude, and season to within tens of ppm, a relative variation of 0.01% (Keeling, 1988; Tohjima et al., 2005). The atmospheric O_2 research community measures such small variations around this value, which are reported as $\delta(O_2/N_2)$ in per meg on a dry-air basis. Dry air has no significant sources or sinks, so a change in a dry-air mole fraction unambiguously reflects a surface flux of the gas, whereas on a moist-air basis every humidity fluctuation would complicate the calculation of flux. Now we clarify in the revised text that the 0.2094 appearing in the per meg conversion of our Appendix C is the reference mole fraction of the Scripps O_2 scale, which is a defined constant of the measurement scale rather than a physical assumption.

We have revised throughout the manuscript to highlight that 0.2094 refers to dry-air mole fraction. For example

L324: X_{O_2} is the dry-air mole fraction of O_2 in the atmosphere (0.2094).

We also added the following text.

L1221-1224: The value of X_{O_2} is the reference mole fraction of the Scripps O_2 Program scale (Keeling and Morgan, 2019), a defined constant of the measurement scale corresponding to an estimate of the atmospheric composition in the mid 1980s derived from Machta and Hughes (1970).

We note that if Dr. Kowalski's claims were correct, they would call for a substantial re-evaluation of the global carbon and oxygen cycles and of roughly 50 years of literature built on the established treatment. We would welcome empirical evidence in support of the proposed

revision, ideally from studies with pre-registered protocols, but in its absence we retain the standard formulation.

References:

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