

Review of “Biological imprint dominates global mesoscale air-sea flux anomalies of carbon dioxide and oxygen”

The manuscript develops an analytical framework to diagnose global patterns of ocean mesoscale CO₂ and O₂ flux anomalies and their drivers. Overall, I find the topic of this manuscript interesting and potentially important. The use of simultaneous CO₂ and O₂ flux variability provides a promising way to investigate the biogeochemical imprint of mesoscale processes at the global scale, and the eddy-rich CM2.6 simulation is particularly well suited for this purpose. However, I have some concerns regarding the framework. Several aspects of the methodology require further clarification and, in some cases, additional analysis before the proposed framework can support the interpretations made in the manuscript.

1. A particularly important missing component of the framework is the contribution of mesoscale variability in the gas-transfer velocity. For a gas flux written schematically as $F = kD$, where D denotes the air–sea concentration or partial-pressure disequilibrium, the mesoscale anomaly can be approximated as

$$F' \approx \bar{k}D' + \bar{D}k' + k'D'$$

The current interpretation focuses primarily on the first term, i.e. changes in gas disequilibrium associated with temperature or biological cycling, and treats variations in gas-transfer velocity as a second-order effect (Line 240). This assumption should be tested more carefully, particularly when the model shows a significant mesoscale wind-stress response. This is important because the sign of the flux anomaly caused by k' depends on the background disequilibrium. If CO₂ and O₂ have background fluxes in opposite directions, the same positive k' can generate anomalous CO₂ and O₂ fluxes of opposite signs, which would then be classified by the proposed framework as “biological” even in the absence of biological perturbation. Conversely, where the background disequilibria have the same sign, variations in k can enhance positive CO₂-O₂ correlations.

Furthermore, I suggest that the authors revisit the associated citation. Salinas-Matus et al. (2025) identifies both piston velocity and $\Delta p\text{CO}_2$ as major contributors to CO₂ flux variability, and finds piston velocity to be more important than $\Delta p\text{CO}_2$ in some regimes.

2. The authors should also clarify whether the air-sea gas-exchange formulation in CM2.6 includes bubble mediated gas exchange. Bubble-mediated transfer depends strongly on wind and wave breaking and may affect CO₂ and O₂ differently, with

potentially greater importance for O₂. Mesoscale wind anomalies could therefore modify the CO₂-O₂ flux relationship through differential physical gas exchange independently of biological production or remineralization. If bubble-mediated exchange is not considered, this should be acknowledged as a limitation, particularly when extending the model-based CO₂-O₂ attribution framework to those strongly wind-forced regions.

Reference

- Deike, L., Zhou, X., Rustogi, P., Stanley, R.H.R., Reichl, B.G., Bushinsky, S.M., Resplandy, L., 2025. A universal wind–wave–bubble formulation for air–sea gas exchange and its impact on oxygen fluxes. *Proc. Natl. Acad. Sci. U.S.A.* 122, e2419319122. <https://doi.org/10.1073/pnas.2419319122>
- Dong, Y., Jähne, B., Woolf, D.K., Krall, K.E., Yang, M., Czerski, H., Liang, J., Brooks, I.M., McNeil, C.L., Wanninkhof, R., Ho, D.T., Atamanchuk, D., Marandino, C.A., 2026. The Role of Bubbles in Air-Sea Gas Exchange: A Critical Review. *Reviews of Geophysics* 64, e2025RG000903. <https://doi.org/10.1029/2025RG000903>

3. I am also concerned by the conceptual argument in Fig. 1 in which an anticyclonic eddy is associated with an initially positive SST anomaly and reduced gas solubility, followed by cooling over the eddy lifetime, increasing solubility, and subsequent ingassing of both CO₂ and O₂. A positive SST anomaly ($T' > 0$), and a negative SST tendency, ($\partial T / \partial t < 0$), are fundamentally different quantities. A warm anomaly by itself indicates warmer water relative to the surrounding large-scale state and it does not imply that the water is currently cooling. Ingassing associated with increasing solubility requires an actual cooling tendency or heat loss and cannot be inferred simply from the existence of a warm SST anomaly. If the proposed mechanism relies on the temporal evolution of eddies from a warm state toward thermal equilibration, this should be demonstrated explicitly using SST tendency, net surface heat flux, or tracked eddy life-cycle composites rather than inferred from SST anomalies. Lagged relationships between heat flux/SST tendency and gas flux would also be informative.

4. I agree with the authors that adding NCP provides useful additional information. However, I do not think that consistency or inconsistency between NCP anomalies and gas-flux anomalies is sufficient to separate “productivity-driven” from “respiration-signal” regimes. In particular, the manuscript interprets CO₂ outgassing and O₂ ingassing accompanied by enhanced NCP as evidence for upward transport of DIC-rich, O₂-poor remineralized waters. This is certainly a plausible mechanism, but it is not a unique inference. NCP integrated over the upper 100 m represents a biological source/sink term, whereas surface gas fluxes depend on the evolution of the

mixed-layer inventories. Between these two quantities lie vertical advection, vertical mixing, mixed-layer entrainment, horizontal advection, storage, and air–sea equilibration. Importantly, the same upward transport event can simultaneously bring DIC-rich/O₂-poor water into the mixed layer and supply nutrients that enhance biological production. Thus, enhanced NCP and an apparent “respiration signal” are not independent drivers; they can be two consequences of the same vertical transport process. I therefore suggest that the latter regime be described more precisely as a vertical-transport/remineralized-water signal rather than “respiration”.

More importantly, because this is a model study, direct analysis of vertical DIC and O₂ transport, as well as entrainment/mixing tendencies would provide much stronger evidence. For example, an upward transport of remineralized water should be accompanied by a positive vertical DIC tendency and a negative vertical O₂ tendency.

Specific comments.

Line 170. A fixed (3x3 degree) window represents very different physical length scales at different latitudes. For example, 3 degrees of longitude correspond to approximately 333 km at the equator but only about ~150 km at 60S. This is important because characteristic eddy scales also vary strongly with latitude. Please discuss this limitation (ideally, test a filtering scale defined in physical distance rather than degrees).

Line 190-200. For CO₂, temperature affects both the solubility coefficient and the carbonate equilibria that determine seawater pCO₂. I therefore suggest using the term like “thermally driven” rather than “solubility-driven”.

Line 240-245. The statement that a modified gas-transfer velocity “does not change mixed layer gas concentrations but the time-scale of equilibration” should be revised. A change in k does not directly change the equilibrium concentration, but it does change the air-sea flux and therefore the temporal evolution and magnitude of the actual mixed-layer concentration and disequilibrium. I suggest replacing this sentence with wording such as: “Variations in gas-transfer velocity do not directly alter the thermodynamic equilibrium concentration, but modify the rate at which the mixed layer approaches equilibrium with the atmosphere.”

Same paragraph. I am also not convinced that excluding sea-ice-covered regions resolves this issue. Wind-dependent gas-transfer velocity is relevant throughout the open ocean, and the manuscript itself demonstrates mesoscale SST-wind coupling (Line 165).

Line 395-400. Since the author highlight the role of heat loss/gain, it would be informative to show the mesoscale correlation between gas-flux anomalies and net surface heat-flux anomalies.