

Authors' response to the comments of the second reviewer

We thank the reviewer for the careful assessment of our manuscript and for the constructive comments and suggestions. Following the same structure as for the first reviewer, the reviewer's comments are reproduced below in italics, followed by our responses. All corresponding modifications to the revised manuscript are highlighted in bold burgundy text.

General points to address:

1. *"More justification is needed for the simplification 'GPPO₂ = 2 × PP'. You state from Juraneck and Quay that the range in the ocean is 1.8-4.5 and that the ratio is close to 2.7, then chose a value of 2. If going for a single number, why not 2.7? Then what is the implication of not considering the spatial variability? For example, if you were to consider spatial correlation between the GPPO₂/PP ratio and e.g. spatial variability in O₂ fluxes, does that lead to a bias in from the mean impact of this assumption that might suggest that you should be using a higher or lower value? As I say, simplification is quite understandable, it just needs more justification and explanation."*

Response: We thank the reviewer for raising this point. We agree that the rationale for using a factor of 2 was not sufficiently explained in the previous version of the manuscript, and that the discussion of the observational GPPO₂/PP ratios could misleadingly suggest that this factor was directly selected from the observational range. Importantly, the factor of 2 corresponds to our assumed GPP/PP ratio, while the value of approximately 2.7 additionally accounts for the conversion from carbon to oxygen units using the Redfield ratio.

In the current version of iLOVECLIM, phytoplankton production (PP) corresponds to phytoplankton growth, which we assume to represent net primary production (NPP). While heterotrophic respiration (R_{hetero}) is implicitly represented through the remineralization of organic matter, gross primary production and autotrophic respiration are not explicitly simulated. We therefore assume that autotrophic respiration (R_{auto}) is equal to PP. Thus:

$$GPP = PP + R_{\text{auto}} = 2 \times PP, \quad (1)$$

and, after conversion into oxygen units:

$$GPPO_2 = 2 \times PP_{O_2} \quad (2)$$

Total respiration, including both autotrophic and heterotrophic respiration, is then diagnosed as $R_{\text{total}} = GPPO_2 - NCP$, thereby preserving the NCP calculated by the ecosystem model and the gross production–respiration budget.

Thus, the factor of 2 arises from the simplifying assumption $R_{\text{auto}} = PP$, rather than from the observational mean GPPO₂/PP ratio. Replacing it directly by 2.7 would imply a different partitioning between net production and autotrophic respiration, with $R_{\text{auto}} = 1.7 \times PP$, and would consequently modify the total diagnosed respiration. However, combined with the Redfield O₂/C stoichiometric ratio (138/106 $\simeq$ 1.30), the assumed GPP/PP ratio of 2 results in an overall conversion factor of approximately 2.6 between carbon-based phytoplankton production and GPPO₂, which is close to the value of 2.7 reported by Marra (2002). This provides support for the order of magnitude of the adopted parameterization.

We agree, however, that assuming a spatially constant GPP/PP ratio is a simplification. Observational estimates of GPPO₂/PP range from approximately 1.8 to 4.5 across the ocean

(Juraneck and Quay, 2013). Neglecting this spatial variability may therefore lead to regional biases in GPPO₂ and respiration, particularly if variations in this ratio covary with environmental conditions or oxygen fluxes. Accounting for such variability would require an explicit representation or independent constraint of autotrophic respiration, which is not currently available in the model. We have revised the manuscript to clarify this assumption and its limitations.

Revised version: To avoid confusion, the definition of PP has been clarified at line 155 as follows: "PP is phytoplankton production and corresponds to phytoplankton growth in the current version of iLOVECLIM. Here, PP is assumed to represent net primary production (NPP)."

Furthermore, the paragraph from lines 158 to 164 has been amended as follows: "Although NCP represents the net oxygen balance, isotopic calculations require an explicit separation of gross primary productivity of oxygen and total respiration. Since GPPO₂ and autotrophic respiration are not explicitly simulated by the ecosystem model, we assume that autotrophic respiration is equal to PP, corresponding to a GPP/PP ratio of 2. GPPO₂ is therefore defined as twice the total phytoplankton production after conversion into oxygen units using the Redfield ratio:

$$GPPO_2 = 2 \times PP. \quad (3)$$

Here, the factor of 2 represents the assumed GPP/PP ratio and not the overall conversion between carbon-based PP and GPPO₂. When combined with the carbon-to-oxygen conversion using the Redfield ratio ($138/106 \simeq 1.30$), this formulation results in an overall conversion factor of approximately 2.6 between the carbon-based phytoplankton production simulated by the model and GPPO₂. This factor is close to the ratio between PP (measured by the ¹⁴C method) and GPPO₂, which is 2.7 according to Marra (2002). However, observational estimates range from approximately 1.8 to 4.5 across the ocean (Juraneck and Quay, 2013). The use of a spatially constant ratio therefore remains a simplification and may lead to regional over- or underestimation of GPPO₂ and respiration."

2. *"I'm not 100 % clear as to whether oxygen is prognostic or not in the model. You state that you are inferring O₂ fluxes from CO₂ fluxes, which in my view may be acceptable in steady state system, but O₂ and CO₂ have vastly different equilibration timescales due to the slow hydration reaction of aqueous CO₂. But you also describe the O₂ flux calculation in the paragraph starting on line 176. I would suggest having a very clearly written summary at the start of the implementation section so the reader is clear when they go into this section what assumptions you are making, then you can fill them in on the detail subsequently. Please then explore with clear justification, how if these assumptions were not made the results would change, and in the context of this, explain why the choices made here are (or are not) acceptable."*

Response: We thank the reviewer for this comment and agree that the previous description was not sufficiently clear regarding the treatment of oxygen in the model. Oxygen is prognostic in our implementation: the three oxygen isotopologues (³²O₂, ³³O₂, and ³⁴O₂) are transported and updated at each model time step according to biological sources and sinks, ocean circulation, and air-sea exchange.

We would like to clarify that only the biological O₂ production and consumption terms are derived from the corresponding biological carbon fluxes simulated by the marine ecosystem model, using Redfield stoichiometric ratios. In contrast, the air-sea O₂ exchange is calculated independently, based on the difference between the surface O₂ concentration and its atmospheric equilibrium concentration, using O₂-specific solubility and gas-transfer formulations. Therefore, the different equilibration timescales of CO₂ and O₂ do not affect the air-sea O₂ flux calculation

in our implementation.

The main simplifying assumption therefore concerns the conversion of biological carbon production and respiration into biological O_2 production and consumption. Allowing variable C/ O_2 stoichiometry or an explicit representation of oxygen-based biological processes could modify the magnitude and spatial distribution of GPPO $_2$ and respiration, and consequently the isotopic signals. However, the present study focuses on equilibrium simulations and on the large-scale controls of oxygen isotopes. Under these conditions, using the biological carbon fluxes to constrain the biological O_2 sources and sinks provides a consistent first-order representation, while dissolved O_2 and its isotopologues retain their own prognostic dynamics.

Revised version: To avoid this ambiguity, we have added a summary at the beginning of the implementation section explicitly distinguishing the derivation of biological O_2 production and consumption from the independent calculation of air-sea O_2 exchange. Lines 145–147 have been revised as follows: "The three oxygen isotopologues are prognostic tracers whose concentrations are updated at each model time step. Biological O_2 production and consumption, through photosynthesis and respiration, are derived from the marine carbon-cycle model implemented in iLOVECLIM. The production and consumption rates in carbon units are converted into O_2 units using the corresponding Redfield stoichiometric ratios. In contrast, air-sea O_2 exchange is calculated independently, using O_2 -specific formulations (Eq. 8)."

In addition, the sentence (line 150) "In the model, oxygen fluxes are not computed directly but are inferred from carbon fluxes using a Redfield ratio" has been removed, as this formulation could incorrectly suggest that all O_2 fluxes, including air-sea exchange, are derived from carbon fluxes.

3. *"The observational comparison is hard to take a lot from in this study, as the model physics and background O_2 concentration are not very good (for understandable reasons given the nature of the model). I wonder if it would be more useful to do the observational comparison in water-mass space rather than in geographical coordinates, so the reader can assess the $\delta^{17}O$ more meaningfully?"*

Response: We thank the reviewer for this valuable suggestion. Following the reviewer's recommendation, we performed an additional model–data comparison in temperature–salinity space (Fig. 1). This provides a complementary evaluation based on the hydrographic properties of the sampled waters rather than solely on their geographical coordinates.

The T–S comparison shows that the observations span a broader range of hydrographic conditions than the corresponding simulated waters: observed salinities extend to approximately 23, whereas simulated values remain mostly above approximately 28. The model therefore occupies a more restricted T–S space, consistent with the simplified ocean circulation and spatial resolution of iLOVECLIM. This indicates that differences in water-mass representation may contribute to part of the point-by-point model–data mismatch, and the T–S diagram is a useful complementary diagnostic for this purpose.

However, when restricted to waters occupying comparable temperature and salinity ranges, the positive $^{17}\Delta_{\text{ocean}}$ bias previously identified in the Southern Ocean remains clearly visible, with simulated values generally higher than observed ones. This suggests that the Southern Ocean bias cannot be explained solely by the comparison of dissimilar water masses, but reflects a more systematic model behaviour in this region.

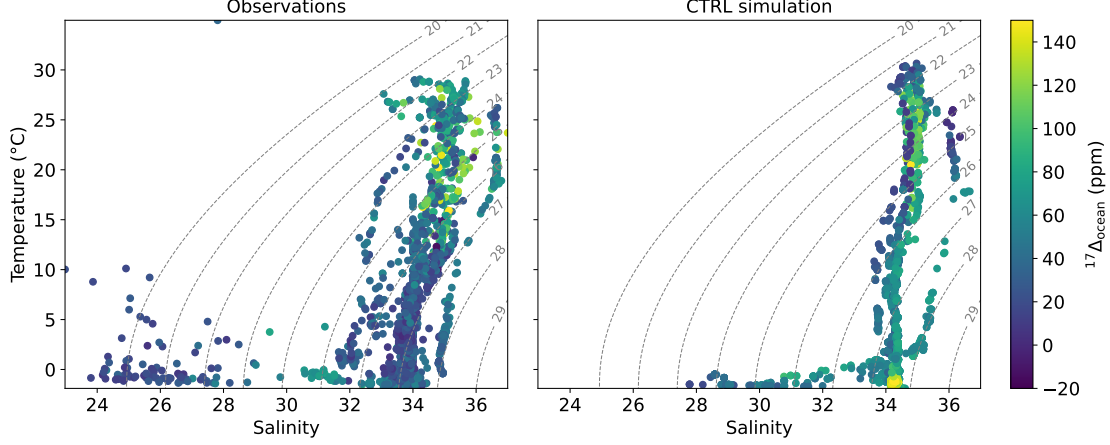


Figure 1: Temperature–salinity (T–S) diagrams for (a) observations and (b) the CTRL simulation, colored according to $^{17}\Delta_{\text{ocean}}$ (ppm). Model values correspond to the grid cells, depths, and seasons associated with the observations. Dashed lines indicate seawater density contours.

We therefore retain the geographical comparison as the primary basis for evaluating the model, for two reasons. First, model values are extracted at the grid cells, depths, and seasons corresponding to each observation, rather than compared as basin-wide averages. The T–S representation provides an additional assessment of whether the hydrographic properties of the corresponding simulated and observed waters are consistent, rather than replacing the geographical comparison. Second, the observational dataset reflects specific locations, depths, seasons, and ventilation histories, whereas our simulation represents an equilibrium state of the model. Consequently, individual observed water masses and their ventilation histories cannot necessarily be directly matched to their simulated counterparts. This limitation applies to any point-by-point comparison, whether expressed in geographical or hydrographic coordinates, and is therefore not resolved by changing the coordinate system. Given that the primary objective of the observational comparison is to evaluate whether the implementation reproduces the first-order spatial distribution and large-scale gradients of $\delta^{18}\text{O}-\text{O}_2$ and $^{17}\Delta_{\text{ocean}}$, the geographical framework remains the most direct and interpretable way to present this evaluation, with the T–S diagram providing a complementary assessment in hydrographic space. We have added Fig. 1 to the revised manuscript (Supplement) and clarified the limitations of the geographical comparison accordingly.

Revised version: To better clarify the purpose and limitations of the model–data comparison, the corresponding paragraph of the manuscript (lines 201–203) has been revised as follows: “It should be noted that the observational datasets used in this study were collected under modern oceanic conditions and represent measurements at specific locations, depths, seasons, and times, whereas the simulations were performed at equilibrium under pre-industrial forcing. Given these differences in temporal representation and the simplified ocean circulation of iLOVECLIM, a point-by-point agreement between model and observations is not expected. Therefore, the geographical comparison is intended to evaluate the large-scale consistency of the simulated oxygen and isotope distributions. As a complementary assessment, the model–data comparison was also examined in temperature–salinity space to evaluate the potential influence of differences in the representation of water masses (Fig. B1).”

In addition, a new section (Appendix B, “Model–data comparison in temperature–salinity space”) has been added to the Supplement, including Fig. 1 and a discussion of the comparison in hydrographic space:

Appendix B: Model–data comparison in temperature–salinity space

To complement the geographical model–data comparison, the distribution of $^{17}\Delta_{\text{ocean}}$ was also examined in temperature–salinity (T–S) space (Fig. 2). Model values correspond to the grid cells, depths, and seasons associated with each observation. The observations span a broader range of hydrographic conditions than the corresponding simulated waters, particularly toward lower salinities, whereas the model occupies a narrower T–S space. This indicates that some local model–data discrepancies may be associated with differences in the representation of water masses in iLOVECLIM. However, for waters occupying comparable temperature and salinity ranges, the positive $^{17}\Delta_{\text{ocean}}$ bias previously identified in the Southern Ocean remains clearly visible, with simulated values generally higher than observed ones. This suggests that the Southern Ocean bias cannot be explained solely by the comparison of dissimilar water masses and may reflect a more systematic model behaviour in this region.

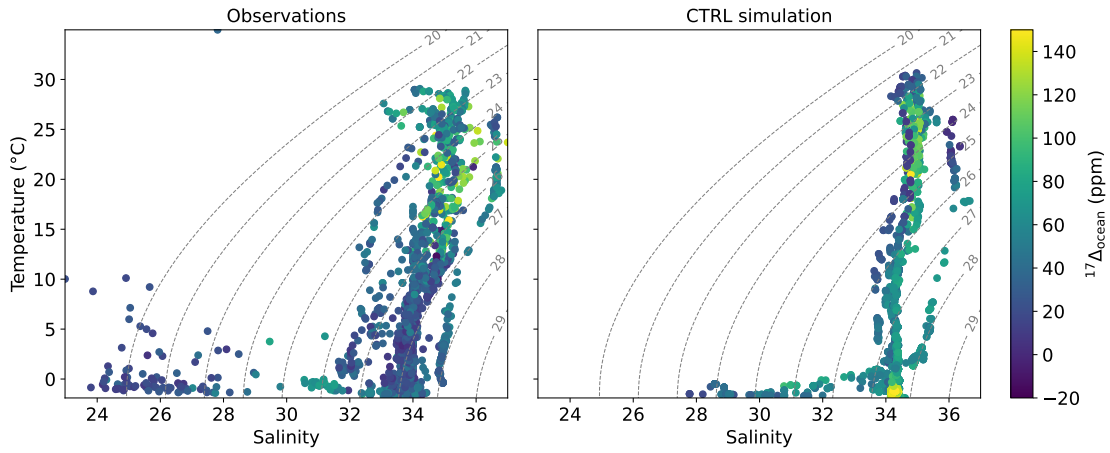


Figure 2: Temperature–salinity (T–S) diagrams for (a) observations and (b) the CTRL simulation, colored according to $^{17}\Delta_{\text{ocean}}$ (ppm). Model values correspond to the grid cells, depths, and seasons associated with the observations. Dashed lines indicate seawater density contours.

Specific points to address:

1, "... provides a global integrator of past biospheric oxygen fluxes, and by extension carbon fluxes". Not all processes change C and O together, particularly on longer timescale - moderate this statement."

Revised version: We agree with the reviewer that the original statement implied an overly direct coupling between oxygen and carbon fluxes, particularly over long timescales. We have therefore moderated this statement: "... provides a global integrator of past biospheric oxygen fluxes, which can also provide indirect constraints on associated carbon-cycle processes." (line 3)

2, Wording flow: "However, deconvolving the signal of $^{17}\Delta$ requires to isolate the oceanic biosphere productivity ($^{17}\Delta_{\text{ocean}}$)."

Revised version: We thank the reviewer for pointing out this ambiguity. The sentence has been revised to specify the contributions from which the oceanic signal must be isolated: "However, deconvolving the signal of $^{17}\Delta$ requires one to isolate the contribution of oceanic biosphere ($^{17}\Delta_{\text{ocean}}$) from those of the terrestrial biosphere ($^{17}\Delta_{\text{terr}}$) and the stratosphere ($^{17}\Delta_{\text{strato}}$)."

3, Line 39, "*have employ isotope-enabled.*" → "*have employed isotope-enabled.*"

Revised version: Thank you for pointing out this grammatical error. It has been corrected accordingly.

4, Line 49, *missing a space in ").T"*

Revised version: Thank you for pointing out this error. It has been corrected.

References

- 1, Marra, J.: Approaches to the Measurement of Plankton Production. *Phytoplankton Productivity*, 78–108, 2002.
- 2, Juranek, L. and Quay, P.: Using Triple Isotopes of Dissolved Oxygen to Evaluate Global Marine Productivity, *Annu. Rev. Mar. Sci.*, 5, 503–524, 2013.