

We thank the referees for their constructive feedback. We agree with all major comments. Our study needed a clearer message and scope, along with a better assessment of its limitations, particularly regarding missing processes, spin-up, and stoichiometry. We now explicitly position this work as a sensitivity analysis using an existing optimized ECCO-Darwin to quantify the response of the ocean carbon cycle to riverine inputs, rather than to deliver a fully optimized solution that includes coastal processes.

To address concerns about nutrient stoichiometry and phytoplankton limitation, we streamlined the experiment set by adding a single dedicated nutrient simulation that includes both phosphorus and iron inputs. This configuration directly responds to referee feedback by accounting for their combined supply relative to global N:P:Si:Fe stoichiometry. Regarding model spin-up and equilibration timescales, we acknowledge that multi-decadal simulations cannot fully equilibrate riverine nutrient perturbations, especially in the Arctic, and we clarify that such equilibration is required for comparison with the long-term river loop. We further highlight that spin-up limitations will be directly addressed in future work, as we are currently developing a new adjoint-optimized ECCO-Darwin configuration (v06) that includes fully-coupled riverine biogeochemical tracers and other coastal and benthic processes.

In summary, we accept the referee's broader point that a more complete and optimized configuration of ECCO-Darwin will be necessary for comparison with long-term cycling of riverine inputs; this manuscript is an important technical milestone towards that effort. Finally, while we recognize suggestions to further expand the analysis, we have deliberately chosen to keep the manuscript focused solely on the implementation of riverine inputs and avoid adding additional layers of complexity, as it is already dense. Our intention in submitting to GMD is for this work to serve as a technical foundation that can be leveraged for more comprehensive scientific exploration in future studies using ECCO-Darwin.

Referee #2

The manuscript describes impacts of adding observationally constrained rates of riverine input of DIC, DOC, DIN, DON and DSi to the global multi-functional type biogeochemical ECCO-Darwin model. The impact of various combinations of riverine carbon and nutrient supplies on model-data misfits with respect to surface pCO₂ and air-sea CO₂ fluxes are investigated, and some improvements are found, particularly when riverine DOC supply is simulated.

The study adds some information to other recent efforts to address the historical lack of realistic descriptions of the land-ocean interface in current global marine biogeochemical models. Results are relatively unsurprising, i.e. CO₂ emissions from the ocean increase when DOC with a lifetime of 100 days is added to the ocean surface at river mouths, and CO₂ uptake by the ocean increases in some regions where additional nutrients are added by rivers. The design of the study results in only limited gain in terms of scientific understanding. I have three main concerns about the present version of the study that limit the gain in scientific understanding:

First, the riverine input of biogeochemical tracers is added to a calibrated model run without riverine input, which -presumably- tries to make up for the missing river input by adjusting model parameters or other control variables. The model configuration with riverine input is not calibrated. Thus, the comparison is between a calibrated and an uncalibrated model version. Difficult to assess. Results are more a sensitivity analysis rather than an assessment of structural model improvements.

We agree that the present experiments should be interpreted as a sensitivity analysis rather than as a full optimization of ECCO-Darwin with riverine inputs. As a data-assimilative model, ECCO-Darwin adjusts biogeochemical control parameters and initial conditions to minimize model–data error, which implicitly accounts for some missing processes such as lateral fluxes. Through the assimilation of observations from 1992–2019, the ocean biogeochemical state simulated by ECCO-Darwin is actually representative of past and present conditions and the anthropogenically-perturbed natural river loop. Consequently, the optimized version of ECCO-Darwin used in Baseline already implicitly accounts for lateral fluxes. However, the coverage of datasets assimilated into ECCO-Darwin remains limited near the coast and river mouths, where terrestrial carbon and nutrients are mostly used or transformed, so a double accounting might be limited. Future efforts will focus on optimizing the model with river inputs, which we anticipate will further improve the structural realism of ECCO-Darwin and better constrain coastal processes. To better showcase the paper, we changed the title to:

Implementing Riverine Biogeochemical Inputs in ECCO-Darwin: A Sensitivity Analysis of Terrestrial Fluxes in a Data-Assimilative Global Ocean Biogeochemistry Model

Second, the simulations with riverine inputs are, if I understand correctly, run only from year 1992 to 2019, i.e. 28 years, of which the first 8 years are taken as spin-up, and 2000-2019 as analysis period. Particularly for the addition of nutrients, this is likely insufficient to reach a steady state. Some time series of relevant model output (NPP in different regions, nutrient and carbon concentrations) needs to be shown to allow the

reader to assess the issue of inferring general results from short decadal-scale simulations only.

We acknowledge that the 30-year model period (1992–2019) does not allow the system to fully equilibrate with the addition of river inputs, particularly the long-term river loop and nutrient inputs. We added Figures S3 and S4 so the reader can see that changes in air-sea CO₂ flux and NPP in all experiments tend to stabilize from 2000, globally. We also computed these figures for our three regional domains in Figures S2–S9. While air-sea CO₂ flux and NPP in TROP-ATL and SE-ASIA equilibrate around the same year as the global domain, they did not stabilize in ARCT, confirming Referee #2's suggestion. It is also consistent with Referee #1 comment on residence time. The Arctic's long residence time prolongs adjustment to riverine perturbations, explaining why this region is slower to equilibrate compared to regions with faster residence time. Extending the analysis over centennial timescales is an important next step, which we highlight in the revised discussion, L276:

“The 28-year model period (1992–2019) does not allow the system to fully equilibrate with the addition of riverine inputs. However, time series of change in air–sea CO₂ flux and NPP with the addition of river carbon and nutrients (Supporting Information Figures S2–S9) indicate that most regions approach quasi-equilibrium by the year 2000, consistent with the global response. In contrast, the change in air–sea CO₂ flux and NPP with the addition of river carbon and nutrients in the Arctic do not stabilize over the model period (Figures S3 and S7). Regional variability in air–sea CO₂ flux responses can be interpreted through differences in coastal residence times, as in the Arctic, long residence times promote remineralization and outgassing of terrestrial organic matter while limiting nutrient-driven uptake due to light limitation (Liu et al., 2019; Lacroix et al., 2021a). These extended residence times also explain why the Arctic response does not stabilize within the 28-year experiment window (Figure S3 and S7), in contrast to other regions where shorter residence times facilitate more rapid equilibration. Conversely, regions such as the Amazon plume display substantial CO₂ outgassing despite shorter residence times, but this is accompanied by offshore transport, suggesting that riverine carbon inputs or remineralization rates may be overestimated in coastal systems where residence time is short.”

Third, several implicit and explicit assumptions may have relevant impacts on the results shown. Some assumptions are stated, e.g. the assumed 100 day lifetime of riverine DOC, or the assumption of zero phosphorus input from land while dissolve silica is included, others are not, such as the presence of denitrification and/or nitrogen fixation. In order to provide “a critical step forward”, as stated in the title of the manuscript, a more comprehensive sensitivity analysis with respect to major

assumptions would be required, possibly extending the analysis to riverine supply of phosphorus.

We agree that our results rely on several key assumptions. In the Discussion section, we elaborate on the necessary and consequential simplifications made in this study. We also describe ongoing and future developments of ECCO-Darwin that will address these limitations and move toward a fully optimized ECCO-Darwin that accounts for coastal margins. To address concerns about nutrient stoichiometry and phytoplankton limitation, we streamlined the experiment set by adding a single dedicated nutrient simulation that includes nitrogen, phosphorus, silica, and iron inputs together. This configuration directly responds to referee feedback by accounting for their combined supply relative to global N:P:Si:Fe stoichiometry. We also agree that the global effect of the implementation is limited in this study. However, we consider the implementation critical for the ECCO-Darwin model and future land-to-ocean model development and studies. Until now, ECCO-Darwin lacked a realistic representation of lateral fluxes of carbon and nutrients, and including these sources is an important step forward for the ECCO-Darwin community, but also for the ocean modeling community in general, as more ocean biogeochemistry models should account for terrestrial carbon and nutrients. Even if the contribution of river carbon and nutrients is small at the global scale, it remains significant for coastal and regional budgets and, thus, is pivotal for future studies using ECCO-Darwin. For this reason, we believe it is critical to move forward with this new ECCO-Darwin capability. We changed the title to make it more reflective of both referees' comments:

Implementing Riverine Biogeochemical Inputs in ECCO-Darwin: A Sensitivity Analysis of Terrestrial Fluxes in a Data-Assimilative Global Ocean Biogeochemistry Model

Individual points:

I.17 'slower' than what?

To clarify, we changed this sentence in the new version of the introduction, L23:

“In the absence of transformation in the coastal ocean, refractory riverine organic carbon can be transported offshore due to its slow turnover time (Hansell et al., 2004; Holmes et al., 2008; Kaiser et al., 2017).”

I.24/25 'excess of alkalinity relative to DIC' Does this refer to concentrations or to fluxes? How does this fit to outgassing (a flux)?

To clarify, we changed this sentence in the new version of the introduction, L21:

“On continental shelves, the outgassing of CO₂ driven by the saturation of surface waters with terrestrial DIC or remineralized terrestrial organic carbon can also be compensated by the excess of alkalinity relative to DIC concentration (Cai, 2011; Louchard et al., 2021).”

I.27 There does not always have to be alkalinity production, e.g. when calcifiers are involved.

We removed the mention of alkalinity production.

I.28 ‘estimated coastal-ocean sink’ of what ? Total carbon, riverine carbon, marine carbon?

We changed it to: “coastal-ocean carbon sink”

I.84 does ‘particulate organic matter’ mean detritus or phytoplankton and zooplankton as well?

It includes detritus, inorganic carbon and living phytoplankton and zooplankton. We added this to the description, L88:

“In the water column, particulate matter (detritus, inorganic carbon, and living phytoplankton and zooplankton) sinks at prescribed velocities and is removed at the ocean bottom to limit the accumulation of particulates on the seafloor.”

I.93. Does this mean there is only 8 years of spin-up? Is the biogeochemistry in some form of steady state after such a short period, and if so, in what regions?

As a data-assimilative model, ECCO-Darwin does not use a traditional spin-up. The model does require a short adjustment period (about 3 years) to equilibrate due to a mismatch between initial conditions and observational constraints. Adding riverine fluxes extended the period of equilibrium, so we used outputs from 2000 onward. We acknowledge that the 30-year model period (1992–2019) does not allow the system to fully equilibrate with the addition of river inputs, particularly the long-term river loop and nutrient inputs. We added Figures S3 and S4 so the reader can see that changes in air-sea CO₂ flux and NPP in all experiments tend to stabilize from 2000, globally. We also computed these figures for our three regional domains in Figures S5-S11. While air-sea CO₂ flux and NPP in TROP-ATL and SE-ASIA equilibrate around the same year as the global domain, they did not stabilize in ARCT, confirming Referee #2’s suggestion in their second major comment. We added the importance of equilibration in the discussion, L273:

“The 28-year model period (1992–2019) does not allow the system to fully equilibrate with the addition of riverine inputs. However, time series of change in air–sea CO₂ flux and NPP with the addition of river carbon and nutrients (Supporting Information Figures S3–11) indicate that most regions approach quasi-equilibrium by the year 2000, consistent with the global response. In contrast, the change in air–sea CO₂ flux and NPP with the addition of river carbon and nutrients in the Arctic do not stabilize over the model period (Supporting Information Figures S3–S7). Regional variability in air–sea CO₂ flux responses can be interpreted through differences in coastal residence times, as in the Arctic, long residence times promote remineralization and outgassing of terrestrial organic matter while limiting nutrient-driven uptake due to light limitation (Liu et al., 2019; Lacroix et al., 2021a). These extended residence times also explain why the Arctic response does not stabilize within the 28-year experiment window (Supporting Information Figure S3 and S7), in contrast to other regions where shorter residence times facilitate more rapid equilibration. Conversely, regions such as the Amazon plume display substantial CO₂ outgassing despite shorter residence times, but this is accompanied by offshore transport, suggesting that riverine carbon inputs or remineralization rates may be overestimated in coastal systems where residence time is short.”

I.96ff The model evaluation addresses surface pCO₂ and air-sea fluxes of CO₂ only. It would be useful to provide some assessment of simulated NPP, biomass and nutrient distributions.

As ECCO-Darwin is a data-assimilative model, it matches very well the nutrient distributions compared to observational datasets used for data constraint (see Figure 2 in Carroll et al., 2020). Our NPP estimate remains in the lower range of literature estimates (30–75 Pg C yr⁻¹). As NPP estimates in literature come with large uncertainties, we preferred to discuss and compare our simulated NPP with existing estimates in text only L254. However, we added a figure that compares ECCO-Darwin Baseline with two state-of-the-art references: the ICON-Coast ocean model (Mathis et al., 2022) and the MODIS CAFE remote sensing algorithm (Silsbe et al., 2016).

“Baseline captures similar spatial patterns of NPP compared to the model ensemble of the REgional Carbon Cycle Assessment and Processes Phase-2 (RECCAP-2) project that aims at constraining present-day ocean carbon from observation-based estimates, inverse models, and GOBMs (Doney et al., 2024) (Supporting Information Figure S12). Many uncertainties remain regarding global-ocean NPP estimates from remote sensing (due to uncertainty in algorithms) and models (due to different conceptual model architectures). Overall, NPP in Baseline (24.5 Pg C yr⁻¹) lies in the lower bound of the wide range depicted by the RECCAP-2 model ensemble (25–57 Pg C yr⁻¹; Doney et al.,

2024) and remote-sensing algorithms (43–68 Pg C yr⁻¹; Behrenfeld and Falkowski, 1997; Silsbe et al., 2016; Carr et al., 2006; Marra et al., 2003; Behrenfeld et al., 2005). This relatively low NPP results primarily from strong iron limitation in the High-Nutrient, Low-Chlorophyll (HNLC) regions in ECCO-Darwin (Carroll et al., 2020). The strong surface-ocean stratification and the weaker winter convection limit the replenishment of nutrients in the euphotic zone. Nevertheless, global-ocean NPP estimates will improve from enhanced space-time coverage of NPP measurements and associated key variables such as chlorophyll, light, nutrients, optical properties, and cell physiology (Bendtsen et al., 2023). An integration of environmental variables along with NPP measurements will greatly reduce models' spread and mismatch with synoptic in-situ observations. The implementation of a radiative transfer package (Dutkiewicz et al., 2019) in the next version of ECCO-Darwin, for which development is already underway, will permit the assimilation of direct ocean-color observations (remotely-sensed reflectance) and improve the model's estimate of global-ocean NPP."

Carroll, D., Menemenlis, D., Adkins, J. F., Bowman, K. W., Brix, H., & Dutkiewicz, S., et al. (2020). The ECCO-Darwin data-assimilative global ocean biogeochemistry model: Estimates of seasonal to multidecadal surface ocean pCO₂ and air-sea CO₂ flux. *Journal of Advances in Modeling Earth Systems*, 12, e2019MS001888. <https://doi.org/10.1029/2019MS001888>

I.138 Would be good to add if the extreme value was high or low, and also provide a very brief explanation for why (only) this value had to be corrected.

The value was originally about 9 Tmol C yr⁻¹ of DIC coming from the Amazon River. It is due to the outstandingly large Amazon watershed area (used for estimating rock weathering) and freshwater discharge compared to other basins that drive a very high load when using equation 9 from Li et al. (2017). We added more details in the manuscript, L150:

"Due to overestimated t_{DIC} inputs in our Global NEWS 2-derived computation for the Amazon River, t_{DIC} inputs for this system were set to a more realistic, literature-mean of 2.54 Tmol C yr⁻¹ (da Cunha and Buitenhuis, 2013; Probst et al., 1994; Li et al., 2017) (for more details, see Appendix A). The outstandingly large Amazon watershed area (used for estimating rock weathering) and freshwater discharge compared to other basins drive a very high DIC load when using equation 9 from (Li et al., 2017)".

Table 2: The units are unclear and likely wrong. If Tg/yr, then the assumed stoichiometry of at least DOC, DIN needs to be provided. References provided in Table 2 are too generic, e.g. some labeled 1-3 do not even mention DSi.

We changed the units to Tg C/yr, Tg N/yr, Tg P/yr and Tg Si/yr so it is less confusing for the reader and moved the references into separate rows. We also removed confusing references and any references that compute inputs for the pre-industrial era e.g. Lacroix et al., (2020) or that use Global NEWS 2 as a source for their estimates.

Fig.3: color scale does not seem optimal. A log-scale might allow easier interpretation.

Given that the colorbar contains negative and positive values, we did not use a log scale. Instead, we narrowed the bounds of the colorbar to make the changes more apparent.

Table 3. Air-sea CO₂ fluxes seem to have the wrong sign

We used the common nomenclature for air-sea CO₂ fluxes with negative sign describing an uptake by the ocean and positive sign for outgassing to the atmosphere (DeVries et al., 2023; Fay et al., 2024; Gregor et al., 2024). To avoid confusion, the direction of the fluxes is now mentioned in the caption of the table.

DeVries, T., Yamamoto, K., Wanninkhof, R., Gruber, N., Hauck, J., Müller, J. D., et al. (2023). Magnitude, trends, and variability of the global ocean carbon sink from 1985 to 2018. *Global Biogeochemical Cycles*, 37, e2023GB007780. <https://doi.org/10.1029/2023GB007780>

Fay, A. R., Carroll, D., McKinley, G. A., Menemenlis, D., & Zhang, H. (2024). Scale-dependent drivers of air-sea CO₂ flux variability. *Geophysical Research Letters*, 51, e2024GL111911. <https://doi.org/10.1029/2024GL111911>

Gregor, L., Shutler, J., & Gruber, N. (2024). High-resolution variability of the ocean carbon sink. *Global Biogeochemical Cycles*, 38, e2024GB008127. <https://doi.org/10.1029/2024GB008127>

I.186. Presumably t_{DIC} plays only a small role compared to t_{DOC} because of the assumed compensation to DIC input by ALK input? Might be good to say this here.

This sentence was removed of the manuscript following the simplification of the experiments.

I.224 'and freshwater discharge' is misleading. If I understand correctly, freshwater discharge is identical in all simulations?

The sentence is indeed confusing and we changed it to, L221:

“The largest differences occur along the coastal periphery, especially near large river mouths, where Baseline's lack of riverine carbon and nutrients underestimates surface-ocean $p\text{CO}_2$ compared to the data-based products.”

I.235 ‘of dissolved carbon input’ - should it read dissolved organic carbon input?

In this sentence, we compared data-based products with ALL_{run} and this pattern simulated near large river plumes is driven by DC_{run} , which includes both dissolved organic and inorganic carbon. We changed the sentence to clarify this, L232:

“In ALL_{run} , the deviation of ECCO-Darwin surface ocean $p\text{CO}_2$ and air-sea CO_2 fluxes from the data-based products is reduced near large river plumes (Amazon, Paraná, Congo, Ganges, Yangtze, Amur) by the addition of dissolved carbon inputs.”

I.237 please provide a brief explanation of why model skill decreases here.

Done. We provide an explanation for each region in the following sentences, L234:

“However, in the Bay of Bengal, Sea of Japan, Canadian Arctic Archipelago, Caribbean Sea, and Siberian Shelf, model skill decreases with riverine inputs. While the data-based products converge on a CO_2 sink over the Arctic basin, adding dissolved carbon in ALL_{run} and the associated CO_2 outgassing increases the model mismatch in this region. However, large gaps in SOCAT data coverage in the Arctic Ocean may be responsible for this discrepancy, especially on the Siberian Shelf. This mismatch in the Arctic and in Antarctica may also reflect known biases in ECCO-Darwin's representation of the seasonal mixed layer and sea-ice dynamics (Lauderdale et al., 2016; Carroll et al., 2020). In the Bay of Bengal and Sea of Japan, the ocean CO_2 sink is increased by the higher uptake from enhanced-NPP in response to the addition of nutrient inputs in ALL_{run} . In the Caribbean Sea, the slight increase in mismatch is associated with carbon inputs from Tropical Atlantic rivers, such as the Amazon and Orinoco.”

I.248 why do you think input might be overestimated rather than underestimated? Could there be positive feedbacks, for example via redox-sensitive Fe and P cycling?

This is a great point, and LOAC can act in both directions by increasing or decreasing the amount of bioavailable material. Thus, we changed the sentence to, L253:

“In these regions, biogeochemical inputs might be overlooked due to the absence of LOAC processes in the model (i.e., parameterizations of estuarine mixing and biogeochemical processes) and the lack of a more realistic representation of organic matter remineralization, allowing for the advection of excess dissolved carbon and nutrients into the open ocean.”

I.282ff would be good to mention possible effects of explicitly accounting for denitrification, as done in some previous studies cited by the authors.

Done. We added a mention to denitrification in estuaries, L330:

“The absence of denitrification within estuaries (3–10 Tg N yr⁻¹; Seitzinger et al., 2005) could alter N:P stoichiometry and downstream air–sea CO₂ fluxes. However, our results do not include air-sea CO₂ fluxes associated with these land-to-ocean components.”

Seitzinger, S. P., Mayorga, E., Bouwman, A. F., Kroeze, C., Beusen, A. H. W., Billen, G., Drecht, G. V., Dumont, E., Fekete, B. M., Garnier, J., and Harrison, J. A.: Global river nutrient export: A scenario analysis of past and future trends, *Global Biogeochem. Cy.*, 24, GB0A08, <https://doi.org/10.1029/2009GB003587>, 2010.

I.296 why overestimated and not underestimated?

The referee is correct. We referred to the excess of advected riverine inputs but the response of the ocean carbon cycle can be either underestimated or overestimated. To clarify, we changed the sentence to, L314:

“As ALL_{run} deviates more from the data-based products in terms of surface-ocean pCO₂ and air-sea CO₂ flux in TROP-ATL, ARCT, and SE-ASIA compared to Baseline, the response of the ocean carbon cycle (source and uptake) to riverine inputs might be inaccurate due to the advection of excess elements to the open ocean.”

I.297 why only faster degradation and not slower?

Here, we mention only faster degradation as it refers to an excess of material that reaches the open ocean. With faster degradation, this excess would be reduced.

I.332 I do not understand ‘lack of nitrogen and silica-limited taxa’ The model explicitly resolves diatoms and 4 other phytoplankton species. Aren’t diatoms nitrogen and silica-limited in your model?

We agree that this is confusing. We referred to diatom species representative of the Arctic as ECCO-Darwin global configuration uses diatoms' traits that are not region specific (i.e., a global ecosystem). To clarify, we changed the sentence to, L351:

“We stress that the phytoplankton functional types in our global model are not representative of the specific Arctic Ocean ecology, and the lack of regionally-adjusted affinity for specific nutrients might hinder the model ecosystem response to riverine nutrients (Ardyna and Arrigo, 2020).”

I.362. I do see that this 'study is a critical step forward'. This would have to be justified in more detail.

We agree that we need to emphasize more the critical aspect of this study. This is addressed in the general response to both referees.

I.403 why only overestimated and not underestimated?

Agree. This goes in both directions. To clarify, we changed the sentence on L410 to:

“Overall, this could lead to an inexact t_{DOC} remineralization in some regions and thus excess of either ocean CO_2 outgassing due to an excess of DIC or advection of organic matter to the open ocean; a limitation that also exists in other GOBMs due to undifferentiated remineralization rates.”