Lai et al., present a new model of the Microbial Carbon Pump (MCP) within the Earth System Model cGENIE. The key novel advancement over other models of DOC is linking the production of recalcitrant DOC (RDOC) to the degradation of semi-labile DOC (SLDOC). cGENIE is a good choice for developing a model of the MCP as it is well suited to quantifying the long timescales needed and is widely applied to past climates where the role of RDOC is potentially important.

The manuscript needs substantial improvement in two key areas. Firstly, the model description has a number of potential errors that need addressing to ensure the manuscript accurately describes the model developments. Unfortunately, I couldn't get the code to compile so this is based on reading the code in the short of amount of time for reviewing so my apologies if anything is missed! Secondly, the authors need to demonstrate that their new development is a significant advance on previous models. Models of DOC can equally replicate observed [(R)DOC] in the deep ocean despite resolving radically different processes, e.g., Lennartz et al., (2024) vs. Hansell et al., (2012). The authors need to demonstrate the significance of their new MCP development beyond just matching the modern distribution, i.e., does it behave differently to previous models?

**<u>Potential issues with the model description</u>**

1) **Model description does not seem to match implementation:**

    a. Lines 126 – 127: "*SLDOC degradation is driven primarily by photodegradation*" – this contrasts against the equation on line 215 stating it is only a function of $\tau_{SL}$. The photodegradation option (bg_ctrl_bio_remin_RDOM_photolysis) in the model configuration file provided is commented out and defaults to off (false) in the definition.xml file.
    b. The gas transfer velocity equation does not match that in Ridgwell et al., (2007) as stated. Parameters l, a, b, c and the 0.25 do not appear in that paper or in genie's piston velocity code. This may be referring to combined processes in the model but I cannot find the a, b, c and 0.25 parameters in the code. The value for the scaling factor "l" has a value of 2.778e-6 which seems like the preindustrial molar ratio of $CO_2$ in the atmosphere but I cannot see how this relates to gas transfer velocity specifically.
    c. Line 159: "*Eppley's initial values, with a = 0.59 and b = 75.80*" – the model default value for b (par_bio_kT_eT) is 15.8. These values appear unchanged in the model configuration file or elsewhere in the code, so I can't see how 75.80 is arrived at.
    d. Lines 169 – 173: "*v=0.66*" – the configuration file has selected the Dunne et al., (2005) option for the particle export ratio (bg_opt_bio_red_DOMfrac='dunne') which is an empirical function dependent on temperature, productivity and euphotic zone depth. This looks like it overrides the default global constant value for v of 0.66, instead creating a spatially variable ratio of particle export to total export (see loc_rPOC on line 991 in biogem_biox.f90) from which v (ratio of DOC

production to total: 1-locrPOC) is calculated. The Dunne et al., (2005) scheme is also described in the manuscript.

e. "*While some POC sinks to the bottom waters and is eventually buried in sediments, a substantial fraction is remineralized within the water column.*" – this model has no sediment module enabled and therefore has a reflective sediment boundary where all POC hitting the seafloor is remineralised in the overlying grid-box (see Ridgwell et al., 2007).
f. Table 1:
    i. Scaling factor "l" does not obviously appear in Crichton et al., (2021) as described here.
    ii. The half saturation constant values do not seem to match those selected in the configuration file (bg_par_bio_c0_PO4=0.1E-6 and bg_par_bio_c0_Fe=0.10E-09 for PO4 and Fe respectively). I'm not sure of the relevance to the Crichton or Matsumoto references here either.
    iii. v: see comment d above, the configuration file appears to choose a spatially variable scheme not a global fixed value.
g. Figure 11: From the equations for LDOC, SLDOC and RDOC production, these are produced in a globally constant ratio of 0.9599:0.04:0.0001. This is also hard-coded in lines 997 – 999 in biogem_box.f90. Therefore, I would expect the LDOC and SLDOC panels at least to have the same spatial variability but scaled in magnitude. I'm also unsure from the text if the RDOC panel includes production from the SLDOC remineralisation.
h. There is no information on the spin-up protocol. The readme provided suggests the model is spun-up for 30k years from initial conditions. If true, this doesn't seem like sufficient time for RDOC to reach steady-state given its lifetime of 16k years.

**2) Model description of new developments is ambiguous and/or incomplete:**

a. POC remineralisation should have a sinking rate described.
b. SLDOC and RDOC remineralisation rates (k) are described as temperature-dependent yet the equations state that k = 1/tau. 1/tau also seems to be applied in the model code.
c. The transformation of SLDOC to RDOC is unclear as to whether the SLDOC is transformed to a RDOC remineralisation flux <u>or</u> to the RDOC tracer. I think the latter is the case from the code but the naming of flux terms is ambiguous.
d. Ideally there should be differential equations for LDOC, SLDOC and RDOC to fully describe the new DOC cycling in the model
e. The degradation rate constant for resolving temperature-dependent DOC (par_bio_remin_DOC_K1) has been changed from the value in Crichton et al., (2021). This should be included in Table 1.

**<u>Significance of MCP model development</u>**

A key feature of this model is the transformation of SLDOC to RDOC via parameter a, which distinguishes it as a representation of the Microbial Carbon Pump vs. other models of DOC cycling where different pools are unconnected (e.g., MESMO3). The manuscript presents the validation of DOC against observations, but the deep [(R)DOC] can be reproduced by very different models (Lennartz et al., 2024 vs. Hansell et al., 2012). This is because rates of RDOC cycling are smaller than circulation rates leading to a near-uniform distribution, such that the key constraint is the inventory which primarily constrains the magnitude of global fluxes in and out. To demonstrate this is a novel development, what new dynamics does cGENIE-MCP resolve?

One way to demonstrate this would be to show parallel results from the model when a=0. For example, it would be illustrative to see what happens to the DOC inventory across a range of perturbations with a=0.015 and a=0. This could be different atmospheric $CO_2$ values, differences in the DOC production or lifetimes, or differences in total production.

My concern is that the implementation of the MCP isn't significantly different to previous models. To illustrate this, I've used a steady-state analysis (see below). The production of RDOC in other models ($Fin_{RDOC}^{fix}$) is typically a fixed fraction of total DOC production:

$$Fin_{RDOC}^{fix} = \Gamma * v * f_{RDOC} \quad (1)$$

where $f_{RDOC}$ is the fraction of RDOC production (e.g., equivalent to $1 - f1 - f2$ in this manuscript) and the parameters follow those in this manuscript. For clarity I have treated this as already integrated over the surface layer, e.g., units of Pg C and Pg C $yr^{-1}$. In the cGENIE-MCP model the production of RDOC has an additional term reflecting the production arising from SLDOC remineralisation:

$$Fin_{RDOC}^{MCP} = \Gamma * v * f3 + a * \frac{SLDOC}{\tau_{SL}} \quad (2)$$

Assuming steady-state, equation (2) can be rewritten in a comparable form to eqn (1):

$$Fin_{RDOC}^{MCP} = \Gamma * v * (f3 + a * f2) \quad (3)$$

where $f2$ is the fraction of SLDOC production from total DOC production. If production and the lifetime of RDOC are similar between models then $f_{RDOC} \equiv (f3 + a * f2)$ to achieve the modern inventory of RDOC at steady-state. In cGENIE-MCP the parameters $f2$, $f3$, $a$ and $v$ are fixed values (note the question about v above however). As such, the two different models (equations (1) and (3)) can be seen to have similar steady-state dynamics where the dynamic term is export production ($\Gamma$) which is scaled by a fixed fraction. The steady-state constraint (eqn 6) also implies that RDOC may not be sensitive to a change in $\tau_{SLDOC}$.

This is a simple analysis which doesn't account for different nutrient feedbacks between SLDOC and RDOC cycling but hopefully it encourages the authors to demonstrate that their model *behaves* in a novel way to other models. What happens if you have a change

in SLDOC production or remineralisation? Do the models behave differently for the same change in atmospheric $CO_2$? If this has a notable effect on RDOC, which would not be the case in previous models, then this model would be a significant advancement in resolving DOC dynamics.

**Choice of results to evaluate the model**

Overall, the results section is heavily weighted towards assessing major ocean tracers not DOC. I understand the authors want to demonstrate their new development doesn't degrade overall model performance but arguably adding a component of organic matter cycling that is characterised by relatively small rates is probably not likely to perturb tracers like PO4, $O_2$ or DIC that much. Table 2 in the manuscript summarises this immediately such that the following text and figures don't add much more highly relevant information. I would suggest minimising this part of the results substantially and/or relocating to supplementary.

It is useful to show temperature as it determines some rates in the model but the only feedback between ocean biogeochemistry and the physical model I'm aware of in cGENIE is atmospheric $CO_2$, which here is restored to a preindustrial value in these runs. Therefore, temperature does not need evaluating to the extent it is here.

The description should increase its evaluation of DOC in the model since this is the key focus of the new development. The existing evaluation is much briefer and less quantitative for DOC than of other tracers limiting comparison against previous developments and current constraints. Some of this appears in supplementary and would be more informative in the main text. It would be good to see some standard metrics comparable to Table 1 in Hansell (2013) such as inventory (Pg C) production rate (Pg C $yr^{-1}$), removal rate, lifetime (years) for each of the DOC components. It would also be helpful to see the cGENIE and cGENIE-MCP comparison which is shown for other tracers but not DOC – is the labile DOC similar between models? It's notable that isotopes are omitted in this manuscript when one of the main advantages of cGENIE is its ability to resolve isotopes! The bulk radiocarbon age is a key constraint on DOC cycling which could be added to the evaluation.

**Functionality of the model**

Several things make the model less functional or harder to use from the description:

a) The tracer names are misaligned between the manuscript and model (LDOC = DOC; SLDOC = RDOC, RDOC = URDOC in the manuscript and model respectively). Ideally, these should be the same.
b) Some MCP parameters are hard-coded (f1, f2, a) which reduces the ability of users to explore the MCP. In your discussion you discuss being able to explore the dynamics of the MCP in detail with this model but not being able to change these parameters is a very big limitation to this.

c) The activation energy of LDOC is hard-coded to the labile POC parameter so there is no ability to decouple these. Is it reasonable to assume POC and LDOC will be always treated the same?

**Evaluation and presentation**

a) The projection choice for Figure 2 limits comparison against other figures. It has no longitude or latitude values.
b) The RMSE presented is actually the centered-RMSE which is the underlying statistic for the Taylor diagram (Jolliff et al., 2009). It would be more informative to show a Taylor diagram so we can assess the spatial distribution and variability of tracers between the two model versions.
c) Please also consider other misfit functions to assess the model. Kriest et al., (2010) provides a good overview of approaches as well as the impact of volume-weighting. This discussion may be relevant here because DOC is considerably more variable in the upper ocean than the deep ocean which these alternative functions can deal with.
d) Why are there no quantitative analysis of the DOC comparison, e.g., RMSE?
e) What is the reason for using specific regions for the model-data comparison? These have been changed from those used in Crichton et al., (2021) with no justification beyond "limited relevance or observational data availability" (lines 251-252). What does this mean and what is the basis for choosing these regions?
f) What do the errorbars represent in the vertical profiles?
g) The model description does not clearly distinguish between *new* developments to cGENIE and previous developments. I would suggest to minimise descriptions of existing processes such as air-sea gas exchange, unless they provide important context, to avoid assumptions that they are included in the new developments of cGENIE.
h) There are numerous descriptions of the model fit to observations throughout that aren't supported quantitatively, e.g., "good accuracy" "show good agreement" "more accurately" "moderate discrepancies" "approximately reasonable". This language should be modified unless it is directly related to a quantitative measure.

**Specific Comments**

Line 56: "observed RDOC" – more accurately this should be observed deep-ocean bulk [DOC] as there are competing hypotheses about whether this is labile or a mixture of compounds with different reactivities, e.g., Follett et al., (2014).

Lines 59 – 60: The deep ocean [DOC] and radiocarbon signature issues can be alleviated by adding a simple RDOC pool without MCP parameterisations. The MCP parameterisations add specific dynamics related to why the RDOC accumulates.

Lines 78 – 81: It would be good to explicitly explain how MESMO represents SLDOC and RDOC to enable direct comparison with cGENIE-MCP here.

Lines 103 – 104: The physical circulation parameters are derived from Cao et al., (2009) configuration but you are using a different continental grid to them (worlg4 vs worjh2). I think the Ward et al., (2018) citation might be more appropriate.

Lines 119 – 120: Though some details in that paper are relevant here, Ward et al., (2018) describes the trait-based ecosystem so it might help to clarify this distinction here. Are the parameter values or equations used developed in the Matsumoto or Tanioka papers?

Line 121: "particulate organic matters (POMs)" – usually this would just be singular not plural.

Line 163: "primary production" – GENIE resolves net export production which is equivalent to net community production (NCP) across large enough spatial scales and temporal scales. Primary production is ambiguous to this difference and should probably be avoided.

Line 228: "a is the conversion rate" – this does not have units of a rate

Lines 310 -311: or equally that the addition of RDOC has a negligible effect on the large-scale $PO_4$ distribution?

Lines 347 – 350: A more appropriate experiment would be a run forced with historical atmospheric $CO_2$ concentrations from the preindustrial to present-day continuing from your preindustrial spin-up. Or alternatively compare against the GLODAP DIC observations with the anthropogenic DIC component removed (Cant in GLODAP).

Line 371: "DOC is a key component of ocean carbon cycling" – maybe my comment is not exactly relevant to here but this refers to multiple things. The recycling of labile DOC is crucial for models to resolve productivity. RDOC is potentially important for carbon storage though this is subject to the timescale discussed.

Lines 377 – 380: "cGENIE-MCP model exhibits improved agreement with observed DOC distributions in both surface and deep layers [compared to MESMO 3]" – please quantify this! Can you get the MESMO-3 results and compare concentrations, distributions, RMSE? Otherwise, this is an unqualified statement that cannot be verified.

Lines 398 – 403: The supplementary figures and tables (Fig S6, Tables S2 and S3) need to be in the main text as these are essential comparisons against observations and models of DOC, which is the key focus of this manuscript.

Line 441: "lifetime of months to years" – you have prescribed a fixed lifetime in the model, it is not variable.

Figure 13: This is an unusual way of plotting meridional distributions which is pretty hard to interpret. It would be much clearer to show zonal averages for each DOC pool such as in Figure 12.

Lines 452 – 454: “Slight elevated RDOC concentrations … may reflect entrainment of surface-derived labile semi-labile DOC…” – is it possible to back this out of the model and demonstrate?

Lines 454 – 457: “These results underscore the dynamic role of LDOC and SLDOC” – I don’t think this statement is supported because you have only shown a steady-state results. To support this you need to show that the model behaves differently to some perturbations compared to the model without the interactions between DOC pools that underpin the MCP concept.

Section 4.2: This version of GENIE resolves net export production not primary production. It has no representation of some of the processes described here like top-down grazing pressure, bacterial and viral lysis, particle solubilization – the net effect of these processes are parameterised by the Michaelis-Menten uptake scheme you are using. This discussion should be amended to better reflect what the model is actually doing and how it represents the complex reality.

Line 518: “huge RDOC” – please quantify this! The DOC inventory is around 700 Pg C whereas the regenerated DIC pool from the Biological Carbon Pump is around 1700 Pg C which undermines this argument. I would argue it is the potential dynamics that are crucial here – how likely, and by how much, could RDOC and regenerated DIC change in response to perturbations or different factors?

Line 529: “coupled of ocean carbon pump” – it’s not clear what this is referring to.

Figure S6: Why is the Sargasso sea singled out here and is this modelled or observed concentrations? What area does panel B correspond to – global average? Can the comparison against Wang et al., (2023) be expanded?

Tables S2 and S3: This is useful but seems like it could easily expanded. Could you add comparison against MESMO here? Are there other datasets and models that could be compared against? Can the analysis be expanded to more regions?

**References**

Follett et al., (2014) Hidden cycle of dissolved organic carbon in the deep ocean *Proceedings of the National Academy of Sciences* , Vol. 111, No. 47 p. 16706-16711

Hansell et al., (2012) Net removal of major marine dissolved organic carbon fractions in the subsurface ocean. *Global Biogeochemical Cycles* , Vol. 26, No. 1

Jolliff et al., (2009) Summary diagrams for coupled hydrodynamic-ecosystem model skill assessment. *Journal of Marine Systems* , Vol. 76, No. 1 – 2, p. 64 – 82

Kriest et al., (2010) Towards an assessment of simple global marine biogeochemical models of different complexity. *Progress in Oceanography* , Vol. 86, No. 3—4, p. 337 – 360

Lennartz, S. T., Keller, D. P., Oschlies, A., Blasius, B., & Dittmar, T. (2024). Mechanisms underpinning the net removal rates of dissolved organic carbon in the global ocean. Global Biogeochemical Cycles, 38, e2023GB007912. https://doi.org/10.1029/2023GB007912

**Steady-state analysis**

Using the same parameters in the manuscript but expanding the remineralisation terms and assuming this is integrated across the surface layer for clarity, the equation for the production of RDOC is:

$$F_{z=he}^{RDOC} = \Gamma * v * f3 + a * \frac{SLDOC}{\tau_{SLDOC}} \quad (4)$$

We can assume steady-state for the governing SLDOC differential equation ($\frac{dSLDOC}{dt} = 0$) and rearrange to find an expression for $\frac{SLDOC}{\tau_{SLDOC}}$ (eqn 6):

$$\frac{dSLDOC}{dt} = \Gamma * v * f2 - \frac{SLDOC}{\tau_{SLDOC}} \quad (5)$$

$$\frac{SLDOC}{\tau sdoc} = \Gamma * v * f2 \quad (6)$$

Eqn (6) can be substituted into eqn (4) and simplified to get an expression for RDOC production that is comparable to previous models:

$$F_{z=he}^{RDOC} = \Gamma * v * (f3 + a * f2) \quad (7)$$