Author's overall comments

We greatly appreciate the time and effort from each of the reviewers along with the constructive comments and feedback. We have summarised here the more significant changes that have been made to the manuscript (all in response to reviewer suggestions) and below answer the specific comments.

1) We have improved the explanations of the novel aspects of the model, why these were made and why they are appropriate for the intended use of the model (investigation of ULN). These changes have been incorporated into the abstract, introduction, discussion and conclusions sections.

2) We have made changes in section 2.3 to the description of the model. This has involved improving some of the model equations and the descriptions of nutrient cycles. We have also re-ordered the section. Changes to this section also include reference to a full list of model tracers (provided in the supplementary information).

3) Apparent Oxygen Utilisation (AOU) and N* data-model comparisons have been added.

4) We have also added a comparison to a similar model (CLIMBER-X).

5) The potential implications of differences/biases between the model and the real ocean are now discussed at greater length.

More specific comments to point raised are provided inline below in blue. On occasions we have pasted the amended text of the manuscript into this reply. When we have done so it is *italicised* and indented.

Review 1

General comments:

Stappard and co-authors detailed the NutGEnIE biogeochemistry module extension to an existing Earth system model. Their motivation was to create a modelling system capable of extended simulations in order to investigate controls on ocean primary productivity over long timescales. While the model would represent a substantial contribution to the field, I found several issues with the manuscript in its current form, which I'm detailing below, that warrant addressing by Stappard et al. before continued evaluation. In particular, the methods section should be heavily revised to improve clarity, and the discussion section should be greatly expanded to include a more thoughtful analysis of the model biases as compared to observational products.

We are appreciative of this review, specific and technical comments. As mentioned above, the methods section has been heavily revised as requested.

Specific comments:

(1). I'm still uncertain of the total number of explicit biogeochemical tracers the model carries. Typically, model development manuscripts include a full list of explicit tracer equations (often in the supplemental material). I recommend this exercise here to improve clarity, especially as it pertains to the iron cycle in the model.

This is a helpful comment and aligns with comments from another review. We have added a full list of tracers with initial values (supplementary information) and referenced this at the beginning of section 2.3 with the sentence.

> *The focus of this section is to describe the nutrient cycles and utilisation of those nutrients by PP; a full list of ocean tracers enabled in the NutGEnIE configuration is provided in provided in Table S2.*

(2). Upon first read, I was confused how POM was treated in the model, since it was discussed heavily in Sections 2.3.1 to 2.3.3. It was not clear until page 8 of the manuscript (Section 2.3.4) that it is implicitly represented. I recommend a reorganization of section 2.3 to (1) first describe the

equations for nutrient uptake, (2) its portioning into DOM/POM, and (3) the remineralization scheme of DOM/POM.

We have reworked the ordering of section 2.3 so that the aspects are covered as follows

1. The concept that nutrient uptake represents ocean primary production and the concepts of partitioning into POM and DOM.
2. Nutrient uptake by other phytoplankton and diazotrophs.
3. Nutrient (phosphorus, nitrogen and iron) cycles
4. The remineralisation scheme.

This is a close match to the suggested reorganisation, and we hope addresses the concerns raised.

(3). It is unclear why NutGEnIE necessitates a burial fraction parameter (k_BF). Shouldn't some portion of the POM flux make it to the bottom of the deepest grid cell, and shouldn't that represent the portion that is buried? Explaining the rationale for such an enhanced burial flux is needed.

We acknowledge that it would have been possible to make the spatial dynamics of burial more realistic, but, as with all models, simplifications have to be made. We have added the manuscript section below to acknowledge the reviewer's point.

> *We intend to use the model to investigate the ULN and for this purpose it is essential that nutrient cycles are open. Therefore, in NutGEnIE a removal (burial) flux is applied by removing a fixed proportion (k_{BF}) of nutrient uptake by other phytoplankton and diazotrophs from the modelled ocean. This represents a simplified instantaneous sediment burial term that is not coupled to POM remineralisation dynamics.*

(4). The authors briefly introduce top-down control of autotrophs by grazers in Section 1.2, yet do not discuss any model caveats by ignoring this process in NutGEnIE. In particular, this can be an important control on surface primary production in HNLC regions (i.e., some suggest it is 'bottom-up' via iron limitation, while others suggest top-down controls lead to higher observed surface nutrients compared to other regions). This caveat should be outlined in greater detail within the discussion section, especially as it relates to biases in model performance (e.g., surface nitrate and phosphate comparisons in HNLC regions).

It is true that top-down control of autotrophs by grazers is not explicitly modelled, and again we feel that is appropriate in this model for the question it is designed to investigate. However, we have added the following sentence at the start of section 2.3.

> *NutGEnIE does not maintain the biomass of phytoplankton or model grazing by zooplankton; it utilises the phosphorus concentration within cells to represent the population size of phytoplankton.*

(5). I recommend elaborating more, or at least clarifying, the iron cycle in the model. Including the equations for each explicit iron tracer would especially helpful in clarifying the underlying dynamics of iron within the model.

The sections relating to the iron cycle have been updated. This along with the specific list of tracers hopefully resolves the request for greater clarity. Specific points are mentioned below in response to other comments.

(6). L. 184 - 185: Based on the wording here, it is unclear if anammox is represented in NutGEnIE at all. In Figure 2., it does not seem represented. If the authors choose to omit anammox, this should be discussed as a caveat to the model, since anammox is responsible for a considerable portion (~28% based on remineralization stoichiometry, see Babbin et al. 2014 - Science) of fixed-N removal in oxygen-minimum-zones.

We confirm that the model does not have a specific anammox removal pathway. We agree with the point that anammox is an important removal pathway in the ocean. As mentioned in the manuscript

NutGEnIE is based on the existing cGEnIE model and we have not made significant changes to the nitrogen cycle of cGEnIE that has been used in previous work (Monteiro et al., 2012; Naafs et al., 2019). With all modelling there are compromises to be made and this is one within NutGEnIE, inherited from cGEnIE. We note that both denitrification and annamox remove bioavailable nitrogen (which is almost all nitrate) and convert it to dinitrogen and that both processes only occur in low oxygen environments. Since their environmental dependencies and biogeochemical impacts are more or less the same, we contend that the model nitrogen cycle is an acceptable representation of the real ocean nitrogen dynamics and that it is not necessary to represent the two processes separately.

(7). L. 224: The authors briefly mention 'particle concentration' (Cp) here, but it wasn't clear to me what they meant by this. POM is only implicitly represented here, so how do they quantify particle concentration?

In the model the scavenging process is computed as a function of particle remineralization and therefore depends on the rate of formation of particles within an upper ocean grid cell (calculated from the rate of nutrient uptake). We appreciate the comment, and we have added the following clarifying sentence directly after the discussion of scavenging (k_{sc}).

> *The rate of scavenging is calculated as part of particle remineralisation which in turn is calculated as a function of the rate of creation of new particles in the upper ocean which in turn is calculated from the nutrient uptake rate.*

(8). Eq (12 - 14): I'm confused by these equations. For example, in Eq 12, this is just a fraction (Michaelis-Menten function ranging from 0 - 1), not the actual rate of aerobic remineralization. It would be helpful to first define the total depth-dependent remineralization rate as the divergence of POM flux in each grid cell (i.e., R_remin) . Then, for example, equation 12 would be better represented as [O2]/(K_O2 + [O2]) * R_remin. Also, it is unclear how the sum of the rates in Equations 12 - 14 equals the total remineralization rate of POM.

We have modified the organic matter (OM) remineralisation section to remove the confusion. OM is remineralised through the water column according to an exponential function of depth. Eq (12 - 14) from the original manuscript (now numbered 17-19 in revised version) are used to determine usage rates of electron acceptors (O_2, NO_3, and SO_4). The equations take account of both electron acceptors abundance and the inhibitory effect of electron acceptors with higher free energy yield.. These equations are now 17 – 19 in the revised manuscript and we make it clear that the R values are the relative amount of OM remineralisation. "the process of OM remineralisation are relative to R_i in Eq. (17 to 19)" Lines 343-349.

(9). Eq (15 - 17): Is particulate organic iron (POFe) not also remineralized in the model? Also, it is unclear where and how the Gibbs free energy yield values are used (I also recommend adding subscripts to the distinct Gibbs values, e.g. DeltaG^(o)_(O2)). Are they folded into the calculation of the inhibition constants? If so, it would be important to include their formulas, either here or in the supplementary material.

Yes, OM contains Fe that is also released as part of the remineralisation process. The elemental ratio of OM is determined by a constant R_{Fe}. Eq (15 - 17) (now numbered 20-22 in revised version) have been modified and are hopefully now clearer to the reader.

$$OM + 138O_2 \longrightarrow 106CO_2 + 16NH_4 + PO_4 + R_{Fe}^{OPhy}Fe + 122H_2O \quad (20)$$

$$OM + 94.4HNO_3 \longrightarrow 106CO_2 + 55.2N_2 + PO_4 + R_{Fe}^{OPhy}Fe + 177.2H_2O \quad (21)$$

$$OM + 53SO_4^{2-} \longrightarrow 106CO_2 + 16NH_4 + 53S^{2-} + PO_4 + R_{Fe}^{OPhy}Fe + 106H_2O \quad (22)$$

The Gibbs free energy yield values were previously provided for each remineralisation pathway however we do not believe these were necessary and have been removed. The values of the inhibition constants are provided in Table 6.

(10). Figure 4: I believe the authors are missing the nitrification source term for the nitrate panel. Also, here they specify nitrogen fixation as an input to the nitrate budget, but it is not represented in the tracer equation (Equation (4)).

In figure 4 we show the nutrient budgets at end of 50 kyr spin up and also the fluxes into and out of the ocean as a whole. Figure 4 does not show fluxes related to internal ocean cycling. Internal cycling through nutrient uptake, remineralisation and nitrification are not shown. We have amended the figure caption to make this clear.

The impacts of nitrogen fixation are in fact included in the equations related to nitrate (Equation (4 and 5) in the original manuscript) but not in an obvious way. We do appreciate the comment and have now modified the relevant equations (Equation (9 to 11) in revised manuscript) to make this clearer. We have also added explanation:

> *Nutrient uptake by other phytoplankton is dependant on the availability of DIN, i.e., the combination of NO_3^- and NH_4^+ concentrations. The uptake rates of NO_3^- and NH_4^+ are represented by Eq. (7 and 8) with NH_4^+ being preferentially utilised (Naafs et al., 2019).*
>
> $$Up_{NH_4} = min\,(\Gamma_N^{OPhy};\ NH_4^+) \quad (7)$$
>
> $$Up_{NO_3} = \Gamma_N^{OPhy} - Up_{NH_4} \quad (8)$$
>
> *The governing equations for NH_4^+ and NO_3^- are below.*
>
> $$\frac{\partial NH_4^+}{\partial t} = -Up_{NH_4} - O_{Nit} + \lambda DON \quad (9)$$
>
> $$\frac{\partial NO_3^-}{\partial t} = -Up_{NO_3} + O_{Nit} - R_{NO_3} + S_{NO_3} \quad (10)$$
>
> $$\frac{\partial DON}{\partial t} = v(\Gamma_N^{NOPhy} + \Gamma_N^{NDiaz}) - \lambda DON \quad (11)$$
>
> *Nitrogen fixation is not explicitly represented in Eq. (9 to 11) but it is reflected. Diazotroph organic matter (Γ_N^{Diaz}) is created without reducing concentrations of DIN, via nitrogen fixation, hence the Γ_N^{Diaz} term is not subtracted from nitrate or ammonium concentration in Eq. (9 and 10). However, all nutrient uptake ($\Gamma_N^{NOPhy} + \Gamma_N^{NDiaz}$) is added to the DON tracer concentration in Eq. (11) and ultimately available for remineralisation.*

(11). In general, text below equations are often missing units for parameters. It would helpful to include the units, and reference tables when mentioning parameters throughout the methods section.

This is an area when differing requests have been expressed by the different reviewers. We have settled on summary tables (in section 2.3.8) where constant values and their units are provided in a single place rather than within the text of the manuscript.

(12). In Section 2.3.7, only Fe is represented in equations (9) and (10), yet the authors mention that complexed iron (FeL) is also available for biological uptake. Did they mean to write FeL in equations (9) and (10)? If so, wherever the authors mention Fe in the text, should they instead write FeL for clarity?

Thank you for this comment which was also mentioned by another reviewer. The equations in question are now equations (3) and (4) in the revised manuscript. Fe has been replaced by Fe_T which is the sum of free iron Fe' and FeL. This revised text and equations are shown below.

> *Primary production (PP) is accounted for via the production of organic matter resulting from the uptake of nutrients. The nutrient framework is configurable but here it is dependant on DIN (= NO_3 + NH_4), PO_4 and Fe_T (= Fe' + FeL). Where Fe_T is the total bioavailable iron, which is discussed , along with the processes of iron complexation, in Sect. 2.3.6. Nutrient*

uptake by non-diazotrophs (other phytoplankton) results in the production of organic matter (Γ^{OPhy}) at a rate subject to Eq. (3) (Monteiro et al., 2012).

$$\Gamma^{OPhy} = V_{max}^{OPhy}(1 - f_{ice})\gamma^T\gamma^I \, min\left[\frac{PO_4}{PO_4+K_P}; \frac{DIN}{DIN+K_N}; \frac{Fe_T}{Fe_T+K_{Fe}}\right] min\left[PO_4; \frac{DIN}{R_N^{OPhy}}; \frac{Fe_T}{R_{Fe}^{OPhy}}\right] \quad (3)$$

.....

$$\Gamma^{Diaz} = V_{max}^{Diaz}(1 - f_{ice})\gamma^T\gamma^I \, min\left[\frac{PO_4}{PO_4+K_P}; \frac{Fe_T}{Fe_T+K_{Fe}^{Diaz}}\right] min\left[PO_4; \frac{Fe_T}{R_{Fe}^{Diaz}}\right] \quad (4)$$

(13). In Section 2.3.7, it is unclear if ligands are an explicit tracer. If they are, then Figure 3 suggests particulate iron can be created in depth cells below the euphotic zone, yet equation (11) states POM flux is only set by euphotic zone values. Please clarify.

This comment has to some degree been covered by earlier responses, but we again thank the reviewer for the suggestion which has allowed us to increase clarity. Ligands are a tracer and are included in the list of tracers in the supplemental materials.

Yes, it is the case that particulate iron can be created at all depths through the scavenging process. This was covered in the response to comment (7) with the key sentence being “The rate of scavenging is calculated as part of particle remineralisation which in turn is calculated as a function of the rate of creation of new particles in the upper ocean which in turn is calculated from the nutrient uptake rate.”

(14). L. 459: There are missing details regarding the initial conditions of the model runs.

The supplemental material (Table S2) now lists each model tracer with initial values. In addition, the value of each constant within the model is provided in section 2.3.8.

(15). L. 477: “ocean physics is not the focus here, so the properties are not discussed in detail”. While I generally agree with this statement, there are some notable biases (compared to WOA, treated here as reality) in both temperature and salinity that could cause stratification and other errors in NutGEnIE. For example, in the Pacific, there is anomalously low temperature in between the subtropics (Figure 5c). Would that not have an influence on the delivery of deep nutrients to the surface there? For instance, there appears to be a very similar bias distribution for surface phosphate and nitrate (Figures 7c and 8c). Similarly, the transect biases in temperature match the patterns in the biases of nutrients. For example, the model appears to be too warm in the deep Atlantic, and is too low in PO4 and NO3. In the deep Pacific, the model is too cold, yet also high in PO4 and, to a lesser extent, NO3. The authors could potentially strengthen their validation exercises by relating some of the nutrient biases to stratification differences.

This is a helpful comment, the model biases in temperature and salinity have been noted in section 3.2 and related to nutrient biases in the discussion.

(16). Similarly, in Section 3.3.4, I believe some of these biases in surface oxygen are related to co-located biases in surface temperature. For example, Figure 5c suggests there is a cold bias in the surface of the subtropical Pacific, where there is also anomalously high oxygen values (Figure 10C).

This is another helpful comment. We have added comments to the discussion section considering the co-location of biases in surface temperature (Fig 5 c) and surface oxygen (Fig 10 c).

(17). Figures 7c and 8c: These surface biases are quite large, especially considering the model is designed to study limiting surface nutrients. For example, in Table 6, the authors report a surface mean nitrate value from WOA of 6.0 umol/kg, whereas NutGEnIE reports nearly half that value (2.9 umol/kg). I would have liked to see a more thoughtful discussion on why these biases don’t impact the authors’ confidence in the model’s performance.

We concur that the biases are significant, and we have commented more closely on them. We suggest that the model performs well in terms of spatial nutrient limitation and spatial dynamics of nutrient concentration. The proposed use will involve control runs in parallel to fertilisation runs, in this scenario we suggest that the dynamics of the model are more important than absolute concentrations.

(18). L. 673: I'm not sure the model results support this conclusion, especially considering the model greatly underestimates both N-fixation (L. 645) and denitrification (L. 654) when compared to other studies (Section 3.1.1.). Why do the authors think that is the case in their model runs? If they believe this does not impact model performance, this should be elaborated on in the discussion.

We do content that the model is a reasonable representation of overall ocean dynamics that have been considered. We have added additional comments relating to biases and additionally caveat those biases when making the comment. We take on board the suggestion to elaborate further on model performance in the discussion and have done so.

(19). I would have liked to have seen some additional validation figures. Comparing model AOU (apparent oxygen utilization) to WOA estimates could help improve confidence in the representation of remineralization in the model. Similarly, N* can be extracted from both the model and WOA to assess model performance in generating spatially-varying N-fixation and denitrification signatures. Finally, oxygen-minimimum-zone (OMZ) thickness comparisons (e.g., thickness of waters within each cell-profile that are less than 60 umol/kg O2) would improve overall confidence in denitrification within the model, since OMZs are crucial regions for balancing the global N-budget.

We have tried to strike a balance with the quantity of comparisons that are made between the model dataset and ocean datasets, including enough to allow readers to assess model-data agreement without including so many that the paper becomes unwieldy. We take on board the suggestions of additional analysis and have added both AOU and *N** analysis in the supplementary materials. Both aspects are also now mentioned in the discussion section.

Technical comments and corrections:

Again, we thank the review for the detailed technical comments and correction. For these technical comments we have largely just added a short comment to state the action that we have taken.

L.30: "Net primary production (NPP) **represents the** total rate..." (suggested edit) - This has been done

L.32: "phytoplankton **produce** biomass" (typo?) - This has been done

L.37: "Nutrient supply to the euphotic zone acts as **a** fundamental control on ocean PP levels". (typo, I also recommend adding a semicolon before "this supply and subsequent growth limitation") - This has been done

L.41: "grazing reduces the total amount of photosynthesis" (this could be rewritten for clarity). - This has been done

L.48: "Again, studies have proposed methods of modeling temperature limitation" (I suggest rewording or merging with previous sentence for improved flow of manuscript). - This has been done

L.50: "Elements (C, H, N, P, O and S)" (I suggest defining these explicitly or omit) - This has been done

L.51: "such as proteins **and** nucleic acids." (I suggest removing 'etc.') - This has been done

L.55: "Addition of the proximate limiting nutrient (PLN) stimulates immediate growth" (I suggest removing the comma after (PLN)) - This has been done

L. 72: “**Their work suggests the** stratified subtropical gyres” (suggested text addition) - This has been done

L.85: ‘The modelling’ should be ‘the modeling’ (no capitalization after semicolon). Or, could be rewritten as “Deutsch et al. (2007) only conducted **model simulations** over short timescales to a modern ocean steady-state” - This has been done

L.90: Recommend removing “but at the same time” - This has been done

L.95 - 99: I’m not sure this paragraph is necessary. We agree and the paragraph has been removed.

L. 102: Consider rewriting the sentence starting with “Such investigations” to place the information and citations outside of the parentheses. - This has been done.

L. 105: Remove comma after “(cGEnIE)” - This has been done

L. 110: Fix bad reference to section. - This has been done

Figure 1: Consider adding longitude and latitude tick labels to this and other map figures. – We made the decision to remove labels from surface figures to gain as much space as possible, this was particularly important when part of a panel figure. We have also decided to do this consistently throughout the manuscript.

L. 138: “biogenically-induced chemical fluxes (ref) and **is** capable of” (suggest adding hyphen and ‘is’) - This has been done

L. 142 - 151: Please be consistent when using chemical abbreviations here and in the rest of the manuscript. Typically, it is best to define chemical abbreviations before using abbreviations throughout the rest of the manuscript (i.e., “that include dinitrogen (N2), ammonia (NH4), calcium (Ca), and sulphate (SO4)”). Also, N2 is italicized here, when other forms in this paragraph are not. Please also include any negative or positive charges on NO3, PO4, NH4, SO4, etc, wherever they appear in the document. - This has been done

L.149: “By default, results are output as annual **averages for each grid cell**” (suggest removing ‘figures’, since models only output numerical data, not visuals). - This has been done

L.150: “giving the possibility of results output relating to shorter timeframes” (suggest rewording) - This has been done

L.153 - 156: Please be consistent when using parentheses to split sentences. For example, here the authors use ‘a)’ ‘b)’ when elsewhere they use ‘(a)’ and ‘(b)’. - This has been done

L. 156: “we detail the most pertinent **features**” (suggested edit) - This has been done

L. 157: “nutrients taken by phytoplankton are **instantly** converted to POM and DOM in the surface ocean” (suggested edit for clarity) - This has been done

L. 166: ”The nutrient uptake terms (…) only have a value in the surface layer”. Is this by design? Does the model restrict uptake to the top grid cell, or is this a result of the model? Please clarify. Yes the design is that nutrient update only occurs in the surface layer.

L. 183: “Like denitrification**, anammox** converts…” (add comma, remove capitalization of Anammox) - This has been done

L. 188: Please rewrite this sentence for clarity (there are some typos), and please define RP, RN, DDFe, and BBFe in the text before using their associated abbreviations. - This has been done The authors could also reference the associated supplemental figures in the caption of Figure 2.

Figure 2: Please define POP, PON, and POFe in the caption, since they are labeled on the Figure panels. - This has been done I also recommend redesigning these figure panels such that only

explicit tracers, and the fluxes that couple them, are represented. For example, here the authors use green circles to define other phytoplankton and diazotrophs, which may confuse readers into thinking these are explicit tracers.

L. 204: Please consider adding units when describing the components of equations (i.e., mmol / m3 d). We have stuck with the approach of providing a table of all components, along with units and values in Section 2.3.8

L. 208: “bit” should be “but” - This has been done

Eqs (2) and (4): How are ’S_(PO4)’ and ’S_(NO3)’ different from ‘RP’ and ‘RN’? If they are identical, consider just using one term. - This has been done Also, the authors mention ’S_(PO4) being configurable by a parameter on line 210? The magnitude of the flux is configurable via a parameter, additional text added to clarify this. In Figure S1, surface nutrient inputs are supplied via a forcing field rather than a parameter. Please clarify what is meant here. Also, I recommend adding supplementary figures (similar to Figure S3) that detail the magnitude of the surface forcing for both nitrate and phosphate. These are both covered by Figure S1.

Eq (4): I recommend using consistent terminology for reaction rates. For example, why use the delta symbol for nitrification and the ‘R’ symbol for denitrification, when they are both reactions in the model? Perhaps it is easier to use ‘R_nit’ and ‘R_den’ for clarity? This has been done

Eq (6); ‘B_(Fe)’ is labeled as ‘BFe’ in Figure 2. Please be consistent between figure labels/captions and equations. This has been done Also, is ‘free dissolved iron III’ another explicit tracer in the model? The model has a single iron tracer and has now been clarified via this list of tracers provided in supplemental materials.

L. 217: Please reference Figure S3 after mentioning ‘DDFe’, either here for after describing the re-gridding of Mahowald et al. This has been done

L. 232: It would be helpful to define units for these terms in the equation. For example, the units of V^(OPhy)_(max) are unclear since the model does not explicitly represent biomass. The authors mention the parameter values on line 255, but please move them to earlier in the text when they are first defined. We have stuck with the approach of providing a table of all equation components, along with units and values in Section 2.3.8

L. 234: I’m confused by the light limitation term. In Figure S37 the text mentions that “lower values of ‘K_light’ indicate that light is more limiting to nutrient uptake”. Where does ‘K_light’ fit into the equation on line 234?

The K temps in Figure S37 are equivalent to the limiting components of the equation on line 234. Additional text has been added to the caption of Figure S37 to S40 as follows

Figure S37 - K_{Light} is equivalent to the γ^{l} term in Eq. (3) and (4).

Figure S38 - K_{Temp} is equivalent to the γ^{T} term in Eq. (3) and (4)..

Figure S39 - K_{Nut} is equivalent to the $\boldsymbol{min}\left[\frac{PO_4^-}{PO_4^-+K_P}; \frac{DIN}{DIN+K_N}; \frac{Fe_T}{Fe_T+K_{Fe}}\right]$ term in Eq. (3)..

Figure S40 - K_{Nut} is equivalent to the $\boldsymbol{min}\left[\frac{PO_4^-}{PO_4^-+K_P}; \frac{Fe_T}{Fe_T+K_{Fe}^{Diaz}}\right]$ term in Eq. (4).

Also, this relates to my previous comment about light limitation. Is it restricted to the surface grid cell? Yes, nutrient uptake is limited to the surface layer.

L. 239: ‘Therefore, DIN and Fe **uptake** are scaled’? (Did the authors mean to include ‘uptake’ here?) - This has been done

L. 242: Please remove the comma after ‘atmospheric transfer’, and the authors have already used ‘N2’ earlier in the text but are just now defining it as dinitrogen. - This has been done

L. 245: I recommend moving the discussion of N_thresh (Section 2.3.5) here to improve clarity. This section has been reorganised in response to earlier comments.

L. 256 - 257: I was confused by this last sentence at first. I recommend reorganizing the text to specifically say that the V^(*)_(max) terms are temperature-dependent (i.e., can reach values both higher and lower than what are reported here). Then, temperature-dependent uptake is scaled by the combined limitation terms. Also, it is unclear why the authors state that “the maximum percentage of the grid cell nutrient concentration taken up by other phytoplankton each time step is 80%”. That would assume that the other limitation terms are equal to 1, but the authors state that the other maximum values are 0.7 and 0.5. Please reword or consider omitting this sentence from the text to improve clarity.

The paragraph was showing that there cannot be a situation where the computed nutrient uptake can exceed the nutrient available within a cell. The sentence has been removed.

Eq (11): Please include units for the flux. I also think the authors can remove the (z = z_o) from the equation and just use z_o. Also, from this equation, it does seem like the model restricts uptake to the top cell of the model. Please detail this earlier in the manuscript so it is more clear.

We think this had been covered in earlier comments and has now been made more clear.

L135, L155 and L163 state

> *Biological activity is estimated from surface nutrient uptake which in turn is immediately converted to particulate and dissolved organic matter (POM and DOM) in the surface ocean (Ridgwell et al., 2007)*
>
> *NutGEnIE described here includes (a) the concurrent use of three open nutrient (N, P and Fe) cycles when determining phytoplankton growth (surface nutrient uptake)*
>
> *Nutrient uptake by other phytoplankton (Γ^{OPhy}) is described in Sect.* ***Error! Reference source not found.****; similarly, nutrient uptake by diazotrophs (Γ^{Diaz}) is described in Sect.* ***Error! Reference source not found.****. The nutrient uptake terms (Γ^{OPhy} and Γ^{Diaz}) only have a value in the surface layer.*

L. 266: This is the first time the authors have mentioned that sulfate is an explicit tracer in the model. It would help improve clarity to have mentioned this much earlier in the methods section, and to provide a tracer equation for sulfate (and all other tracers) in the supplemental material. A list of tracers is now provided in supplementary materials. Sulfate does not play any role in nutrient limitation and therefore its representation in the model is not described.

L. 277: Is the model configurable to represent non-Redfield stoichiometry? The stoichiometry of “phytoplankton” can be configured via parameters, R_N^{OPhy}, R_{Fe}^{OPhy} and R_{Fe}^{Diaz}. The parameters are referred to in the manuscript text and summarised in Table 2.

L. 286: Since bacteria are not an explicit tracer, it is not necessary to say ‘by bacteria’ here. - changed

Eq (18): It might improve clarity to separate the two Michaelis-Menten functions rather than showing the product. Also, please include the units for maximum rate of nitrification, and consider using a different symbol (i.e., R_(nit)) to match the style of other reactions in the manuscript.

As with other parameters the value and units of maximum rate of nitrification is provided in Table 4. The symbol Δ^{Nit} has been changed to O_{Nit}.

L. 301: Please reword this sentence. Reworded as follows

> *The iron cycle involves two processes that act on Fe', scavenging and complexation. Fe' can be scavenged and is lost as particulate iron, which thereafter is not available for biological uptake. Secondly, Fe' can be complexed with a free ligand (L') to form complexed iron (FeL).*

Figure 3: Please be consistent with terminology elsewhere in the manuscript. Does 'PartFe' represent 'POFe' in Figure 2? No, POFe in Figure 2 is derived from OM and is available for remineralisation. PartFe in Figure 3 is scavenged iron that is lost from the available inventory.

L. 310: It was confusing where the caption ends and the next sentence begins (minor comment). An additional blank line has been added to make the end of the caption more clear.

L. 312: Please move this text on the equilibrium between Fe, ligands, and complexed iron to line 304 for clarity. The position of Figure 3 has been changed to achieve this.

Tables 1 - 5: References to these tables should be placed in the appropriate locations in the text when first mentioning these parameters. This has been added.

L. 340 - 344: "Parameters were adjusted to result in a combination that showed best agreement with **observed nutrient distributions..."** (suggested edit). Thank you, revised.

L. 404: I recommend converting +- 26% into Tg N yr to match the other estimates. This denitrification amount along the Wang et al. (2019) nitrogen fixation rates have been changed from % to Tg N yr^{-1}.

L. 405: "Wang et al. (2019) provide location of fixed nitrogen due to…". Do the authors mean fixed nitrogen **loss**? Changed

L. 420: Here the authors italicized PO4, NO3, and O2, whereas in other areas they are not italicized. Please be consistent. - Changed

Figures 5 - 8: Please provide labels on the maps and transects so that it is easier to identify which panels represent model results vs. which panels represent validation products. In all Figures, it would also help to extend the colorbar limits slightly to better represent values beyond their current ranges, since the values often reach the maximum/minimum limits. This is most notable in Figures 7 and 8, since the representation of these nutrients are a central point to the manuscript. Finally, please include latitude ticks (higher priority) and longitude ticks on map figures.

Our interpretation of the Copernicus guidance relating to figure labels it that "must be included with brackets around letters being lower case" the example sequence provided is (e.g. (a), (b), etc.). We have therefore followed that guidance. Where we have used panels, for example Fig 5 to 8 we have been consistent regarding layout and always used top left = WOAR surface, middle left = NutGEnIE surface, bottom left = surface delta (NutGEnIE - WOAR), top right = WOAR transect, middle right = NutGEnIE transect, bottom left = transect delta (NutGEnIE - WOAR). We have reviewed the colour bar limits and changed them for both delta panels of figure 7 and 8. We have commented earlier on surface map tick marks and labels, our aim has been to give as much space as possible for the figures.

Figure 9: Can the authors please convert longitude labels from 0 - 360 format to -180 to 180 format (with E and W labels)? This has been done. The inset map is also quite small and could be resized for clarity. This is difficult without detracting from the priority of the iron depth profiles. We have increased the size of the data point and legend of the insert, we think this has improved the readers ability to determine the cruise locations.

L. 610: "For all **variables**…" (typo). - Corrected

Table 6: Please use the same numerical precision between surface and interior reported values (i.e., surface value of 0.58 vs 2 for PO4). This has been done.

L. 651: “Denitrification can occur throughout the water column”. Ideally, this won’t be the case. Instead, NutGEnIE should restrict denitrification to only very low oxygen regions. Perhaps reword this to be clear. Also, I suggest removing ‘by bacteria’ since the model does not resolve bacterial biomass or their metabolisms. This has been done.

Figure 15: Consider rewording the caption. This has been done.

Review 2

Scope of the manuscript, major comments, and recommendation

Stappard et al. discuss a multi-nutrient-cycling extension to the established cGENIE ocean biogeochemical model. The extended model is simple in the sense that the biological cycling of the three nutrients phosphorous, fixed nitrogen, and iron is strictly implicit, without explicit description of living or dead biomass. Instead, export production by non-nitrogen-fixing organisms for example is described as being proportional to the most limiting nutrient, assuming a fixed Redfield-like stoichiometry, with the proportionality factor taking into account temperature, light and nutrient effects on the maximum growth rate of phytoplankton in a way as it is done in many other models that do describe biomasses explicitly.

This simplification, and the comparatively coarse resolution, allows the model to be integrated with large timesteps, making it possible to be integrated over tens of thousand of years, longer than the residence time of phosphorus in the ocean, and hence allowing it to treat all major nutrient cycles in the ocean as an open system, prescribing just the external inputs of nutrients, e.g. from riverine input, and letting the system decide which average nutrient levels are ultimately reached in the ocean. Most other models at least treat the phosphorous cycle, some also the nitrogen cycle as closed systems, neglecting inputs to and losses from the ocean and setting a fixed average nutrient concentration.

Indeed, the main motivation that the authors give for developing this model is to investigate what is the 'ultimate limiting nutrient' in the ocean, in the sense that its inventory sets the overall strength of the biological carbon cycling. But clearly, the usage of that model needs not be limited to that rather specific geochemical question: The model could also be used how nutrient inventories in the ocean change over time when e.g. external climate and nutrient influxes change, such as happened over glacial-interglacial timescales.

The model described in this manuscript, with its simplicity and consequent speed, fills a niche at one end of the different approaches for modelling the ocean carbon cycle and is therefore a quite useful addition to the literature. Model results for present-day climate are compared to nutrient and oxygen climatologies; I especially liked that modelled iron distributions (for which no climatology exists yet) are assessed against an important subset of the GEOTRACES intermediate data product. Overall, the manuscript is well written. It fits thescope of the journal, and in the end I think it should be published in

Geoscientific Model Development. I have three main criticisms, however, and the manuscript should be revised accordingly before being published.

The first and most important criticism is that there are several unclear points in the model description of the modelling of different nutrient cycles involved. These need to be clarified in a manner that a reader can understand the critical details of the model without having to dig into the model code. I will detail the points where I found something unclear in the minor comments below.

We have attempted to make the model description sections clearer. A table of model tracers is now included for reference in the supplemental information that should hopefully assist clarity. Section 2 of the manuscript has been reorganised so that the overall flow is now: (1) nutrient uptake, (2) the three nutrient cycles, followed by (3) organic matter remineralisation. Again this hopefully improves clarity. We have also enhanced the descriptions of some of the nutrient cycles, picking up points made in this and other reviews

The second criticism is that the discussion of the limitations of the model is still a bit weak. One major point here is that it is not really discussed how much assumptions in the model parameterizations, such as the choice of stoichiometric N:P:Fe ratios influence the results,

especially with respect to the question of the ultimate limiting nutrient. This will probably be done in detail in subsequent papers that use the model for that purpose, but some discussion here would be in order.

We have enhanced the discussion section of the manuscript. Where there are differences/biases between the model and the real ocean these are now more clearly assessed; where biases exist and a compromise has been made to accept these biases, the potential implications of the compromises are considered. On the specific point of stoichiometric N:P:Fe ratios, we have added a discussion relating to this and the possible influences that ratio choices could have.

And thirdly, I think the manuscript should describe a bit the differences and similarities of their model to the carbon cycle component of the CLIMBER-X model (Willeit et al, 2023, doi: 10.5194/gmd-16-3501-2023), because it fills a rather similar niche in the ecology of carbon cycle models.

We have added a comparison to the CLIMBER-X model and appreciate the suggestion. The fact that there are several similarities between NutGEnIE and CLIMBER-X is encouraging. Lines 816-823 in revised manuscript.

Minor comments

It is unclear to me whether the removal of a fixed proportion of nutrient uptake, mentioned in line 168ff is done uniformly over the ocean or locally.

Locally: the removal happens across all surface cells; in each cell the loss is based on the nutrient uptake in that cell. We have added the line below for clarity

> *Within each surface cell a proportion (k_{BF}) of the nutrient uptake in that cell is lost and unavailable for remineralisation.*

In line 229 it is mentioned that the nutrient framework is configurable; I understood this as that there is an option to add further possibly limiting nutrients. Is that so? It would be good to give a few more details on this.

The only additional nutrient is silicon. It is also possible to configure fewer combinations of nutrients, for example just P and Fe. The line has been changed to the following.

> *The nutrient framework is configurable, with an option to include silicon, but here it is dependant on DIN (= NO_3^- + NH_4^+), PO_4^- and Fe_T (= Fe' + FeL).*

In equation (2) and (3) why is the somewhat uncommon small delta is used instead of the more common partial derivative sign?

This has been changes along with other similar equations.

In equation (4) a nitrification term is mentioned; but the model, as far as I can see does not include ammonia as an explicit variable, only nitrate. On the other hand, if I look to equation (18) I see an explicit dependence on ammonia concentration. Can you explain? Does the model contain a prognostic ammonia variable that is also advected by the currents? I also find it slightly confusing that in Figure 2, middle panel, the heading says nitrate, but the arrows are annotated as DIN, which to me is the sum of nitrate/nitrite and ammonia.

Thank you, we have improved the nitrogen cycle description to cover the point you make you. The model does have ammonium as a tracer; this is now clear from the list of model tracers. We have also included an equation, eq. (9), for the tracer. Figure 2 has also been amended following your comments. The model does not have a tracer for nitrite so that DIN (= NO_3^- + NH_4^+) which we believe we have provided clarity on.

In equation (7) a term proportional to the biological degradation of dissolved organic Fe is given. It is is unclear to me how to think about this organic Fe. Is it different from the ligand-bound Fe? How would one distinguish the two?

We have clarified these points. As per the list of tracers there is both a Fe_T and Total Ligand tracer. The ligand tracer has a fixed inventory. Fe_T is the sum of Free iron (Fe') and complexed iron (FeL) and is available for biological uptake. For example the uptake equation for other phytoplankton now reflects this.

$$\Gamma^{OPhy} = V_{max}^{OPhy}(1 - f_{ice})\gamma^T\gamma^I \, min\left[\frac{PO_4^-}{PO_4^- + K_P}; \frac{DIN}{DIN + K_N}; \frac{\boldsymbol{Fe_T}}{\boldsymbol{Fe_T} + K_{Fe}}\right] min\left[PO_4; \frac{DIN}{R_N^{OPhy}}; \frac{\boldsymbol{Fe_T}}{R_{Fe}^{OPhy}}\right] \quad (3)$$

The organic iron that is available for remineralisation could have come from either Fe' or FeL' which is the Fe_T inventory it is then added back to the Fe_T inventory. The ligand tracer has a fixed inventory.

$$\frac{\partial Fe_T}{\partial t} = -\Gamma_{Fe}^{OPhy} - \Gamma_{Fe}^{Diaz} + \boldsymbol{\lambda DOFe} + DD_{Fe} + B_{Fe} - k_{sc}Fe' \quad (13)$$

$$\frac{\partial DOFe}{\partial t} = v(\Gamma_{Fe}^{NOPhy} + \Gamma_{Fe}^{NDiaz}) - \boldsymbol{\lambda DOFe} \quad (14)$$

We hope this adds clarity.

In equation (8) scavenging is made proportional to particle concentration, but is not explained whether this particle concentration varies with space and time, and what determines it.

Particle concentration is not a tracer in the model but rather is something that is calculated at every surface grid cell at each timestep and then is immediately turned into particle flux that is remineralised won the water column. The scavenging is calculated as part of the remineralisation process. This point has been included in the manuscript as follows.

$$k_{sc} = \tau k_0 (C_p)^{\phi} \quad (15)$$

where k_0 is the scavenging rate (when particles are abundant), C_p is the particle concentration, and ϕ is a constant coefficient. The rate of scavenging is calculated as part of particle remineralisation which in turn is calculated as a function of the rate of creation of new particles in the upper ocean, which in turn is calculated from the nutrient uptake rate.

In line 239 it is mentioned that phytoplankton nutrient uptake of Fe and P happens in a fixed proportion. As approximation this is ok, although the Fe:P ratio is much more variable in phytoplankton than the N:P ratio. But given the variability of Fe:P it may be a good idea to discuss a bit how this assumption may influence model results, especially concerning the question of the ultimate limiting nutrient. There are several recent papers discussing the effects of a variable C:Fe ratio, e.g. Wiseman et al. (2023, doi:10.1029/2022GB007491).

See comment above, we have added discussion related to this.

In line 259 ff it could be mentioned that an approximation of the vertical dependence with two exponentials is actually fairly similar to what one would obtain from the classical Martin (1987) curve, but is maybe somewhat more mechanistic. Just a suggestion.

An alternative reviewer has commented that the model description section could be more concise and focused. We have therefore added clarity when needed but are conscious not to expand this section unless necessary. When have therefore not added this suggestion.

In line 268 it is said that the consumption rate of electron acceptors in the remineralization is given in equations (12) to (14), but in fact these equations only give the inhibitory factors (dimensionless numbers between zero and one), not the rates themselves.

This is helpful, we have changed the line to make it clear that the equations give the relative rates of electron acceptor consumption rather than the rates. The text is now as below.

> *Consumption rates of electron acceptors in the process of OM remineralisation are relative to R_i in Eq. (17 to 19) and take account of both electron acceptors abundance and the inhibitory effect of electron acceptors with higher free energy yield (Reinhard et al., 2020).*
>
> $$R_{O_2} = \frac{[O_2]}{K_{O_2}+[O_2]} \quad (17)$$
>
> $$R_{NO_3} = \frac{[NO_3^-]}{K_{NO_3}+[NO_3^-]} \frac{K_{O_2}^i}{K_{O_2}^i+[O_2]} \quad (18)$$
>
> $$R_{SO_4} = \frac{[SO_4^{2-}]}{K_{SO_4}+[SO_4^{2-}]} \frac{K_{O_2}^i}{K_{O_2}^i+[O_2]} \frac{K_{NO_3}^i}{K_{NO_3}^i+[NO_3^-]} \quad (19)$$
>
> *where R_i indicates the relative fraction of each electron acceptor consumed……*

Also, it is mentioned that sulfate reduction is included here as a degradation process, but it is not made clear whether there is an explicit equation for sulfate concentration. Is sulfate simply made proportional to salinity or is it a prognostic state variable?

Sulphate is a tracer and is included in the newly provided list of model tracers. We have focused on the nutrient cycles in Section 2.3 and not included equations for other tracers.

The section on ligand scheme enhancement (lines 300 ff) is somewhat confusingly written in several aspects. First, the classical one-ligand scheme with constant total ligand concentration is explained in some detail, without clearly marking that this is the old state of how iron is treated. Then in lines 318 ff the new scheme is explained, without clearly delineating it from the older approach. And then, it is described that the ligand stability constant is made dependent on depth. So is this the way that two ligands are described, by having one ligand, but with vertically varying stability constant? And if so, how is the vertical variation described, is it a step function, high at the surface, low below a certain depth, or something different? And what is the concentration of the ligand(s), is it/are they constant? Looking at the table 5 and carefully re-reading the text, my conjecture is that there is a constant ligand concentration (but unclear what it's value is), and that the stability constant is approximately doubled in the uppermost surface layer. Is that it?

Some of the earlier points also touch on this aspect. There is a ligand tracer, and it has a fixed inventory; this is now stated in Table S2. The ligand stability constant K_{FeL} has been extended so that it can be set independently at each of the 16 depth levels. We only override the default value in the surface level. This is covered in the sentences below.

> *A default value of k_{FeL} ($log\ (k_{FeL}) = 11$) is configured with the ability to override this at each depth level by providing a depth level specific stability constant k_{FeL}^n, where n denotes the layer, from 16 (surface layer) to 1 (deepest layer). The existence of a strong binging ligand class predominantly present in the upper water column is represented by setting a higher value of k_{FeL}^{16} ($log\ (k_{FeL}^{16}) = 11.4$) in the surface layer of NutGEnIE. For all other layers the value is the default value.*

In line 376 ff it is explained that the modeled Fe distribution is compared to profiles obtained from GEOTRACES data, which is a feature that I really liked. But 'GEOTRACES data' is a bit vague; it should be noted which data set exactly is used (i.e. which intermediate data product) and the data should also be cited.

Thank you for your comment. We have made the product version IDP2021 clear and included the appropriate citation.

In line 383 ff it is stated that the model NPP is compared to a composite of several satellite-based estimates of NPP. But is it unclear to me how this comparison works: The model calculates net

nutrient uptake in the uppermost model layer, and that can of course be converted to a net carbon uptake. But this, in my view at least, is more a calculation of the export production at the lower depth of the first layer at 88m, approximating the net vertical flux of carbon out of the euphotic zone, and not net primary production, which is significantly higher and also includes the carbon that is heterotrophically respired within the upper ocean. Or did I misunderstand something here?

This point is appreciated, the attempt to compare model nutrient uptake to ocean NPP data is not suitable. We have changed this section to be a simple qualitative comparison of total nutrient uptake to ocean NPP and discuss a qualitative spatial comparison. The line below is added to the text and the delta panel of figure 12 has been removed.

> *A direct quantitative comparison is not appropriate because NutGEnIE nutrient uptake is equivalent to net ecosystem production whereas the OPR product is gross primary production; however, we have provided a qualitative comparison of spatial similarities and differences below.*

Line 502: I first stumbled across the description; maybe just add that you are talking primarily about the deep phosphate concentrations here, not the surface.

This phrase “ocean interior” has been added to the sentence as well as reference to the ocean interior panel of figure 7 at the end of the sentence.

Concerning the discrepancies to WOA surface fields mentioned for phosphate and nitrate (e.g. lines 515 ff): I think that some of the patterns look as if iron limitation is somewhat to weak in the major Fe-limited areas, like the Southern Ocean, the Equatorial and subpolar North Pacific. Maybe you can check this briefly? I would not be too surprised, especially in the Southern Ocean, and given the vertical resolution of the model. Overall the comparison looks quite good to me for a relatively simple model.

Based on Figure 15 the model is iron limited across the Southern Ocean and the North Pacific, Equatorial Pacific iron limitation is restricted in comparison to observations. We have extended the discussion of model biases in the discussion section and this point is included in that text.

line 575: Maybe it should be mentioned that the fact that WOA does not contain iron data has to do with the limited amount of data and is not just an oversight by the makers of WOA data.

This is a good point, we have changed to say that the comparison has been made with GEOTRACES and not commented on WOA.

Figure 13: It is interesting that the nutGENIE model does a quite reasonable job in reproducing the patterns of N2 fixation, although it does not use a stronger temperature dependence for nitrogen fixation than for other phytoplankton, as many other models do. This is encouraging.

There are a few smaller typos in the references, e.g. in Ballantyne et al., and a few missing subscripts and capitalizations. Please check this once more.

The Ballantyne reference has been corrected; several other minor corrections have also been made to references.

Review 3

General comments:

This study aims to develop a global model to investigate the ultimate limiting nutrient (ULN) for ocean primary production. It relies on the existing cGENIE model, which includes key biogeochemical cycles, including N, P and Fe. The authors extend the model by incorporating P and N surface inputs and sediment burial, as well as an improved representation of the iron cycle. The authors provide a comprehensive validation of the model, comparing key characteristics with available observations. They also present an insightful analysis of the environmental factors limiting ocean production.

This model represents a valuable tool for addressing the long-standing debate about nutrient limitations in the ocean, which ultimately controls ocean production, a topic that dates back to Redfield's foundational work on the biological regulation of nutrient ratios in the ocean. Historically, this debate has been explored using box models. Hence, the development of this 3D ocean model within an Earth System framework offers a promising avenue for exploring nutrient limitation and revisiting the ULN concepts over long timescales.

I have a few major comments, as well as some minor issues, that should be addressed before publication.

Main comments

- The abstract lacks sufficient details on the study's key outcomes. In particular, it does not clearly explain why the model is an appropriate tool to investigate ULN.

The abstract has been re-written to more clearly indicate the key features of the model and the rationale behind suggesting it is appropriate to investigate the ULN.

- The introduction could benefit from improved flow, with sentences and ideas better connected. For instance, L51-55 list different nutrient regimes, without clearly explaining the aim of defining them and how they relate to one another. Additionally, the introduction should focus more directly on the concept of the ULN and how it has been addressed in the existing literature.

These lines introduce concepts such as nutrient stress, deficiency and co-limitation that are more relevant to the investigation into the ULN but are not needed in this model description manuscript. The paragraph has been simplified.

In general, the introduction has been amended and hopefully the flow and focus has been improved. We have added more context to the concept of the ULN but are also conscious that this manuscript is a model description paper and we do not investigate the ULN within it.

- The model description is at times a bit wordy and could be made more concise and focused.

This comment contrasts with comments from other reviewer requesting more detailed information in the model description. We have reworked section 2.3 of the manuscript and tried to improve flow whilst keep the details focused. There have also been some amendments to section 2.3 as a result of comments below.

- The novelty in the model is not highlighted clearly enough. For instance, the inclusion of DOM uptake by phytoplankton (L205-206), a revised iron cycle (including iron input from the seafloor, dual iron ligand classes). It would be valuable to assess the impact of having dual iron ligand in cGENIE, comparing results with 1 or 2 ligand classes.

We have aimed to give more focus and clarity to the novelty in the model both in the model description and discussions sections.

- Why rename the model NutGENIE?

NutGEnIE is used to refer to the variant of cGEnIE with the described changes in place. There are features such as nutrient cycle fluxes, ligand dynamics, diagnostics relating to limiting factors and the reports of nutrient limitation dynamics that are not present in cGEnIE. We feel that continuing to refer to cGEnIE would be confusing to readers and potentially users of cGEnIE would expect those features to be accessible in cGEnIE.

> *For that to be successful the **c**arbon-centric **G**rid **En**abled **I**ntegrated **E**arth system model (cGEnIE) has been configured and modified to include the three open nutrient cycles for nitrate, phosphate, and iron to create a variant referred to hereafter as **nut**rient-centric **G**rid **En**abled **I**ntegrated **E**arth system model (NutGEnIE).*

- The discussion lacks sufficient comparison with previous studies. In particular, the nutrient limitation patterns identified in this study should be compared with observational data and outputs from other models.

Comparisons to observational data have been included. We have also added some comparison to the CLIMBER-X model.

Specific comments:

L34-36: This does not strike me as the best example of promoting BCP importance. Can you provide better examples?

We are a little confused by this comment, we don't see these lines as relating to BCP importance.

L93, 148: need full dot.

This has been done.

L110: Amend reference error

This has been done.

L144-146: Would make more sense to say the model represents NO3 and NH4 as forms of fixed nitrogen or DIN. The model also represents H2S for the sulphate cycle.

This has been done.

L154: Make it clearer why adding 3 nutrients (N, P and Fe) is a new feature, when Monteiro et al. (2012) and Naafs et al. (2019) already present a cGENIE model version with these.

The point should have been clear that it is the fact that the 3 nutrient cycles are open. The two papers mentioned have been referred to and the relevant text revised.

> *The novel features of the configuration of NutGEnIE described here includes (a) the concurrent use of three open nutrient (N, P and Fe) cycles when determining phytoplankton growth (surface nutrient uptake) and (b) the representation of a second iron binding ligand class with stronger binding in the upper water column. The three nutrients N, P and Fe have been included in previous versions of cGEnIE (Monteiro et al., 2012; Naafs et al., 2019) but without the surface and seafloor input fluxes introduced here and with uniform ligand binding within the iron cycling.*

L155: "the representation of a second iron binding ligand class with stronger binding in the upper water column" seems like an important concept for the model development. Could you elaborate on it more and justify it in the method?

This was covered in section 2.3.7 of the original manuscript and is updated and now covered at greater length in section 2.3.6 of the revised manuscript.

L161: 0.5 yr-1 seems very low. Do you mean per day?

This has been checked in the model code and should be 0.5 yr^{-1}. Previous papers relating to this cGEnIE parameter also provide it as 0.5 yr-1 (Ridgwell et al., 2007; Reinhard et al., 2020).

L168-169: Can you justify your assumption that the burial flux is not related to the POM flux, as this seems to be a big assumption? Also, have you considered using the available simple sediment burial scheme? Not published though.

The burial flux removal term is related to nutrient uptake and is referred to as a "a simplified instantaneous sediment burial term". This is a simplified mechanism that has been employed and ensures that each nutrient cycle is open. The justification that this mechanism is acceptable is the good degree of agreement between model and datasets with it in place.

L172-173: Can you describe more what gamma All stands for and how you rely on Equation (1)?

Γ^{OPhy} is nutrient uptake by other phytoplankton and Γ^{Diaz} is nutrient uptake by diazotrophs. These terms are discussed in the manuscript in the paragraph that precedes Equation (1). As mentioned in the reply to the previous comment the burial flux removal term is related to nutrient uptake.

Equations 2 and 3: Why omit the transport terms? You could write the equations as d/dt (which would look the same, but including transport terms within the full derivative term).

We do not feel adding transport terms would assist the explanation of the nutrient cycles. This also seems to be the standard approach in other papers describing cGEnIE nutrient cycles e.g., Ridgwell et al. (2007), Naafs et al. (2019) and Reinhard et al. (2020)

Equation 3: I struggle with why you assume that the burial of OM is instantaneous (gamma all).

This has been covered in the replies to the comments related to L168-169 and L172-173. This is a simplified instantaneous sediment burial term, we have taken this approach and then validated the model against ocean datasets and discussed model biases and compromises made. Given the timescales that we are considering (100's of ky) instantaneous OM removal versus a more realistic removal mechanism becomes a less significant point.

L208: typo "bit"

This has been corrected.

Line 210: surface NO3 and PO4 input, sea floor Fe input, need to highlight as novelty.

We have added summary of the novel aspects of each nutrient cycle to the relevant sections in the revised manuscript (sections 2.3.3, 2.3.4 and 2.3.5 for P, N and Fe respectively).

Equation 4: Where is the equation for NH4? And why do phytoplankton not take any NH4? Why does remineralisation go directly into the NO3 pool (lambda DON)?

Thank you for these comments, the equations relating to the nitrogen cycle have been changed and are now as follows.

> *Nutrient uptake by other phytoplankton is dependant on the availability of DIN, i.e., the combination of NO_3^- and NH_4^+ concentrations. The uptake rates of NO_3^- and NH_4^+ are represented by Eq. (7 and 8) with NH_4^+ being preferentially utilised (Naafs et al., 2019).*
>
> $$Up_{NH_4} = min\,(\Gamma_N^{OPhy};\ NH_4^+) \quad (7)$$
>
> $$Up_{NO_3} = \Gamma_N^{OPhy} - Up_{NH_4} \quad (8)$$
>
> *The governing equations for NH_4^+ and NO_3^- are below.*
>
> $$\frac{\partial NH_4^+}{\partial t} = -Up_{NH_4} - O_{Nit} + \lambda DON \quad (9)$$

$$\frac{\partial NO_3^-}{\partial t} = -Up_{NO_3} + O_{Nit} - R_{NO_3} + S_{NO_3} \quad (10)$$

$$\frac{\partial DON}{\partial t} = v(\Gamma_N^{NOPhy} + \Gamma_N^{NDiaz}) - \lambda DON \quad (11)$$

L219-222 and Section 2.3.7: Make it clearer how the iron representation compares with the previous cGENIE Fe representation and what is novel here. The same applies to N cycle and nutrient dynamics.

We have added summary of the novel aspect of each nutrient cycle to the relevant sections in the revised manuscript (sections 2.3.3, 2.3.4 and 2.3.5 for P, N and Fe respectively).

Equation 10: refer to Monteiro et al. (2012).

This has been done.

L255-256: Please present parameter values in a table, not in the text. You have Table 2 that you should refer back to

We have had requests from reviewers to provide parameter values in the text which is at odds with this request. We have taken the approach of providing parameters in Tables in Section 2.3.8 and referring to the relevant table which aligns with this request. However, there was a specific request to provide the iron ligand association constant in the descriptive text and this has been done.

L260: refer to Reinhardt et al. (2020), not Monteiro and Ridgwell (2023)

This has been done.

L285: Not exactly correct. Equation (13) represents the limitation term of denitrification. Please amend.

This has been changed.

L290: Please explain more about what you mean by dynamic threshold here and refer back to Monteiro et al. (2012), which explains this concept. You could also not mention the dynamic threshold as it is not used here.

As the model is configured for a modern setting with the condition $DIN < N_{thresh}$ used where $N_{thresh} \approx 2\ \mu mol\ DIN\ l^{-1}$, Monteiro et al. (2012) is also referenced. This is contained in Sect. 2.3.2 of the revised manuscript. Therefore the text referring to the dynamic threshold has been removed.

Section 2.3.6: Please refer to Naafs et al. (2019) for this, where this formulation of nitrification in cGENIE was first introduced.

Section 2.3.6 has been reworked and incorporated into Section 2.3.4 and references to Naafs et al. (2019) are now included.

L333: “an implicit ecosystem and therefore appropriate values for constants are not immediately apparent from observations or experimental outputs” not clear

Table 1: Please specify that the 3 last parameters are for iron.

This has been done the 3 parameters are now described as “Initial iron scavenging rate”, “Exponent coefficient of iron scavenging” and “Iron scavenging scaling factor”

Section 3 (L368 and 3.1): Here, it is mentioned that the model represents PP and how it might compare to observations of satellite NPP. It is essential to note that this version of the model does not explicitly model PP but rather exports production, as the nutrient uptake term in equation (9) is not a direct representation of NPP but rather the result of NPP minus grazing pressure. I suggest that you remove the comparison with satellite NPP, as it is not meaningful. Also, it is important to

recognise that the cGENIE resolution is not high enough to capture physical dynamics and associated biogeochemistry in the Arctic Ocean and Mediterranean Sea, so a comparison might not be useful.

This point is appreciated, the attempt to compare model nutrient uptake to ocean NPP data is not suitable. We have changed this section to be a simple qualitative comparison of total nutrient uptake to ocean NPP and discuss a qualitative spatial comparison. The line below is added to the text and the delta panel of figure 12 has been removed.

> *A direct quantitative comparison is not appropriate because NutGEnIE nutrient uptake is equivalent to net ecosystem production whereas the OPR product is gross primary production; however, we have provided a qualitative comparison of spatial similarities and differences below.*

Define WOAR in the figure captions.

The change has been made in figures 5 to 11 captions.

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