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Geoscientific Model Development
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Interactive comment on "A new terrestrial biosphere model with coupled carbon, nitrogen, and phosphorus cycles (QUINCY v1.0; revision 1772)" *by* Tea Thum et al.

Anonymous Referee #3

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This paper presents a novel model (named QUINCY) for the coupled cycling of carbon, water, N, and P in terrestrial ecosystems. This is a substantial contribution to a relatively small set of available models with a comparable scope: possible global applications [although only site-scale simulations are presented here], mechanistic representation of processes that determine the response of the terrestrial biosphere to global environmental change, applicability within a Earth System Modelling framework. The model is evaluated with respect to GPP and NEE data from FLUXNET, NPP/GPP ratios from paired FLUXNET and forest inventory data, and foliar d13C - a proxy for leaf-level water use efficiency.

The main innovation of the present model lies in the coupled representation of N, P, and C cylces; and in the model's entirely (?, please clarify) newly written code, that is designed in a modular way (p. 3, l.19) and allows for an appropriate design of the basic model structure to accommodate the new modelling capacities of simulating interactive carbon and N/P cycling as opposed to adding respective processes onto a "first-generation" C-only model. Parts of the model, however, are process parametrisations that are implemented as such in other Dynamic Global Vegetation Models (see also comments below).

It is highly challenging for a reviewer to assess whether the present model is appropriate and accurate in simulating all key processes that determine the coupled C, N, P, and water cycling. Especially given the immensity of the number of equations and paremeters implemented in the model (see SI). Therefore, I'm trying to evaluate how far the present paper got me to being convinced that this model works.

In summary, I am convinced that this model is a highly valuable contribution and that its description should eventually be published in GMD. I am less convinced that the model works (practically and off-the-shelf) and can be used by the wider community, since the code is not made fully publicly accessible ("The source code is available online, but its access is restricted to registered users and the fair-use policy stated on https://www.bgc-jena.mpg.de/bgi/index.php/Projects/ QUINCYModel. Readers interested in running the model should contact the corresponding authors for a username and password."). Therefore, I could not apply the model myself and my assessment is merely based on the descriptions in the text. I am always disappointed to see model code not being made fully open access along publications in GMD (an open-access journal!). In that sense, and very strictly speaking, what is the purpose of a publication in GMD? Shouldn't such a model description just remain an internal technical document then? I leave it to the editors to handle this and will evaluate the further aspects of the paper assuming that the editors support non-open access code in GMD.

Below, I'm listing a few MAJOR points that I would like to see addressed in a resubmission, followed by a number of MINOR points that I hope would improve the manuscript.

1 MAJOR

1. Evaluation. It is a practically impossible task to comprehensively evaluate a model that simulates virtually every important process that operates in a terrestrial ecosystem (and is typically represented in comparable models). I also consider that a complete and detailed description of the model itself may be the main part of a GMD paper, and that the evaluation with data may be secondary and addressed by further studies. However, as the paper is designed now, the "meat" is in the SI (all equations and parameter values), while the main text provides a rather brief description of basic model concepts and approaches in intuitively accessible language, and provides a rather brief evaluation against a small set of observational data and and overview of the model sensitivity. I think this is generally a good form of presentation. However, the evaluation becomes a central point of the paper and the evaluation presented here is relatively slim. The key challenge is to identify what we learn from including N and P cycling and limitation in a vegetation model and to identify key phenomena that can only be explained with including nutrient cycling (What are the key phenomena that can only be explained with including nutrient cycling?). I was intrigued by the evaluation of carbon use efficiency, CUE (Fig. 6c) but would have liked to understand more about why the model captures the overall magnitude of observed values, but does not explain the substantial variability in observations within vegetation types (e.g. NE forests). I would also have like to see how foliar stoichiometry, C allocation, the root:shoot ratio, soil respiration, or N fixation change across climatic and N (and P)-deposition gradients and how it (broadly) compares to observations. These processes have been identified previously as key mechanisms

that determine the coupled C and nutrient cycling (Medlyn et al., 2015). I was less convinced that the diurnal and seasonal GPP evaluation (Fig. 3) provides much insight in that respect. I suspect that the model can easily be tuned to match the magnitude of observed fluxes for each model setup (C, CN, CNP), and it is stated in the text that nutrient limitations do not affect diurnal and seasonal C dynamics (p. 11, l. 16). An explicit representation of chlorophyll (Chl) was included in the model in order to provide a useful diagnostic (with readily available Chl data), but no evaluation was shown.

2. Sensitivity analysis. I am most interested to learn about which parameters the modelled variables X are most sensitive to, and not primarily about how much X varies when several variables are varied at the same time (which is shown now in Fig. 8 if I understand this correctly). Could the results of the sensitivity analysis be shown differently? Also, in my interpretation, the sensitivity analysis primarily reflects the choice of the range over which the model parameters are chosen to vary. Therefore, the conclusion on p. 10, l. 18 that "the model output (Fig. 8) is well constrained and centred around the results of the standard parameterisation" is mainly an implication of this choice. If the range over which the parameter values were sampled was larger, then the range of simulated variables would be larger ("less well constrained"). However, I agree with the authors that non-linear, interactive effects could lead to assymetric simulated distribution. Anyway, I think this sensitivity analysis as presented now does not provide very useful information. Providing information about the sensitivity of modelled variables w.r.t. a selection of the most important parameters, and to clearly show which variables are most important in a figure, would be more useful.

3. Model description - several points here:

    - In the main text should provide an intuitively understandable description of the model, a characterisation of its behaviour, and a clear identification of the most important assumptions and choices made for model structure. This is done on p. 3 l. 1-18, however I would have liked to see this description more comprehensive and better referenced to the existing literature. In particular, I encourage the authors to make some of the central assumptions underlying the model more explicit, e.g. the following - if I'm correct:

  - A "sink limitation term" (function of temperature, soil moisture, and nutrient availability) is included on Vcmax and Jmax, Eqs. 7d.
  - Using air temperature for photosynthetic rates
  - Canopy N determines photosynthetic rates. This implies that photosynthetic capacity (A for saturating light conditions) is strongly controlled by N availability, and not by climate.
  - Biochemical (acting on Vcmax and Jmax) and stomatal limitations by low soil moisture considered
  - Acclimating basal respiration following Atkin et al., 2014
  - Resource uptake respiration depending on the form of N uptake (NO3 or NH4)
  - Root respiration scales with temperature but not with N or P uptake capacity.
  - Strict space constraint in forest stands by prescribing a maximum foliar projective cover. Constrains the number and size of individuals.
  - SOM turnover is N limited.
  - Labile pool dynamics determined by sources and sinks, sink limitation on growth by temperature and soil moisture
  - P just limits (imposing a "cap") growth (unlike N which also regulates the photosynthetic capacity)

- Model structure (and complexity): The model contains a very large number of parameters and it remains unclear how the parameters can be constrained from observations, or whether they are relatively well known from

independent measurements. E.g., the fraction of C allocated to fruit production (Eq. 29) seems enormously complex. Is the complexity chosen here always necessary? Equations are presented mostly without reference to justify the choice of the model structure. It is unclear whether the structures of equations used to describe the many processes are adopted from other references, are grounded in fundamental laws that are sort of standard representations, or whether they are designed here for the first time. If so, it may require some additional words on the motivation. For example, the photosynthesis scheme in SI Sec. 2: Is it adopted from Kull Kruijt (1998) or what parts of what's implemented here are new? Reference for N retranslocation upon heartwood formation (Sec. 3.5)? Many of the parameters are "shape parameters" of the functions used, and the systems dynamics may not be very sensitive to these. It would be useful to identify the most important feedbacks and discuss how these may shape the system dynamics in response to manipulations of temperature, CO2, N-input, etc.

- Motivation and description of advantages of this new model:
  - Merit of model is described as "decoupling of photosynthesis and growth" (p. 11, l. 6). This is unclear.
  - The model is described as "modular" (p. 3, l. 19), but then, the model description refers to specific model representations, not alternative ones within the same model. It remains unclear, what "modular" means in this sense.

2 MINOR

2.1 Main text

- p. 2, l. 3: "induce" instead of "provide"

- p. 3, l. 17 “nutrient uptake” instead of “root uptake"
- p. 4, l. 16: From what sources were these inputs prescribed? In particular: What is the source for rooting depth?
- p. 4, l. 31: Description of plant nutrient uptake
- p. 4, l. 22: turnover at two time scales: What is the motivation and the effect of this fast nutrient turnover and resorption/remobilisation to/from the labile pool?
- p. 6, l. 9: ‘Microbes’ or ‘microbial’ is mentioned at several instances, yet a microbial biomass pool is not explicitly modelled. Please specify how this is to be understood.
- p. 9, l. 3: Table 2 does not provide information about model performance. Can it be replaced by something that gives insight into performance?
- p. 9, l. 5: Should mention modelled value next to observed value in the text.
- p. 9, l. 18: Is there no data available to support this statement?
- p. 10, l. 22-23: How does this statement relate to the results shown in Fig. 8?
- p. 11, l. 5-6: What does “decoupling of photosynthesis and growth” refer to?
- p. 11, l. 12-13: This is not shown, is it?
- p. 11, l. 16: This does not seem to be what the figures suggest (substantial effect by CN and CNP vs. C)
- p. 12, l. 12-13: give modelled values here too
- Table 2: Just showing modelled values, without observational data is not very informative.

2.2 SI:

- p. 2, l. 8: Worth noting that layer 1 is the top layer.
- p. 2., l. 9: Worth noting that this is the total canopy N content (if that’s correct?).
- Eq. 7: Why is the CO2 compensation point not subtracted from ci in the numerator?
- p. 4, l.19: Introduce the term Av again.
- Eq. 15: It’s described on p. 3, l. 26 that A is the minimum of two rates (Ac and Aj). It appears confusing that Ah is introduced here as another limiting rate. Isn’t it just determining the Aj rate (actually, it may also appear confusing that Aj is independent of light, as of eq. 7).
- Eq. 16: should spell out ‘for’ or use appropriate mathematical symbol
- Eq. 17: Is aerodynamic conductance a fixed parameter?
- p. 8, l. 4: Why “co-limitation” and not (just) limitation?
- p. 8, l. 4/5: Should mention here that this refers to the turnover rate of the labile pool and that the labile pool turnover defines this part of the growth limitation.
- Eq. 28: Should mention the exponent 2 also in the text below.
- Eq. 30: Better write functions as f(N, P, H2O) instead of arguments as subscripts. In general, Eq. 30 needs an explanation/motivation.
- Eq. 37: What are lambda and k?
- Eq. 39b: $k_{reserve}$ not k_{store}?
- Eq. 45b, ‘dt’: clarify that this refers to daily.
- p. 15, l. 5: Is the seed-bed pool and fruit production related?

- p. 16, l. 3/4: But later, C pools of newly established individuals are averaged with C pools of existing ones, leading to a reduction in the average-individual C pool, right?
- p. 16, l. 17: 'met, str, ...' Introduce these abbreviations at first mention.
- Eq. 65b: What is Ed,decomp?
- p. 19, l. 5: 'increased' At first I though this should be 'reduced'? I thought that the fast and slow SOM pools have a lower C:N ratio than the structural pool and mass transfer from the structural to fast/slow SOM leads to net immobilisation. If not, please state upfront which step of mass transfer leads to immobilisation and relate it to respective pool stoichiometries.
- Eq. 73a: Point out in the main text that uptake is linear w.r.t. fine root biomass.
- Eq. 94: Start with stating what the reflection coefficient determines. Maybe better to start with something "high-level", like the surface energy budget? Or just start with equation 97.
- Sec. 6.3: Start stating what sort of scheme is applied for soil hydrology, how many layers, . . .
- p. 29, l. 10: Need to introduce the meaning of "skin" here.
- p. 29, l. 19: I'm confused: field capacity is not part of Eqs. 114.
- Eq. 114: Throughfall is not defined. Is sl=1 the topmost layer? In general, I don't understand Eq. 114.
- Eq. 116: What is Ei? Evaporation of intercepted water? What is ra? aerodynamic resistance? Repeat here to clarify. It would be helpful to start with the high-level water budget.
- p. 30, l. 5: How is surface temperature calculated? Please add reference to equation.

- p. 31, l. 1: This is better put upwards (start with high level description of first principle (water/energy conservation).

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