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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 #1

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Thum et al describes a new terrestrial biosphere model, called QUINCHY, and presents a first evaluation of the carbon, nitrogen and phosphorous cycle against site-level data. Although the quality of what has been presented is good, I'm concerned about what has not been presented in the manuscript and its supplement: (1) the benefit(s) of starting a new terrestrial biosphere model, (2) the impact of the "consistent model formulation" (as called by the authors), (3) a clear overview of what makes QUINCHY stands out among the existing terrestrial biosphere models, (4) an evaluation of the energy and water balance, and (5) the target/criteria used to decide that the model's performance is "acceptable".

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(1) The authors remind the readers that “Many process-based models of the terrestrial biosphere have been gradually extended from considering carbon-water interactions to also including nitrogen, and later, phosphorus dynamics.” and state that “This evolutionary model development has hindered full integration of these biogeochemical cycles and the feedbacks amongst them”. Although I fully agree with the first part of their assessment, models like CLM (10.5194/bg-11-1667-2014), CABLE (10.5194/bgd-6-9891-2009; 10.5194/gmd-2017-265), ORCHIDEE (10.5194/gmd-10-3745-2017) and JSBACH (10.5194/bg-9-3547-2012) show that the second part of the statement needs to be toned down unless the authors can provide evidence in support of their claim. The current presentation contains no elements that demonstrate that the technical and/or scientific performance of QUINCHY was only possible due to the fact that the group started their model developments from scratch. Most of the groups that maintain and develop a terrestrial biosphere model that has a history that goes back to over a decade are likely to have considered a rewrite of their model at one point. Most of these groups, however, decided to continue with “evolutionary developments”. If this evolutionary approach really hinders scientific progress (as the authors seem to claim), this is an important message but it should be supported by evidence.

(2) It is mentioned several times that QUINCHY has “a consistent representation of element cycling in terrestrial ecosystems”. It remained unclear to me what is meant by this. Towards the end of the manuscript I was under the impression that “consistent” referred to the fact that all processes in QUINCHY are calculated at the same half-hourly time step. Although I can appreciate that such an approach makes the code easier to read and maintain, I’m less sure this approach can be claimed to be “consistent” because the time step of the model itself is still arbitrary (1800 seconds) when compared to the actual time step of the processes. Moreover, the idea to use different time steps for different processes has been justified by a more efficient use of limited computer resources. This far most terrestrial biosphere models favored speed above accuracy for the calculation of the non-linear processes. The QUINCHY group choose to trade computer time for an expected increase in accuracy. Can you demonstrate that there

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was an increase in accuracy? Based on your experience and findings can you recommend other groups to make the same choice? Will you maintain this "consistency" in the near future when adding landscape-level processes to the model such as plant biogeography and disturbances?

(3) The authors claim that QUINCHY is a new model. Although I have no doubt that this assessment is correct from a technical point of view, it is less clear whether this is also true from a scientific point of view. It would be interesting to present the family tree of QUINCHY as it seems to be strongly inspired by O-CN (10.1029/2009GB003521). When thinking about weighting models in the IPCC context (10.1038/s41558-018-0355-y), would you argue that QUINCHY is independent or do you expect similarities with for example ORCHIDEE (10.5194/gmd-10-3745-2017) in which the C and N-cycle seems to be very similar to the one used in QUINCHY. If I understood the model legacies correctly, O-CN partly relied on ORCHIDEE and subsequent versions of ORCHIDEE (10.5194/gmd-10-3745-2017 and 10.5194/gmd-2018-261) relied on O-CN. Given that QUINCHY adopted many approaches from O-CN is it fair to assume that both models are likely to have some similar behavior? As a reader it is not clear at all what makes QUINCHY unique. After reading the current manuscript and its supplement, I expect that prospective model user will still have no idea when they should choose QUINCHY over CABLE, CLM, ORCHIDEE, JULES, JSBACH,...

(4) Although the SI presents the formalisms used to simulate the water and energy budgets, these processes are not at all discussed in the manuscript. The whole point of having a terrestrial biosphere model (especially when it will be coupled to a general circulation model which is the case for QUINCHY) is that the terrestrial biosphere model links carbon, nutrients, water and energy cycles in a quantitative way. In my opinion, the most telling evaluation targets for a terrestrial biosphere model are those showing the skill of the model in jointly reproducing two or more cycles. Such analyses has not been presented.

(5) The evaluation is sound but routine meaning that no clear effort was made to go be-

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yond the typical “acceptable performance” where “acceptable” remains undefined and “performance” is limited to a RMSE or a correlation. I do realize that this represents a common modus operandi in the community but the tools and data exist to do better. Hence, there is no excuse for a leading journal as GMD not to raise the bar by insisting on more ambitious evaluation practices. Could you, for example, set quantitative targets, i.e., reproducing 95% of the seasonal cycles in addition to 50% of the residuals data structure (i.e. observations minus the seasonal cycle)? Or using a simple purely climate driven statistical model as the reference to beat? Subsequently, quantify whether these targets were met or not. The statistical methods for such an approach are available and have even been proposed for spatially explicit analysis (see SI of 10.1038/nature02771). Furthermore, the study somewhat overlooks the concerns of the community who wants to learn about the performance of QUINCHY who presents itself as “the new kid in town”. From a community point of view it would make sense to run the model through the ilamb benchmarks (10.1029/2018MS001354) and compare QUINCHY’s performance relative to what is considered state of the art within the community (in addition to the evaluation shown by the authors).

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