Review comments on Moreaux et al.

Overall comments:

This paper is a very detailed and thorough description paper of the newly developed model GO+ version 3, which is a model for simulating carbon, energy and water fluxes in temperate managed forest ecosystems. The model description is detailed, the sensitivity analysis and the model evaluation is extensive, and the discussion of the usefulness and limitation of the model is also adequate. I don't have major issues with the overall content of the paper. I think the paper and the model are both valuable additions to the community, and the paper will be a great contribution to GMD. Below I list several relatively major comments and my detailed comments are available thereafter.

Major comments:

The authors claim that the model is novel in that, it combines biophysical and biogeochemical processes of natural vegetation dynamics with different representations of forest management, and thus the model allows the explicit simulation of both short- and long-term impact of forest management and climate change across multiple scales. Realistically speaking, the biophysical perspective of this model is somewhat overly simplistic, as compared to many land surface models or dynamic vegetation models out there (e.g. OCHIDEE, CABLE-CNP etc.). While I agree with the authors that there may be a balance between realism and scalability, the authors failed to convince me that their model implementations are adequately enough to make some novelty claims that they stated in the manuscript (I detail these in the specific comments). For example, the authors claimed that their independent simulation of stomatal conductance and its linkage with plant hydraulic is a novelty of this study (e.g. P39, L3-4), but they never demonstrated how their simulated photosynthesis was coupled/decoupled with stomatal conductance, how water availability affected this relationship, and how well the model performed in relation to data. I suggest the authors to demonstrate the performance of this "novelty" in order to claim it.

Moreover, the other novelty that the authors claim was that, the model offers a large range of options of management. I think these management options are easily implementable in land surface models, and some may have already been implemented (detailed comments in specific comment). I think it's OK to claim these additional modelling implementations as novelty. However, I do feel that they haven't really demonstrated well enough how each, and the combination of these management options affect the simulated results. Their figure 3 and 4 for example, did not convince me that the new simulation really significantly improved the comparisons with observations. I suggest further sensitivity analyses on this point. Further along this line of thought, some representations of management and their effect on vegetation dynamics are supported with no literature evidence, and it seems that some are rather simplistic (i.e. without species/climate/soil –specific effects). I think this warrants some discussions.

Furthermore, I think there is a possible missed opportunity with regard to nutrient cycles. The authors claimed that they had some nutrient contents simulated, and leaf respiration depends on nitrogen. I understand that incorporating a full nutrient cycle may be beyond the current paper, but the authors really didn't test how nutrient affect photosynthesis. I suggest the authors to justify the reasoning for not relating nutrient availability with photosynthesis (especially given the relationship between respiration and nitrogen), or make some simple tests to see how well/poorly their modelled nutrient availability was. This would point to interesting future research to improve the model, I think.

Specific comments:

Abstract: the 2nd half of the abstract only described what the author have done – i.e. examines the sensitivity of the model, compares the model performance with observations. I would like to see some more explicit descriptions of the results of these actions.

P4, L2: This statement really depends on your definition of representations. Many land surface models did incorporate empirical relationships on management effect on soil and vegetation carbon. A recent literature is Felzer and Jiang (2018), who assessed the effect of different land uses on vegetation and soil carbon sequestration, including forest harvests. The relationships in their model are empirical, but so does some relationships described in this study.

Table 1: Any particular reason why atmospheric O_2 concentration is an input in this model?

P8, L13: What depth is the reference depth? Can you specify?

P9, L8: So stomatal conductance is simulated independently of photosynthesis. Can you show, in your model evaluations, how photosynthesis and stomatal conductance is coupled/decoupled under different weather conditions? I think it's important for the readers to know the performance of these two fluxes, especially given the current way you represent these two inter-related fluxes.

P9, L9: Can you perform a sensitivity test on the time constant? This constant seems to potentially have a big effect determining your drought responses.

Equation 13: How could one derive relationships from observations to drive your model? I can see many assumptions must have gone into the parameterization of this equation. How much confidence can we trust the model prediction, if these parameters were only empirically-determined/assumed?

P10, L25: You have maximum root depth as an input parameter, but how root depth changes with plant age?

Equation 24: You did not have nutrient effect on photosynthesis, but you included N effect on respiration? Can you justify the reason to not include N effect on photosynthesis then? That seems a missed opportunity given the current momentum in including nitrogen and phosphorus cycle processes in land surface models, which has been quite nicely reviewed in Achat et al. (2016) and evaluated in Fleischer et al. (2019).

P13, L2: "than" grammar issue?

P13, L6: Does allocation only respond to this water stress index and nothing else (e.g. nutrient, competition, phenology)? This could be quite an important weakness that needs further justification. Also, it seems that this water stress index only changes at annual timestep (P14, L5). Is this too coarse a resolution to simulate drought effect on growth and transpiration fluxes? The model certainly resolves energy, water and carbon budgets at hourly timestep, which implies that the model has the capacity to investigate detailed water-carbon relations under extreme conditions. But if the water stress index is only updated at annual timestep, I see little possibility for a realistic simulation of the diurnal and intra-annual variability in carbon-water coupling.

Figure 2: Allocation partitioning into different root components – how do you parameterize and evaluate this? For such a simple allocation scheme, maybe the authors want to justify the need for additional complexity in representing root dynamics. What additional insights do you gain by compartmenting roots into 4 categories?

Section 2.7: The representation of vegetation phenology includes very little mechanistic understanding – from what I can see, some part of the model only still uses date of year to change phenology. Maybe that's a point of future model improvement, but some acknowledge of the limitation may be needed.

P16, L21 – 22: From reading of this, it appears to me that you consider a tree dead once you can't close the carbon mass balance. Is this a realistic/safe assumption? The thing is, this assumption ignores the role of plant hydraulic and physiological traits in modulating plant responses to extreme conditions. I think some acknowledgement on the lack of process-based representation of tree mortality is needed here.

P16, L25: If I understand this correctly, here potentially coarse woody debris is added to soil pool?

Figure 3: Clearly the new prediction still can't capture the exact management effect, so what's the point of including these management options in your model? Yes the simulation is better matched with observation over the long-term, but the immediate impact should also be represented, I would argue.

Figure 4: Prediction not necessarily improved, is it?

P21, L10 – 14: these assumptions seem to be very arbitrary – no citations, and not species-specific.

Section 2.9.4: I don't think there is much mechanistic basis in these model implementations. And if you have nutrient concentration in leaf, it seems to be logical to include nutrient effect on plant photosynthesis, at least that's what the authors did for respiration. Some justifications are needed as to why the authors did not consider nutrient effect on photosynthesis. There are relationships available to do so (e.g. Walker et al., 2014).

The following section on sensitivity and parameterization test seems thorough, but I do note that the model was parameterized, so it's reasonable to see the model simulation matched with observations to some extent. I think it's more important to test the sensitivity of the assumptions that determine the CO_2, temperature, precipitation, etc. responses, which is a different suite of sensitivity test. This different suite of sensitivity test would allow one to really entrust the model mechanisms to predict future climate change impact.

Table 6: why “continued”?

P39, L3-4: You haven’t evaluated how photosynthesis couples/decouples with stomatal conductance under water stress. I think you need to demonstrate it before you call it a novelty of the paper.

P40, L5: there is an extra comma in the citation bracket.

Reference cited:

Achat DL, Augusto L, Gallet-Budynek A, Loustau D. (2016). Future challenges in coupled C–N–P cycle models for terrestrial ecosystems under global change: a review. Biogeochemistry 131: 173–202.

Felzer, B.S. & Jiang, M. (2018). Effect of land use and land cover change in context of growth enhancements in the United States since 1700: net source or sink? Journal of Geophysical research, Biogeosciences, 123, 3439-3457.

Fleischer, K., Rammig, A., De Kauwe, M.G. et al. (2019). Amazon forest response to CO2 fertilization dependent on plant phosphorus acquisition. Nat. Geosci. 12, 736–741.

Walker, A.P., Beckerman, A.P., Gu, L., Kattge, J., Cernusak, L.A., Domingues, T.F. *et al.* (2014). The relationship of leaf photosynthetic traits - V_{cmax} and J_{max} - to leaf nitrogen, leaf phosphorus, and specific leaf area: a meta-analysis and modeling study. *Ecology and Evolution*, 4, 3218-3235.