

Interactive comment on “A new version of the CABLE land surface model (Subversion revision r4546), incorporating land use and land cover change, woody vegetation demography and a novel optimisation-based approach to plant coordination of electron transport and carboxylation capacity-limited photosynthesis” by Vanessa Haverd et al.

Anonymous Referee #1

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Haverd et al present a set of updates to the CABLE model, including the "Populations Order Physiology" (POP) model representation of land use, an algorithm depicting photosynthetic optimality principles, and several other updates presented as appendices.

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Numerous integrations of the model with different land use and climate drivers are presented, along with a comprehensive model evaluation exercise.

While this is a substantial paper that should almost certainly ultimately be published, and while it includes many interesting a novel benchmarking approaches that the land surface modeling community would do well to take notice of any repeat with other models, I find this version of the manuscript in need of considerable work in terms of the model description presented and in terms of discussion of the uncertainties inherent in both the POP approach and the other updates.

Firstly, the authors rather over-zealous ‘selling’ of the POP concept in the manuscript strikes me as not particularly objective and thus quite unconvincing. Further, given the lack of critical discussion of the approach, I am left unsure in which circumstances POP might act as an appropriate simplification, and those in which it would not. For example, there is no discussion of how PFT competition might be represented in this framework, nor of how it would respond to the implementation of partial disturbance processes. The somewhat heuristic and undocumented disaggregation of grid cell fluxes into patches and cohorts (which is the critical central assumption) is also presented without any consideration of whether it is realistic or appropriate. I realize that it is imperative to illustrate in some way the basic competence of an LSM, in order to allow the following experiments to be seriously analyzed, but this must be balanced with some humility about how much can really be read into the conclusions, given the vast difficulties of parameterization and appropriate validation of these models.

Secondly, the model description is inadequate and confusing throughout much of the methods section. I have detailed specific instances of this below, but in general, the description is vague, not accompanied with technical equations nor any accompanying documentation, and is not up to the standards that are found within a typical GMD article. I suggest that this section needs completely re-writing with transparency and provenance tracking in mind. In my view it requires a full separate technical note to allow proper assessment of the methods employed, which again, would be normal practice

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within GMD.

Thirdly, the manuscript focuses in great detail on the POP land use and the photosynthetic optimization modifications, then almost ignores the other myriad of modifications that have been made to the model. Why are these two modifications selected for special treatment? Maybe there is a good reason, but it needs to be made clearer.

Lastly, the paper is essentially presents a new version of CABLE with many updates, but the performance of this new version in contrast to any previous versions is not considered and the impact of the implementation of the different model features is in general ignored, nor is the performance compared to any other LSM. Thus, the skill of this model version is presented in isolation, and is quite difficult to assess other than broadly stating that it performs reasonably well.

Specific Comments P1 L5: Critical for what?

P1 L15: This theory has been proposed previously (Xu et al. 2012), so is not novel

P1 L21: “state of the art” is jargon and should be replaced by a statement with some clear scientific meaning.

P1 L25: I wasn't aware that we had any credible estimates of global GPP, let alone centennial trends therein.

P3 L8: These two developments seems quite arbitrary and distinct from one another. Why are they the joint focus of this one paper?

P3 L10: Now there is a new list of developments. Why is this list different from the last list?

P3 L33: “second generation” in what sense?

P3 L36: Many current DGVM models using some sort of similarity clustering to deal with the problem of expanding numbers of disturbances classes to track. (see all implementations of ED...) This might be difficult, but it is nonetheless the ‘state of the art’,

if we are going to use that sort of terminology.

P4 L5: POP has some advantages in terms of computational time, but the rules used to disaggregate the big leaf fluxes into size classes of vegetation are necessarily arbitrary. There has not been, as far as I know, any attempt to investigate the uncertainty introduced by not resolving vertical light partitioning in POP. Thus, it is not clear to me that is all that useful of an idea.

P4 L 12: As above, this idea was also proposal by Xu et al. (2012) and it's global implementation presented by Ali et al. (2016)

P5 L11: Define what is meant by 'offline' in this context?

P5 L27: This sentence is really just hype and doesn't add anything of scientific value to the paper.

P5 L30: If the timestep is one year, how is the growth of leaf tissue, and disturbance events from individual fires resolved with appropriate fidelity?

P5 L31: Input variables to POP, not CABLE, I assume.

P5 L35: Surely neglecting partial disturbance from fires and mortality will introduce a large bias? How can this decision be justified?

P5 L37: Need to define the nature of a 'cohort' here. Are they all of similar height, age, dbh? Similarly, are the 'patches' spatially explicit or implicit? To what does the term 'neighborhoods' refer?

P6 L1: Does this just mean that stem biomass is a fixed fraction of NPP?

P6 L2: GPP and Ra at the grid-scale level?

P6 L3-6: This is not an adequate description of the disaggregation process, which is the most important assumption in this POP system. How are gap probabilities evaluated? How is light interception of the different cohorts and patches evaluated? With what

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set of assumptions? I think the authors would do well, if they genuinely wish this approach to become more broadly accepted, to apply some more critical thinking to this particular aspect of the model and to be much more transparent with the limitations and strengths of the assumptions here. Maybe it is defensible to assume that NPP is directly proportional to light interception, or maybe it isn't, but the absence of discussion and questioning of this topic is frustrating. I was a referee on the original POP paper too, and continue to find this to be a limiting aspect of this exercise.

P6 L11: I thought there was only catastrophic disturbance?

P6 L13: There should, at the very least, be a reference to the place where one can find an actual description of this mortality function. Growth efficiency is often also defined as NPP/LAI (in LPJ, for example). Hence this needs more careful definition. There is no description at all of how the crowding mortality works.

P6 L 17: In what sense are the patches 'replicates'?

P6 L24: How are the state variables interpolated? This sentence doesn't make sense to me, nor does the one that follows. Is this a new feature of POP? In which case, it needs much, much clearer documentation.

P6 L30: "The resulting tree biomass turnover" : resulting from what? The combination of the mortality rates discussed above?

P6 L32: Thus far the distinction of how CASA-CNP and CABLE interact has not been made clear.

P6 L35: Is this a feature of POP, or of CASA-CNP?

P6 L36: I thought NPP, GPP and Ra were all calculated at the grid scale level, so how can NPP thus be dependant on stand age?

P7 L 12: What is a biome in this context? Is each grid cell really only populated by one or two PFTs?

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P8 L5: How many age classes are there? Is this dynamic or fixed?

P8 L10: I don't think the description in section 3.1 was sufficient to let the reader understand how this interacts with the age structure tracking in secondary forests.

P8 L14: typo in 'pools'

P9 L5: Again, this seems like a huge shift in focus from land use and demography to fast timescale photosynthesis. Further, a very similar method was suggested by the studies of Xu, and Ali.

P11 L23: So, shouldn't the BGC model be CASA-CN, not CASA-CNP?

P11 L31: Which version of CRU-NCEP are you using here?

P12 L9: None of these scenarios explores the impact of the photosynthetic optimization approach that you just documented in considerable detail? This happens later, but that is mixing of methods and results and is confusing.

P15 L4: From where are these successional data taken? Surely there is massive geographical/climatic variance in these rates? Is the model sampled to make sure that it has the same climatic regimes as the dataset?

P16 L27 : Papua New Guinea??

P26 L10: Arguably, if one is going to say 'state of the art', this model should already include fire effects on vegetation, croplands and dynamic biogeography and PFT interactions already, since those are things that are included in many other models. Saying a model is the 'state of the art' is a bold statement, given both the complexity and the wide range of approaches within this field. Further, it is not really necessary for the purpose of model documentation. All LSMs have strengths and weaknesses in different areas. Progress can only be made by careful and objective analysis of the uncertainties inherent in different types of structural assumption, parameters and boundary conditions. I found this paper somewhat lacking in any thoughtful discussion of these

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things, whether the model is 'state of the art' or not.

References

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