Geoscientific
Model Development
Discussions

Interactive comment on "Competition between plant functional types in the Canadian Terrestrial Ecosystem Model (CTEM) v. 2.0" *by* J. R. Melton and V. K. Arora

Anonymous Referee #1

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This paper presents a comprehensive analysis of a new version of the Canadian Terrestrial Ecosystem Model (CTEMv2.0). Three versions of the model are tested, one without dynamic vegetation, and two with dynamic vegetation changes. Two DGVM simulations are presented, one with a formulation of the Lotka-Volterra equations that tends towards mono-dominant ecosystems, and another where the equation is recalibrate to generate much greater co-existence between plant functional types. The latter produces a better distribution of PFTs relative to observations.

The model as presented is a pragmatic approach to generating a functional DGVM, and the authors do a good job of describing the model structure and the experimental setup.

My major comment on the manuscript, expanded in the specific comments below, is that the re-calibration of the model to generate realistic carbon and water fluxes in the DGVM format is poorly described and justified. There is little relation of the parameters to observational constraints, nor any description of the process of model calibration or the sensitivity of the model outputs to particular parametric changes. While it is difficult to investigate all of the degrees of freedom implicit in DGVM models, on account of their complexity, it is common practice in the LSM literature to illustrate how parameters relate to empirical observations, and if this is impossible or inappropriate be done, to explain why.

P4854 L30: Arguably, a model isn't a DGVM if it doesn't simulate vegetation distribution?

P4855 L16: Arguably, ED and other cohort based approached (LM3-PPA, TREEMig) are a mid-way point between gap model dynamics and a normal DGVM. The criticisms leveled at gap models are those that ED is specifically designed to circumvent, so I am not sure that this argument (of computational load) is the right line to take here. P4855 L17: The SEIB approach -is- included in an Earth System Model, and if I understand it, requires that some of the physiological processes are calculated daily. There are also newer references for LPJ-GUESS now (Smith et al 2014) P4856 L2: Given that TRIFFID is a L-V model, do you mean to lump it in with this criticism? P4856 L3: I feel like some high-level philosophy or justification for the use of the L-V approach might be useful here. Is the purpose to modulate the tendency towards mono-dominant vegetation caused by the NPP-based approaches? From first appearances, it isn't clear why a predator-prey model designed for trophic interactions is the right way to simulate competition for resources, and so I think it needs a little bit more introduction. P4864 L5: Given the amount of discussion devoted to the comparison of the alternative parameterization of 'b', it would be useful to see more discussion of the ecological interpretation of this number, and some justification of why it might be parameterized

as either '1' or '0'. Is there no appropriate middle-ground? P4869 L25: Again, revising the same point as above, does the CTCOMP simulation simply increase the competitive inhibition of expansion of the dominant PFT? Is it a proxy for landscape variability and the processes controlling coexistence? P4873 L23: Some discussion of Reich et al. (2014) and their work on needleleaf tree parameterizations might be appropriate here. P4875 L1: How are they parameterized as tropical? Through the climate envelopes, or some other feature of the parameterization? P4876 L20: It would seem intuitive that drought deciduous trees should naturally be more successful than other vegetation types in seasonally dry climates? Is a climate model necessary to exclude them from wetter areas? P4876 L27: Why use this dataset for validation if it is itself based on unreliable latitude bands? There are many landcover datasets in existence which potentially do not introduce these artifacts. P4877 L16:The process alluded to earlier, of adjusting the bioclimatic envelopes to get a good distribution of BDL-DCD-DRY trees, means that the comparison to this data in the results section is necessarily circular. It would be very helpful and illuminating if this process were a good deal more transparent, potentially including sensitivity tests either to the climate envelopes, or the physiological parameters depicting differences between the PFTs. P4879 L25: How were these parameter values determined? Were they fitted in an ad-hoc fashion? In which table are they listed? Presumably the net result of the bare-ground and expansion requirements is that the productivity needed to be increased in the DGVM simulations? It would be good to add a note to that effect here. P4880 L14: Which parameters? Surely that is a relevant thing to include here? P4881 L13: With this, and all similar results, I do not know how to interpret the goodness of fit between the models and the data, because the parameterization process is so opaque. Were the data specifically fitted to the Amazon biomass data, or is this a fortuitous result that illustrates the skill of the model process representations? P4884 L15 - P4885 L12: This is a very useful discussion and analysis. P4886 L 9: The meaning of the altered parameter in the LV equations is still unclear at this stage. Does it have an interpretation in reality, or is it's function simply to reduce the intensity of competitive exclusion

processes leading necessarily to greater co-existence? There is an argument that this is a reasonable approach, given that many of the processes determining co-existence remain uncertain in the ecological literature, and even if we can simulate co-existence in a given place, it is much more difficult to do so across a heterogenous griddle. I think the authors could actually write a much more robust defense of this strategy, which at present comes across as a simple calibration tweak.

Figures 2,3: The maps are quite hard to see in this configuration. I think they would be more efficiently presented in a rectangular projection, since in mulit-panel figures the elliptical projection loses quite a lot of space.