Dear Editor,

We thank the reviewers for their time in providing feedback on our paper. We have now included a section on code availability that details the Environment Canada requirement of agreeing to a licensing agreement to obtain the code. We would also like to point out that the manuscript provides an exhaustive appendix detailing exactly how the model code works as we believe it is likely much easier to understand than the actual code itself.

Below are the reviewer comments with our replies on a point by point basis in bold. We first address the comments of Reviewer 1.

Anonymous Reviewer 1

This paper presents a comprehensive analysis of a new version of the Canadian Ter- restrial 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 recali- brate 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.

We agree that that more insight into how parameters were changed to recalibrate the model will improve the manuscript. We have now included some discussion related to this in the manuscript. As the reviewer notes, some of the parameters were changed in line with available observations while other paramters, those that have no available observations, were tuned to give appropriate model behaviour.

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

There are two aspects of vegetation dynamics - structural and areal, as mentioned in the manuscript. Even if spatial vegetation distri-

bution is not dynamically simulated, a model may still stimulate structural aspects of vegetation dynamically. So, while we agree that DGVM generally should mean dynamic vegetation spatial distributions and terrestrial ecosystem models (TEMs) should be models with prescribed fractions, in practice the naming in the literature seems to be flexible.

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.

Yes, we agree that models like ED are a mid-way between gap models and DGVMs, however they are still not commonly applied at global scales. For example, the recent paper by Fisher et al. (2015) that incorporates ED into the CLM is only over eastern North America. We have included the reference to Fisher et al. (2015) in this context.

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)

Thank you for pointing out the application of SEIB-DGVM in the Kyousei2 model, which we have now noted as a global-scale application, and also the LPJ-GUESS reference. We had chosen the original reference (Smith et al., 2001) as it presents the standard reference for that model (similar to Sitch et al. (2003) for LPJ).

P4856 L2: Given that TRIFFID is a L-V model, do you mean to lump it in with this criticism?

Yes, that was intentional as TRIFFID does suffer from the amplified expression of dominance as that sentence points out. This is also evident in our work where we adjust our parameterization to be more similar to that of TRIFFID (the LV-COMP simulations).

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 veg- etation 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 sim- ulate competition for resources, and so I think it needs a little bit more introduction.

The L-V approach can suffer from a tendency to over-represent dominant PFTs, as we noted for TRIFFID and demonstrate in our LV-COMP simulations. However, we demonstrate here in our CT-COMP simulations that a slight change in the parameterization can result in more realistic PFT distributions. We agree that using -unmodified- the L-V approach, as it is designed for predator-prey interactions, is

not the right way to simulate competitive interactions between PFTs and that is part of the purpose of this paper and was also demonstrated by Arora and Boer (2006). We have added some text to try and expand upon the point raised by the reviewer.

P4864 L5: Given the amount of discussion devoted to the comparison of the alterna- tive 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?

The binary nature of the treatment of the b term is related to the manner in which two PFTs interact represented by $f_\alpha^b f_\beta$ in equations (2)-(4). When $b = 1$, the interaction between PFT α and β occurs over the fraction $f_\alpha f_\beta$. If the fractional coverages f_α and f_β are taken to indicate the probability of finding a particular PFT in some region of the grid square, then the probability of independently finding both is the product $f_\alpha f_\beta$. Interaction occurs in these common regions in what might be termed the "random interaction" case. The choice of $b = 0$, by contrast, implies that the dominant PFT has full access to all subdominant PFTs and invades them in proportion to their coverage in what might be termed the "full interaction" case. This case may be thought of as corresponding to the general availability of the seeds of the dominant PFT that may germinate and invade the coverage of the subdominant PFT provided the climate is favorable. We have added this explanation into the MS.

P4869 L25: Again, revising the same point as above, does the CTCOMP simulation simply increase the competi- tive inhibition of expansion of the dominant PFT? Is it a proxy for landscape variability and the processes controlling coexistence?

As discussed above the full interaction case allows PFTs to invade other PFTs and the bare fraction in proportion of their coverages. In the case where only two PFTs are present (e.g. a dominant and subdominant PFT) this implies the sub-dominant PFT has "full access" to the bare fraction and the dominant PFT has "full access" to the sub-dominant PFT. The former effect more than compensates for the latter and the sub-dominant PFT is able to coexist, at equilibrium. The approach is thus likely a proxy for landscape scale processes.

P4873 L23: Some discussion of Reich et al. (2014) and their work on needleleaf tree parameterizations might be appropriate here.

We have added some discussion in to the manuscript about this paper. This is an interesting avenue for exploration for future model development but at present we use fixed leaf turnover times.

P4875 L1: How are they parameterized as tropical? Through the climate envelopes, or some other feature of the parameterization?

Yes, the climate envelopes are the primary control on the evergreen broadleaf PFT distribution but this PFT also has other parameters like Vcmax or cold thresholds to induce leaf fall that are specific to the intended population of EVG-BDL trees. We have included an additional sentence in the manuscript to clarify this.

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?

That is an interesting point. Yes, they do tend to do better than other PFTs in seasonally dry climates (as one would expect). The difficulty is that they also then do too well in wet areas where they behave like BDL-EVG species since they don't have their leaf loss triggered. The model is missing some physical processes to counter this, which is one reason we took pains to discuss the use (and misuse) of bioclimatic envelopes in the Summary and Conclusions section. Bioclimatic envelopes are required to counter the influence of missing or poorly represented physiological processes.

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.

Determining the triggers for leaf fall of deciduous trees is not possible from remotely-sensed datasets to our knowledge since the actual mechanism is not directly (remotely) observable. We are not aware of any landcover datasets that could provide this information. In addition, the reason for using the modified W2006 dataset is that it specifically maps the 22 GLC200 land cover types to CTEM's nine PFTs.

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.

The distribution of the BDL-DCD-DRY in the observation-based dataset (Wang et al. (2006)) is based upon a transition between the two deciduous PFT traits (BDL-DCD-COLD and BDL-DCD-DRY) delineated by latitudional bands. To be clear, the model does not use latitude to determine the distribution of these PFTs but instead relies upon competitive interactions and some bioclimatic limits. In the absence of an observation-based geographical distribution of the trigger of deciduousness (temperature or moisture), the assumption made by W2006 is perhaps not unreasonable. We now note in the manuscript that a bioclimatic index that has been evaluated at few

sites in its ability to predict the trigger of deciduousness would likely be a better predictor.

P4879 L25: How were these parameter values determined? Were they fitted in an ad-hoc fash- ion? 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.

This main comment was address above and as we said in our response above, we agree that this information should be included in the MS. The parameters values that differ between the model versions when PFTs' fractional coverages are specified and when they are dynamically modelled using competition between PFTs are shown in parentheses in Table 1 and several of the tables in the Appendix. For more clarity, we have now also included a listing of all tables that contain these parameters with values that changed. The reviewer is correct that to account for the production of bare ground and consideration of spatial expansion of PFTs we reduced their respiration rates. The parameters most closely related to productivity (Vcmax primarily) are better known due to compendiums like Kattge et al. (2009) and thus we prefer to adjust values with a less empirical basis, i.e. respiration observations are much more sparse.

P4880 L14: Which parameters? Surely that is a relevant thing to include here?

We have included a listing of them around the earlier comment. While they were listed and highlighted in all the tables, we do also see the value in listing them in one succinct location and this change has been made to the manuscript.

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?

No, the model was not fitted to the Amazon data. This was simply a model result. The parameterization process is expanded upon in the revised MS in response to the earlier comment above.

P4884 L15 - P4885 L12: This is a very useful discussion and analysis.

Glad to hear it.

P4886 L 9: The meaning of the altered parameter in the LV equations is still unclear at this stage. Does it have an interpreta- tion 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.

With this, and the earlier comments of the reviewer that were of a similar vein, we have included more discussion of the choice, and interpretations, of the b term in the L-V parameterization.

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.

The projection was chosen since it presents a reasonable compromise between the necessary evils of distortion in shape and distortion of area of the land surface. This projection, while maybe not as good on covering all the page as some, does allow for a reasonably undistorted view of the globe as a whole which we viewed to be of greater importance.

Anonymous Reviewer 2

We would like to especially thank Reviewer 2 for their care in providing comments on our (lengthy) appendix.

General comments

Representation of plant interactions remains a challenging question for vegetation distribution modelling. The purpose of this paper to improve the representation of plant competitive interactions in the Canadian Terrestrial Ecosystem Model is therefore totally relevant.

In this new model version (v. 2.0), the authors use a modified version of the LotkaVolterra predator-prey equations to represent competition between PFTs. The authors show that modifications improve model results compared to results obtained with unmodified L-V equations as well as with prescribed PFT fractional coverages. The new parameterization of L-V equations allows the coexistence of more species than with unmodified L-V, reducing notably the dominance of tree PFTs on grass PFTs.

This paper is first a global validation of the model after different re-parameterizations required by the new plant dynamics and other improvements made since the CTEM version 1.0. It does not present actually any new modelling concept or tool. The competition scheme using modified L-V equations was already presented in a previous CTEM paper (Arora and Boer, 2006, Earth Interact. 10, 1-30) and it is not the first model which uses L-V equations to represent competition. Moreover, the number of simulated PFTs remains very low (7 PFTs) and the spatial resolution quite coarse (3.75 degrees) compared notably to the Community Land Model (CLM) (integrated in the Community Earth System Model) which can simulate 16 PFTs in finer scale simulations (Oleson et al., 2013, Technical Description of version 4.5 of the CLM).

Yes, the competition parameterization used in CLASS-CTEM was first presented in Arora and Boer (2006) as we explain in the last paragraph of the Introduction. The fact it is not the only model to use L-V relations is an important point of this paper. The other applications of L-V relations use these in an unmodified form which we demonstrate in our paper to lead to excessive areal coverage of dominant PFTs. We agree that other models, like CLM, will use more PFTs at finer spatial resolutions, but if the underlying competition parameterization (in the case of CLM this is the LPJ - highest NPP wins parameterization) leads to inaccurate global distributions, the extra PFTs and spatial resolution do little to help achieve more accurate results (as indeed appears to be the case for the version of LPJ presented in Cramer et al. (2001)). Our paper presents the first global validation of the competition parameterization and also a full model description and evaluation of CTEM v.2.0, which falls well within the scope of GMD.

Descriptions of changes performed for this study are very detailed and adaptions made since CTEM version 1.0 in related works are integrated in Appendix and well documented. Nevertheless, the paper is quite long and some parts, e.g. the description and discussion of results, could certainly be reduced. Some very long sentences and misplaced punctuation make sometimes the reading difficult.

Specific comments

Though model descriptions are very complete, the modifications of the L-V equations through the empirical parameter b (p. 4859 and 4864) are yet rather poorly justified. How has the value of b been determined? It is surely explained in Arora and Boer (2006, Earth Interact. 10, 1-30) but authors should develop again here. Maybe could they show some tests of the sensitivity of the results to this parameter b?

This question has been highlighted also by the first reviewer (a more detailed response was included above). We have now included text explicitly detailing how the b term changes the behaviour of the LV equations in the revised manuscript.

Same comment for the re-parameterization required after the modification of the competition scheme (p. 4879). It is difficult to find which parameters have been changed and the consecutive impacts on carbon and water fluxes. Authors should clearly indicate the modified parameters and the tables where the new values are presented. How did you get the new values? By optimization using observation-based datasets?

This point also came up with reviewer 1 and we have added text into the MS detailing these changes. Please also see our response to reviewer #1.

Even if some statistics are presented about how the different simulations compared to observation-based estimated (principally Figure 5), the frequent use of expressions like "compare reasonably with" are not very indicative of the agreement level with observations. P. 4870 line 8, authors describe as "fairly reasonable" a correlation of 0.38.

On p. 4870, we are comparing the spatial patterning of the grass cover to an observation based dataset. We use the term 'compare reasonably' as the spatial pattern of the model is similar to that of the observations. However, the correlation is only 0.38 as the model has large amounts of grass in the arctic regions. Thus our indication of comparing reasonably is describing the grass cover over much of the globe while the correlation is reduced by the overestimated grass cover in the arctic. We describe the reason for this overestimation starting on line 18 of the same page.

P. 4867 line 11, authors should directly present some global statistics.

We have added these in.

Concerning the structure, the results section is very long. Authors should reduce it. There are some repetitions between section 4.2 (Geographical distributions) and section 4.3 (Individual PFTs).

In the revised MS we have removed any unnecessary overlapping content.

In section 4, the comparisons between the three simulations (CTCOMP, LV-COMP and PRES) are sometimes irrelevant (e.g. p. 4880 lines 27-28-p. 4881 lines 1-4). You should only focus on comparisons CTCOMP-observation-based estimates. Similarly, Figure 7 should display a column with observation-based estimates (even if estimates are not available globally).

The point of comparing between LVCOMP and CTCOMP is to understand how the choice of the LV parameterization influence the model outcomes (this addresses the reviewer's request for sensitivity study of the influence of choice of the b term above). This appears to be a relevant outcome. The observation-based estimates maps have been added to the figure, but due to the size of the figure (it is now a 4x4 grid) we have had to split it into two.

Summary and conclusions section should only focus on main outcomes of the study. The approaches currently used in other models and their limitations have been already listed in the introduction section (p. 4855). So, this paragraph can strongly be summarized (p. 4884 lines 1-14). The discussion about bioclimatic limits within models (p. 4884 line 15-p. 4885 line 12) should appear earlier and surely not in conclusion (section 2.1.4 ?).

The start of the Summary and conclusions section does have some overlap with the introduction, however it does lead into a discussion on bioclimatic limits that we feel is useful and relevant to our paper (a sentiment also felt by Reviewer #1 who is appreciative of this discussion). We do introduce our bioclimatic limits earlier in the paper (section 2.1.4) but we are choosing to use this section to also address how bioclimatic limits are being used in other models and what the downsides of their use may be. This discussion does belong in this section thus we have renamed it from Summary and Conclusions to Discussion and Conclusions section.

Technical corrections

For the technical corrections, we provide replies only to comments that we wish to discuss or are directed at clarity or scientific content. Otherwise, any typographical comments were simply adopted.

p 4853 line 13: please use singular for "respond" and "influence"

p 4853 line 22: remove comma

p 4854 line 28: use plural for "adds"

Singular is correct here since it is actually only one action being described.

p 4858 line 22: maybe change "During competition"

p 4859 line 4: replace "with" by ";"

Prefer original wording

p 4863 line 12: what is a e-folding sense?

e-folding is time over which an exponentially growing quantity increases by a factor of e (https://en.wikipedia.org/wiki/E-folding). It is analogous to doubling time for base-e. We have clarified this term.

p 4872 line 23: remove comma between "grass" and "cover"

p 4875 line 16: change "at" by "with"

p 4876 line 13: use plural for "precludes" p 4876 lines 14-20: I suggest to move the paragraph "While. . . " in line 7, just after the sentence starting with "The bioclimatic indices. . . "

This appears to already be the case (the 'While . . . ' paragraph already starts there and there is no 'While' on line 7).

p 4877 line 25: use plural for "grass"

p 4877 line 27: use plural for "response"

p 4878 line 25: I do not understand ". . . which may be important is parts of. . . "

Should have been 'which may be important in parts of'

p 4879 line 9 and line 18: use plural for "coverage"

p 4880 line 16: remove "s" to "simulations"

p 4881 line 8-12: maybe sentence could be simplified ("CTCOMP and LVCOMP simulations" twice in the same sentence)

p 4881 line 10: use plural for ''simulation"

p 4881 line 27: Please explain why annual fire emissions are highest in the CTCOMP simulation

Fire emissions in CLASS-CTEM are calculated from the fire extent in a region and the amount of vegetation and litter biomass available for burning. The fire extent is determined from the moisture conditions, availability of fuel, and presence of an ignition source. All simulations have the same ignition source availability but differ in their moisture conditions (which are represented by the root zone moisture content) and availability of fuel (vegetation + litter biomass). While the CTCOMP simulation has the lowest vegetation biomass of the three simulations, it has the highest litter mass. Litter is assumed to have high flammability (its dryness is calculated from only the top soil layer, not the root zone weighted value as is done for the dryness of the vegetation biomass) and it has the second highest combustion factors after leaves (Table A7). Thus for an equal size fire, an area with more litter will have higher emissions. This yields the highest fire emissions for CTCOMP while its area burnt is the lowest. This result demonstrates the complex interactions that occur in the model's fire parameterization.

p 4882 line 3: Which contemporary observation-based estimates did you use?

This was referencing Table 3 which lists the Mu et al. (2011) and Giglio et al. (2006) estimates. We have made this more clear.

p 4882 line 5: use plural for "coverage"

p 4882: Why comparing only with PRES simulations and not with observational estimates?

Since the PRES simulation uses prescribed PFT fractions, the difference between the model runs (CTCOMP and LVCOMP) and the PRES simulation demonstrate the impact of the dynamically determined PFT distributions on the model outputs, which is the intent of this section. Earlier CTEM publications with prescribed fractional coverage of PFTs have been compared directly with observation-based estimates. In any case, Figure 6 does compare zonal distribution of GPP, vegetation biomass and soil carbon mass for three simulations with observation-based estimates.

p 4883 lines 17-20: I do not understand this sentence

Removed.

Summary and conclusions

p 4884 line 18: remove "s" to "PFTs"

p 4885 line 11: use plural for "distribution"

p 4885 line 14: what do you mean by "fairly relaxed"?

There is no strictly quantified way to describe how strongly the model uses bioclimatic limits, thus we use the term 'fairly relaxed' to describe how much the model imposes bioclimatic limits. This viewpoint is expanded in the sentences that follow the use of the term in the manuscript.

p 4885 line 19: add a comma after PFTs

p 4885 line 24-25: remove commas in ", and modified,"

Appendix

p 4887 line 23: please use plural for "process"

p 4890 equations A9-A10: what are (2.1) and (1.2)?

These are the values used in the standard Q10 function as defined below equation A4.

p 4891 line 10: add commas for "as a result"

p 4891 lines 17-18: add a dash for "leaf level"

p 4892 line 10: I do not understand "nitrogen/time"...

Nitrogen in the canopy has been found to, over time, become distributed in a similar manner to how photosynthetically active radiation (PAR) is absorbed. There is thus more at the top and less at the bottom of the canopy which allows plants to take advantage to how the light is distributed in their canopy. We have expanded on this in the manuscript to make it more clear.

p 4893 line 13: remove commas after "(gc)" and "(gb)" p 4893 line 15: remove dot

p 4894 line 5: use plural for “respiration”

p 4894 line 20: use plural for “sensitivity”

p 4895 line 7: remove “by”

p 4896 line 13: add “on” before “a PFT-dependent”

p 4897 line 13: use singular for “temperatures”

p 4897 line 18: what is a ‘’log math”? not very clear…

Changed to: math with logarithms

p 4897 line 21: please correct “mircobial”

p 4898 equations A35-A36: Is it “100.0” not “1.0”?

No, it is correct as is. It is a scalar that varies between 0 and 1.

p 4898 line 11: use plural for “comes”

p 4898 line 14: change “fashion” by “manner”

p 4898 line 14: use singular for “respirations”

p 4899 line 7: use singular for “biomasses”

p 4900 line 18: add “up” to “add up to one”

p 4901 line 3: use plural for “biomass”

p 4901 line 27: add “the” to “all the NPP”

p 4902 line 15: change “is not” to “are not”

p 4902 line 25: please add ‘’that’‘before’‘a give amount” to clarify the sentence

p 4905 line 6: use plural for ‘’maintenance and growth respiration”

p 4905 line 7: add a comma before “it is possible”

p 4908 line 1: specify “fire disturbance”

p 4908 line 11: Why a representative area of 500 km2? Maybe explain…

The representative area was chosen based on observed fire counts in a single day period. Since the CTEM parameterization is based on one fire per day per representative area, the area had to be sufficiently small to allow only one fire per day. Based on Figure 1 in Li et al. (2012), a 500 km^2 is an appropriate size to not have more than one fire per day and still be a large enough area to be assumed representative of the gridcell as a whole. We have added this explanation into the MS.

p 4910 line 15: add ‘as’ before ‘a surrogate’ p 4915 line 4: use plural for 'contributes'

p 4917 line 8: Please split sentence "Crops increase…" in two phrases. Start a new sentence from "However"

p 4917 line 10: why not use the sum of degree-days for harvest?

We believe it is more realistic that crops would be harvested when they have reached maturity (here we use LAI as a surrogate). Assumedly degree-days sums could also work to some extent but that might not account for droughts which would cause the plants to take longer to reach maturity (as determined by LAI) but not be captured in a degree-day formulation.

p 4917 line 15: use plural for "leads"

p 4918 line 22: change "," to ":"

p 4918 line 26: use plural for "depends"

References

p 4865 line 4: Ramankutty and Foley (1999) does not appear in the bibliography
p 4921 line 17: correct "CMPI5"

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