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Interactive comment on "Robust Ecosystem Demography (RED): a parsimonious approach to modelling vegetation dynamics in Earth System Models" *by* Arthur P. K. Argles et al.

Anonymous Referee #1

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In this paper, Argles and co-authors introduce the 'Robust Ecosystem Demographic' (RED) model. RED is introduced as an alternative to cohort-based vegetation demographics models, and its justifications are largely presented as being in opposition to more complex approaches that discretize tree size and age since disturbance. Instead of discretizing age since disturbance and tracking individual cohorts performance, RED makes several simplifying assumptions including:

1. Productivity for each plant size class is not calculated as a function of it's resource availability within a PFT x age class matric (as for a typical ecosystem-demography based VDM) and instead is assumed to scale with plant size, as per the idealized

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‘Metabolic Scaling Theory’.

2. Thus, there is no possibility of relative plant size determining competition for light, and hence productivity and growth, and thus all plants of a given PFT are supposed to occupy the same area.

3. The horizontal area is divided into said PFT tiles, and another tile wherein disturbance and seed establishment occur.

On account of these various simplifications the model can be solved analytically for a given productivity and mortality rate and RED is generally proposed as an alternative method for the simulation of some aspects of vegetation demographics in Earth system models (ESMs). I appreciate the novelty of this approach, and think it is important that a diversity of avenues are taken towards improving the representation of the terrestrial carbon cycle within ESMs.

While this is an interesting set of concepts, and potentially an interesting ‘middle ground’ in complexity of representation of vegetation demographics, there are numerous issues with the presentation, description and validation of the model that I find problematic in this paper.

First, in the introduction, there is insufficient explanation of the existing diversity of approaches to the simplification of forest models. A class of models already exists which is much closer conceptually to RED, e.g. the POP (Harvard et al. 2014) and ORCHIDEE-MICT (Yue et al. 2018) models, that also track different size or age cohorts within a single tile devoted to each PFT. While RED additionally provides a DGVM capacity in the form of the competition for seed recruitment, it seems that this class of models certainly requires description at the very least.

Further, despite the numerous mentions of the PPA approach, the paper does not actually describe this alternative approach to defining ‘tractable’ solutions to demographic modeling. A comparison of the RED and PPA approaches would be interesting, in particular given the fact that the PPA requires slightly more parameters than RED. This is particularly relevant given that the PPA is also implemented in the GFDL ESM.

Instead of a description of the relevant literature, the current justification statements in the introduction focus on somewhat vague assertions that full ED-type size-and-age structured approximations are too cumbersome. A comparison with more similar models would be helpful, as would a more general depiction of the pro's and cons of the approach used here. The model clearly has some benefits in terms of simplicity and tractability, but also has some drawbacks in terms of reduced ecological fidelity compared to real ecosystems. Given this, it would be good if the paper at some point addresses the questions for which RED would and would not be appropriate.

Many demographic model development activities, for example, as specifically motivated by a desire to include greater diversity of functional types in ESMs, and to predict their distribution as a function of their plant traits, which in most models primarily impact upon growth. Removing the ability of the model to simulate growth-based competition for light, and indeed, to simulate a diversity of trees within the same class, means that this RED would not be suitable for that problem.

Further, the introduction suggests that part of the motivation for resolving tree size is to introduce size-dependant physiological processes, but by introducing the metabolic scaling of productivity from an arbitrary reference size to all of the other classes, RED is also unable to simulate how tree size actually affects physiology - e.g. plant hydraulics, light availability, fire damage, allometry (and thus allocation and demand for nutrients in pools of different stoichiometry), size dependant rooting depth (and thus uptake of water and nutrients) , burial by snow, etc. Many developments of demographics models are specifically motivated by the representation of size, so again,RED could not be used for those types of question.

As a parallel, RED also does not provide discretization of the time-since-disturbance continuum, and instead really divides the grid cell into various PFT tiles, with resolved height, and one ‘gap’ tile, where new seedlings compete for space. Many demographic model developments are motivated by the ability to represent how the development along the successional trajectory impacts physiological boundary conditions. Examples of this include simulating the dominance of N fixers in early succession, of the matrix of post-fire disturbance conditions (including the vertical co-existance of grass and trees), representation of variation in light conditions to capture successional composition shift and the horizontal variation of vegetation height in systems which are buried by snow.

I am also highly skeptical of the authors claim that mortality rates can be backed out from spatial coverage of a particular PFT. Given that only a single not very convincing validation is presented, I remain far from convinced that this is a reasonable model inversion method. While it might be mathematically plausible, given the myriad simplifying assumptions of the model, I’d like to see how robust the mortality estimates are to variations in the seed production rates and minimum size, as well as assumptions on the spatial arrangement of crowns, and indeed, the uncertainties in the estimates of PFT areal coverage.

Lastly, the paper has some issues with clarity that I have tried to cover in some detail in the following comments. Some of the excessive mathematical details could be moved to the appendix as they rather detract from the flow of the paper. A more informed and nuanced description of how the model fits into existing demographic model literature and of it’s strengths and weaknesses would, I think, be more useful for the general readership.

Specific Comments.

P1, L21: The statements in these first three sentences all need references.

P2, L7: I’m not sure what to take from this assertion that uncertainty ‘can be attributed’ to CO2 responses and regrowth. It can also be attributed to a lot of other features of LSMs. Is it really necessary to state this so definitively?

P2, L9: You didn't really describe or define what a DGVM is yet.

P2, L15: Here it is indicated that 'processes that are dependant on size' is a core motivation for the implementation of this concept, but RED actually ignores that size of all except the reference tree, using an assumption to scale to the other size classes. There are lots of processes that do actually depend on size (hydraulics, allocation, fire mortality, competition for light, wind damage, snow burial, etc.) and so this is a genuine justification for using a size-structured model, but it does not apply to RED. Therefore, a different justification is required.

Further, in ED-type models, the faster regrowth after disturbance is typically predicted on the use of multiple tree types that exist in early, mid and late successional systems (as opposed to an average, slower growing tree).

P2, L21: This is true, but you are also going to get lots of different outcomes of climate change from alternative parameterizations of RED - parameters that are absent from the simpler model are really just assumed to be fixed in RED (e.g. the decay coefficient of productivity with size, seed production, competition parameters). Making the parameters either assumed constants or round numbers doesn't make their uncertainties go away. It would be more interesting to actually look into these uncertainties and illustrate a succession experiment under a range of model assumptions.

P2, L24: Cohort models are numerically unwieldy and no-doubt more expensive, but as you attest later in the paper, it is disingenuous to state that they make a new patch every timestep when in fact ED-derived models immediately fuse the newest patch to the next largest one.

P2, L26: Cohort models can either track tree age or tree size, so adding this here to distinguish RED from a cohort model doesn't really make sense.

P3, L5: The way in which this equation is presented seems overly contrived. Surely it can be presented such that the dn/dt is the sole term on the left hand side?

P3, L7: Neither g(m) nor lambda(m) appear in the actual equation, so this is again a little hard to get ones head around.

P3, L11: What did Niklas and Spatz find or do, briefly?

P3, L16: I do not understand how the last term translates into fractional area, when it looks like it should just return 'area'. Further, is there no constraint on the area the trees can occupy? That seems strange and needs further discussion.

P4, L1: I'm not sure why you need to state that the model conserves carbon three times. All vegetation models must conserve carbon. This isn't very surprising.

P4, L3-15: I'm not sure what purpose is served by this sequence of equations.

P4, L20: This equation would be easier to read if it were split into terms for seed recruitment and growth.

P4, L23: The PPA assumes minimum overlap of crowns within each layer of the canopy. It distinctly does not assume no overlap of PFTs. It assumes that canopies are arranged into layers and within each layer there is no overlap. Competiton for light occurs at the boundary of the layers, and is a strong control on ecosystem assembly. In fact, much of RED is highly contradictory to the PPA concept, given the MST rejects the need to different growth parameters as a function of light availability (as demonstrated convincingly for tropical forests by Farrior et al. 2016). I think it's thus a little disingenuous to cite the PPA here as a justification for this assumption.

P4, L25: "injected"? How do trees get injected?

Figure 1: I don't find figure 1 particularly informative. It would be better to have a depiction of the actual area available for seeds and to illustrate how the different PFTs might affect the allocation to each PFT. This figure just tells me that shrubs are smaller than trees.

P5, L4: The calculation of the area occupied by each PFT, as it is introduced here, needs a lot more explanation. In the description on L16 of P3, it simply states that the area of all the mass classes is added together, such that there is no overlap between the canopies of the trees in each plant type. This implicitly assumes that all the trees are in the 'canopy' layer, (using PPA terminology) and thus by implication that they should all get the same amount of light. Of course, via use of equation 2, the actual light environment of the plants is divorced from assumptions about their spatial arrangement, but it seems like a strong assumption to me to include no possibility of additional canopy layers. What happens when the total amount of space occupied by the plants exceeds the ground area available?

P5, L5: It should be noted here that the Cox 2001 paper is at-least inspired by the Lotka-Volterra approach, to better allow connection of this concept to community ecology literature.

P5, L7: Later on you state that the coexistence between PFTs of the same type doesn't actually work, so this statement that Eqn 12 allows for coexistence is a little misleading.

P5, L8: This allows succession as you note, but only between the PFT of different classes, not within a given class, unless I'm mistaken? . Figure 3: I'm not really sure what this Figure is supposed to illustrate. What are the red dotted lines in the middle of the triangle? There are three heavy double headed black arrows and not one (as implied by the legend).

Eq 28 and 29: These equations need a bit more explanation and description. This section feels like you are making a concerted effort to lose readers. Is it really necessary that everyone understands how the equilibrium solution of the model is derived? Could this go in an appendix?

P10, L1: As I said above, I am highly skeptical that this is a robust way of estimating turnover, given the uncertainties to do with seed production and spatial extent.

P10, L8: So, productivity was derived from JULES using TRIFFID? Were the outputs saved for each month? Is there interannual variability? This needs a bit more detail.

P11, L3: Is this really how succession works in Amazonian forests? I think it's really mostly trees that are present in the formation of small to medium sized gaps.

P11, L5: Can you illustrate the dependance on alpha and m0?

P12, L11: What are we to take from this illustration of 'succession' in the model? There isn't any comparison with data, nor an illustration that the model fixes the issue of slow recovery from disturbance that was raised in the introduction. What controls the area fractions of the smaller PFTS? Is there always some gap fraction dedicated to them? How is this equilibrium maintained?

Figure 6: The inputs of productivity taken from JULES do not, for example, allow BETs to grow outside of the tropics, and so many of the critical questions related to the prediction of biome boundaries that are asked of DGVMs cannot be addressed in this circular analysis.

P14, L1: It seems that reproducing the PFT map should be a trivial matter given the productivity inputs illustrated in Figure 6.

P16, L8: This is confusing because the reference to Figure 10 comes before it is described. The use of the mortality rates in these simulations is not described in this section until now.

P17, L1: To what does this 'diagnosed mortality rates' refer? Isn't this sentence about diagnosing mortality rates? This adds another layer of confusion onto my previous comment.

Figure 9: This color map does not allow one to distinguish between most of the lower turnover areas. You need some sort of logarithmic variation in color with mortality rate.

P17, L8: How influential is the minimum recruit size? This definitely needs to be illustrated.

P17, L10: The sentence that begins “Under the assumption” isn’t a whole sentence. Moreover, what is the aim of defining a ‘healthy’ environment? You need to state what you are trying to achieve first. . .

P17, L12: This is a very quick and potentially confusing switch to discussing the growth-mortality ratio and not mortality (you should maybe also re-state what ðİIJĞ is as this is a non-standard quantity.

P17, L13: This number seems extraordinarily high for the stem turnover rate of tropical forests? Comparison with data is, of course, where this aggregation idea is problematic, as mortality rates have clearly been shown to vary with tree size (Lines et al. 2010, Johnson et al. 2018), and thus the range of tree size with which one can compare these rates is unclear, particularly the lower size boundary.

P17, L13: Table 3 contains goodness of fit metrics, and not estimates of mortality.

P17, L15: The ‘value within the paper’ doesn’t state which paper, nor why it needs converting. Thus is very confusing.

P18, L1: This text on the differences between the Moore paper value and this value (which are indeed extraordinarily close and probably don’t need excusing) would be better spent describing first how the Moore method differs from RED. This section assumes the reader is familiar with, for example, the non-discretized nature of the Moore method.

P18, L5: “Potentially providing a future constraint on ESM growth rates for PFTs.” is not a whole sentence.

Figure 10: The mortality numbers in figure 10 for tropical forests seem too high. (0.07-0.08). Again, it’s hard to know what mortality rates they can be compared to. In Table 4, the numbers are different from the figure, perhaps because they are area weighted, but this isn’t really clear from the text.

P18, L11: I’m not sure what “within the top 25% of coverages” means, nor what this

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is trying to achieve. Further, there is no data in figure 10, so I am not sure why one is supposed to conclude that the model captures the data well. Maybe you actually mean figure 11, which reduces the RED estimate, but only down to about double the observations. Given the a doubled mortality rate is approximately equal to a halved biomass, I'm not sure that this provides a very convincing validation. Further, many estimates of mortality are lower than this. Lewis et al. 2004 find mortality rates of tropical forest from 1.5-1.7%, for example.

P20, L4: I could not find a definition of DET prior to this usage here.

P22, L1-10: I'm not sure what to take from this section about fire. The last line seems to suggest that RED overestimates fire mortality, when figures 12 and 13 seem to show the opposite. The logic of this section needs tightening.

P23, L1-6: This, and the paragraph above, are in need of more references.

P23, L6: This statement about patch merging is incorrect in its assertion that patches can only be merged after a certain age in ED-type models. Further, it does not illustrate that this is actually problematic, and simply asserts as such. Fusion criteria are indeed to some extent arbitrary, but that this is a genuine problem has not actually been demonstrated.

P5, L7: Which important features is it designed to capture exactly? This hasn't really been stated.

P23, L16: Metabolic scaling theory has been widely debunked by numerous studies comparing its predictions with observations (MullerâĂŘLandau et al.20016; Russo et al. 2007; Coomes et al. 2010, Ruger and Condit 2012) in particular where asymmetric competition for light (e.g. in forests) is important.

P23, L18: I am not sure how the seed model allows you to capture the effects of light competition. It allows you to represent the impacts of recruitment competition, but seems to me that it explicit does not include light competition.

P24, L1: It is stated here that equation 12 is a promising method to deal with the problems of coexistence in RED, but equation 12 is already part of RED, thus how can it be the solution? Further, I do not know what 'gap boundary conditions' refers to here.

P24, L4: I am skeptical, without further much more robust testing and illustration, that these relationships would be meaningful.

P24, L13: I do not think that this model is 'based on' the ideas of the PPA in any meaningful way. The idea of the PPA is primarily concerned with how trees fill space, which is specifically ignored by RED, and also on the division of the canopy into discrete layers, which is definitively at-odds with the metabolic scaling method of disaggregating production solely based on tree size. P24, L16: It apparently can be fitted, but I'd argue that there has been no validation presented to show that this is 'effective'.

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