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Comment

Interactive comment on “A stand-alone tree demography and landscape structure module for Earth system models: integration with global forest data” by V. Haverd et al.

V. Haverd et al.

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Received and published: 7 May 2014

Anonymous Referee #1

Comment 1.1

This (POP) remains an interesting idea, but I have the impression that the authors are over-selling its utility as a full alternative to the dynamic vegetation models that they introduce at the beginning. It seems that the approach as proposed will only work under static disturbance regimes where the rate of return of catastrophic events is constant, and therefore the equilibrium landscape distribution can be estimated trivially.

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Response 1.1

It is not our intention to suggest that we are solving all problems with current DVMs. Indeed, POP is not a full DVM but, as stated in the title of the paper, a scheme for adding woody vegetation demography (size structure) to the land surface component of an ESM. We have added a new statement from p2347,L23 to make this as clear as possible: “POP is not a new DVM, but a scheme for dynamically estimating size structure and turnover of woody vegetation, forced by productivity information from an external LSM.” However, we think it is relevant to provide an overview of the major issues with the vegetation dynamics schemes in large-scale models, some aspects of which we are attempting to address with our approach. We have added “some aspects of” on p2347,L10 to make this absolutely clear. We defended the landscape scaling component of POP in the cited earlier paper (Haverd et al. 2013). The present paper is concerned with the patch-scale dynamics; an in-depth discussion of the landscape scaling method would be out of scope and would distract the reader. For the record, we expect the equilibrium assumption implicit in the Poisson-based method of weighting patches of different time-since-last-disturbance will be robust to a gradually changing disturbance regime (typical for most climate change studies), though the approach will break down in the case of a step change in mean disturbance interval.

Comment 1.2

There is no mention of how carbon resources might be partitioned among different plant functional types, nor how competing plant functional types might obtain more or fewer resources in different light regimes represented by the model.

Response 1.2

Again, POP is not a full DVM, but a scheme for adding woody vegetation demography/size structure to land surface models. Current LSM's generally do not feature competing plant types, but may have fixed tiles representing grid cell fractions dominated by different types of vegetation. As many LSMs currently lack any representation

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of size structure dynamics - and it is likely to be important for getting carbon dynamics right in transient climate simulations - we think POP represents a relevant advance. However, we plan to introduce PFTs and to distinguish canopy and understorey strata in a later development of the approach.

Comment 1.3

Only one number (biomass increment or biomass turnover) is passed between the CABLE and POP models.

Response 1.3

A good principle is that a model should be as simple as possible but no simpler (with respect the intended application). Our goal (p2347,L13-15) was to design an approach that would be technically straightforward to couple to an existing LSM but still improve on the large-area approximations of many first-generation DVMs, shown to be inadequate in earlier studies (e.g. the cited paper by Wolf et al. 2011). As pointed out on p2346,L23-27, coupling of more detailed schemes typical for second-generation DVMs may be time-consuming and technically challenging, due in part to overlapping process parameterisations in the LSM and DVM models, and a large number of drivers and response variables needing to be exchanged between the models on different time steps and scales. Our approach is (deliberately) much simpler and technically trivial to couple to an existing LSM. We believe this will be appreciated as an option by the community now setting up ESMs for upcoming model intercomparison projects.

Comment 1.4

How would the structure deal with multiple plant types contributing to both of these pieces of information? Maybe these are implicit, or the model is not supposed to predict these properties, but either way, the approach is introduced and its use is promoted without any discussion of what the potential caveats or limitations might be, compared to the models they are supposed to replace.

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Response 1.4

Like most LSMs, CABLE does not distinguish multiple woody plant types within a simulated tile. We agree this is a limitation compared for instance to a full second-generation DVM. But again, POP is not a full DVM but an intermediate option that in terms of structural dynamics fills a gap between a full DVM and the large-area approach of most current ESMs. As demonstrated in Figure 4, CABLE-POP outperforms current LSMs and even two second-generation DVMs included in the Wolf et al. (2011) study. We discuss limitations imposed by the LSM tile architecture and the modular coupling strategy of our approach in a new caveats section s4.2 of the discussion:

“The approach presented and demonstrated in this paper offers a potential alternative, suitable as a replacement for the Big-Wood approximation, for the representation of biomass structural dynamics for woody vegetation in large-scale models. POP is not a replacement for a full-featured DVM. It does not represent biogeochemical processes, nor in its current form competitive interactions among PFTs. POP is designed to be readily coupled to a biogeochemical LSM as implemented in many current climate and Earth system models. Such LSMs generally do not feature competing plant types, but may have fixed tiles representing grid cell fractions dominated by different types of vegetation. POP simulates size structure dynamics separately for each (woody) vegetation tile, based on the principle of asymmetric (i.e. size-dependent) competition between co-occurring individuals, but with no competition among PFTs (tiles). Competition between trees and grasses, deciduous and evergreen vegetation, and C3 and C4 plants provide an important explanation for global biome distributions and may modulate the responses of vegetation to future climate and [CO₂] forcing (Smith et al. 2014). We plan to introduce PFTs and to distinguish canopy and understorey strata in a later development of the approach.”

Comment 1.5

Maybe the authors intend to develop these capacities later, but it still needs to be

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mentioned. This opaque discussion makes this paper much less interesting to me, as the approach appears promising and parsimonious, in a field where such innovations are clearly required.

I hope the authors can modify the paper so that the pros and cons of their method are clearer to other researchers who might like to adopt a similar method in their own studies.

Response 1.5 We discuss limitations imposed by the LSM tile architecture and the modular coupling strategy of our approach in a new caveats section s4.2 of the discussion. (See Reponse 1.4 above).

Specific Comments

Comment 1.6

2345:Line 16: 'as' not 'ass'

Response 1.6

Corrected

Comment 1.7

Line 2346: Line 16 - The ED model, as I understand, does not have a stochastic component.

Response 1.7

ED works by fitting a set of partial differential equations to the size- and age-structure differentiated output from a gap-type stochastic stand simulation. The latter is described in detail in the original ED paper by Moorcroft et al. (2001), Ecological Monographs 71: 557-586.

Comment 1.8

2348: Line 15 - You haven't defined here what is meant by a 'patch'. Given the compli-

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cated and inconsistent use of this term in vegetation model literature, this is extremely important.

Response 1.8

We define “patch” to mean a stand of vegetation of sufficient extent to encompass a neighbourhood of individual woody plants, competing with one another in the uptake and utilisation of light, soil resources and space. In our model, a patch also represents a statistical sample of local stand structure within the landscape of a simulation grid cell. We now explain this on p2348,L25 (S2.1.1): “A patch thus represents a stand of vegetation of sufficient extent to encompass a neighbourhood of individual woody plants, competing with one another in the uptake and utilisation of light, soil resources and space. Patches are not spatially referenced, but represent a statistical sample of local stand structure within the overall landscape of the grid cell. Individuals are not distinguished within a cohort, but each cohort has an associated mean individual stem biomass (see below), from which other size metrics (height, stem diameter and crown area) can be derived (see Appendix A).”.

Comment 1.9

2348: Line 18 - why mention the second class of disturbance here if it is not used at all?

Reponse 1.9

We are here providing an overview of the whole model; disturbances and their impact on vegetation at the landscape scale are relevant as a context for discussing the role of stands and patches within the model.

Comment 1.10

Is something an ‘input variable’ if it is a constant parameter?

Response 1.10

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Disturbance return times are fixed in time in this study, where the focus is on the patch-scale dynamics and landscape structure merely provides a boundary condition. In the more general case, return intervals for the two classes of disturbance may vary in space (e.g. along a rainfall gradient, as in the earlier study by Haverd et al. 2013) or be prognosed by a forcing model. See Response 1.1 for comment on time-varying disturbance intervals.

Comment 1.11

2348: Line 21 - Do you mean age or size? They are not the same thing, as it is possible to have old, small individuals with suppressed growth, etc. Can you make this clearer?

Response 1.11

Our model explicitly distinguishes age classes (cohorts) but not individuals within a cohort. Each cohort has an associated mean individual size (in terms of height, stem biomass, diameter and crown area). We now state this.

“State variables are the density of tree stems partitioned among age/size classes (cohorts) of trees and representative neighbourhoods (patches) of different age-since-last-disturbance across a simulated landscape, representing a spatial unit (tile or grid cell) of an LSM. Hereinafter we use the term “grid cell” to refer to the spatial unit at which POP is coupled to the host LSM, in our study a vegetation tile comprising either evergreen needleleaved or deciduous broadleaved forest. A patch thus represents a stand of vegetation of sufficient extent to encompass a neighbourhood of individual woody plants, competing with one another in the uptake and utilisation of light, soil resources and space. Patches are not spatially referenced, but represent a statistical sample of local stand structure within the overall landscape of the grid cell. Individuals are not distinguished within a cohort, but each cohort has an associated mean individual stem biomass (see below), from which other size metrics (height, stem diameter and crown area) can be derived (see Appendix A).”

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Response 1.12

2348: Line 22 - Are the 'neighborhoods' spatially explicit of statistical concepts?

Comment 1.12

They are statistical concepts, see Response 1.11 above, including new text.

Comment 1.13

2348: Line 25 - Is this the total biomass increment of the whole grid cell? What about variation between plant types?

Response 1.13

Variation between plant types is accounted for in CABLE, as in most current LSMs, by distinguishing tiles corresponding to grid cell fractions dominated by different vegetation types, here evergreen and deciduous forest. POP is coupled to CABLE at the level of a tile. This is now explained on p2348,L13 and onward: "POP is designed to be coupled to a land surface model (LSM) or the land surface component of an ESM (Section 2.1.2) which provides forcing in terms of the annual grid-scale stem biomass increment (ΔC (kg C m⁻²)) for woody vegetation, as an average across a simulated tile or grid cell. In LSM's such as the CABLE model employed in the present study (see below), each tile represents the proportion of a grid cell dominated by one major vegetation type, such as evergreen or deciduous forest."

Comment 1.14

2349: Line 4 - This method for dividing up the NPP between cohorts is so central to the argument that I think it should be in the main section of the paper

Response 1.14

We have expanded the explanation of the NPP partitioning procedure (Eq 4) in the methods section of the main paper.

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Comment 1.15

For example, it isn't clear to me at this point how the model deals with cohorts of the same size that might be shaded in some late successional patches and fully lit in early successional patches.

Response 1.15

NPP is partitioned among cohorts following Eq 4, which is applied separately for each patch. A cohort of the same size that is fully lit on one patch will receive a larger proportion of the NPP than a shaded cohort in another patch that has accumulated more biomass since the last disturbance.

Comment 1.16

2349: Line 14 - If the disturbance is episodic, and the patch is reset completely by it, then how can this not invoke some kind of stochastic behavior? Is 'episodic' the right word to use here?

Response 1.16

The use of "episodically" was misleading and has been deleted.

Comment 1.17

2349: Line 16 - Where you say "this' threshold' I'm not sure what 'this' refers to in the context of the sentence or the following equation.

Response 1.1.7

The threshold referred to is defined in Eqn 2. We clarify in the manuscript by :

"We characterise the response of resource-limitation mortality to growth efficiency (GE, i.e. growth rate as a function of size) by a logistic curve with the inflection point at G_{Emin} "

Comment 1.18

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2350: Lines 1-5: The parameterization of the first two terms in this growth efficiency based model need more detailed justification. Models of mortality are notoriously poorly parameterized, and so a description of why these numbers (0.75 and 0.3) are used is needed. What data or methods were used to justify them originally? It is OK if this is a difficult subject, and a discussion of the provenance of the model would make this more interesting.

Response 1.18 - The 0.3 value is simply a proxy for the reciprocal of the number of years of stressful conditions plants may be expected to survive, i.e. a resilience parameter. It is not based explicitly on observational data but the same value is used in the LPJ-GUESS DVM (described in cited paper by Smith et al.). The power term 0.75 (s) dictates the expected proportionality between the area of plant resource uptake surfaces (leaves and roots) and stem biomass, justified on the basis of allometric scaling theory, and shown to hold empirically across a wide range of plant taxa from around the globe in the cited paper by Enquist and Niklas. We have added this information to the text: "Individuals are thus assumed to capture resources in proportion to the area of their resource uptake surfaces, estimated as the s power of stem biomass following the allometric scaling theory of (Enquist and Niklas, 2001)."

Comment 1.19

2350: Line 19: Ac_y is defined in the text, but doesn't seem to be actually used in the equation?

Response 1.19

This term appears in the inline equation for cpc_y on p2350,L18.

Comment 1.20

2351: Line 4 - I think there is some punctuation missing here.

Response 1.20

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Corrected.

Comment 1.21

2351: Line 12 - Does this assumption still hold if the disturbance interval is not static in time? (if not, this caveat need to be mentioned here, because it is likely that fires, pest and wind throw will all change in a non-static climate)

Response 1.21

It does not strictly hold if disturbances are variable in time. We have added a caveat on p2351,L17: "Strictly, the Poisson assumption demands that the mean disturbance interval is invariable over time, a difficult assumption to uphold in practice, as disturbance agents such as wildfires, windthrow, pest or pathogen attacks may increase or decrease depending on variations in climate and other drivers. A constant past disturbance regime was assumed in the present study."

Comment 1.22

2352: Line 3 - It is assumed, then, that all of the patches are biogeochemically equivalent, and that the lag in recovery of all the other processes (LAI, in particular) is negligible?

Response 1.22

Correct, this is a (simplifying) assumption. NPP (and therefore LAI) is assumed to be uniform among patches, while structure is assumed to vary.

Comment 1.22

2352: Line 15 - is each grid cell just one plant functional type?

Grid cells in CABLE are subdivided into tiles corresponding to different major vegetation types. See Response 1.13 above.

Comment 1.23

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2352: Line 21 - cold deciduous, presumably?

Response 1.23

CABLE makes no distinction between winter and drought-deciduous vegetation. We have added this information on p2352,L16.

Comment 1.24

Also, why is phenological habit a relevant input if LAI is specified by MODIS?

Response 1.24

CABLE has different plant physiological parameters for each PFT. We have made this clear in the manuscript by the following addition to Section 2.1.2: “Vegetation cover was prescribed as one of three of the CABLE plant functional types: Evergreen Needleleaf; Evergreen Broadleaf or Deciduous Broadleaf, each with its own set of physiological parameters.”

Comment 1.25

2352: Line 28 - Why is the model set up like this - (driven by LAI, only for some grid cells, no vegetation dynamic predictions etc.) I guess it is to compare against the biomass data with as few degrees of freedom as possible, but some sort of justification statement would be useful here (of what you are and are not testing). It is quite strange for a paper whose introduction is about DGVMs to specify both vegetation cover and disturbance rates as static, so at this point in the paper I am a little confused about the direction it is taking.

Response 1.25

The purpose of the CABLE-POP simulations presented here is to test CABLE-POP predictions of leaf-stem allometry against observations. This test is possible using simulations only at gridcells corresponding to the locations of the observations, with prescribed LAI and without vegetation dynamics. We now precede our description of

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the model setup with: “The model set-up in this study was designed to permit evaluation of CABLE-POP predictions of leaf-stem allometry.”

Comment 1.26

2355: Line 9 - The model self-thinning algorithm is calibrated against all of the forest data.

Response 1.26

No, as stated on p2354/L16-17, the model self-thinning algorithm is calibrated against 30 points from the upper bound of the full data set (comprising 482 points in total) when biomass is plotted against stem density.

Comment 1.27

Given that self thinning is driven by growth rates, ultimately, and that these will likely change through time, is this empirical fitting process applicable to future simulations?

Response 1.27

Yes this parameterisation is independent of growth rates and assumes only that the proportionality between log biomass and log density revealed by the observed data continues to hold true in a future simulation. This assumption is reasonable if mortality depends more on biomass than on growth rate. The robustness of this proportionality across wide climatic gradients is clear from the observed data as plotted in Figure 3.

Comment 1.28

2356: Line 12 - On the previous page, you describe how the parameters controlling these observations are fitted to the data, so which parts of the model-data comparison illustrate the model structure is performing adequately, and which illustrate that it has been tuned to the data against which it is being tested?

Response 1.28 As noted above (Response 1.27), and stated in the paper on

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p2354/L16-17, the calibration only uses 30 points out of the total dataset of 482 points. The calibration is limited to optimising the biomass to density relationship for these 30 points. The evaluation by contrast uses the entire data set and includes additional data (foliage biomass) and relationships (foliage:stem biomass). It is doubtful whether the strong overall agreement between the modelled and observed relationships depicted in Figure 4 and Table 1 could be achieved by tuning alone, in the absence of inherent (structural) model skill. We tried excluding the 30 calibration points from the full dataset prior to evaluation, but this made no material difference to the results.

Comment 1.29

2357: Line 15- This section (4.1) seems more like results than discussion to me.

Response 1.29

Section 4.1 has been moved in its entirety to the results section.

Comment 1.30

2539: Line 8 - I do not yet have a feeling, so far in this manuscript, for why it is important to specifically simulate the size distribution of trees in the forest, and how, for example, altering this property might change the overall response to forcing variables, in this framework (given they all have the same physiology anyway). I can imagine many possible reasons, but I think the specific motivating factors need to be spelled out here.

Response 1.30

We have added the following text on the specific motivating factors on p2358, L28 and onward: “Cohorts of different age will face different mortality rates depending on the microenvironment imposed by realised stand structure, and this in turn will vary among patches in a landscape, depending on the disturbance history. This suggests that it is important to specifically simulate the size distribution of trees in forest vegetation and account for how changes in this distribution may alter the response of ecosystem functions like NPP to forcing variables.”

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Comment 1.31

2360: Line 2 - You can use inventory data in outer ways - e.g. to determine the gross turnover and recruitment rates, not to mention total biomass.

Response 1.31

Inventory data can of course be used to evaluate a model with respect to gross turnover rates or total biomass. The problem is to build a model that can adequately prognose turnover rates or biomass. The study by Wolf et al. whose conclusions are summarised here convincingly demonstrates that big wood models commonly used in current ESMs lack such skill and that the root problem is that they fail to discriminate different size classes of trees. Quoting Adam Wolf's review of the present paper: "because these different cohorts face different mortality rates owing to asymmetric competition, the effective turnover rate is different from the diagnosed turnover rate for an "average" individual."

Comment 1.32

Figure 6: The text here is very small, and the labels (i,ii, etc.) appear to be missing

Response 1.32

The labels appear near the top left corner of each frame.

Anonymous Referee #2

Comment 2.1

One comment is related to the motivation of the model development. The POP model is presented in the context of DGVMs and it is presented as an important innovation for earth system models. I fully agree that DGVMs need to be improved and that the representation of vegetation structure is an issue in these models. Yet, my personal opinion is that model efficiency is not a problem and that improving model performance does not really bring dynamic vegetation modeling forward. I am sure that most DGVM de-

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velopers have huge computer resources and compared to climate models, vegetation models are cheap in terms of CPU time. I rather think that DGVMs can be improved by increasing the ecological realism of these modes for instance by improving competition models or by making use of the huge amount of plant trait data that is now available. Therefore, more sophisticated models such as ED or LPJ-GUESS or individual-based models such as SEIB-DGVM or aDGVM are necessary.

Response 2.1

As noted in our response to Comment (1.1) from Anonymous Review #1, POP is not a full DVM and is not being advocated as a replacement for full DVMs. Rather, POP is designed to fill a methodological gap between large-area parameterisations in many current ESMs that lack ecological realism and full second-generation DVMs that have greater realism but are considered too complex and technically challenging to be an option for most ESMs. We are involved in ESM coupling efforts ourselves and know the computational demands in terms of CPU time and memory usage associated with stochastic representations of vegetation processes are seen as a serious limitation by climate modellers. We feel we are describing the motivation and context of our study clearly enough, but in response to this and a similar comment from Anonymous Referee #1 have added a statement that “POP is not a new DVM, but a scheme for dynamically estimating size structure and turnover of woody vegetation, forced by productivity information from an external LSM.”

Comment 2.2

I do also not agree with the statement that DGVMs should be deterministic and that stochasticity in models is a potential disadvantage (p. 2346, l. 17). Nature is stochastic and there is also evidence for bistability in certain ecosystem types. For example, many regions where we find savannas and grasslands could also support closed forests in the absence of fire (Staver et al., 2011, Hirota et al. 2011, Higgins and Scheiter, 2012). The observed ecosystem type is not necessarily deterministic but it may be related to

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the system's history and stochasticity in vegetation dynamics and disturbance regimes.

Response 2.2

All real-world processes are stochastic, but due to aggregation averaging many appear deterministic when viewed at a large enough scale. Deterministic representations are generally preferred by ecosystem modellers. Canopy photosynthesis is for example always represented as a deterministic dependency of biophysical drivers even though light assimilation, nitrogen allocation, stomatal exchange etc vary randomly depending on the microenvironment of each leaf and in time due to varying sky conditions, sun flecks etc. ESMs and DGVMs are designed for continental-global and multi-decennial studies, and on these scales we would argue that deterministic representations are reasonable also for population and landscape processes. The performance of our model as depicted in Figure 4 supports this claim. The issue of bistability in ecosystem structure is certainly an interesting one. Our approach is amenable to representing feedbacks between vegetation structure and disturbances such as wildfires that could lead to alternative stable states depending on past disturbance episodes. However, such issues are well beyond the scope of the present paper, where the focus is on population processes at the stand scale.

Comment 2.3

I am not convinced that the model structure can adequately represent competition and the dynamics of different cohorts because interactions between cohorts are fully deterministic (but maybe I am wrong or misunderstood details of the model). For example, if several communities are initialised at the same time, then the properties and dynamics of all cohorts are identical and a cohort model is not really required. This could happen in a catastrophic disturbances where all cohorts are affected. When one cohort is introduced per year as it is done in the model experiments, the carbon allocation to a younger cohort is by definition less than carbon allocation to older cohorts. Hence, it is necessarily outcompeted.

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Response 2.3

This is asymmetric competition, is well-described in forest ecology, and is intentionally reproduced (as an emergent property) by our model. A cohort model is needed because trees of different ages experience differential mortality depending on the degree of suppression by larger neighbours, represented in our model by the NPP partitioning rule of Eq 4. The size structure of a stand thus determines the relationship between standing biomass and mortality-driven biomass turnover. “Big wood” models lacking size structure fail to represent this relationship, as demonstrated by the Wolf et al. (2011) study cited in our paper. Our model performs better as the results in Figure 4 show. It is not obvious why a stochastic representation should improve performance further.

Comment 2.4

p 2345, l 16: Replace ass by as

Response 2.4

Corrected

Comment 2.5

p 2348, l 21: Does the model simulate size or age classes? In fire driven systems such as savannas, trees have a high capacity for re-sprouting from their root resources such that they can be old but small.

Response 2.5

Each cohort has an associated average individual size, i.e. cohorts can be thought of as representing both age and size classes. We do not consider resprouting.

Comment 2.6

p 2352, l 1: Calculation of D_y not described in main text

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Response 2.6

We have added a reference to Eq A6-A9 in Appendix A on p2351, Line 1, which allow mean tree height and diameter to be derived from stem biomass for each cohort.

Comment 2.7

p 2352, l 10: Disturbances are, in the model, not linked to climate or the ecosystem state.

Response 2.7

Disturbance return times are fixed in time in this study, where the focus is on the patch-scale dynamics and landscape structure merely provides a boundary condition. In the more general case, return intervals for the two classes of disturbance may vary depending on climate (e.g. along a rainfall gradient, as in the earlier study by Haverd et al. 2013) or be prognosed by a forcing model, such as a wildfire module, that includes ecosystem state as a driver.

Comment 2.8

p 2352, l 15: It is not clear to me how the link between POP, CABLE and functional types works. These models only exchange biomass and turnover but no information about PFTs. However, in the analyses, needle-leaved and broad-leaved trees are considered. Are there two cohort models for two PFTs or is POP only used for the dominant PFT?

Response 2.8

Variation between plant types is accounted for in CABLE, as in most current LSMs, by representing separate tiles corresponding to grid cell fractions dominated by different vegetation types, here evergreen and deciduous forest. POP is coupled to CABLE at the level of a tile. This is now explained on p2348,L13 and onward.

Comment 2.9

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p 2354, l 24: fc not mentioned in eq A19, do you mean eq. 5?

Response 2.9

We meant Eq A.11, which also appears in the main section of the paper as Eq 5. Corrected.

p 2356, l 5: insert "=" after alpha

Corrected.

Comment 2.10

Large parts of the discussion could be moved into the results section.

Response 2.10

Section 4.1 has been moved to the Results.

Comment 2.11

p 2358, l 1: "trajectory never reaches the upper bound of the C-U data" Fig 6i suggests that the trajectory exceeds C-U data fit but does not converge towards this line
Response 2.11

The reviewer has misunderstood that this trajectory corresponds to the "low production simulation", which is not expected to reach the upper bound of the C-U data because of the sparseness of the stand. This has been clarified in the text of Section 4.1: "The ageing trajectory of the low-production patch never reaches the upper bound of the C-U data (representing self-thinning due to crowding mortality) because the stand is relatively sparse (Figure 6(ii)) and resource-stress mortality prevents crowding (Figure 6(iii))."

Comment 2.12

p 2360, l 21: The authors argue that big wood models "should be phased out from use in carbon cycle studies" which implicitly suggests to the reader that the presented POP

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model should be used. However, the authors do not provide evidence that the POP can improve our understanding of future vegetation dynamics or reduce uncertainties. The model is fitted to biomass and stem density data measured under ambient conditions and there is no benchmarking against carbon, water or nutrient fluxes.

Response 2.12

Wolf et al. 2011 demonstrated that big wood models are unable to reproduce basic structural information about the vegetation biomass compartments of forest ecosystems. The link between stem biomass and carbon storage is very tight, forests account for a major part of the vegetation carbon pool at the global scale, and most of this carbon is in tree stems. We have shown that POP, as one possible alternative, overcomes the shortcomings of big wood models in terms of representing vegetation structural dynamics across the range of climate zones spanned by the global forest data. We do not assert that our approach is the only possible alternative; we described the advantages of second-generation DVMs that explicitly represent size structure in the introduction on p2346,L9-15. We do not repeat this information here as the point we wish to make - no more and no less - is that big wood models are bad, and structural dynamics are needed. We are not implicitly or explicitly advocating POP as the best specific alternative.

Comment 2.13

Fig. 4: How good is the fit in non-log coordinate system? Fig 4v and vi suggest huge model uncertainty in biomass.

Response 2.13

Figures 4v and 4vi are in the non-log coordinate system. As shown in Table 1, the model explains 57% (R^2) of variation in observed biomass for broadleaved forests. For needleleaved forest the agreement is poorer (24%) and the model shows a positive bias in simulated biomass. As discussed on p2356,L21.27, lower observed biomass

may be expected due to silvicultural management; the model is only simulating natural mortality, not timber extraction during thinning.

Adam Wolf (Referee)

Comment 3.1

General Comments This paper describes a new, and relatively simple, parameterization for treating forest stand dynamics for use within global carbon cycle models. The motivation for this work is in noting (1) the importance of representing woody turnover rate in carbon cycle models and (2) the poor apparent performance in global carbon cycle models in representing this turnover, owing the reliance on a first-order kinetic model of woody biomass loss. The key suspect in the poor performance in this regard is the absence of demographic models of tree growth and mortality - - where demographic mortality is the process that leads to transfer woody biomass from the living to the dead pool. The authors have shown that it is possible to represent the process of allocating biomass increment among the different sized cohorts quite simply, using metabolic scaling theory. This biomass increment (framed as a relative growth rate) is directly used in the calculation of mortality by low resource availability and overcrowding. The authors have shown that with one tuning step, the model is able to reproduce realistic patterns of size-number allometry and component biomass allometry, consistent with global databases of forest component biomass in the temperate and boreal regions. The authors have also shown that these patterns of size-number allometry are contingent on growth potential, which is the source of variation in the size-number allometry below its upper bound. This observation shows that it is essentially impossible to diagnose the number of trees from a stand-level biomass, which highlights the importance of implementing stand dynamics explicitly in carbon cycle models, as opposed to diagnosing the size of the mean individual and computing allocation based on this assumed size. The important distinction is the presence in a grid cell of a number of patches of different age, possessing cohorts of different size. Because these different cohorts face different mortality rates owing to asymmetric competition, the effective

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turnover rate is different from the diagnosed turnover rate for an “average” individual.

Specific comments I don't have strong criticisms of this work and I think it shows an elegant solution to a widespread problem. However, if I may there are a few comments that might improve this work

Comment 3.2

- in eqn 3 it is not clear if the exponent is in the denominator, or the entire fraction.

Response 3.2

Following normal convention, the power is bound to the C_y in the denominator, not the whole fraction.

Comment 3.3

Actually, it's not clear why this value needs an exponent on it at all, esp since p itself is an arbitrary parameter.

Response 3.3

The power term (s) dictates the expected proportionality between the area of plant resource uptake surfaces (leaves and roots) and stem biomass, justified on the basis of allometric scaling theory, and shown to hold empirically across a wide range of plant taxa from around the globe in the cited paper by Enquist and Niklas. We have added this information to the text at this point.

Comment 3.4

GE is generally called the Relative Growth Rate or RGR.

Response 3.4

We now mention this on first introduction. GE is an established term in the DVM modelling community.

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Comment 3.5

- in Eqn 4, it would help to flesh out your logic for the reader, ie that individual growth (say G) is allometrically related to individual mass to the $3/4$ power, ie $G \propto M^{0.85}$ (C/N) $\propto 0.75$. Thus G_y/N_y is the growth fraction of the given cohort, relative to the growth fraction to all cohorts.

Response 3.5

We have fleshed out the logic for the reader in the new text adjacent to Eq 4: "Individuals are thus assumed to capture resources in proportion to the area of their resource uptake surfaces, estimated as the s power of stem biomass following the allometric scaling theory of (Enquist and Niklas, 2001)."

Comment 3.6

- in Eqn 5, the assumption that crown projected area follows a random overlap model is shaky - you would need to have hugely large crown area to get complete ground cover in this model, but in reality if trees avoid each other, c can be close to A . That is, instead of $C = (1 - \exp(-A))$, why not try $C = A$? I recognize that you parameterized this model initially in savannas, where there are legitimate ecological regions for trees to clump. But in temperate and boreal regions, this understanding is not correct, despite the fact that you are certain to find this assumption employed widely in the forest literature. I assume that the tuning exercise you employed compensates for this representation, and in the end doesn't profoundly affect your results. But I still think it should be corrected.

Response 3.6

We prefer to retain the assumption of random overlap of crowns. In the case of very low vegetation cover these formulations are approximately equal. However, crowded, non-overlapping crowns ($C=A$) are rare (occurring eg in even-aged plantations) because stands generally comprise trees of differing heights (with crown projections overlap-

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ping) and crown locations are set by locations of seeds and hence stems, which don't avoid each other. Indeed, in Australian savannas, seedlings emerge in the shade, leading to clumping of stems, which would be poorly approximated by $C=A$.

Comment 3.7

- this is outside the purview of this paper, but the use of fixed allocation fractions is questionable, although it too is a common assumption. There is another Wolf et al paper on this topic . . .

Response 3.7

- We agree, the use of constant allocation fractions is common practice in global vegetation modelling but undoubtedly introduces uncertainty.

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