Author’s response to reviews of “*The biophysics, ecology, and biogeochemistry of functionally diverse, vertically- and horizontally-heterogeneous ecosystems: the Ecosystem Demography Model, version 2.2 – Part 1: Model description*”

We would like to thank Dr. Baker and Dr. Olin for reviewing this manuscript, and providing constructive feedback on our model description. We also thank Dr. Kim for the suggestion to include the differences between the ED-2 versions in this manuscript (see separate response, submitted earlier).

Below we a detailed point-by-point response to comments, suggestions, and questions raised by each referee, and how we plan to address them in the revised manuscript. We also include proposed text modifications following the referees’ suggestions (“quoted blue text”).

Responses to Dr. Ian Baker

IB Comment 01: Larger Impressions. This paper describes the code used in ED2.2. That’s pretty much it. There isn’t really any ‘new science’ here, and in fact any text not devoted to explaining equations is just showing that the model gives reasonable results and conserves energy (there are a few paragraphs showing differences in size/age distributions for two tropical sites with different disturbance regimes). It can be hard to get a paper like this through review, but the authors are lucky to have me for a reviewer. I know the value of papers like this (e.g. the BATS NCAR Technical Manual by Dickinson et al., the LSM Technical Note by Bonan, and the Sellers SiB papers from 1986 and 1996). However, the people reviewing the methods paper want to see results, and vice versa, and they want the paper to be short. But these ‘code papers’ have value for the people who use models, and I appreciate that because I am one of them.

> \> Response: We are glad the Reviewer (Dr. Baker) appreciates the value of this model description manuscript. We believe that the ED-2 model needs a detailed technical description The GMD *model description paper* type expects: “detailed, complete, rigorous, and accessible” descriptions of the model, and to present examples of model output and model verification. We are glad that the Reviewer identified all these elements in this manuscript.

IB Comment 02: I understand there is no way to combine the Parts 1 and 2 of the ED2.2 paper. This weighty tome already comes in at over 100 pages (paper plus supplements for Part 1), yet it is critical to make the code information available for people who will use the model. Some might suggest a technical manual (like is done for CLM), and the authors have in fact done this; I took a quick look at the wiki, and I think it has very useful information for users, but it doesn’t really lay out the rational for the code. Also, the authors want to get journal citations and credit for the work they’ve done, and I don’t blame them one bit.

> \> Response: We are glad that the Reviewer agrees in the value of a manuscript describing the model in detail. The online wiki explains how to run the ED-2.2 model whereas, as the reviewer correctly pointed out, this manuscript describes the rationale

and the parameterizations needed to solve the energy, water, and carbon cycles within the ED-2.2 framework.

IB Comment 03: I'm not going to download the code and study it line by line to see if the explanations make sense. There is no way I could do that and get a review back in under a year. Therefore, it is incumbent upon the authors to very carefully go over the manuscript and check for typos in the equations as they appear in the paper.

> Response: As per the Reviewer's suggestion, we will carefully revise the paper and look for typos.

IB Comment 04: Initially, I thought that perhaps I was the wrong person to review this paper. I have years of experience with SiB and CLM, but none with ED. But then I realized that makes me the perfect person to review; someone familiar with ED will already know much of the material. But if I can understand how ED2.2 works after reading the paper, then the authors have done their job. And I think they've succeeded. I feel fairly comfortable, for the most part, about the ED2.2 framework after reading the paper (multiple times). This paper will be useful for researchers learning or developing ED, and other models, in the future.

> Response: We thank the Reviewer for thinking that the paper was useful to understand the concepts of ED-2.2. We sought to make the manuscript accessible to a broad community that may or may not be familiar with the ED-2.2 model.

IB Comment 05: My formal recommendation is to accept the paper for publication, with minor revisions. I don't need to see it again.

> Response: We thank the Reviewer for the encouraging recommendation.

IB Comment 06: I really like the use of enthalpy; that is an innovative way to demonstrate conservation of energy, and I'm not sure it has been used before.

Response: We also prefer the equations to be based on energy (or enthalpy) as the response variable, as this simplifies the tracking of energy and helps to enforce energy/enthalpy conservation. We too are not aware of energy/enthalpy being used as the energy-related state variable, and thus represents a novel feature ED-2.2 model.

IB Comment 07: I'd like to see more emphasis on what is new in ED2.2 (final paragraph in the Introduction). A bullet list might draw the reader's eye to the new features in this version of the model.

> Response: This suggestion is also in line with Dr. John Kim's iterative comment. We propose to modify the next-to-last paragraph of the introduction to briefly describe and itemize the main changes between ED-2.0 and ED-2.2:

"The original ED model formulation was an off-line ecosystem model describing the coupled carbon and water fluxes of a heterogeneous tropical forest ecosystem (Moorcroft et al., 2001). Subsequently Medvigy et al. (2009) applied a similar approach to develop the Ecosystem Demography model version 2 (ED-2) that describes coupled carbon, water and energy fluxes of the land surface. Since then, the ED-2 model has been continuously developed to improve several aspects of the model (see Supplement S1 for further information): (1) the conservation and thermodynamic representation of energy, water, and carbon cycles of the ecosystems; (2) the representation of several components of the energy, water, and carbon cycles, including the canopy radiative transfer, aerodynamic conductances and eddy fluxes, and leaf physiology (photosynthesis); (3) the structure of the code, including efficient data storage, code parallelization, and version control and code availability. ED-2 has been used in many studies including offline simulations (e.g. Medvigy et al., 2009; Antonarakis et al., 2011; Kim et al. 2012, Zhang et al., 2015; Castanho et al., 2016; Levine et al., 2016), or interactively with a regional atmospheric model (e.g. Knox et al., 2015; Swann et al., 2015)."

In addition, we will include a new Supplement (S1) that will describe the timeline of changes between ED-2.0 and ED-2.2, based on our previous reply to John Kim (https://doi.org/10.5194/gmd-2019-45-AC1).

"S1 ED-2 developments since ED-2.0 and ED-2.2

In this Supplement, we list the main developments in the Ecosystem Demography Model version 2 (ED-2), with focus on mentioned in this manuscript (Fig. S3). The complete list of implementations, improvements, and code fixes are available on the GitHub website (https://github.com/ EDmodel/ED2).

S1.1 Version 2.0 (ED-2.0)

This is the version described in Medvigy (2006); Medvigy et al. (2009), and it is the first version of the ED model that implements energy and water cycles at sub-daily scale. The biophysics core was adapted from the LEAF-2 land surface model (Walko et al., 2000), which is part of the Regional Atmospheric Model System (RAMS). The main differences in the ED-2.0 biophysics core include (1) solution of the energy and water cycle for each cohort and patch; (2) use of 4th order Runge-Kutta solver to improve numerical stability. In addition, this version allowed leaf phenology to be prescribed from external data (Supplement S3.1.1). The photosynthesis solver was largely the same as in ED-1.0 (Moorcroft et al., 2001).

S1.2 Version 2.0.12 (ED-2.0.12)

Most developments between ED-2.0 and ED-2.0.12 relate to code organization and structure. ED-2.0 was partly written in C (legacy from ED-1) and partly written in Fortran (legacy from LEAF-2). To simplify the code and ensure data were correctly transferred between subroutines, we rewrote most of the code in Fortran. The only exceptions were a few file handling functions that remained in C because we could not find equivalent functions in Fortran.

In addition, this version uses Hierarchical Data Format 5 (HDF5) format and libraries (The HDF Group, 2016) to generate model outputs. HDF5 allows a more efficient framework to output variables in the dynamic patch and cohort structures. It also introduced an XML model parameter input file, rather than relying solely on hard-coded defaults, which makes it easier to perform model calibration, sensitivity analyses, and ensemble error propagation. Importantly, this was the last version of ED-2 that used temperature as prognostic variable for leaves and canopy air space.

S1.3 Version 2.1 (ED-2.1)

Most ED-2.1 developments aimed at improving the energy cycle representation in ED-2.1. Importantly, leaf enthalpy and canopy air space enthalpy replaced temperature as the prognostic energy-related variables (Eq. 4; Sec. 3.2.3-3.2.4). The main advantages of energy-based prognostic equations include: (1) simplification the numeric integration, as total energy changes must be equivalent to net energy flux; (2) improved conservation of energy when water fluxes are large and cause rapid changes in heat capacity of the thermodynamic systems; (3) elimination of singularity at the water's fusion point (0°C, when enthalpy changes due to freezing or melting, but the temperature remains the same.

To ensure the model was thermodynamically consistent, we also: (1) implemented a mechanistic representation of heat capacity for vegetation (leaves and branches, Supplement S6.2) that is scaled with leaf and branch biomass (e.g. Dufour and van Mieghem, 1975); and (2) replaced the original LEAF-2-based surface layer model (that was based on Louis, 1979) with the parameterization by Beljaars and Holtslag (1991), as the latter parameterization improved numerical stability of eddy covariance fluxes under thermally stable conditions; (3) included an option to prescribe silt, clay, and sand fractions to define site-specific soil texture characteristics (Supplement S8) instead of the original ED-2.0 implementation that required soils to be assigned to one of the 12 fixed classes originally defined in LEAF-2 (Walko et al., 2000); (iv) implemented the capability of saving the entire ecosystem and thermodynamic state of the model into HDF5 files, which can be used to stop and start simulations and yield the same results of uninterrupted simulations, a desirable feature for simulations with long runtimes.

S1.4 Version 2.2 (ED-2.2)

The ED-2.2 version implemented several improvements and fixed inconsistencies in the representation of the energy, water, and carbon dioxide cycles. First, we redefined enthalpy (S5), to ensure that it would be a true thermodynamic state variable (i.e. path independent, see Dufour and van Mieghem, 1975), by making latent heat of vaporization a linear function of temperature (Eq. 72-73). Moreover, we identified missing components of the energy cycle that precluded the conservation: (1) the transfer of internal energy from soils to leaves before transpiration (Eq. 97); (2) the enthalpy exchange associated with vaporization and condensation also accounts the mass transfer of water between the thermodynamic systems (e.g. Eq. 75; 98. Furthermore, to ensure results from ED-2.2 consistently conserve mass and energy, we implemented detailed conservation verification during the model execution, which now reports any violation of energy, water, and carbon conservation, generates detailed output of the violation, and interrupts the simulation. Finally, to improve computational efficiency of the energy, water, and carbon cycle solvers at sub-daily time steps, we implemented a shared-memory parallelization of the most computationally-intensive subroutines. The parallelization was written to allow users to select any number of cores (depending on core availability), and to account for patch ages in order to balance the load among cores.

In addition, we rewrote the photosynthesis to allow temperature-dependent functions to be expressed as functions of Q_{10}. We retained the original Arrhenius-based functions as legacy options, but the new option increases the options for assimilating data into the model. The current Q_{10}-based parameters fix the low-temperature optimum in tropical plants previously noted by Rogers et al. (2017). Importantly, we rewrote the photosynthesis solver to ensure that it would always converge to a unique solution for net assimilation rate, stomatal conductance, and intercellular carbon dioxide concentration given the environmental conditions (Supplement S15).

The ED-2.2 version also includes improvements in the representation of conductances between different thermodynamic systems. First, the leaf boundary-layer conductance now accounts for differences in leaf and branch characteristics of each cohort, and to account for both free and forced convection under both laminar and turbulent flow (Supplement S13.2). Second, we implemented ground-to-canopy conductance formulations (Sellers et al., 1986; Massman, 1997; Massman and Weil, 1999) that account for the cumulative drag profile of vegetated areas obtained from the cohort structure, as well as the stability of the surface layer (Supplement S13.3).

Finally, in ED-2.2 we replaced the version control to GitHub, which makes the new code developments readily available to the scientific community and encourages users to post issues, code fixes and model improvements and developments to the main code repository in open and collaborative forums."

Specific Comments

(many of these are suggestions for grammar, and in some cases need not be implemented exactly as I suggest. They are just places where I noted typos and grammar issues. I also apologize for location indicators; in my copy there were new line numbers on each page, and after about page 26 I found a line numbered 5 at the bottom of the page sometimes.)

IB Comment 08: Abstract, line 11: "out and is presented"

> Response: We will modify the text as suggested.

IB Comment 09: Page 2, lines 5-15: This description of generational advances in model development does not align exactly with Sellers et al. (1997). I think it would be helpful to acknowledge the Sellers paper and put the descriptions here in that context.

> Response: As suggested, we will cite Sellers et al. (1997) and revise our narrative to be consistent with Sellers et al. (1997):

"As described by Sellers et al. (1997), the first generation of land surface models (LSMs) were limited to provide boundary conditions to atmospheric models, and only solved a simplified energy and water budget, and accounted for the effects of surface on frictional effects on near-surface winds (e.g. Manabe et al., 1965; Somerville et al., 1974). These models, however, did not account for the active role of vegetation. The second generation of LSMs considered the active role of vegetation and represented the spectral properties of the canopy, the changes in roughness of vegetated surfaces, and the biophysical controls on evaporation and transpiration (Sellers et al., 1997); examples of these models include NCAR/BATS (Dickinson et al., 1986) and SiB (Sellers et al., 1986). The increasing recognition of the role of vegetation in mediating the exchanges of carbon, water and energy between the land and the atmosphere led to the third generation of LSMs, which incorporated explicit representations of plant photosynthesis, and resulting dynamics of terrestrial carbon uptake, turnover and release within terrestrial ecosystems (Sellers et al., 1997); examples of such models included LSM (Bonan, 1995) and SiB2 (Sellers et al., 1996). While the fluxes of carbon, water and energy predicted by these models would change in response to changes in their climate forcing, the biophysical and biogeochemical properties of the ecosystem within each climatological grid cell was prescribed, and thus did not change over time.”

IB Comment 10: Page 3, line 7: SiB does not have an explicitly layered canopy or sunlit/shaded leaves separately treated.

> Response: As suggested, we will remove the reference to SiB.

IB Comment 11: Page 3, lines 12-13: I’m confused here. I thought models were transitioning from broadly-defined ‘biomes’ to a PFT-based mosaic structure. This sentence says the opposite.

> Response: We intended to say that the lack of functional diversity and the mechanistic representation of ecological processes such as competition cause models to predict ecosystems comprised of single homogeneous vegetation types. We will rewrite the sentence to clarify this point:

“The lack of significant variability in resource conditions limits the range of environmental niches within the climatological grid cells of terrestrial biosphere, and makes the coexistence between PFTs difficult. Consequently, models typically predict ecosystems comprised of single homogeneous vegetation types (Moorcroft, 2003, 2006).”

IB Comment 12: Page 6: The full set of PFTs is not listed. In table S5 we’re shown parameter values for the tropical grasses and trees used here, but if this paper is going to be the ‘go to’ manual for ED2.2, all PFTs should be listed in a table somewhere. Don’t worry about the extra length-this paper is already incredibly long.

> Response: As suggested by the reviewer, we will include the parameter values for all the default PFTs. Specifically, we will add a separate table for temperate PFTs, revise the Leaf phenology supplement (formerly S2, now S3) to include descriptions of the cold-deciduous leaf phenology, and update Supplement S16 (Allometric equations) to include non-tropical PFTs.

“S3.1 Leaf phenology

The phenological strategy of the plant functional types, can be evergreen, drought-deciduous, or cold-deciduous. The plant’s phenology strategy is defined by two functions: (i) the leaf elongation factor ($\hat{e}_{lk}$), defined as the ratio between the environmentally-constrained leaf biomass and the potential (maximum) leaf biomass, and Iii) the rate of leaf shedding ($\omega_{lk}(t)$). These functions can either be prognosed or prescribed from observations.

S3.1.1 Evergreen Plants

For evergreen PFTs, the elongation factor is always 1, the rate of leaf shedding ($\omega_{lk}(t)$) is zero, and their rate of leaf turnover is governed by the PFT-dependent leaf turnover parameter (τ_l, see Eq. S5, and Tables S5 and S6). The leaf phenology of tropical trees can also be represented by an empirical model that is driven by the seasonality of light availability (see Supplement S2 and Kim et al., 2012).

S3.1.2 Drought-deciduous tropical phenology

For drought-deciduous tropical plant functional types, the leaf elongation factor is governed by:

$$e_{l_k} = \begin{cases} 1 & \text{, if } s_{l_k} \geq 1 \\ s_{l_k} & \text{, if } 0.05 \leq s_{l_k} < 1 \\ 0 & \text{, if } s_{l_k} < 0.05 \end{cases}, \tag{S4}$$

$$s_{l_k} = \frac{1}{|z_{r_k}|\, \Delta t_{\mathrm{El}}} \int_{t'-\Delta_{\mathrm{Phen}}}^{t'} \left(\sum_{j=j(z_{r_k})}^{N_G} \left\{ \frac{\max\left[0, \Psi_{g_j}(t') + \frac{1}{2}\left(z_{g_j} + z_{g_{j+1}}\right) - \Psi_{\mathrm{Wp}}\right]}{\Psi_{\mathrm{Ld}} - \Psi_{\mathrm{Wp}}} \right\} \right) \mathrm{d}t, \tag{S5}$$

where s_{l_k} is a running average of soil moisture accessed by cohort k (normalized by the difference between Ψ_{Ld}, and Ψ_{Wp},the difference between soil matric potential below which plants start shedding leaves and the soil matric potential at wilting point), z_{r_k} is the rooting depth of cohort k (Supplement S19), Δt_{El} is the time scale for changes in phenology (assumed to be 10 days), $j(z_{r_k})$ is the soil layer containing the deepest roots of cohort k, and z_{gj} is the depth of soil layer j, ($z_{gN_{g+1}} \equiv 0$). Leaf shedding occurs whenever soil is drier than the threshold defined by Ψ_{Ld} and drought conditions are increasing. Specifically:

$$\omega_{l_k} = \frac{1}{\Delta t_{\mathrm{Phen}}} \max\left[0, \frac{C_{l_k}}{C_{l_k}^{\bullet}} - f_{\mathrm{El}}\right]. \tag{S6}$$

S3.1.3 Cold-deciduous phenology

The prognostic cold-deciduous leaf phenology approach is a thermal sum and chilling sum-based model identical to that of Albani et al. (2006), which, in turn, is based on Botta et al. (2000). At each site, growing degree-days (GDD) are accumulated during the extended growing season (t_{GS}, January–August for the Northern Hemisphere, and July–February for the Southern Hemisphere), and the chilling days (CHD) in the extended senescing season (t_{SS}, November-June for the Northern Hemisphere, and May-December for the Southern Hemisphere):

$$\mathrm{GDD}(t) = \sum_{t'=t_{\mathrm{GS}}(0)}^{t} \max\left(0, \overline{T}_c(t') - T_{\mathrm{Phen}}\right), \tag{S5}$$

$$\mathrm{CHD}(t) = \begin{cases} 0 & \text{, if } \overline{T}_c(t) \geq T_{\mathrm{Phen}}, \text{ or } t \notin t_{\mathrm{SS}} \\ \mathrm{CHD}(t - \Delta t_{\mathrm{Phen}}) + 1 & \text{, otherwise} \end{cases}, \tag{S6}$$

where $\overline{T}_C$ is the daily average canopy air space temperature, Δt_{Phen} = 1 day is the phenology time step (Table 2), $t_{GS}(0)$ is the beginning of the growing season, and T_{Phen} = 278.15 K (0°C) is the leaf phenology temperature threshold (Albani et al., 2006). The valued elongation factor $\hat{e}_{lk}$ is then determined by the following series of conditions:

$$\hat{e}_{l_k}(t) = \begin{cases} 0 & \text{, if } \overline{T}_s(t) < 275.15 \text{ K} \\ 0 & \text{, if } \overline{T}_s(t) < 284.30 \text{ K and } t_\odot < 655 \text{ min} \\ 1 & \text{, if GDD} \geq -68.0 + 638.0 \exp\left[-0.01\,\mathrm{CHD}(t)\right] \\ \hat{e}_{l_k}(t - \Delta t_{\mathrm{Phen}}) & \text{, otherwise} \end{cases}, \tag{S7}$$

where $t_\odot$ is the daytime duration.”

If desired, cold-deciduous phenology can be prescribed rather than prognosed, as described in Medvigy et al. (2009) and Viskari et al. (2015). The timing of leaf onset and leaf senescence are empirically determined from either field observations or from remote sensing (e.g. Zhang et al., 2003) by fitting the following curves, which are then used to determine $\hat{e}_{lk}$ in the model:

$$\hat{e}_{l_k} = \begin{cases} \dfrac{1}{1 + (y_0\, t)^{y_1}} & \text{, if } t \in t_{\mathrm{GS}} \\ \dfrac{1}{1 + (y_2\, t)^{y_3}} & \text{, if } t \in t_{\mathrm{SS}} \end{cases}, \tag{S4}$$

where y_0, y_1, y_2, and y_3 are empirical parameters, determined from data prior to running the ED-2.2 model and provided to the model as inputs; t is the time, provided as day of year (i.e. 1 for January 1st, 365 for December 31, and 366 for December 31 in leap years); t_{GS} is the extended growing season (e.g. January-July for the Northern Hemisphere, and July-January for the Southern Hemisphere); and t_{SS} is the senescing season (e.g. August-December for the Northern Hemisphere, February-June for the Southern Hemisphere).

IB Comment 13: Page 7, line 15: I'd like to see the index k introduced here. I had to wade through a bit of text in the supplements before I realized that k addressed cohorts (this might also have to do with the fact that I had a hard time seeing k in the lettering in Figure 1. It might be helpful to have a small table showing the indexes used to address sites, patches, and cohorts. By the time I had read the paper several times I think I had it figured out, but a more explicit explanation might be helpful.

> Response: As suggested, we will include a new Table 1 with indices associated with patches, cohorts, disturbance types and PFTs. The new Table 1 caption will also refer to

existing Table S1 that lists all the subscripts used in the manuscript. We will also revise the text to describe the subscripts, and refer to the new Tables 1 and S1.

"Equation (2) and Eq. (3) cannot be solved analytically (except for trivial cases) and therefore were solved numerically using the method of characteristics. The age distribution is discretized into a series of patches (subscript u, $u \in 1,2,\ldots, N_P$; Table 1) of similar age and same disturbance type, and the population of plants living in each patch u is discretized into a series of size-cohorts (subscript k, $k \in 1, 2,\ldots, N_T$; Table 1) containing plants of similar size and of the same PFT (Fig. 1)."

IB Comment 14: Page 9, lines 31-32: How do you specify CO2 mole fraction on the timescale of the model? I'm not aware of CarbonTracker or GlobalView products that give that kind of resolution, and products with temporal averaging will cause issues with your carbon exchange during diurnal cycles (I think Jih-Wang Wang et al.,2007, talks about this). I don't see any mention of CO2 drivers in the wiki either. We've always calculated atmosphere-CAS CO2 exchange using a constant atmospheric value, and the flux can be easily scaled during a mesoscale- GCM- or transport-model application when a time- varying atmospheric CO2 value is available in the lowest atmospheric level. This may be a recommendation more appropriate for the github wiki, but I think the authors need to explain to the user how to deal with it.

> Response: Above-canopy CO_2 is read-in as part of the meteorological drivers read routines. It therefore must be provided by the user, along with other meteorological variables. Further details on the initialization procedure can be found on ED2 site GitHub (https://github.com/EDmodel/ED2/wiki/Drivers). Ideally, atmospheric CO_2 should be provided at comparable temporal and spatial resolution to other meteorological variables, to avoid the issues raised by the reviewer. In the case of coupled ED2-BRAMS simulations (e.g. Knox et al. 2015; Swann et al. 2015), this happens automatically since CO_2 is explicitly tracked as an atmospheric state variable whose value at the lower boundary value is passed into ED2 thereby eliminating the need to scale the fluxes.

In offline simulations, CO_2 data are not generally available at comparable spatial scale or similar temporal resolution as other meteorological drivers. For these cases, ED-2.2 allows the user to provide time-varying CO_2 that is constant in space (this option is available to all meteorological drivers, as ED-2.2 can be driven by micrometeorological tower data). Alternatively, the user can set a constant CO_2 mixing ratio when no CO_2 data are available.

In line with the Reviewer's suggestion, we will add a sentence clarifying that ED-2.2 can read CO_2 data when available. We will also explain that ED-2.2 can read data from a single site (eddy flux tower) and gridded drivers (re-analysis):

"Meteorological drivers can be either at a single location (e.g. eddy covariance towers), or gridded meteorological drivers such as reanalysis (e.g. Dee et al., 2011; Gelaro et al., 2017) or bias-corrected products based on reanalysis (e.g. Sheffield et al., 2006; Weedon et al., 2014). Whenever available, CO_2 must be provided at comparable temporal and spatial resolution as other meteorological drivers; otherwise, it is possible to provide spatially homogeneous, time-variant CO_2, or constant CO_2, although this may increase uncertainties in the model predictions (e.g. Wang et al., 2007). Alternatively, the meteorological forcing (including CO_2) may be provided directly by BRAMS (Knox et al., 2015; Swann et al., 2015)."

IB Comment 15: Page 10, lines 12-13: "aboveground part each cohort" Huh? I think there is some re- wording needed here.

> Response: As suggested, we will replace this text with "leaves and branchwood portion of each cohort", to clarify the meaning of the sentence.

IB Comment 16: Page 11, line 11: "components on the right-hand side"

> Response: As suggested, we will correct this typographical error.

IB Comment 17: Page 15, line 1: I'm not sure I understand exactly what j-prime means. I think I know, as in there is no sub-surface runoff from any soil level above the bottom one, and no ground evaporation from layers below the surface, but this is not made explicit to the reader.

> Response: We used j' to refer to two different soil layers (g_j and $g_{j'}$). For example, in the third term on the right-hand side of Eq. (8) ($\delta_{gjgj'}$) is 0 for all layers except layer g_1. We did not want to add yet another index because it could be even more confusing and therefore plan to keep the j' notation; however, in light of the Reviewer's suggestion, we will modify the text to clarify the meaning of j':

"where $\delta g_j g_{j'}$ is the Kronecker delta for comparing two soil layers g_j and $g_{j'}$ (1 if $g_j = g_{j'}$; 0 otherwise),…"

We will make a similar modification to the text after Eq. (11):

"where $\delta s_j s_{j'}$ is the Kronecker delta for comparing two TSW layers s_j and $s_{j'}$ (1 if $s_j = s_{j'}$, 0 otherwise),…"

IB Comment 18: Page 10, line 3: I'm not sure that holding energy, enthalpy, and water fluxes to zero is consistent with the explanation given on page 9, lines 24-26. If free drainage is allowed out of the bottom of the soil column, won't W-dot g0,g1 be nonzero? This needs to be made more clear.

> Response: We believe the Reviewer was referring to Page 15, line 3. In the original submission, we had defined sub-surface runoff (drainage) as a separate term ($\dot{H}_{g1,o}$; $\dot{W}_{g1,o}$), and thus we defined $\dot{H}_{g0,g1}$ and $\dot{W}_{g0,g1}$ to be zero in section 3.2.1. However, we agree with the Reviewer that this is confusing, and also is inconsistent with the text in section 4.1. To remove the ambiguity, we will refer to sub-surface runoff fluxes as $\dot{H}_{g0,g1}$ and $\dot{W}_{g0,g1}$ and use subscript o exclusively for surface runoff. Specifically, we propose the following three changes:

(i) Re-define the flux notation at the beginning of section 3.2:

"For any variable X with that has flux between a system m and a system n, we assume that $\dot{X}_{m,n} > 0$ when the net flux goes from system m to system n, and that $\dot{X}_{m,n} = -\dot{X}_{n,m}$. Arrows in Fig. 2 indicate the directions allowed in ED-2.2."

(ii) Replace the notation g_1,o with g_1,g_0 in Table 4 (which will become Table 5), and in section 4.1 (Hydrology sub-model and ground energy exchange).

(iii) Remove the sub-surface runoff term from equations (6) and (7), and describe that the terms $\dot{H}_{g0,g1}$and $\dot{W}_{g0,g1}$ are the (negative) sub-surface runoff fluxes:

In the equations above, we assume $\dot{Q}_{g0,g1}$ to be zero (i.e. bottom boundary in thermal equilibrium); $\dot{H}_{g1,g0}$ = - $\dot{H}_{g0,g1}$, and $\dot{W}_{g1,g0}$ = - $\dot{W}_{g0,g1}$ to be sub-surface runoff fluxes (see section 4.1). In addition, $\dot{Q}_{gNG,gNG+1}$, $\dot{H}_{gNG,gNG+1}$, $\dot{W}_{gNG,gNG+1}$ are equivalent to ($\dot{Q}_{gNG,s1}$, $\dot{H}_{gNG,s1}$ $\dot{W}_{gNG,S1}$), which are the fluxes between the top-most soil layer and the bottom-most temporary surface water layer (please see also section 3.2.2)."

IB Comment 19: Page 16, lines 1-2: If layer Ns+1 does not exist, why mention it at all? Does it exist in the code as a placeholder? If so, that should be stated.

> Response: We made the comment because the second term on the right-hand sides of Equations (9–11) for layer $s_j = s_{\mathrm{NS}}$ implies the existence of such layers. We will rewrite the sentence to clarify this:

"When solving Eq. (9)-(11) for layer s_{NS}, we assume the terms $\dot{Q}_{sj,sj+1}$, $\dot{H}_{sj,sj+1}$ and $\dot{W}_{gNG,gNG+1}$ to be all zero, as layer N_{S+1} does not exist."

IB Comment 20: Page 17, line 3 or so "that changes we obtain" could be "then we obtain"

> Response: As suggested, we will replace "that changes we obtain" with "then we obtain."

IB Comment 21: Page 21, line 3: "because the enthalpy" could be "due to the enthalpy"

> Response: As suggested, we will replace "because the enthalpy" with "due to the enthalpy."

IB Comment 22: Page 28, line 22: "surface x is at temperature T with a liquid"

> Response: As suggested, we will replace "and a liquid" with "with a liquid."

IB Comment 23: Page 28, line 5 (bottom of page): "and Leuning" could be "and the Leuning"

> Response: As suggested, we will insert the missing word "the" before "Leuning."

IB Comment 24: Page 31, lines 9-15: This temperature restriction is similar to what we've used in SiB for years. We also have a frost 'delay' term, where plants do not rebound immediately to photosynthesize during periods where temperatures may go below freezing (think spring in higher latitudes). I'd be happy to share it with you. Also, we have a humidity restriction term.

> Response: We thank the Reviewer for kindly offering to share the parameterization. We will consider implementing this parameterization in future versions of ED-2.

IB Comment 25: Page 33, water extraction by roots: OK, so plants can extract water from all layers "to which they have access" (which, in a 3-layer soil I imagine is all of them), but roots have a uniform mass distribution. I think I might know why this is done. In the real world, I would expect a shorter/younger cohort to be less deeply rooted than an older/taller cohort, and grasses to be shallower rooted still. I also imagine that when this was done in ED, the *short/young/grass cohorts might have died due to lack of water because the old/tall trees took it all. This is fine, but you can't have it both ways. In Section 6.2, "Heterogeneity of ecosystems" the authors claim ED 2.2 "...improves the characterization of heterogeneity...by the number of individuals, their height and rooting depth, and their traits and trade-offs that determine their ability to extract soil moisture..." which contradicts what is described in Section 4.6. These stories need to be made consistent.*

> Response: We first must clarify that ED-2.2 does not necessarily have three soil layers: the user can specify how many layers to use and the thickness of each layer. This confusion may have arisen from Figure 2, which depicted three layers. In light of the Reviewer's comment, we will modify the diagram to show a generic number of layers. Not all cohorts have access to all layers: access is determined by the root allometry. We agree that Sections 4.6 and 6.2 should be consistent and we believe that the ambiguity will be resolved by redrawing Figure 2, and rephrasing the text in the beginning of section 3.2.1 to the following:

"In ED-2.2, the soil characteristics (number of soil layers, thickness of each soil layer and total soil depth, soil texture, soil color) are defined by the user, and assumed constant throughout the simulation."

A few additional factors must be accounted for in the below-ground competition. (1) Even though the biomass of each cohort is uniformly distributed, the amount of water extracted by each cohort from each layer is proportional to the available water in each layer (i.e., if the shallower soils are drier, deep-rooted plants will remove most of the water from the wettest soil layers. This is described in Eq.s 95–96. (2) Because ED-2.2 represents cohorts with different sizes in the same patch, when large and small trees coexist, the water demand of small cohorts is typically reduced, because these cohorts are light-limited. (3) Small cohorts have generally lower carbon demand because the biomass of their living tissues, and consequently their maintenance costs, is smaller than the live biomass of large trees. Therefore, it is not necessarily true that the higher soil moisture stress of small cohorts will lead to their extinction. In ED-2.2, the success or failure of plants to survive droughts is governed by tradeoff between soil moisture stress, size-specific carbon and water demands, and maintenance costs under drought scenarios. These interactions have been previously explored in Longo et al. (2018). The results indicated increases in the abundance of small-sized plants under more frequent and extended droughts, as high-mortality of large, water-demanding trees improved light conditions in the understory.

IB Comment 26: Page 37: "nonexistent"

> Response: As suggested, we will replace "inexistent" with "non-existent."

IB Comment 27: Page 37: "stand-level" is not defined in the paper. Does this mean polygon, site, or something else? Also, I'm not sure the significance of the paragraph comparing stand variability to patch variability. What does it mean?

> Response: The notation was indeed confusing, and we will remove "stand" from the paragraph, and it replace with "polygon" to be consistent with the rest of the narrative. The goal of the analysis in this section was to compare and quantify the impact of structural variability of complex ecosystems such as forests with the climate variability.

Climate variability is represented by most dynamic global vegetation models, but structural variability cannot be properly represented by big-leaf models. The goal here was to show that this variability is relatively important. We will re-write the paragraph to clarify these points:

"The impacts of simulating structurally and functionally diverse ecosystems are also observed in the fluxes of energy, water, carbon, and momentum. For example, in Fig. 9 we show the monthly average fluxes from the last 40 years of simulation at GYF, along with the inter-annual variability of the fluxes aggregated to the polygon-level (hereafter *polygon variability*, error bars) and the inter-annual variability of the fluxes of the different patches within polygons (hereafter *patch variability*, colors in the background). The polygon-level variability can be thought as the variability attributable exclusively to climate variability, whereas the patch variability also incorporates the impact of the structural and compositional heterogeneity on the degree of variability. Most highly aggregated ("big-leaf") models characterize the polygon-level variability, but not the patch variability. However, in all cases, the patch variability far exceeded the polygon variability, indicating that structural and compositional variability is as important as the inter-annual variability in complex ecosystems. In the case of sensible heat, the polygon variability was between 39 and 64% of the patch variability (Fig. 9a). The polygon-to-patch variability ratio was similar for both friction velocity (19 − 39%) and water fluxes (17 − 44%) (Fig. 9b,c). In the case of gross primary productivity, the relevance of patch variability was even higher, with polygon-to-patch variability ratio ranging from 3.7% during the dry season to 17% during the wet season (Fig. 9d). Importantly, the broader range of fluxes across patches in the site can be entirely attributed to structural and functional diversity, because all patches were driven by the same meteorological forcing."

IB Comment 28: Page 38: "density in the canopy air space"

> Response: As suggested, we will replace "density at the canopy air space" with "density in the canopy air space"

IB Comment 29: Page 39: SiB has had a prognostic CAS since 2003 (Baker et al), based on Vidale and Stockli (2005). Just sayin'.

> Response: Thank you for the additional references, we will re-phrase the sentence to include SiB as another model that represents canopy air space:

"Unlike most existing terrestrial biosphere models (but see SiB2, e.g. Baker et al., 2003; Vidale and Stöckli, 2005), in ED-2.2 we explicitly include the dynamic storage of energy, water, and carbon dioxide in the canopy air space."

IB Comment 30: Page 40, line 12: "access to and competition for"

> Response: We will modify the text as suggested.

IB Comment 31: Page 40, lines 29-30: "is fundamental to explaining"

> Response: We will modify the text as suggested.

IB Comment 32: Page 41, lines ?: “degradation is pervasive”

> Response: As suggested, we will correct the sentence to “degradation is pervasive.”

IB Comment 33: Page 42, line 29: “has excellent conservation”

> Response: We will replace this sentence with “has a high degree of conservation.”

On to the Supplements! (I do not have specific line numbers in my supplements file; I’ll just have to do my best with explaining where the comments address)

IB Comment 34: Table S2: might help to add bulk specific enthalpy, and Temporary Surface Water.

> Response: As suggested, we will add bulk specific enthalpy and temporary surface water to Table S2.

IB Comment 35: S2: What is a “leaf elongation factor”, and how is it determined? There is a long equation to describe slk, but we aren’t told what it means.

> Response: We will modify the first paragraph of the Leaf phenology appendix (formerly S2, now S3) to clarify the meaning of leaf elongation factor:
>
> “The phenological strategy of the plant functional types, can be evergreen, drought-deciduous, or cold-deciduous. The plant’s phenology strategy is defined by two functions: (i) the leaf elongation factor ($\hat{e}_{lk}$), defined as the ratio between the environmentally-constrained leaf biomass and the potential (maximum) leaf biomass, and the rate of leaf shedding ($\omega_{lk}(t)$) which either be prognosed, or prescribed from observations.”
>
> We will also include a description of s_{lk} beneath Eq.s S4 and S5 (please see our response to comment 12 above).

IB Comment 36: S7: is the ‘b’ term the Clapp and Hornberger b? If not C+H, where does the value come from?

> Response: Yes, the Reviewer is correct. We will include the citation to Clapp and Hornberger (1978) when b is first defined:
>
> “… b is the slope of the logarithmic water retention curve (Clapp and Hornberger, 1978)…”
>
> We will also include the information that the parameterization of soil matric potential is derived from Clapp and Hornberger (1978):
>
> “The equation that describes soil matric potential as a function of soil moisture is taken from Clapp and Hornberger (1978); soil hydraulic conductivity…”

IB Comment 37: S7, field capacity: I’ve seen several definitions for determining field capacity from things like moisture potential. Is there a reference for what is being used here?

> Response: We used the definition by Romano and Santini (2002) and references therein. We will include the reference to Romano and Santini (2002) and the rationale in the revised manuscript.

"Field capacity (ϑ_{Fc}) is often defined from soil matric potential (e.g. Hodnett and Tomasella, 2002; Saxton and Rawls, 2006). However, this definition is based on field measurements and the definition of ϑ_{Fc} from soil matric potential can substantially across studies, with values ranging from −0.1 kPa to −0.5 kPa (Romano and Santini, 2002). In ED-2.2, we follow Romano and Santini (2002) and define field capacity in terms of hydraulic conductivity, and assume that the drainage flux of water becomes negligible at hydraulic conductivity of 0.1 kgW $\mathrm{m^{-2}}$ $\mathrm{day^{-1}}$."

IB Comment 38: S9: "contains contributions from reflectance and transmittance"

> Response: As suggested, we replaced "contribution" with "contributions." We did not replace "contain" with "contains" as we are referring to "both the bulk diffuse backscattering (β_{mk}) and forward scattering ($1-\beta_{mk}$)."

IB Comment 39: S12.1: Are you really able to avoid the 'material surface at the top of the CAS' problem under stable conditions? This has been a problem for years, and may be worth a publication of its own. If you've already written it, advertise it here.

> Response: We did not develop any new parameterization that avoids the flux underestimation under stable conditions. Instead, we implemented the Beljaars and Holtslag's (1991) empirical formulation of the flux profile functions. Beljaars and Holtslag (1991) found that their empirical model resulted in more mixing under stable conditions, so this is their result, not ours. We rewrote the paragraph to clarify these points:

"The ED-2.2 model uses the empirical parameterization of the originally developed by Beljaars and Holtslag (1991). For the unstable cases, Beljaars and Holtslag (1991) used the Businger-Dyer flux profile equations (Businger et al., 1971). For the stable cases, Beljaars and Holtslag (1991) implemented an empirical formulation that improved the vertical mixing between the canopy air space and the air above under stable conditions:"

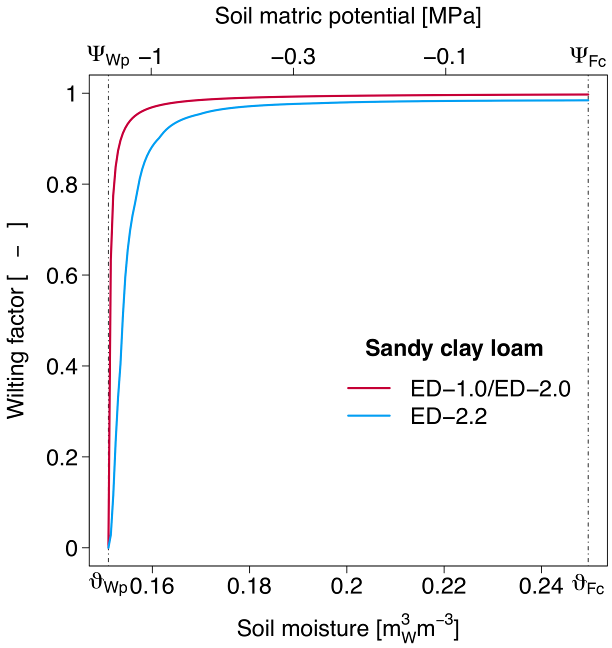
IB Comment 40: S15: soil moisture limitation on photosynthesis. There's been a lot of work done on this with regard to the fact that individual plants maintain photosynthesis as soil dries down from wilt point, until suddenly closing stomates (Colello et al., 1998; Kim et al., 2010). This behavior, while well-known on the plant scale, is problematic when imposed on the ecosystem scale, as it frequently results in binary, or 'on-off' behavior. Many methods have been utilized to deal with it (e.g. Laio et al., 2001; Porporato et al., 2001, 2002: Rodriguez-Iturbe 2000; Baker et al., 2008, 2013; Wood et al., 1992, to name just a few). I'd like to see more explanation of what you're doing. A graph showing how stress is imposed, from field capacity to wilt point, would be helpful. Is stress imposed in a linear fashion, or does it behave like the btran function in CLM? Is this function based on previous research (which should be cited), or something incorporated specially for ED2.2? If so, why?

> Response: The formulation of the soil moisture stress is mostly based on previous versions of ED (Moorcroft et al., 2001). The only difference in ED-2.2 is that we re-defined the soil water term to be a function of soil matric potential, similar to CLM

(Oleson et al., 2013), because the response of the wilting factor to drying the new formulation is slightly more gradual than the original formulation. We will modify the text to include these citations:

"To account for soil water stress, we define a phenomenological scaling function f_{Wlk} (wilting factor). The functional form of f_{Wlk} follows the previous versions of ED (Moorcroft et al., 2001; Medvigy et al., 2009). However, in ED-2.2 we define water availability ($W_g{}^{*}$) in terms of soil matric potential, similarly to CLM (Oleson et al., 2013), which produces a more gradual transition from no-stress conditions to completely closed stomata as soil moisture approaches the wilting point (Fig. S9)."

We will also include a new Supplemental Figure that shows the response of the soil moisture stress using both the original ED formulation and ED-2.2, which highlights the less steep transition from no stress to extreme stress when soils are dry. The proposed figure and caption are shown below:



"Figure S9: Example of the wilting factor (f_{Wlk}, Eq. S192) response to soil moisture change for the original implementation in ED (ED-1.0 and ED-2.0, Moorcroft et al., 2001; Medvigy et al., 2009) and the ED-2.2 model approach. Results here are shown for the idealized case with constant soil moisture profile in a 3-m deep, sandy clay loam soil, for a mid-successional tropical cohort with default parameters (Table S5 with diameter at breast height of 30 cm and leaf area index of 1 $\mathrm{m^2_{Leaf}\,m^{-2}}$, non-limited leaf-level transpiration rate $\dot{E}_k = 9.0\ \mathrm{kg_W\,m_{Leaf}^{-2}\,day^{-1}}$. Values are shown for soil moisture columns ranging from wilting point (ϑ_{Wp};Ψ_{Wp}) to field capacity (ϑ_{Fc};Ψ_{Fc})."

We should also clarify that the soil moisture stress is calculated for each cohort and each patch, and as explained before, plants with different sizes and PFT have different rooting depths, and may have different water use strategies, expressed through different stomatal conductance parameters. The predicted response to soil water depletion in ED-2.2 does not show the "on-off" response. For example, in Figure S14 of Longo et al. (2018), we found that the soil moisture stress factor ($1-f_{Wlk}$) would increase more quickly for small cohorts (DBH < 10cm) than larger cohorts (DBH > 35cm) under recurrent fire regimes. However, that did not cause stomatal conductance to decrease for the small cohorts because once large trees started to die, the small trees experienced more light that compensated the additional soil moisture stress.

The diversity of cohort responses to droughts are further enhanced when using TOPMODEL (one of our current developments, as explained in Section 6.3). This approach accounts for both edaphic heterogeneity and lateral moisture transfer, and can represent that mesic lowlands do not dry as quickly as ridges, for example. Finally, please note that a version of ED-2 has been developed that mechanistically solves the plant hydraulics (Xu et al., 2016). This is currently not integrated with the ED-2.2 described here; however, work is underway to include this new formulation into the main distribution of the model on GitHub.

Figures

IB Comment 41: Figure 1: White text was difficult for me to read. It might be worth sacrificing the pretty clouds/sky background for something more simple. Or maybe just use red lettering.

> Response: In light of the Reviewer's feedback, we will increase the contrast between the background and the text, and replace the cloud picture to avoid white letters on white background. We will also increase the contrast in Figures 2 and 3. In addition, we will change the symbols in the regional maps (Figures 7 and S4), following a suggestion we received for Part 2.

IB Comment 42: Figure 2: caption should say "dashed yellow arrows"

> Response: We will correct the caption as suggested. We also noticed that the colors were incorrect in Figure 3 caption, and will correct these as well.

IB Comment 43: Figure 3: caption should say there are 3 cohorts shown.

> Response: In line with the changes described in our response to *IB Comment 25*, we will update Figure 3 to show a generic number (N_T) of cohorts.

IB Comment 44: Nice paper, people. Good work.

> Response: Thank you Ian for reviewing the manuscript and for your constructive thoughts and feedback.

Responses to Dr. Stefan Olin

SO Comment 1: The model description did not leave out any details, which is a very good thing and it is not very common for many of the existing model description papers. The downside of that is of course that the manuscript is rather long, too long in my opinon.

> Response: We are glad that the Reviewer thinks the model description is detailed. Our goal was indeed to describe how the processes are actually solved within the ED-2.2 framework; we believe that providing the complete description allows ED-2.2 users to understand the rationale behind each module, and researchers using other models to understand and reproduce our methods should they decide to implement our developments in other models. We tried to keep the main text as concise as possible, by keeping only the main description of the fluxes in the main text, and algorithms and details on other variables (specific heat, conductances, definition of state variables, among others) in the Supplements. However, because the model solves many processes at multiple time scales, the final manuscript is still very long.

SO Comment 2: One thing I miss from the very thorough walkthrough of vegetation models in the introduction are references to the DGVMs that are closer to ED such as LPJ-GUESS (for disclosure, I am an LPJ-GUESS developer).

> Response: Following the Reviewer's suggestion, we included a new paragraph in the introduction describing the emergence of cohort-based models and included references to some of them, including LPJ-GUESS.

"The need to represent vegetation structure in terrestrial biosphere models, without the computational burden required to simulate every tree at regional and global scales, led to the development of cohort-based models (Fisher et al., 2018). In the cohort-based approach, individual trees are grouped according to their size (e.g. height or diameter at breast height); functional groups, which can be defined along trait axes (e.g. Reich et al., 1997; Wright et al., 2004; Fortunel et al., 2012), and micro-environment conditions (e.g. whether plants are living in a gap, recently burned fragment, or in a patch of old-growth forest). Over the past two decades, many cohort-based models have emerged, including the Ecosystem Demography Model (ED, Moorcroft et al., 2001; Hurtt et al., 2002; Albani et al., 2006; Medvigy et al., 2009); the Lund-Potsdam-Jena General Ecosystem Simulator (LPJ-GUESS, Smith et al., 2001; Ahlström et al., 2012; Lindeskog et al., 2013); and the Land Model version 3 with Perfect Plasticity Approximation (LM3-PPA, Weng et al., 2015), and the Functionally-Assembled Terrestrial Ecosystem Simulator (FATES, Fisher et al., 2015; Huang et al., 2019). Because these models also represent functional diversity and heterogeneity of micro-environments, the ecosystem's structure, diversity and functioning also emerge from the interactions between plants with different life strategies under different resource availability, albeit at a lesser extent than individual-based models (Fisher et al., 2018)."

SO Comment 3: The text is easy to read, and the refernces to equations, sections and tables are good. One comment regarding the referencing to equations is that it should be concistent, for example on line 14, page 7. In my opinon it should read: Eq. 2-3 cannot ... .

> Response: We believe our equation references are consistent with the GMD style guidelines. We do not have a strong preference and we will change the notation if the editor considers the reference style suggested by the Reviewer more appropriate.

SO Comment 4: Another comment I have regarding the equations (or symbols) is the sometimes odd choice of symbols. Like Eq. 36-38, why choose the same symbol for a variable that you are using as an operator?, that is very confusing.

> Response: We sometimes resorted on less conventional symbols because we used all letters of the Latin and Greek alphabet (lower case, upper case, and calligraphy, see Table S2). However, we agree with the reviewer that the use of Π for plant area index in Eq. 36–37 is potentially confusing. To address this issue we will replace the plant area index symbol from Π to Φ (upper case), and plant area density to ϕ (lower case), and use the symbol ϖ (originally used for plant area density) for oxygenase:carboxylase rate. We tried to restrict odd symbols to variables that were only used in specific equations, to avoid distractions.

SO Comment 5: The same goes for the use of exp instead of e, and on the note of the letter e, you are using it as pool (ej) and as a scaling factor (eH ot), I'd say that it is better to use the letter e as the mathematical constant it is, and then use some other symbol to denote your pools. And for your factors, use q or f.

> Response: Both "*e*" and "exp" standard mathematical notation to refer to an exponential function. Regarding the scaling factors, we believe the Reviewer was referring to Eq. 103 and 104. However, we agree that using "*e*" is this context is potentially confusing, and therefore we will replace it with "*f-hat*" (f_{Cold} *and* f_{Hot} are already used in Eq. 85, and q is used for specific heat). We will also replace e for the elongation factor (Supplements) with $\hat{e}$. We did not use f in this case because it would lead to too many levels of subscripts, and no other letter was available. Regarding the soil carbon pools, we think that the subscripts make them clearly distinct from actual variables, and we plan to keep them the same, as all other Latin letters have already been used for something else.

SO Comment 6: On the same topic of mathematical operators as variables, in Eq. 76, maybe something went wrong, there is a definition character instead of an equal sign. And again, why use operators as super scripts, just adds confusion.

> Response: We believe that the Reviewer was referring to Eq. 75. We agree that the choice of the equivalent sign to indicate phase equilibrium (also known as saturation) was confusing, and that the superscript was also confusing. In light of the Reviewer's comment, we will refer to phase-equilibrium partial pressure as p_{Sat} and to phase-equilibrium specific humidity as w_{Sat}.

SO Comment 7: And likewise, in Eq. 56, is that an equal sign as a superscript or do you have an assignment within the equation? Or is it a pre-request? Either way, that equation is confusing.

> Response: In light of the Reviewer's suggestion, we will split Eq. 56 into two equations to eliminate the ambiguity.

SO Comment 8: With such an explicit formulation of the exchange of heat and water I find it rather strange that the incoming water does not have an explicit energy level specified. If 15 deg. C water lands on a surface that is 25 deg. C, there would be a cooling taking place. Maybe this is of minor importance in the Amazon, but in colder places this would matter. Or did I totally misread what is written in the beginning of Sect. 4.2, if so, I suggest you clarify this.

> Response: Precipitation has an explicit energy level associated, and this energy input is defined in the paragraph that starts on Page 21, line 15, including equation 39. Most meteorological drivers (including eddy covariance towers) do not provide precipitation

temperature. Therefore, we assumed it to be the same as the temperature of the air above canopy (T_a, Eq. 39), which is not the same as the leaf or ground temperature; therefore the model does account for the cooling effect of precipitation. We will rewrite the paragraph to make these important points more explicit:

"Precipitation is a mass flux, but it also has an associated enthalpy flux ($\dot{H}$) that must be partitioned and incorporated to the cohorts and temporary surface water. Similar to the water exchange between soil layers, the enthalpy flux associated with rainfall uses the definition of enthalpy (Supplement S5). Because precipitation temperatures are seldom available in meteorological drivers (either from towers or gridded meteorological forcing data sets), we assume that precipitation temperature is closely associated with the free-air temperature (T_a), and we use T_a to determine whether the precipitation falls as rain, snow, or a mix of both. Importantly, the use of free-air temperature partly accounts for the thermal difference between precipitation temperature and the temperature of intercepted surfaces…"

In addition to the temperature difference, precipitation in the form of snow has varying density, which in turn affects the density of developing snowpack. The dynamics of the snowpack was missing in the original manuscript, and we think it is an important process for non-tropical simulations. To avoid extending the main manuscript, we will include the description of the snowpack depth dynamics as a new supplement, and indicate this in the text:

"In the case of liquid TSW, the layer thickness of the single layer is defined as $\Delta z = \rho_{\ell}^{-1} W_{s1}$, where ρ_{ℓ} is the density of liquid water (Table S3). In the case of snowpack development, the snow density and the layer thickness of the TSW are solved as described in Supplement S7. The thickness of each layer of snow (Δz_{sj}) is defined using the same algorithm as LEAF-2 (Walko et al., 2000) and described in Supplement S7."

The new Supplement that briefly describes the snowpack depth dynamics:

"S7 Snowpack depth dynamics

In addition to enthalpy and total water, we must also track the changes in snowpack depth of each layer (Δz_{sj}) and density (ρ_{sj}) over time. The ordinary differential equation that governs changes in depth over time is defined as:

$$\frac{\mathrm{d}\Delta z_{s_j}}{\mathrm{d}t} = \begin{cases} \underbrace{\rho_{wa}\,\dot{W}_{a,s_j}}_{\substack{\text{Throughfall}\\ \text{precipitation}\\ (\boxed{4.2})}} + \underbrace{\left(\sum_{k=1}^{N_T} \rho_{wt_k}\,\dot{W}_{t_k,s_j}\right)}_{\substack{\text{Canopy dripping}\\ \text{from cohorts}\\ (\boxed{4.2})}} - \underbrace{\rho_{ws_j}\,\dot{W}_{s_j,o}}_{\substack{\text{Surface runoff}\\ (\boxed{4.1})}} - \underbrace{\rho_{wx}\dot{W}_{s_j,c}}_{\substack{\text{Surface water}\\ \text{evaporation}\\ (\boxed{4.5.2}\text{ and }\boxed{4.5.3})}} - \underbrace{\delta_{s_1 s_j}\,\rho_{ws_j}\,\dot{W}_{s_1,g_{N_G}}}_{\substack{\text{Surface water}\\ \text{percolation}\\ (\boxed{4.1})}} & \text{, if } s_j = s_{N_S} \\ 0 & \text{, otherwise} \end{cases} , \qquad \text{(S68)}$$

$$\rho_{ws_j} = \frac{W_{s_j}}{\Delta z_{s_j}} \qquad \text{(S69)}$$

$$\rho_{wx} = \begin{cases} \rho_{ws_{N_S}} & \text{, if } \dot{W}_{s_{N_S},c} \geq 0 \\ \rho_{wc} & \text{, if } \dot{W}_{s_{N_S},c} < 0 \end{cases} \qquad \text{(S70)}$$

where $\delta s_j s_{j'}$ is the Kronecker delta for comparing two TSW layers s_j and $s_{j'}$ (1 if $s_j = s_{j'}$, 0 otherwise), ρ_{wa} is the precipitation density, ρ_{wtk} is the canopy interception density, ρ_{wc} is the density of condensing water vapor. Precipitation density is defined based on Jin et al. (1999), but slightly modified to make it continuous:

$$\rho_{wa} = \frac{\rho_{ia}\,\rho_\ell}{\ell_a \rho_{ia} + (1 - \ell_a)\,\rho_\ell}, \qquad \text{(S71)}$$

$$\rho_{ia} = \begin{cases} 169.16 & \text{, if } T_a > 275.16\ \text{K} \\ 50. + 1.7\,(T_a - 258.16)^{1.5} & \text{, if } 258.16\ \text{K} < T_a \leq 275.66\ \text{K} \\ 50. & \text{, if } T_a \leq 258.16\ \text{K} \end{cases} , \qquad \text{(S72)}$$

where ρ_ℓ is the density of liquid water (Table S3). For the canopy dripping flux, water density is similar to Eq. (S71), except that we assume the density of frozen water to be the same as frost density (ρ_{*}, Table S3). A similar assumption is done for water condensing from canopy air space, with the additional assumption that the liquid fraction of condensation is the same as the liquid fraction of the top TSW layer:

The maximum allowed number of snow layers is determined by the user, but the actual number of snow layers is dynamically determined, following the same algorithm as Walko et al. (2000). Multiple layers only exist when ice is present, otherwise a single layer (N_S = 1) is enforced. When ice is present, the model selects NS to be the maximum number of layers that satisfies

$$\rho_{wt_k} = \frac{\rho_{\divideontimes}\,\rho_\ell}{\ell_{t_k}\rho_{\divideontimes} + (1-\ell_{t_k})\,\rho_\ell}, \tag{S73}$$

$$\rho_{wc} = \frac{\rho_{\divideontimes}\,\rho_\ell}{\ell_{s_{N_S}}\rho_{\divideontimes} + \left(1-\ell_{s_{N_S}}\right)\rho_\ell}. \tag{S74}$$

$W_{sj} \geq 5$ $\mathrm{kg_W\,m^{-2}}$ for all layers s_j, $j \in 1,2,\ldots,N_S$, to ensure numerical stability. The layer thickness distribution (Δz_{sj}) for any given N_S is defined as:

$$\Delta z_{s_j} = z_s \, \frac{2^{\min(j-1,N_S-j)}}{2^{\lfloor \frac{N_S+1}{2} \rfloor} + 2^{\lfloor \frac{N_S}{2} \rfloor} - 2}, \tag{S75}$$

$$z_s = \sum_{j=1}^{N_S} \Delta z_{s_j}, \tag{S76}$$

where z_s is the total depth of the snow, and $\lfloor x \rfloor$ is the floor function (i.e. the nearest integer value to x that is not greater than x). The layer distribution described by Eq. (S75) ensures that the layers near the ground and near the canopy air space are thinner than the intermediate layers, to improve the representation of exchanges between the snowpack and the canopy air space, soils, and incoming irradiance (Walko et al., 2000)."

SO Comment 9: In the first paragraph of the discussion you are writing that you have demonstrated a functional diverse canopy, from the supplements I get that you have three PFTs along one functional trait axis.

> Response: The reviewer is correct that there are three default tree PFTs for tropical South America, and for the purposes of this manuscript we only used these three tree PFTs. However, in ED-2.2 model users can specify additional PFTs, or modify the existing PFTs, using XML files that are read during the initialization, and several published ED2 studies have modified or added tropical PFTs according to the scientific questions (e.g. Xu et al. 2016, Trugman et al. 2016, Feng et al. 2018).

This information was missing in our manuscript, and we will include the following paragraph in Section 2.3 (Model inputs):

"*Plant functional types.* The user must specify which plant functional types (PFTs) are allowed to occur in any given simulation. ED-2.2 has a list of default PFTs, with parameters described in Tables S5-S6. Alternatively, the user can modify the parameters of existing PFTs or define new PFTs through an extensible markup language (XML) file, which is read during the model initialization."

We will also update the discussion (Section 6.2) and point readers to these previous studies that have used non-default PFTs in the tropics:

"In this manuscript, we presented the functional diversity using only the default tropical plant functional types (PFTs), which describe the functional diversity along a single functional trait axis of broadleaf tropical trees. However, the ED-2.2 framework allows users to easily modify the traits and trade-offs of existing PFTs, or include new functional groups; previous studies using ED-2.2 have leveraged this feature of the code to define PFTs according to the research question both in the tropics (e.g. Xu et al., 2016; Trugman et al., 2018; Feng et al., 2018) and in the extra-tropics (e.g. Raczka et al., 2018; Bogan et al., 2019)."

SO Comment 10: Results are not really discussed nor shown, but one result that there is much focus on is the closed energy budget. Is it really closed if there is a 0.01 deviation? Is there not a great risk of error propagation if the bar is set that low? In LPJ-GUESS we are concerned if the mass balance is off by 10^{-12}.

> Response: We agree with the reviewer that we should impose stricter tolerances by default, and we will modify the default to be 10^{-5} and update the code available in the permanent repository. We cannot impose a tolerance as strict as LPJ-GUESS: to reduce the size of the output files, variables in ED-2.2 are stored as single precision (truncation error of the order of 10^{-7}), even though the biophysics solver uses double precision. We tested the model with the new tolerance, and the model ran without problems. We will revise the paragraph to include the updated information:

"The ED-2.2 simulations show a high degree of conservation of the total energy, water, and carbon (Fig. 6). In the example simulation for one patch at Paracou, French Guiana (GYF), a tropical forest site, the accumulated deviation from perfect closure (residual) of the energy budget over 50 years (2,629,800 time steps) was 0.1% of the total enthalpy storage — sum of enthalpy stored at the canopy air space, cohorts, temporary surface water and soil layers (Fig. 6a), and 0.002% of the accumulated losses through eddy flux, the largest cumulative flux of enthalpy. Results for the water budget were even better, with maximum accumulated residuals of 0.04% of the total water stored in the ED-2.2 thermodynamic systems, or 0.0006% of the total water input by precipitation (Fig. 6b), and the accumulated residual of carbon was 0.008% of the total carbon storage or 0.017% the total accumulated loss through eddy flux. The average absolute residual errors by time step, relative to the total storage, ranged from $3.6 \cdot 10^{-11}$ (carbon) to $3.8 \cdot 10^{-10}$ (energy), and thus orders of magnitude less than the truncation error of single-precision numbers ($1.2 \cdot 10^{-7}$) and the model tolerance for each time step ($1.2 \cdot 10^{-5}$)."

We had already included text in the discussion proposing areas of improvement of closure, but we will insert the following sentence acknowledging that the closure should be improved before the suggestions for improvement:

"Nonetheless, the residual errors in ED-2.2 are larger than the error of each time steps after integrating the model over multiple decades (Fig. 6), which suggests that the errors may have a systematic component that deserves further investigation."

Regarding the absence of results, we only showed model verification and some examples of simulations in this paper, in line with the GMD guidelines for model description manuscript. The companion manuscript (Longo et al., 2019), also in review for GMD, has extensive model evaluation for tropical South America.

SO Comment 11: Some specific comments in addition to those spotted by Ian Baker: Line 3, page 41: remove the 'a'.

> Response: As suggested, we will remove the "a".

SO Comment 12: Line 3, page 21: intercepted instead of intercept.

> Response: As suggested, we will replace "intercept" with "intercepted."

SO Comment 13: Line 1, page 33: What is a decay rate due to respiration? Do you mean turnover?

> Response: The original sentence was incorrect. This section is describing metabolic fine-root respiration, not turnover. We will replace the sentence with the following:

"where r_{rk} (s^{-1}) is the PFT-dependent factor that describes the relative metabolic activity of fine roots at the reference temperature (15°C)..."

SO Comment 14: Page 34: GYF is not defined, comes later.

> Response: We will include the following explanation:

"In the example simulation for one patch at Paracou, French Guiana (GYF), a tropical forest site, the accumulated deviation from perfect closure (residual)…"

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