

[Interactive  
Comment](#)

## ***Interactive comment on “Analysing Amazonian forest productivity using a new individual and trait-based model (TFS v.1)” by N. M. Fyllas et al.***

### **Anonymous Referee #2**

Received and published: 12 April 2014

#### General Comments:

The authors present a new trait-based and individual-based formulation of terrestrial vegetation. This paper has been fun to read, exciting, and I am looking forward to seeing this model address more science questions in the future.

The methodology, as far as I know, is novel. The novel contribution to the modeling community is the method of randomly assigning traits to individuals from a reference stand-sample of traits with limited size. There are also various adaptations of biophysical processes, but the dividing line between new contribution and existing method is not as clear. The strengths of the paper are as follows: appropriate review of literature, novel science, appropriate level of comparison of model performance with observations, acceptable coverage of model mechanics and evaluation of the trait variability

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



space and other emergent properties.

The manuscript has modest shortcomings, that can be easily addressed: redundancies in the explanation of the model, the order of which concepts are introduced and explained, amount of emphasis placed on explaining both new and existing model concepts.

#### Specific Comments:

The most interesting and novel components of the model is the choice of the trait set, and how the sample of traits is generated. The authors also create apparently unique adaptations of water controls and light interception too, but it is less clear. If it is new, you've got bragging rights! Tell us at the beginning of the model description! They need to state out front, other than the trait sampling and generation process, what model mechanics are novel. The model description in the main text should emphasize these points. For instance tessellation is a concept brought up by Purves 2007 (which you acknowledge), but its not so clear if and how your adaptation of tessellation and ultimately light scattering differs. \*Note that this is definitely not an issue about giving an acceptable level of credit to prior research, that is fine.

The authors walk through how the methods of light interception, stomatal conductance and water controls are integrated into the model, yet they do not place the same attention on how the method of Taylor and Thompson is specifically applied. I would strongly recommend \*\*giving more treatment to how the Taylor and Thompson method works in this model\*\*, paragraph 2.1.1 is helpful but it needs more substance.

The supplemental material is a trove of interesting results and explanation of model mechanics. However, it is not clear why the authors decided to put some description in the body text, and some description in the supplemental material. There is also some redundancy. For instance the "daily fractional available water content" equation is displayed twice, and the leaf photosynthetic capacity equation is displayed twice. The logic behind what model mechanic is explained in the body text must be stated up front.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



For instance one way to separate the explanation, is to only cover direct functional relationships that involve the 4 base traits. Otherwise, condense and combine the explanations of non novel model mechanics from the main text with the supplemental material; then make a concise summary of these modules for the main text.

It would be really interesting to see a diagram showing how the 4 traits tie into the model mechanics of the stand, although I think the manuscript is fine without.

Some commentary in the discussion, or even a sensitivity analysis could be made, regarding how the size of the trait sampling space impacts model response and validation with observation. I am on the fence whether this type of analysis is really necessary. Although, I think it would be very useful.

The introduction gives a background on the four traits, but I would recommend that the discussion address other traits that were considered and rejected. The authors acknowledge works of Scheiter and Higgins, and Falster et al. 2011 and explain the differences in the three modeling schemes. For instance though, why was a trait such as seed size not included? A perfectly acceptable reason would be that you simply wanted to keep the trait set small or lack of data, but the decision making process would also be interesting and informative.

Technical comments:

This list of technical comments, appologetically, is definiately not complete. While the paper read smoothly in general, there were some awkward phrases here an there, and some overly verbose deliveries of concept. These are things that will smooth out with a few re-reads and the normal word smithing.

L26 1418: It is unlcear what "functional dimensions" means here.

L28 1418: change "leaf N and P dry mass concentration" to "leaf nitrogen and phosphorous dry mass concentration"

L5 1419: awkward sentence "Co-varying ..."

C356

GMDD

7, C354–C357, 2014

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



L10 1419: awkward sentence "The fourth trait..."

L19 1433: cite Moorcroft et al. 2001 along with Fisher et al. 2010, capitalize Ecosystem Demography

The font sizes in the figures need to be en-biggered in some places: figure 1 is not even close to readable, figure 4 axis tick labels are just barely readable, S1.1 is a mirror of figure 1 and enlarged but some axis are stil completely unreadable, the axis ticks and labels in the lower panel of S1.5 are also too small.

---

Interactive comment on Geosci. Model Dev. Discuss., 7, 1413, 2014.

**GMDD**

7, C354–C357, 2014

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper