

Interactive comment on “Taking off the training wheels: the properties of a dynamic vegetation model without climate envelopes” by R. A. Fisher et al.

E. Weng (Referee)

weng@princeton.edu

Received and published: 6 June 2015

This paper describes a new version the CLM model that incorporates a set of individual-based competition procedures according to the concept of the ED model, which allows the model to predict forest distributions based on competitions among individual trees. So, theoretically, it doesn't need the climatic envelopes imposed on plant functional types to define their geographical distributions. The authors tested the model's performance of predicting distributions of evergreen and deciduous forests in Eastern North America. The authors also parameterized a set of key processes by the correlations of plant traits (e.g., leaf nitrogen, V_{cmax} , respiration, and leaf life span) to improve the

C987

performance of the coupled model. Because of the complexity of individual based forest models and debates on the distributions of deciduous vs. evergreen trees regarding to their physiological and morphological traits, this study is a good try at coupling the processes ranging from leaf physiology to individual behavior. The paper is well written and the model is clearly described in the main text and supplemental materials. (But the tech note seems to be independent of this paper since it has a different author list.)

My major concerns are about the costs and benefits analysis that relate to the fundamental theories/principals about the relative advantages and competitiveness of deciduous vs. evergreen trees. I think the most valuable part of this paper is its tests and discussions about the parameter sensitivity and uncertainties of the relationships of plant traits in affecting the predictions of the distributions of evergreen and deciduous trees. The tests presented in this paper may not show how perfect the model is, but they can tell us why the model performs good or bad. This information can help in developing a better model. Here, I'm not criticizing the analyses. Presenting more details that explain the model behavior may improve the value of this paper and help the readers to understand the simulated results.

1. Costs and benefits analysis of deciduous vs. evergreen leaves

The authors mentioned “carbon economy” and “costs and benefits” in introduction, but I don't find any such analysis in the methods and results. I'd like to see such analyses at different simulated biomes so that we can know why one outcompetes the other one and how the parameterization schemes affect the fitness of deciduous and evergreen trees. The cost-benefit analysis can explain the simulated distribution patterns. Basically, one plant can't distribute in some particular regions by two reasons: one is that it can't survive the climatic conditions of those regions. For the ensemble simulations in this study, there may be the third reason: the plant traits combinations may be carbon negative in some grids. The cost-benefit analysis can explain this. Therefore, we can check if the simulated distributions are resulted from correct reasons.

C988

2. Distribution patterns and successive patterns of evergreen vs. deciduous forests

This model is a carbon-only model. But ecologists has found the needle-leaved evergreen trees usually distribute in nutrient poor soils while broadleaf deciduous trees with fertile soils and theoretical explanations have been proposed to explain this pattern (e.g. Givnish 2002). Can this model predict this pattern? What reasons made this pattern happen or not happen in the model? are they the same or different with the theories proposed by those ecologists? Because there are so many empirical relationships in a model, it always happens that one can get correct results by wrong reasons. I want the authors to check the details of why a particular PFT (evergreen or deciduous) wins or fails at some grids. Needle-leaved evergreen trees are usually pioneer species and dominant at early succession stage in temperate regions. Is this pattern observed in this model?

And, how costs and benefits of leaves explain these two patterns?

Specific questions:

1. Page 3303, line 18: "ENT have much lower nitrogen use efficiency than DBT".

It depends on how to count it. Per unit time, ENT may have lower carbon gain per unit nitrogen. But as for "nitrogen use efficiency", it should be counted as the carbon gain during the lifetime of nitrogen in a leaf. Since evergreen leaves have much longer lifespan than deciduous leaves, the carbon gain per unit nitrogen through the whole lifetime is higher than deciduous leaves.

2. Page3305, lines 16~18: "we ran the models 30 years"

I was wondering if the authors let the mode run the whole period of forest succession? (30 years is too short for succession) So, how to determine who wins eventually at a grid? For some places, evergreen trees may occupy the stand for 30~50 year and then replaced by deciduous trees gradually.

3. Page 3307, line 16: "the number of leaf layers over the footprint of the tree"

C989

"number of leaf layers" and "footprint of the tree" are not clear to me. According to the equation 3, they are individual tree's LAI and crown area, respectively.

4. Page 3308, lines 1~2: "the net assimilation cost of the bottom leaf layer does not fall below zero" Theoretically, it could "fall below zero", only if they could worsen others. (I was just thinking of this when reading it. It's ok here to have "zero" as the criterion.)

5. Page 3308, lines 7~11 and 16~20.

Here, it seems there are some foes in the base model in parameterizing LMA-leaf mass-LAI and the authors used another assumption (Eq 5) to correct it. Ideally, evergreen trees should grow faster than deciduous trees during the early succession stage because of its high LAI. After forest closed, deciduous trees will gradually replace evergreen trees because of successful regeneration. A carbon-only model should be capable of simulating this pattern.

For me, it's not necessary to specifically set the initial LAI same for DBT and ENT. This would complicate the model. A delicate design of LMA and leaf lifespan can solve this problem. For the same allocation of NPP to leaves, ENT should have much higher LAI because of its long leaf lifespan, and the high LAI and long growing season of ENT leaves will make evergreen trees have a high productivity. So, the authors don't have to set a high LAI for ENT.

Anyway, this is a new model and shouldn't have too many such kind of compromising design. It also brings me a question: In those grids that ENT wins, what makes them win?

6. Page 3317, Lines 18~22 and lines 4~6 in page 3321:

Since the leaf lifespan is a function of temperature, there are still some kinds of "empirically derived climatic constraints" in this model. The relationship between temperature and leaf lifespan is a result of competition and it will change with other factors, such as CO₂.

C990

7. Table 1

Some of the combinations are not realistic. For example, the ENT of the Run ID 15 has a short leaf lifespan (0.6839 yrs), high LMA (483.6 gC/m²), and high Narea (4.95 g/m²). According to Osnas et al. (2013, Science) and many other studies, leaf lifespan has a good linear relationship with LMA. And it will be great if I can see a table or figure in the results showing carbon economy of these combinations. It will be helpful for readers to understand spatial distribution patterns of the traits combinations.

Interactive comment on Geosci. Model Dev. Discuss., 8, 3293, 2015.