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Comment on gmd-2022-72

Anonymous Referee #2

Referee comment on "Modeling demographic-driven vegetation dynamics and ecosystem biogeochemical cycling in NASA GISS's Earth system model (ModelE-BiomeE v.1.0)" by Ensheng Weng et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-72-RC2, 2022

The authors present a model development work on vegetation demographics and seek to implement it into an Earth system model. The new model features include a greater diversity of global plant functional types and a new phenological scheme. They also compare the behaviour of this model to the eight observational locations and six MsTMIP simulations. As seen from the results within the paper it is possible to capture vegetation structure and dynamics reasonably even within such a parasimonious model at a global scale. Overall, this work is well structured and is useful in highlighting some new issues in improving the representation of the terrestrial carbon cycle within ESMs. I think the manuscript could be publishable after some major revisions. I have a few comments below.

Major comments:

- One of the most representative feature of this model is the fulldemographic processes. The authors mainly compare the differences between the simulations of the full demography and the single cohort settings of BiomeE. I think authors should separately compare simulations of these two versions of BiomE with observations, if possible, to show the advantages of full demography in reproducing ecosystem dynamics (Figs. 6-10).
- Lines 157-158: "A set of continuous plant traits are used to define the distinctive plant types". Please specify the continuous trait assignments for plant functional types, especially the differences with traditional PFT-based model.
- To represent the major variations in plant functional diversity, the authorschose four plant traits as the primary axes to define PFTs: wood density, leaf mass per unit area (LMA), height growth parameter, and leaf maximum carboxylation rate (*V*cmax). I would suggest a sensitivity analysis of these plant traits to different ecosystem functions, which would be very instructive for futher model improvement and localization.

- Methods Line 184: it is unclear to me about the assumptions in the phenological scheme. Why to define the nine PFTs as these four phenological types? Regarding the comparative advantage and competitiveness of deciduous vs. evergreen trees, are there any basic theories that evergreen species are more resistant to cold and drought than deciduous tree species? According to the global vegetation distribution, evergreen broadleaf species are usually distributed in warm and moist environments. What kinds of functional traits suggest that evergreen species are adaptive under water limitation and cold conditions?
- Methods Line 254: PFT-specific parameterisations for the mortality parameter are used, so are there different PFTs each with their own cohort structure? How are PFT-specific background mortality parameters set in the model? Are they all come from observations across different vegetation types? Related reference is missing in the main text. Since the most size-dependent mortality research focus on closed-canopy forest system, whether the "U-shaped mortality pattern" can be extended to other vegetation systems, including forbs, shrubs, grasslands, systems with open canopies and systems experiencing different risks in different environment?
- Methods Line 374: how does disturbance history set in the MsTMIP simulations? I'm wondering whether the large inter-model discrepancy in simulating plant biomass is caused by disturbance dynamics? For clarity, can the authors be a bit more explicit about the experimental design of the MsTMIP.
- Figure 4d: the authors point out that model analyses are based on equilibrium simulations without explicit disturbances. But the critical height across forests shows an abrupt decrease in the 100 years of model run. What reasons made this pattern happen in the model? Is that driven by the aging-related mortality of canopy trees? Could you discuss more the underlying mechanisms behind the emergent ecological phenomena?
- Result Line 515-517: the formulation of allometry makes the tree height growth as a function of tree diameter (Eq. 5 in the main text). Since the two model versions have similar stem growth and tree size distribution, I would assume that tree height growth is stable as well. Why the full demography model shows higher tree height than the single-cohort model (Figure 11c)?
- Result: the authors evaluate the model outputs with the MsTMIP simulations in the Result section. The simple intercomparison would be invaluable to help determine which model behaviour is more realistic. I think it would be interesting to have a section in the discussion tracing the variability that emerges among the models and informing what modeling structural choices or assumptions lead to improved model estimates. Since this paper is a model description paper, further discussion by model developers on the potential reasons for the biases would be much appreciated.

Minor comments:

- The abbreviation of the term CAI on Line 409 should be put in parentheses for the first time on Line 407.
- Lines 71-75: it is unclear to me what is "the legacy of land models and the technical requirements of reversibility in model development"? Could you explain or rephrase this sentence?
- Lines 225, "H is tree height"should be modified to "Z is tree height".
- In equation(10), *k* is ground area? Not defined.
- Figure 3. How to understand the constant LAI value of KZ?
- Figure 9. Please add units of LAI.