

Interactive comment on “The biophysics, ecology, and biogeochemistry of functionally diverse, vertically- and horizontally-heterogeneous ecosystems: the Ecosystem Demography Model, version 2.2 – Part 2: Model evaluation” by Marcos Longo et al.

Anonymous Referee #2

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The authors present an interesting model evaluation of a new version of the Ecosystem Demography Model (version 2.2) for the Amazon region. They show strengths and weaknesses of the modelling approach and identify priorities for further model development. For the model evaluation, the authors use observational data from four specific sites, inventories and remote sensing and nicely present the comparison with the simulation results. Below, I have some comments and remarks that will hopefully help to improve the manuscript.

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General comments:

- Title: The model evaluation has been done for the Amazon region, please state this in the title.
- The Methods section could profit from more structure and detail. First, although the manuscript has a companion paper that provides a detailed model description, I would recommend to give a summary of the model and a short overview over the relevant processes that are evaluated, in the beginning of the methods section. Additionally, it is to my opinion not fully clear, which datasets are used as forcing data and which are used for model evaluation (e.g. p.4, l.18ff).
- In the Methods section (p. 4, l. 17ff and p.5, l. 4) it is not clearly described how the model sensitivity was evaluated. Did you systematically vary different parameters or driver data?
- The assessment of forest function and structure is interesting and it is nice to see that the model can reproduce the functional relations (Fig. 9). I think it would be important to explain the reader that the mortality rate and wood density (e.g. Fig. 9b,c) are simulated by the model and not prescribed parameters, probably this could be done in the model summary in the methods section as suggested above. Regarding the unexplained bias in simulated mortality rates and wood density, it would also be interesting to see the relation between productivity (NPP) and mortality, which are probably the two dominant drivers for AGB. When looking at Fig. 5, it seems that (at plot level) GPP is underestimated and respiration is overestimated (Fig. 6), thus, NPP might be underestimated. With high mortality rates (Fig. 9) I wonder why biomass is overestimated (Fig. 7)?

Specific comments:

- Section 2.1: Which climate data were used to drive the model for the assessment of short-term fluxes? L. 18: What do you mean by “we aggregated the model results

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to polygon level hourly averages"? Please explain the model setup more clearly (as in section 2.2). L. 19-22: The explanation in these sentences is difficult to understand. Did you compare NEE? Why is it relevant to compare "all times in which the net ecosystem productivity could be estimated..."?

- p. 4, l. 13: "We initialized soils with texture obtained from Quesada et al." What has exactly been done for the initialization?
- p. 4, l. 21ff: You assume a constant monthly evapotranspiration of 100 mm month⁻¹ for calculating MCWD. This assumption is not valid in arid regions (L. 25). How reliable is the application of MCWD with the assumptions you made? Please also explain how the yearly MCWD is calculated, is it done for the hydrological year?
- p. 5, l. 4-5: "To evaluate the sensitivity of mortality and the growth rate": Sensitivity to what? Please specify.
- Page 5 line 13: How was outgoing shortwave radiation calculated by the model? Depending on the method, isn't outgoing-sw a proxy for LAI or FPC for which also site data should be available?
- p. 5, l. 19 and Fig. 2: Please define "TAI". The message of Fig. 2 is not clear. Is this related to LAI?
- p. 12, l. 5: Please move this sentence to the methods section and explain how the comparison of MODIS-LAI has been set up (which spatial resolution, for which time period, how did it match with information on deforestation considered in the simulations?)
- Figure 7: The symbols indicating the locations of the focus sites are very hard to see in the map.
- p. 13, l. 7: First time that fire is mentioned, leaving the reader a bit puzzled. It would be good to evaluate the occurrence of fire in more detail and to mention it in the methods section.

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- Page 17, figure 17: ED2 shows (almost) constant mortality; density-independend and dependent. Why is it constant? Was there only background mortality occurring throughout the simulation period? What about the effects of the drought years (e.g. 2005 and 2010)? I would expect to see an effect in the model results.

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