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Geoscientific Model Development Discussions

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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 #1

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The manuscript by Marcos Longo and colleagues performs a detailed evaluation of the ED-2.2 model (described in an accompanying discussion manuscript) for two sites in the Amazon and for the regional patterns simulated for the northern part of South America. The results are mostly presented in a comprehensive way and evaluate many different aspects of the model, both with regard to the short-term behaviour as "land surface scheme" and carbon cycle model and with regard to the long-term vegetation dynamics. It is hard to approach completeness in such an evaluation with all differ-

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ent properties and processes simulated by a model of this complexity, but I think that the authors have presented a nice selection of results in which the most important processes as well as different types of processes are addressed.

These results, while not providing scientific novelties in themselves, provide a thorough evaluation of the model presented in the accompanying manuscript and provide good insight in the strengths and weaknesses of the current model implementation, and I think that these are presented in a balanced manner. I have some remarks about the way of describing and presenting some parts of the results that I would recommend the authors to address. With these adjustments, I expect the manuscript to be acceptable for publication in GMD.

I describe my remarks in more detail below, with some major issues and a list of smaller suggestions for edits and clarifications.

Major remarks:

- Evaluation of ED-2.2 is undertaken for the tropical forest in the Amazon in this study. Such a regional focus is understandable (and enough to be published separately), but as such the evaluation provided in the manuscript is for these tropical conditions specifically. The title of the manuscript could reflect the Amazon focus of this work. Also, it would be good to stress (p. 22, l. 21) that the model was evaluated for Amazon conditions specifically. E.g., this tests indeed "multiple biophysical and biogeochemical mechanisms", but also leaves many "mechanisms" that are typical for non-tropical conditions (e.g. those related to temperature-induced phenological changes or interactions with snow cover) unevaluated. I miss this aspect in the discussion and conclusion of the manuscript. The authors highlight that earlier versions of ED have been tested for other ecosystems as well (p. 21, l. 30ff), but do not discuss to what extent the current version of the model is expected to behave in a similar way or not.

- For the comparison of light extinction in the canopy (p. 3, l. 32; Fig. 2), it seems crucial that atmospheric conditions (e.g. the ratio of direct and diffuse light) are comparable to the average of the simulation period if you do not use the same days. Has this been tested? Even without detailed meteorological information for the time of measurements of the profile, I expect that there is some basic characterization of the weather conditions for those days that could be tested.

- It is unclear how the sensitivity of the model was tested (using average conditions from different forcing data sets, p. 4, l. 17), and how this relates to the regional simulations mentioned earlier. Are these separate simulations? Or are these meteorological drivers merely used to compute statistics to separate grid cells for determining relationships? In the case of the former, the description should be extended to describe the simulations properly, in the case of the latter, I am unsure why the authors have not used the existing model forcing to perform that separation?

- p. 5, l. 4: Yes, net radiation is partly determined by the incoming radiation (which is an input), but so is outgoing radiation of course. I expect the seasonality shown in Fig. 1 to be primarily determined by the seasonality of incoming radiation, and absolute deviations (in both outgoing and net radiation) are probably more informative for understanding model biases than the model's ability to represent the seasonal cycle.

- Fig. 2: Shading (confidence interval) appears to be missing in the figure.

- I like the summaries of functional relationships provided in Fig. 8 and 9. They are very informative to express the model's ability to represent spatial variations for the wider Amazon area.

- p. 22, l. 16: The ability of ED-2.2 to represent fine-scale heterogeneity is an interesting aspect, but was not evaluated in this study. Remarks to this would fit better in the accompanying "Part 1" paper than in this one.

Minor remarks:

- p. 1, l. 4: "excellent" could be removed here: You have verified the conservation of energy and other properties, not its excellency. In general, there is a tendency in

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the manuscript to describe this conservation as an “excellent” property of the model – I trust that the authors are glad with that result, but conservation of properties is typically considered a technical prerequisite rather than a scientific breakthrough in model development.

- p. 3, l. 5: does “variability” here refer to spatial variability, temporal variability, or both?

- p. 3, l. 19: remove “the” in front of “each”

- p. 3, l. 19: Please clarify this sentence. I guess that GPP and Reco are “modelled statistically” from observed NEE, right? And have you tried to use NEE as well (next to the evaluations of GPP and Reco)? This is not clear to me at this point in the manuscript (it becomes more obvious when the results are presented).

- p. 4, l. 16: What was the spatial extent and spatial resolution of the regional simulation?

- p. 4, l. 29: The text is hard to follow here. Remove “the” in “the three remote sensing estimates,” they are only introduced in the next sentence. And then, it seems that there are only two estimates introduced, whereas the third one (from the two sites only) cannot provide information on the spatial relationship. How was this used?

- p. 5, l. 4: The description of this sensitivity test is hard to follow – sensitivity to what is tested here, and what is the rationale of testing these different settings of the model?

- p. 5, l. 31: Apart from canopy air temperature, it would be interesting to learn whether surface temperature of the canopy is in agreement with observations (if those exist), to investigate whether the overestimated sensible heat fluxes are caused by too high temperatures.

- p. 12, l. 11: add “estimates of” between “remote sensing” and “biomass”

- p. 13, l. 5: Estimates of AGB from ED-2.2 appear to deviate substantially from observed ones, and the cloud of black points in Fig. 9a does not provide any confidence in

ED-2.2 to accurately predict AGB for different sites within the tropical forest. “Generally well characterized” seems a bit too optimistic for this.

- p. 20, l. 9: Sentence should probably read “. . . is accounted for for energy, water and carbon dioxide”.

- p. 20, l. 10: remove “may”

- p. 21, l. 7: remove “significant”

- p. 22, l. 10: Correct “ecosystems”

- Fig. S3: remove “based on” from figure caption

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