Geoscientific

Model Development
Discussions

[GMDD]

Interactive
comment

Interactive comment on "Accounting for forest age in the tile-based dynamic global vegetation model JSBACH4 (4.20p7; git feature/forests) – a land surface model for the ICON-ESM" *by* Julia E. M. S. Nabel et al.

Anonymous Referee #1

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This manuscript presents an interesting and likely significant update to the dynamic global vegetation model JSBACH4. Specifically, a methodology is presented for more accurately simulating forest age structure. It is found that the new model version mostly has lower predictions of gross primary production, aboveground biomass, and leaf area index than the old model version. These lower predictions typically bring the model into closer agreement with the observations. Given the global importance of land-use change and forest harvesting, this paper presents an important step forward. However, there are several areas where I think the manuscript can be improved.

Specific issues

1. The text raises two important expectations that were not fulfilled in the results. First is the issue of land-atmosphere interactions. I would argue that it is conventional to think of land-atmosphere interactions in terms of energy and water budgets, and indeed some authors (e.g., Santanello Jr et al. 2018) define land-atmosphere interactions exclusively in terms of energy and water budgets. Yet the current manuscript contains no results related to energy and water budgets. Personally, I think that such results would be an interesting addition to the paper. Without such an addition, I think the authors need to change the text to more specifically refer to carbon fluxes and budgets. Second is the issue of forest management. The authors repeatedly state (see P11, L10-11 for one example) that an advantage of the model is in simulating different forest management scenarios. However, this is not exploited in the paper (I know that the forest management scenarios in Fig. 7 are different, but so is the climate, so one cannot isolate the effect of the management scenario). Why not illustrate the power of the new modelling approach by running different forest management scenarios for a single grid cell?

2. Some of the methods were not adequately justified. First, I am concerned that the initial condition is unrealistic. Why did simulations begin in 1860 from bare ground rather than from a spin-up? Of course, the 1860 initial condition would more realistically be represented by many forested areas. Second, I could not determine from the paper how one goes from the 2010 age-class distribution to time-dependent (1860-2010) harvest rates. This procedure should be described. Third, it seems like there is an inconsistency between the definition of the model's PFTs (tropical evergreen and deciduous, extratropical evergreen and deciduous) and the Poulter et al. PFTs (broadleaf evergreen and deciduous, needleleaf evergreen and deciduous). What is the correspondence?

3. The comparison to observations can be made more substantial. RMSE is a helpful statistic, but I wonder what is being missed by only considering this statistic. For exam-

ple, I wonder what can be learned from Taylor diagrams? I am certainly not asking that the paper include Taylor diagrams for every variable, but rather such diagrams could be analyzed in a preliminary analysis and the most exciting ones presented in the paper or supporting information.

4. There are some problems with the interpretation of the results. First, I think it misses the point to repeatedly state that the new model is better. Rather, the fundamental result is that new model tends to reduce GPP and LAI relative to the old model. The new model is better because the old model was biased high. If the old model had been unbiased, then the new model would have been biased low. Alternatively, suppose that there is another modeling group excited by this study, and that that modeling group has a model that is biased low. Then implementation of this scheme would probably make that model worse. Second, I think that more care needs to be taken in the interpretation of Figure 6. While the curves in panels a-c are decreasing, the authors do not quantitatively support their assertion that the curves are decreasing exponentially (and not, say, quadratically). Exponential fits should be done and the quality of the fits should be analyzed if the authors want to assert that the declines are exponential. Related to this, the assertion that there is "no offset" in panel d is unsupported. A linear fit should be done, and analysis of the residuals would inform whether there is an offset.

5. In Section 3.3, note that much of the discussion is also relevant to cohort-based models (or at least the ED family). The ED approach involves discretization of a partial differential equation (equation 5 in Moorcroft et al. 2001), and thus there are again questions of the optimal number of age bins, whether the bins should be of different or equal sizes, and criteria for merging.

Technical corrections

P1, L8: do you mean "simulation" rather than "implementation"? This paper, of course, deals with the simulations rather than actual implementations of forest management.

P1, L9: not clear what "hierarchy" is being referred to here

P2, L11: replace "extend" with "extent"

P2, L13-16: there are a couple of sentences where a plural verb "are" is used with a singular subject ("one example")

P3, L5: this sentence seems to have missing words or typos

P3, L6: I am comfortable with the idea that this is a frequently applied approach, but do you have evidence that this is the "most frequently" applied approach?

P3, L30: Perhaps instead of "In this paper we try to", use "The objective of this paper is to"

P7, L4: Note that "data" is plural. Hence, "these data".

Throughout: My sense is that the word "exemplary" is not being used appropriately in the text. Exemplary denotes a particularly good example, whereas I think the authors are oftentimes just referring to an example of the typical sort.

References

Moorcroft, P.R., Hurtt, G.C. and Pacala, S.W., 2001. A method for scaling vegetation dynamics: the ecosystem demography model (ED). Ecological monographs, 71(4), pp.557-586.

Santanello Jr, J.A., Dirmeyer, P.A., Ferguson, C.R., Findell, K.L., Tawfik, A.B., Berg, A., Ek, M., Gentine, P., Guillod, B.P., van Heerwaarden, C. and Roundy, J., 2018. Land–atmosphere interactions: the LoCo perspective. Bulletin of the American Meteorological Society, 99(6), pp.1253-1272.