Geoscientific Model Development Discussions

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Comment on gmd-2020-440

Anonymous Referee #1

Referee comment on "Accounting for forest management in the estimation of forest carbon balance using the dynamic vegetation model LPJ-GUESS (v4.0, r9710): implementation and evaluation of simulations for Europe" by Mats Lindeskog et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2020-440-RC1, 2021

General comments

As I think it is urgently necessary to consider more realistic stand conditions as well as management impacts into estimates of carbon and wood storages, I welcome the incorporation of such features into LPG-GUESS. I think that the results obtained for managed forest area are indeed advancing modelling science. Despite being not the first modelling approach addressing biomass development in dependence on forest management, I guess it is worthwhile publishing the respective components specifically for LPJ, in particular since the now available options are quite comprehensive and the question addressed is still not well investigated. In my opinion, the presentation is well structured and the language is fluent (some spelling errors may occur that are not specifically pinpointed but this can easily be corrected). Some specific recommendations related to references, figures and issues that might be better addressed are given below.

Two main concerns remain that may require some more elaboration. Firstly, I am a bit irritated about the presentation of large-scale simulations that use the new features at a large scale without presenting any stand-level evaluation of managed sites in the paper. As it is, it is implicitly assumed that if the model represents PFTs under natural conditions correctly, it is also appropriately simulating the PFTs under management. Since it is known that growth depends on structural features (e.g. Bohn et al.) and this is what is changed here, this assumption might not hold, and thus should be demonstrated, at least in case of some examples. In fact, I think that lots of specific cases are available that could be used for such an exercise (e.g. sites in the Profound database). Please apologize in case such evaluations have been presented in other publications and I have overlooked the references.

With this respect, I am particular puzzled about the example simulation, where beech seems to perform the superior growth relative to spruce in the PNV (for the first 100 years) as well as (and particularly in) all managed forest options. This seems to be

counter-intuitive to what I would expect for a site in southern Sweden (see e.g. Bolte et al. 2010), although I am aware of only one extensive biomass study which gives almost the same biomass/production values for both species in this area (Nihlgard 1972). However, although the particular conditions seem to be more representative to Poland than to Sweden (see e.g. Jagodizinki et al. 2020), I might misjudge the growth and will happily accept if I am proven wrong. In any case, I think that particularly (the) illustrating example(s) need some data or references that support their realism.

Second, it is unclear why old forests (one third of the area) are considered as PNV despite it is acknowledged by the authors that this leads to a simulated species composition widely divergent from the actual forests. It also leads to total forest estimates of carbon in the different compartments that are heavily influenced from this (unrealistic?) assumption. I guess that considering even a species composition from coarse data as is available from literature would largely decrease uncertainties related to the inventory as it is now.

Specific comments

L17/18: How is the underestimation of carbon sink explained if stock and net growth is fairly well met?

L29: Morin et al. 2017 not in ref. Do you mean Morin et al. 2018?

L41: Are the Pan et al. data really providing below- and aboveground carbon stocks for Europe? The numbers given here don't seem to match the same values indicated for the same source in Table 2. Estimates from other sources seem to be also far different as e.g. app. 5/5 (Liski et al.), 9/14 (Goodale et al.), and 9/8 PgC (Pilli et al.). Please check. Consider to introduce the given references into Table 2-4 if appropriate.

L69ff: It is not clear, what are newly developed features that justify this publication and what are features that are only described in order to understand the scenarios modelled. Please rephrase and better indicate the expected benefit of the model improvement. (In other words: clarify the objective.)

L76ff: What is (are) the timestep(s) of the model?

L132ff: For the within-patch mixture of Fig.4a, the 60/40 ratio refers to while for the among-stand-types mixture of Fig. 4b the 60/40 ratio refers to groundcover area. I think that this should be indicated when presenting the different options.

L163: define LUH2

L192/Tab. 1: consider changing 'N fertilization' to 'N fertilization/deposition' since the continuous kind of application looks more similar to a deposition regime (and is more likely to happen anyway).

L205/Fig. 5: This is a nice presentation indicating the potential options that can be selected. However, I wonder what happens, if 'old' would be selected together with 'small' or vice versa in the thinning rule options? What would happen? Wouldn't be a preference 'from above' or 'from below' be more suitable? What effect does this have on the simulation if biomass is taken e.g. preferentially from larger trees? I guess that the effect would depend on underlying assumptions about the biomass in different size groups – what are these assumptions?

L215ff: I am a bit unhappy with the unit 'densmax' being trees per ha since it implies that the stand is harvested after a certain number of trees has reached. However, the number is actually not defined and can be very different dependent on the diameter distribution. Densmax thus is an arbitrary density value in 1/m2 or similar unit. Similarly, I guess that 'dens' is also not indicating real tree numbers.

L232/233: As indicated in Tab. 1, irrigation seems to simply bypass drought stress. Here it is said that you actually calculate the (minimum) amount of water that is necessary to do this. Is this true? Check and homogenize.

L308: harvested volume was calculated from 'killed vegetation carbon'? consider rewording.

L311: What does 'reductions in wood products and residuals' mean? Do you mean 'due to' instead of 'in'? Consider that the argument is repeated a couple of lines later.

L330-332: irritating punctuation (needs ":" after forest, and "," after pubescens)

L486: The simulations are initialized and driven by the given information but not 'constrained', correct? The word would be correct, if repeated inventory data would be

used to parameterize or adjust the simulations which I think has not been done.

L494: As I take it, there are no observations but only estimates of 'mean growing stock' that are based on different information, sometimes on rather inhomogeneous sources, correct?

L526: You mean that the NPP decline that ORCHIDEE simulates is not simulated by LPJ, which might have various reasons that are caused by the model structure and processes. You don't mean that the decline is a function as such that is 'included' or not 'included', correct? Could such a decline be related to nutrient depletion? How is the effect of nutrient export – that is a main concern of a sustainable management – considered anyway? I think it should at least be part of the discussion (see e.g. Parolari and Porporato; Sverdrup et al.)

L645: I don't see the usefulness of this plot. The development of stand density in dependence on automated or self-thinning seems to be the same for all countries as is not surprising since it is treated with the same functions. So what would you like to show here?

Mentioned references

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