Geoscientific
Model Development
Discussions

Open Access

EGU

Interactive comment on "Energy, water and carbon exchanges in managed forest ecosystems: description, sensitivity analysis and evaluation of the INRAE GO+ model, version 3.0" *by* Virginie Moreaux et al.

Anonymous Referee #2

Received and published: 4 July 2020

The paper "Energy, water and carbon exchanges in managed forest ecosystems: description, sensitivity analysis and evaluation of the INRAE GO+ model, version 3.0" provides a comprehensive model description and results of additional model tests. It is a well written and well structured contribution and will be a solid base for future model applications and model users. One point of critique is a tendency for over-enthusiasm regarding the model. This does not support the understanding of the material and is not necessary in a modeling journal. For instance, in many instances the text reads (almost) as if trees were explicitly modelled ("... among individual trees ..."), which is

not the case. Similarly, phrases such as "each canopy layer..." are a bit misleading, as there is exactly one tree canopy layer (and an understorey layer) in the model. Also the introduction overstretches a bit (in my opinion), e.g. when hinting at the applicabilty at global level, while GO+ is (currently) applicable for a subset of the temperate biome (evenaged, mono-specific, managed). I would suggest to tone down the marketing language throughout the paper - the model is great and can speak for itself. Another major point is the "verifcation" section of the paper. In my point of view a technical test to show that a model is not leaking (or creating) energy or matter is such a basic step in model development that it does not merit a section on its own. Of course I might be wrong here - and reading about substantial differences (9%) is an indication that I am wrong here. Please explain better why such a test is (a) non-trivial and (b) avoid to create the impression that empirical data was involved in this test (in addition to initial conditions; see previous point). Some clarifications would also help the reader to understand the model. One is the spatial dimension which is never really mentioned; Is there a conceptual stand area for which the model designed? This may also help to better understand the management options available in the model. A second point is that the response functions (e.g., f_SW to calculate stomatal conductance) are not described (page 9). Later (page 24), it sounds as if those functions are specifically developed for each species, which would be an approach that is hard to scale up to many more species. Please include this information early in the manuscript.

A more minor point is that oftentimes abbreviations - particularly in figure captions - are not explained. Moreover, the paper is quite long already and the authors should consider shortening here and there (some suggestions in the details).

All in all I think the paper is an excellent piece of work that fits very well in the journal. The evaluation exercises are nicely executed and highlight results for different processes at different time scales. The publication of the model source is clean (github) and the use of publicly avaialable data sets (e.g., ISI-MIP) is another plus.

Details: page 3, line 4: better explain the mechanisms (sensible heat flux)

p3L18ff: you do not consider the life cycle of wood products. Your model can provide data that could allow such an analysis....

p6L1: "releasing model calculations...": nicely put!

p7l5: "latter" instead of "later"

p9l1: "is taken" ... "extended *the* use of Eqs"

p9l13: please be a bit mor

p10l5ff: the concept of dynamic layer dimensions is an interesting approach!

p11l19: extra space after "presented here"

p13: please make clear what "indivdual trees" are and what not

p16l17ff: you state that you do not model tree mortality; what about regeneration? Is this also limited to managed forests (with planting)? You could mention this here (growth/mortality/regeneration as the major demographic processes). And what is the "carbon balance of individual trees"???

p17l27: double ".."

p19l10: it is not clear what the "number of trees felled" means. How does the number of trees change in the model? By management (thinning, final cuts), right? How is that related to the mortality of trees? You see, I am confused.

p20 Fig5: please add explanations for the pools (DPM, RPM, ...) in the caption. There is a stray label ("L2") in the figure. Captions (e.g. "Long", "Beech" are way too big - or the other text labels are way too small.

p22l12: maybe use a different term instead of "verification". Maybe something like "testing conservation principles"?

p26/p27: in my opinion the discussion of results of the sensitivity analysis is too long and too verbose. It is meaningful to have such an analysis, but it is just limited what

can be derived from such a general +-10% approach.

p27Figure7: spell out abbreviations or refer to some table.

p30l1: How did you select the SDs of the parameters?

p33l30: "annual increase" instead of "time derivative".

p34Table 5: Can you add observed/predicted values here?

p35 Table 6: remove "continued"

p35l10: the sentence is duplicated ("The random errors...")

p36 Table 7: Is this analysis useful? It does basically say that the model is able to discern between summer and winter. I think this is a candidate for shortening the paper

p39l17: Remove the "the" after GO+

p39L22: confused again. How do you simulate the dynamics of size distribution??

p40L1: not true. There are gazillions of forest management practices that are not covered. What about single tree selection approaches or other spatial explicit small scale interventions? What about everything related to mixed forests?

p40l28: "Model performance" either remove or create paragraphs in the discussion

p45: Leaf area is m2/tree, not m-2/tree. same for BA. LAI is m2/m2, not m-2/m2. Stem volume is m3, stocking densitiy is stems... this page is a bit messed up.