Final response to referee comments

Anonymous Referee #1

Received and published: 30 June 2019

1. The authors present a new forest growth model HETEROFOR, which is a process-based model including comprehensive ecosystem and eco-physiological processes. As the title indicates, the inclusion of the competition of light for photosynthesis and structure growth between individual trees is one of the main contributions of this model. This manuscript focuses on the carbon budget and growth parts of the model. The processes behind them are well described in detail. The model was tested against inventory data and could predict the growth of trees very well. The authors further demonstrated the scenario simulations of future climate change using the model. In general, this is a good manuscript and suitable for publication here.

Author response (R):

Thank you for these encouragements!

2. In many places throughout the manuscript, the authors mentioned that there exists very few spatially-explicit forest growth models (e.g. P4L22 "Given the lack of process-based models with detailed spatial representation: : :"). That's one of the motivations for developing this new HETEROFOR model. However, to my knowledge there are several excellent individual-based models owning this functionality, e.g. the iLAND model (http://iland.boku.ac.at/startpage) and the FORMIND model (http://formind.org/model/). The authors may want to survey the published models again and renew the manuscript.

R:

There are indeed several spatially explicit process-based models but some models account only for the radiation absorption (such as FORMIND) or describe the processes with an intermediate level of detail and simplified eco-physiological concepts such as the radiation use efficiency approach (e.g., iLAND). The FORMIND model was considered in the review of Pretzsch et al. (2015) but not retained according to our criteria (water balance and nutrient budget not accounted for). Regarding iLand, we added a sentence in the introduction to explain its specificity (description of the process at an intermediate level of detail in order to simulate forest dynamics at the landscape scale) and to explain the differences with our approach. We also rephrased some sentences such as that mentioned by the reviewer to be less categorical.

3. In the last two paragraphs of the introduction, the scope as well as the strengths of the new HETEROFOR model are stated: it uses ray-tracing approach, hourly time step for calculating photosynthesis and transpiration, complex water balance module, detailed nutrient cycling module, and the CAPSIS platform. It would therefore be exciting for the readers, to explore these strengths by reading this paper. My major concern to this manuscript comes from this point: for what reason should the description of the whole model be separated into two (or more?) papers? I (and the readers) would like to know the main advances of this new model at once, instead of first knowing the carbon budget and growth parts, and waiting for the rest to come up later. The publication of a new and complex forest growth model has often been done in series papers, e.g. (Paper 1): full description of the model; (Paper 2) Verification, validation, and sensitivity analysis of the model; and (Paper 3) Application of the model. In the current manuscript of HETEROFOR, the general structure of the model is given in section 2.1 "Overall operation of the HETEROFOR model". However, the detailed description of water budget and nutrient cycling, and more importantly, their coupling to

carbon budget are lacking. The RCP scenario analysis of the forest growth was performed and presented in the last part of this manuscript. This has been well done and the potential of the HETEROFOR model is well demonstrated. However, due to the lack of detailed model description on water and nutrient modules, we are actually not able to comment on the outcome of this scenario simulations, or, even to give fair comments on the simulation results of individual radial growth and size-growth relationships.

R:

We have long thought about the best way to present the HETEROFOR model. Given its complexity, it seemed to us that it was better to present separately the carbon-related processes from the water balance module. Therefore, I submitted the first paper on carbon flux and tree dimensional growth and my PhD student (Louis de Wergifosse) submitted the second paper. Both papers are under review in GMD and are already available as discussion paper which gives the opportunity to the reader to better understand the functioning of the water balance module. Given the length of the papers (49 equations in the first one and 83 in the second) and the fact that the second paper is part of the thesis of Louis de Wergifosse, it is not anymore possible for us to merge them in the same paper. Regarding the nutrient budget module, the implementation is not yet finalised and some tests must be performed. Given the complexity of the processes at play, it will require an article on its own. However, the model is fully functional without activating this module. It was therefore not considered in the scenario analysis.

4. If the manuscript should still be kept in its current scope, the authors may want to provide more information in section 2.1., including (a) the spatial resolution of the soil chemistry; (b) how the phenological periods are coupled to the ecosystem processes; (c) how does the stand-scale evaporation calculated by the Penman-Monteith method is distributed between soil, bark, and foliage. And how do the latter two compartments are further distributed to individual trees, such that the tree-scale throughfall and stemflow could be calculated? (d) how growth will be exactly regulated by the nutrient cycling? (e) Figure 1 should give an overview of the complete model, including the water and nutrient modules

R:

In the revised version of the manuscript, we provided the spatial resolution for the soil chemistry. We better explained how phenology interacts with the other processes and how the evaporation is distributed between soil, bark, and foliage based on the absorbed solar radiation by each ecosystem compartment. As bark and foliage are calculated at the stand scale as well as throughfall and stemflow, they are not distributed to individual trees. We did not describe with more details how tree growth is regulated by nutrient availability since the complexity of the nutrient cycling and tree nutrition module really disserves a paper on its own. Accordingly, we have integrated the phenology and water balance in Figure 1 but not the nutrient cycling in order to keep it readable. In the paper of de Wergifosse et al. currently in discussion in GMD, a figure describing the water balance module with more details is provided.

5. P12L21: The HETEROFOR model takes into account 5 nutrients (N, P, K, Mg, Ca, in descending importance) in calculating the allocation of carbon in fine roots. How does the model deal with the weighting of the 5 nutrients?

R:

For each nutrient, a fine root to foliage ratio is calculated and the maximum is retained in order to account for the fact that the most limiting nutrient has the dominant effect. In the revised manuscript, we have completed our description to make this point clearer.

6. In section 3.1, the npp of individual trees is compared with the modeled gpp. Please describe the method of calculating npp from the inventory data. On the other hand, why not directly compare the derived npp with the modeled npp?

R:

The method used to calculate npp from the inventory data is described in section 2.2.7 on growth reconstruction. In the revised version of the manuscript, we provided more details on this method in order to make it perfectly clear for the reader.

To transform the modelled gpp into npp, we used either a ratio or the routine for respiration calculation. In both cases, we used a parameter fitted based on tree growth data. In order to keep the two variables completely independent, we did not transform the modelled gpp into npp.

7. When discussing the performance of calculating npp from gpp (P24), the authors focused only on the maintenance respiration. How about growth respiration?

R:

In HETEROFOR, the growth respiration is estimated as a proportion of the total biomass increment as in many other models (e.g., 3D-CMCC, CASTANEA). The growth respiration is therefore simpler to estimate than the maintenance respiration and the challenge for estimating growth respiration is to correctly estimate the biomass increment.

8. P26L29: the term “threshold” is used here and in Figure 4 and is defined as “the minimum girth for radial growth to occur”. I don’t think that it is a proper way of description. The radial growth is too small to be properly displayed in the figure. However, the small trees do grow with girth smaller than the threshold.

R:

We agree with the reviewer that trees smaller than the threshold still grow even if this growth is very limited. We redefined the threshold as the girth beyond which radial growth linearly increases with girth.

Some minor suggestions:

9. P3L6: propose -> proposed

10. P3L18-19: a stable systems -> a stable system

11. P3L21: short and long-term -> short- and long-term

12. P3L22: response -> responses

13. P3L33: horizontal dimension -> horizontal dimensions

14. P3L33: in both dimensions -> in three dimensions

15. P4L19: short -term -> short-term
16. P4L24: I am wondering if the authors are going to write: HETEROgeneous FORests?
17. P9L20: (The, 2006) -> (Teh, 2006)
18. P9L23: the extinction coefficient should be unitless
19. P17L27: LIEBL. -> Liebl.
20. P20L14: run with height different -> run with different
21. P23 Figure 5: use Sessile Oak instead of Common Oak for consistency
22. P29L4: NTOG 3D -> NOTG 3D
23. P44L28: Teh, C. -> Teh, C. B. S.

R:

All minor suggestions were accepted and implemented accordingly in the revised manuscript.

Anonymous Referee #2

Received and published: 9 July 2019

The manuscript by Jonard et al. presents a new spatially explicit forest growth model which aims to incorporate the structural and/or compositional complexity in simulate forest growth, and associated ecological, biogeochemical, ecohydrological, physiological processes. This manuscript focuses on the carbon fluxes and tree growth and validates the model performance against inventory measurements at an oak/beech forest.

The manuscript also demonstrates the potential applications by simulating forest growth based on several projected climate scenarios.

Overall, the manuscript is well written and carefully crafted. The study's objectives and scopes are also generally justified. Below I list my few general comments. I would recommend the manuscript to be published in the Geoscientific Model Development after addressing/considering my general comments.

[1] This is a somehow very complicated model in terms of the number of parameters and model structures, but the model validation really falls short. The model predictions are tested against very sparse observation (i.e., two-time inventory at a single forest stand, only a few types of measurements). Given that this is a new and complicated model; I would question whether such validation is sufficient and robust. Strictly speaking, the comparison between gpp and npp isn't a valid comparison. Also, several parameters used (e.g., Table 2) are fit against the measurements at this specific site. I'd urge the authors should make a stronger case about the model robustness by considering a couple of options, e.g., test against more than one single sites, sites with different structure compositions, or multiple types of observations (e.g., those intermediate variables like respiration, leaf area, biomass). For observations that may be unavailable at the moment, the manuscript should at least point out the critical variables/ parameters that need future data collection.

R:

Individual gpp is obtained with the photosynthesis routine of CASTANEA based on the intercepted radiation while the reconstructed npp is obtained from the observed increment in dbh and height. The tree growth measurements are just used to calculate the reconstructed npp. For the gpp, the CASTANEA model was calibrated independently on other sites. In conclusion, we consider that the two variables are independent.

The first objective of the paper was to present a description of the model and secondary to make a first evaluation of the model using an uneven-aged and mixed stand. We are well aware that this first evaluation is insufficient and we are preparing a larger one at the European level using ICP forest level II plots (cf. Conclusion and future prospects). For the present article, we will expand this first evaluation to 3 stands covering contrasted stand structures and compositions. For this first evaluation, we will use only the tree growth measurements. However, the companion paper which focus on phenology and water cycle (de Wergifosse et al., in review in GMD) is using phenological data as well as throughfall, stemflow and soil water content measurements for the model evaluation. For our evaluation at the European scale, we could eventually also use litter fall data and eddy covariance data on some sites of the ICOS program.

[2] I suggest considering removing or revising the parts of simulating forest growths based on projected climate scenarios. 1) The current model validation (as pointed out above) doesn't test the

extrapolation capability of the model, e.g., either in time or under different climatic conditions. If the authors intend to keep the simulation part, they should consider/discuss those aspects in model validation. 2) For this manuscript, I think it may be a better idea to use simulations to demonstrate the capability or powerfulness of this model in simulating the spatially-explicit forest growths, e.g., simulations on forests with a different degree of heterogeneity in compositions/structures. I think it may help elaborate the reasons of why we need such type of model.

R:

In the revised version of the manuscript, we have evaluated HETEROFOR in 3 stands showing contrasted species composition and stand structures, which is in line with the second suggestion of the reviewer. For the part with the simulations based on projected climate scenarios, we agree to remove it if necessary. However, we suggest to keep it as we think this is a good illustration of the potential of HETEROFOR (see comment n°3 of reviewer 1). Now that the companion paper is also accessible in GMD, the reader has all the information to interpret it. Indeed, this second paper describes processes that are sensitive to climate conditions (phenology, water balance) and evaluates them against observations.

Anonymous Referee #3

Received and published: 3 August 2019

This piece is sophisticated and well written. It may be publishable in GMD with some moderate revisions. However, the proposed model is not free and this gets on my nerves : : : a little bit. Open access has been the prevailing trend in academia and is good for science. There are many free codes available. If HETEROFOR is not free of charge, I am not sure the point of getting this piece published. I urge the authors considering to release the codes for the public.

R:

HETEROFOR is actually free and we decided to distribute it freely. The misunderstanding probably comes from our wording which was not clear enough and from the fact that we did not choose any specific license. In CAPSIS, the models have by default no license and the rights belong to the model authors. In order to further clarify it, we decided to adopt a LGPL license for HETEROFOR.

We have now clarified the wording regarding the code availability section and mentioned that a LGPL license was adopted for HETEROFOR.

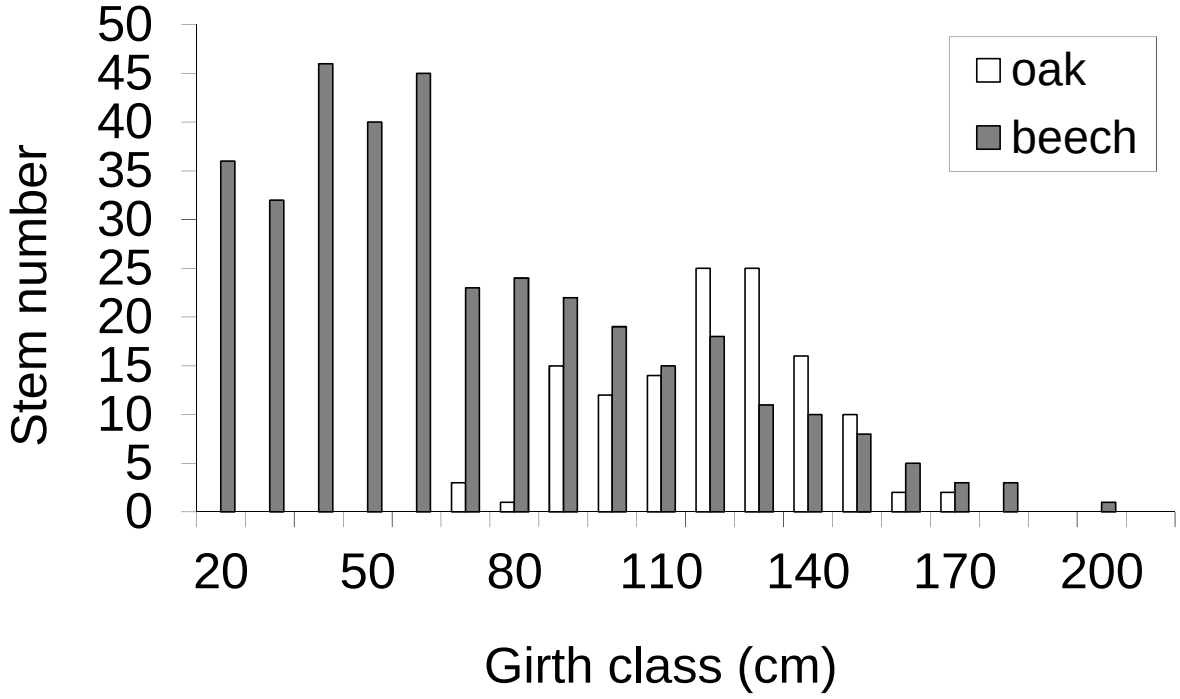
My main comment is on the name of the model HETEROFOR, since I am not quite sure if the validation data is heterogeneous enough (only 2 broadleaf species). The site was pretty homogenous to me comparing to the canopies in the tropical region. In addition, how representative is the validation site and data?

R:

Compared to forest in tropical region, the mixed broadleaved stand is indeed less heterogeneous. For temperate forests, it is however already quite heterogeneous (especially when compared to monospecific even-aged stands). In this stand, part of the heterogeneity comes from the size class distribution (see below).

The idea was just to make a first evaluation of the model performances using a mixed and uneven-aged stand of oak and beech. A broader evaluation using stands from the whole Europe is currently done and the results will be available later.

Nevertheless, we decided to expand this first evaluation and use 3 stands with contrasted stand structure and composition.



Here are my specific comments:

The abstract is a little bit disjointed. More information should be provided to clarify the sentences such as: Why the models called HETEROFOR and CAPSIS (acronyms for what), and how well is the radical growth prediction? Also, did you mention the effects of thinning in the abstract?

R:

In the revised version of the abstract, we defined the acronyms HETEROFOR (*HETEROgeneous FORest*) and CAPSIS (*Computer-Aided Projection of Strategies In Silviculture)* and provided the Pearson's correlation between observed and predicted radial growth. We did not mention the effects of thinning since we did not test it in this paper.

P2L19-20: These are not news and we know these all along. Why we need HETEROFOR?

R:

We removed this sentence from the manuscript and explained later in the introduction the specificity of HETEROFOR.

P3L14: "To explore forest response to new silvicultural practices : : :" Did you do that in this paper?

R:

No, because the manuscript is focused on the model description but we developed HETEROFOR with this goal and are currently using HETEROFOR in this way in several projects.

P5L12-13: "As the whole model could not : : :" Why is that? Please elaborate on it.

R:

This sentence was removed from the introduction and the explanation regarding the model description is given at the end of section 2.1.

P10L30: I am not sure about eq. 6. Why the NPP/GPP ratio depends on the crown to stem diameter ratio?

R:

We realized that, for some tree species, the NPP/GPP ratio varies with tree characteristics and observed a clear effect of the crown to stem diameter ratio regarding oak whose crown development strongly depends on the stand density (and past silvicultural treatment). This possibility was therefore integrated in the model. However, if the user does not want to use it, the corresponding parameter (β) can be fixed to 0 (Eq. 8).

P17L12-13: "Tree mortality occurs when trees reach a defoliation of 90%, considering that a tree with less than 10% of its leaves will never recover." Any reference for the statement?

R:

In the revised version of the paper, we rephrased it "…considering that a tree with less than 10% of its leaves is in an advanced stage of decline and is unlikely to recover (Manion, 1981)." and we added a reference.

Manion, P.D., 1981. Tree Disease Concepts. 1st Edn. Prentice-Hall, Englewood Cliffs, NJ, 402 pp.

P17L25-26: More detailed geographic and topographic information should be provided.

R:

We added in the revised version that the stands are located on a tableland. We also provided the geographic coordinates, the altitude and the region in which the stands are located.

P18L13: Please justify the use of the Wallonia data.

R:

We used data collected in the ICP Forests level II plots of Wallonia only for fitting the parameters of the fruit production equation. We chose the plots from Wallonia to have the same ecological conditions than the study site and since we had access to this data. However, the user can provide a file with fruit production data that will be used by the model to adapt the parameters of the fruit production equation.

P18L16: If the mean temperature of the site is 8 degrees C, why you used 15?

R:

15°C is the reference temperature at which the parameter is evaluated (the maintenance respiration per g of N at 15°C) but, in Eq. 11, the temperature effect is taken into account which enables to calculate the maintenance respiration at all temperatures.

P20L6: Statistics to show no difference between the intercepts?

R:

In Figure 2, the values provided between parentheses are half the confidence intervals. Since the confidence intervals of the intercepts overlap, we considered that the difference was not significant.

P23L10: CASTANEA

R: OK

P46: Table 1 is not indexed in the ms.

R: OK