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Interactive comment on "HETEROFOR 1.0: a spatially explicit model for exploring the response of structurally complex forests to uncertain future conditions. II. Phenology and water cycle" *by* Louis de Wergifosse et al.

Anonymous Referee #1

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I was curious to learn about new routines for phenology and water balances that can be used with a single-tree physiologically based model. However, I am quite disappointed because what is presented is not very well connected to the specific issues that should be addressed with the new spatially explicit model. For example, it should be crucial for a model which is particularly designed to represent the competition between individual trees under changing environmental conditions, to capture the differences in budburst or drought stress between understorey and dominant trees. I feel that this hasn't been addressed adequately, neither theoretical (neglecting several issues that are relevant

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for individual modelling, not differentiating current model approaches appropriately, and lacking many relevant references) nor practically (i.e. without evaluation of individual transpiration rates, simulation of soil water development seems not very indicative).

The paper is quite long but not always stringent and to the point. If I understood it correctly, phenology is calculated species-specifically but with uniform drivers, while water balance is considering individual flows but not the spatial heterogeneity of water availability in the soil. So where is the benefit in comparison to cohort-based approaches? The evaluation of the water balance is done with averaged results from the literature where the influence of a structural component is not visible or by integrated values from the same sites that are (partially) also used for parametrization. So not even the impact of the differently structured forests that are described here has been evaluated. Furthermore, a sensitivity analysis, e.g. to explore the effect of other potential stand structures, has not been carried out. Therefore, I cannot say that the overall benefit of the new model has been sufficiently demonstrated.

More specific comments

Abstract:

It is a bit surprising to me that no reference to the specific demands on phenology and water balance regarding the spatial differentiation is made in the abstract. Also, the good results that the evaluation seems to provide cannot be judged if it is not indicated what kind of forests have been investigated. In particular, I would expect that the structure of the evaluation sites and not only the number is highlighted. Also, throughfall and average soil water development might not be the most important processes to be judged in a model that is designed to represent heterogeneous forest conditions. What about individual transpiration as could be determined by sapflux measurements?

Introduction:

It is of course an important motivation for the development of new models to better

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judge the impacts of climate change. However, although the list of references about potential changes that are going to happen to forests in Europe in general is rather long, a reference about the impact of extreme weather events (which would be smaller in mixed forests) seems to be missing (e.g. see Kornhuber et al. 2019). You also might consider that the references about phenology as well as tree mortality are a bit outdated and should be replaced or complemented by newer ones (e.g. Piao et al. 2019, Waldau et al. 2018 and Klein et al. 2019, Etzold et al. 2019, Greenwood et al. 2017, respectively). Also, possible negative developments related to climate changes need to be mentioned (e.g. Liu et al. 2018) and the need to develop more mixed and structured forests should be much more emphasized (e.g. Rasche et al. 2013).

Overall, the introduction is rather lengthy with regard to general issues while quite short when it comes to reasoning about the importance of how individual phenology and water conditions drive the competition within a mixed and structured forest (see e.g. Jolly et al. 2004, Grote et al. 2016, Schaefer et al. 2018). In addition, please check English in places (e.g. page 4, line 3 or page 5 lines 11ff) and if the given references actually support what you want to say (i.e. is Fontes et al. 2010 really requesting a spatially differentiated modelling approach?).

Model description:

The phenological models include chilling and warming influences (any other as indicated by 'mainly' in line 31, page 7??) and comparisons are always interesting. With this respect, I wonder if the respective temperature is the air temperature as read from the input files or if the temperature is somehow processed before used here (considering e.g. a partial canopy cover that changes the temperature for the smaller trees). This is important because using the same temperature for understory and overstory trees is bound to lead to mismatches in phenology in structured forests. I was also a bit surprised about the reasoning that three models are necessary to be applied for specific sites or regions. Since the model has been designed to investigate climate change impacts – how can you ever know which model will work best on these new

conditions? In other words: If you are asked to select a phenological model for future conditions in northern regions, would you apply one that now works best in the North or one that now works best under the expected climate conditions – which are now in more southern regions? There certainly should be more deterministic criteria for model selection – otherwise a choice of models only increases the uncertainty connected to future model investigations. Since only one of these models has been used in the evaluation, I would advice to frame it so that this is the standard module for the model but that others (such as. . .) can be added (and shorten the description). Alternatively, all three models should be applied and results shown (at least in a supplement).

The water balance model looks quite complicated with many equations that do not necessarily be indicated as an equation. Therefore, I would like to see a much better scheme of flows than indicated in Fig. 1. The model seems to calculate interception and evaporation from the bark in unusual detail – although I guess that the estimation of bark surface and the interception of water at this surface is rather uncertain. I wonder if this can be justified by the size of the flux and the sensitivity of water balance fluxes to this compartment's properties? Furthermore, I think the ground/ soil vegetation evaporation needs a better description. As far as I understand, ground vegetation LAI dynamics are not part of this model (which is a pity) and are derived from 'ecosystem LAI' which needs to be given by the user (correct?). Doesn't this lead to quite some large errors due to ground vegetation being much more abundant during the early spring (e.g. Schulze et al. 2009)? Finally, water content in different soil layers seems to be calculated on a stand basis only, which assumes that water is sufficiently fast transported from places where transpiration is low and/or stemflow is high to sites with high water uptake and less input (this assumption should explicitly be stated). What I couldn't find is that the water uptake of smaller trees with less or no roots in deeper soils should be restricted to the upper soil layers. Did I miss this somehow?

Further Questions are: What is the difference between 'leaf biomass' and 'green leaves' (page 6, line 14)? How is it decided if maintenance respiration is calculated

as a fraction of gpp or as a separate process (page 6, line 25ff) and what are 'structural components' (stem, branches, coarse roots, bark? What about reserves and fruits? (page 6, line 31) – which are questions that are also not answered in Jonard et al. in review (at least not in the present form of the manuscript). I also wonder, if 'runoff is not included' means that percolation rates are bound to be unreasonable large at times or if there is an unreasonable large surface water pool if the rainfall is high. Why does the bark water storage depend on the state of the leaves? In the calculation of throughfall as difference between rainfall and stemflow (eq. 21) the canopy interception seems to be missing. Is the radiation used in eq. 29 the same as calculated in eq. 23 and 24? Please explicitly refer to eq. 29 and that ra and rs are estimated as 1/ga (gs), similarly to rs_foliage in eq. 53. How can the leaf width be fixed to be 4cm for all species reaching e.g. from Juglans to Salix? How can pre-stemflow be independent from tree species (page 16, line 29) if it is calculated for species- and individual-specific stemflow; and why is it calculated at all (being a part of the bark-intercepted water pool that is available for evaporation independent of its particular location)? Are there any references for the derivation of the different modifiers for gs_foliage (if not, a separate derivation based on data needs to be presented)?

In addition, I would recommend that names of parameters used and listed in table 1 are exactly the same (with/without sp indicator, y small or capital). Also, there shouldn't be parameters with the same name even in different equations (e.g. a, b, c, d in eqs. 4 and 12, 13, 16, . . .). If parameters are derived from literature data rather than directly taken from literature, the derivation should be shown (if only in a supplement). It is also strange, that the evaluation is carried out with stands that consist of three species, while parameters in table 1 indicate only two species. Where is the third? The derivation of some other variables is not explained either (e.g. BAI). Also check, if the range of the respective equation is valid, e.g. in eq. 5 it looks that RId can get larger than 1 which would result in leafProp values larger than 1 which is not possible based on what has been said before. Again, English needs to be checked in places (e.g. page 7/line 4, page 10/line 16, page 8/line 11, . . .) and sentences have to be evaluated for their logic

(e.g. how can the calculated leaf biomass 'proportion' (of what?) allow to 'predict' the seasonal foliage development of individuals (a development predicted by a single mass??); a general model does not calculate a term only at a 'this moment'; ...). In fact, I couldn't at all understand what is meant by the paragraph at page 8 lines 27ff.

Results:

None of these results couldn't have been produced with a plantation model. Why isn't there at least a differentiation by species for the phenological evaluation? The throughfall data and water content might be used for a minimum sensitivity test if measurements would have been compared using for example a) full individual data, b) no species differentiation, c) or no dimensional differentiation in order to demonstrate that some benefit arrives from the proposed model.

Discussion:

As the authors say, there are not many models that calculate carbon- and water fluxes and pool sizes with physiologically-based principles on the individual scale. A gradient might be indicated that starts with highly structured individual models (e.g. LIGNUM, Perttunen et al. 1988) which are too computationally expensive and would need too much spatially distributed boundary conditions to be used at the stand scale. Other models can be applied on stands but at the expense of a rough representation of physiological processes (e.g. SORTIE, Coates et al. 2003) or a neglect of some part of the full balances, i.e. the soil carbon processes (e.g. MAESPA but also FORMIND, Köhler and Huth 1998). Cohort-based models represent another compromise because horizontal differentiation is simplified (apart from 4C and ANAFORE there is also LandscapeDNDC-PSIM, Grote et al. 2011). The usefulness of such approaches has been discussed in reviews (e.g. Pacala et al. 1995, Berger et al. 2008, Bravo et al. 2019) and the role of a differentiated phenology (e.g. Gressler et al. 2015) as well as water balance (Roetzer et al. 2017) for individual competition and stand development has been demonstrated. I think that these approaches that all try to better account

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for stand-structural issues should be separated from individual empirical approaches (e.g. FOREST) on the one hand and homogeneous physiological models (GOTILWA, CASTANEA) on the other. This differentiation, pros and cons are not nearly reflected in Table 5. In the current discussion, the model results are only judged by the closeness to the averaged measurements. There is no differentiation even between different species and no discussion about the effect of different sizes or particular positions of trees (see e.g. Wesolowski et al. 2006, Simioni et al. ...). This all doesn't seem related to the particular emphasize of the model. Also, potential improvements that could well be related to individual stress conditions (e.g. locally different water availability due to stemflow pattern or rooting depth) – eventually combining phenology and water balance issues (e.g. Sanz-Perez and Castro-Diez 2010, Xie et al. 2018) - are not mentioned.

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