Geosci. Model Dev. Discuss., 8, C2090–C2093, 2015
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Geoscientific Model Development Discussions
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Interactive comment on "Validation of 3D-CMCC Forest Ecosystem Model (v.5.1) against eddy covariance data for ten European forest sites" *by* A. Collalti et al.

Anonymous Referee #1

Received and published: 18 September 2015

This paper presents the modifications brought to the 3D-CMCC ecosystem model that simulates growth of heterogeneous forest ecosystems with the representation of different species, several vertical layers and age classes. The representation of vertical and horizontal heterogeneity is still very rare in most ecological models and this approach is needed within the community. In this study the authors evaluate the model's ability to simulate GPP at site level by comparing the simulations to FLUXnet-derived GPP data with a focus on the time scale for data aggregation, and level of details in the initialization of the model. The paper is well written, however the split between results and discussion makes the 2nd part of the paper more difficult to follow through. Some important aspects of this type of work however seem to be lacking and are described

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Interactive Discussion
Discussion Paper

below.

Improvements to the model

From the text, the reader is expecting to see the effects of the changes implemented in the version 5.1 of 3D-CMCC FEM: new carbon allocation, phenological, and autotrophic respiration modules, a 3D-canopy representation, tree carbon-nitrogen allocation and water flows that differentiate 3D-CMCC v5.1 from its previous version. The equations of the modifications brought to the code are shown in the appendix without a lot of details. Moreover, in the description that is given, it is not clear what has been changed from the previous version of 3D-CMCC. For example, in the text, L136 & L176, the authors say that the phenology and the canopy representation have been improved, in appendix A, nowhere is explained the difference between the previous and the new implementation: L 721 'the C flux is still estimated by . . .'. The interest of the work described in this study as a model improvement would be more convincing if the modifications implemented were clearly shown in regards to the previous versions of each element. The authors should explain clearly for each of the improvement what was used in the previous model and how it has been changed. The validation would also strongly benefit from the comparison of the simulations with the previous versus the new version to see where the addition of details in processes actually improves the model simulations and where some more improvements are still needed.

Tuning

The tuning is only quickly mentioned and the paper lacks a clear description (that could be put in appendix) of the tuning process. Also, related to the previous comments, I could not understand if more tuning was done after the changes were implemented, and if so a list of the species-specific tuned parameters with the values in the previous version and in the new version is needed. The authors choose to adopt a generic parameterization for species-specific parameters in order to make the model applicable to different sites without prior site-specific calibration. They acknowledge on L453

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that this approach yields large uncertainties but do not give any element about how big this uncertainty is. It would be interesting to have an idea of the magnitude of this sensitivity/uncertainty, maybe using a Monte-Carlo approach to propagate parametric uncertainty into the simulations or testing the same simulations with site-specific calibrations to get a feeling of how much is lost through this approach, even if genericity clearly is a choice for future applications.

Validation

The validation of the model is the heart of the paper. The authors try to be exhaustive by using several performance indices, but this section lacks some crucial elements to be fully convincing. For example, many performance indices are calculated and shown but nowhere in the text it is explained or analyzed if they are consistent with each other, if not why and what is the difference in their meaning. In Fig 1, the succession of indices is even harder to follow because all indices do not go in the same direction, i.e. a good model will have a NRME close to 0 but a correlation coefficient close to 1. It would be very useful to find a way to express all indices so that they would all go in the same direction, at least visually on Fig. 1. Also, plotting the daily and monthly indices on the same graphs with different colors would ease the comparison. For interannual variability, I found that the bar plots are hard to read. Since it is an evolution with a x-axis with years, maybe line plots would help the reader in understanding the results. Also, I regretted that the simulations with varying degrees of complexity in the model initialization were not shown along with the other figures.

The 3D-CMCC model represents carbon, (energy?) and water fluxes. GPP in itself cannot be sufficient to evaluate the relevance of a model in simulating the functioning of an ecosystem. GPP is highly intertwined with other variables and this only appears discretely in the paper when the water stress is suggested to explain the bad performance of the model at the Mediterranean site. It is important to show the outputs of the model as for water and energy variables. I don't know if water vapour variables are available for the 10 fluxnet sites used in the study but latent and sensible heat

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fluxes must be available. With such data, the validation of the model would result much stronger, else, the authors could still show the model's outputs for water and energy variables to try to explain the water problem at the Mediterranean site and give a less partial overview of the model's performances.

Specific comments & typos

L113 set of annual series

L123 answer the following

L496 as like as

L567 Del Pierre instead of Delpierre

In paragraph 2.1 several times maintenance is written mainteinance

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Interactive comment on Geosci. Model Dev. Discuss., 8, 6867, 2015.