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Interactive comment on "Monte Carlo based calibration and uncertainty analysis of a coupled plant growth and hydrological model" *by* T. Houska et al.

Anonymous Referee #3

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General comments

The authors performed a calibration and uncertainty analysis of the coupled CMF-PMF model using a Monte-Carlo approach. The topic fits very well into the scope of BGD, and the manuscript is well prepared. The text is well written, and I enjoyed to read the manuscript. The novelty of the presented 1D simulation runs is somewhat limited, because such model systems are available for more than 20 years, but I understand that a first step has to be done to parameterize such a new model framework. Technically the Monte-Carlo analysis was performed in an immaculate way. My major criticism, however, is that the plant model is behind the state-of-the-art. The model was calibrated for a winter cereal (winter wheat). The authors do not explicitly state that, but from

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reading the manuscript I got the impression that the crop model, besides senescence, does also not include vernalization. Simulating plant growth of winter wheat without vernalization and senescence is like simulating the hydrology of a catchment without considering snowfall in winter and capillary rise in summer. That are central plant physiological processes that must be captured by every crop model. At least, if the model is applied to a crop that has a vernalization requirement and is maturing at the end of the growing period. During the senescence phase of wheat (about 4 weeks) photosynthesis continuously declines and transpiration collapses. The crop stops growing, and assimilates are translocated from vegetative organs to grains. This period is highly dynamic and is crucial for simulating crop dynamics as well as the water fluxes, what should be also of interest for the hydrological model component. Such structural model deficiencies are in my view unacceptable and also not understandable. These two processes are-well understood and algorithms for including them into PMF are readily available from literature. That the authors come up at the end nevertheless with good a fit is a "nice" example for a good fit for the wrong reason. The final yield, for example, is not matched well because during senescence assimilates are translocated from vegetative to generative organs but due to a continuously ongoing photosynthesis during a period where in the real world green living leaves are absent. In my view, the authors must extend PMF for these two processes before this manuscript can be published. I would like to encourage the authors to revise the manuscript accordingly, because I am convinced that the concept to couple a 3D hydrological model with a process-based plant growth model has a high innovative potential.

Specific comments

p. 19512, line 11: You did not iterate simulation runs. You screened the hyper-dimensional parameter space for behavioral model runs. Please rewrite!

p. 19514, line: This sentences is misleading. At the first view one gets the impression that you used two years for calibration (1993-1994), but what you actually did is that you tested the model for the growing season 1993/1994. Please rewrite!

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p. 19514, line 25: Root, shoot etc. are not a physical component of the plant. Rewrite it as plant parts or organs.

p. 19516, line 1: Here it is unclear what you mean with “which interact on a numerical level”. Please rewrite!

p. 19516, line 5-25: How are the assimilates are partitioned between the plant organs. Are the partitioning factors tabulated or do you use a more sophisticated approach such as the functional balance theory? Please explain and include it in the model description. Sub-chapter 2.1.3: Here it remains vague how the dataflow between the models was organized in detail. A figure could help to make this point more clear here.

Subchapter 2.3: 1) I think this chapter would better fit at the very beginning of Chapter 2. 2) I wonder why the authors did not use any phenological data in their calibration. I read the publication of Wegehenkel (2000). There it is stated that besides the above-ground biomass also the phenology was determined. Calibrating the phenology is usually the very first step in the calibration of a crop model calibration and that would already heavily distill the parameter space. That may explain also why the parameter space of the phenological plant parameters was not well-constrained (see p. 19521, line 3-4). The dataset does not contain information on phenology, so what should constrain the parameter space with regard to the different temperature sums? If phenological data are available the authors must include them in the calibration. They are of central importance!

p. 19525, line 21: It would be better to introduce the cross-validation method already earlier in Chapter 2.

p. 19529, line 9: The growing season of winter wheat is from October until July. Please correct!

p. 19527, line 8: How was fertilization included in the crop growth model. Please explain?

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p. 19528, line 16: Please add “at least under similar soil and weather conditions”

Technical comments

Eq. (1): There is a typo. The minus sign must be replaced against a multiplication sign

p. 19520, line 1: Cancel “primarily”

p. 19520, line 1: Usually the texture fractions are give in percentage by weight and not volume. Please check!

p. 19520, line 5: Replace “climate data” against “weather data“.

Fig. 4 was not shown and plotted in my pdf.

Interactive comment on Biogeosciences Discuss., 10, 19509, 2013.