

## ***Interactive comment on “Influence of feedbacks from simulated crop growth on integrated regional hydrologic simulations under climate scenarios” by P. E. V. van Walsum***

**Anonymous Referee #3**

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Paper “Influence of feedbacks from simulated crop growth on integrated regional hydrologic simulations under climate scenarios”

General: The paper deals with the description and application of a modeling system consisting of a groundwater flow model (MODFLOW), a SVAT-Model (MetaSwap) and a crop growth model (WOFOST6.0). The paper focuses mainly on the application and description of the crop growth model (WOFOST6.0) with some modifications regarding the description of interception, evapotranspiration and crop water stress. In the study, the impacts of two different climate change scenarios on the simulation of crop growth

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and water balance with a so called static (without vegetation feedback) and a dynamic (vegetation feed back included) crop growth model are compared and discussed. The paper deals with an actual and very interesting scientific topic. However, from my point of view, the paper is sometimes difficult to read as a standalone publication in the actual state and should be restructured and rewritten in a major revision. For many relevant informations and arguments stated in the paper, the author refers to publications without any further explanation. In addition, from my point of view, relevant informations regarding e.g. Model description and Model set up for the simulation runs with the different climate change scenarios are either missing or the description is not detailed enough. Specific comments and suggestions for an improvement of the submitted paper are following.

Section 1 Introduction: Page 10154 line 7-17: This part should be moved to the section model description. Line 18-25: The author should focus more on the specific objectives of the paper and the expected benefits of the application of his modeling approach e.g. for a better simulation of the impact of climate change on agricultural flat and groundwater influenced areas.

Section 2 Methods and materials Section 2.1 Model Fig.1: A short description of the fluxes between the three different models (e.g. output groundwater recharge from MetaSwap is input and upper boundary condition for MODFLOW) would improve Fig.1 Information about the most important input data (not only that for WOFOST) for the modeling approach, the modeling time step (daily or hourly ?) and the modeling units (grids or hydrotopes ?) should be provided here. Furthermore, the description from page 10154, line 7-17 should be inserted here.

Page 10155, line 15 As far as I know, in the WOFOST-version described by Supit et al. (1994), potential transpiration, actual transpiration and interception evaporation are calculated inside the WOFOST-code, external inputs are potential evaporation from a free water surface, potential evaporation from a moist bare soil surface and potential transpiration from a crop canopy. Are these descriptions modifications of WOFOST

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described in the sections 2.1.1 and 2.1.2 ?

Section 2.1.1 In the actual state, only interception is described. I would like to recommend to reorganize this section in the following manner: -description of Makking approach for ETp and the crop factor KCMAK including the description of the impact of LAI on KCMAK . The description of the evaluation of the function LAI versus KCMAK at page 10160. Line 8-18 is not clear for me. A better version of this description should be inserted here. -after that the description of the split ETp into the single components. Page 10156, line 1-24: Obvsiously the approach of Valente et al. (1997) was designed for tree vegetation, How was this approach modified for the application for agricultural crops ? In addition, this approach should described in more detailed and concise way. E.G.: The meaning of Wfrac is not clear for me - is Wfrac a part of the modeling time step of one day ?

Section 2.1.2 Correct if I am wrong, but the implementation of EiC in equation (4) makes no sense for me. From my point of view, the ration Ta/Tp is the most essential one.

Section 2.1.3 Here the determination of the input parameters of the WOFOST-model and the parametrization of vegetation related parameters should be described in a more understandable and concise way. E.g. why potatoes for all areas with arable land and why fixed day 111 ? This fixation ignores the impact of a potential warmer climate on sowing. What means the 30yr-run ? Which data were used for that purpose ? Actual data from meteorological stations or generated data ? Again, it is not clear how the curves in Fig. 2 were evaluated.

Section 2.1.4 As the author claimed later in the paper, the estimation of the impact of CO2 on crop growth in his paper is one of the essential improvements in comparison with the other studies cited in the paper. In this case, I would have expected a short description of the approach.

Section 2.2-3.1 I would like to recommend that the author should describe the model

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set up in more detail including -initial conditions of simulations (soil water contents etc.) -meteorological input (which station ? which time period for control period ? which time period for climate change scenario ? which parameters ? ) -soil data (pF-data ? texture ?) - comparison of only two years 2003 vs. 2063 ? From my point of view, a comparison of only two selected single years for the estimation of the impact of climate change enables only a very limited analysis of this impact and ,therefore, only a limited analysis of the sensitivity of the static and dynamic vegetation model. Why not longer periods ?

Section 3.2 Fig. 8: It is difficult to distinguish between the different lines. Furthermore, I assume that CIW+-\_LuGr.Stat is CIW+Ãñ\_LuGr.Dyn ?. The author pointed out, that the dynamic model was applied with the option of hay making. This means that at certain days in the year or if above ground biomass or LAI is above a defined threshold value, grass is cutted and removed from the field and grass growth starts again. This option was obviously not used in the static model. Why ? This should be explained in more detail by the authors. In addition, LAI simulated by the dynamic model showed high values near 9, this is really high from my point of view. Are these typical conditions in the study region ?. Without a short description of the expected climate changes such e.g. higher temperatures in winter and longer drought periods in summer, the results in Fig. 8 are difficult to interpret (see also the comment for the use of only two years 2003 vs. 2063).

Section 3.3 Page 10167 line 11-25 and page 10168 line 1-23: Are these calculated differences up to 45 mm a-1 shown in Table 5 and Fig. 10-11 really significant or still in the uncertainty bounds of the model ?.

Section 4.1 I can only agree to the statement at page 10169 line 1-5 in the case of the impact of CO2 on crop growth. If there is a lack of other experimental data, the author should shortly summarize the parameters and assumptions, which cannot be validated until now.

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Section 4.2 The author cites only one study in this section. Is the limitation due to the application of WOFOST in the cited study or is there really only one study available ? I would like to encourage the author to carry out a more intensive literature review and to include the findings from other studies in the discussion of the results. This would substantiate the discussion about the findings in the recent study.

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