

## ***Interactive comment on “Capillary rise affecting crop yields under different environmental conditions” by Joop Kroes et al.***

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Received and published: 6 February 2017

Comment on major issue 1): The motivation behind this manuscript is not forthcoming. The authors fail to mention if other commonly used crop simulation models (e.g., Decision Support System for Agrotechnology Transfer or DSSAT) have the capability to simulate capillary rise or not. It seems like a very basic thing that should already be accounted for.

Our reply to major issue 1): The referee is right, we will extend the introduction with a paragraph on other similar models. We will make it clear that our modelling approach is a combination of a detailed soil hydrology model based on a solution of Richard's equation and a crop growth model. This combination is not common.

Comment on major issue 2): It seems that the second objective of the paper with

C1

regards to quantifying the impact of soil types on capillary rise has already been answered by previous work by Rijtema (1971). This is another reason that the authors need to clearly describe the differences with past studies and the motivation behind current work.

Our reply to major issue 2): Quantifying impact of soil types on capillary rise indeed has been carried out by others (Rijtema, 1971 as example) however these studies generally focus on capillary rise and do not explicitly include recirculation under free drainage conditions. We also calculate this contribution in a systematic way for 45 years and 72 soil types. Furthermore the link with crop growth and yield is seldom made.

Comment on major issue 3): The experimental and the modeling setup are not clear. The authors need to point out what input parameters were derived directly from the data, and what measurements were used for calibration versus validation for the model. It seems hard to believe that crop management parameters mentioned in Table 2 were the only model inputs for the coupled SWAP-WOFOST model.

Our reply to major issue 3): We will adjust Table 2 and combine this with Tables 1 and 3. Furthermore we will add a paragraph about calibration versus validation as part of a section about model setup.

Comment on major issue 4): Table 2: It is also not clear why 3 drought stress values were used for the first case study.

Our reply to major issue 4): The referee has a good point. These 3 values are part of the Feddes water stress-function This will be clarified in the text and in the subscript of the table.

Comment on major issue 5): Line 155: If the management factors seemed too high than expected after calibration, are the authors confident that they chose the right parameters for calibration or were some other processes missing that are not accounted

C2

for?

Our reply to major issue 5): The management factors are relatively high because the case studies have good management. It is very likely that we miss some processes, because our modelling approach has a physical base, but is still relatively simple. Some processes like pests and diseases are not included and may play a role in the field; the calibration was done on experimental farms where the impact from diseases and pests is minimal.

Comment on General concerns 1) 85-90: It seems weird that authors are talking about different boundary conditions in the introduction section. It is also not clear what the focus of Kroes and Supit (2011) study was or what their study entailed. This whole paragraph needs rewriting. Our reply to General concerns 1) We will do as suggested

Comment on General concerns 2) 125: Please mention that WOFOST is a crop growth simulation model.

Our reply to General concerns 2) We will do as suggested

Comment on General concerns 3) 130: Please define crop assimilation before using the term.

Our reply to General concerns 3) We will do as suggested

Comment on General concerns 4) Table 2: Please explain the term  $T_{pot}$ . Please do not use abbreviations like red. for reduction.

Our reply to General concerns 4) We will do as suggested

Comment on General concerns 5) I feel that this paper needs an editorial review as an improvement in writing will definitely make it easier to extract the scientific value of the paper. For example, see 85, 155, etc.

Our reply to General concerns 5) We agree with the Referee that several explanations are not very clear. We therefore agree on a strongly revised version. We will adapt the

C3

text and introduced a Figure to explain hydrological conditions in the model simulations. Part of this has been done in our comments to Referee #1, but we will extend this.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-598, 2016.

C4