

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

Anonymous Referee #3

Received and published: 28 January 2017

This paper describes the impact of capillary rise on crop yields at seven locations in the Netherlands. They use the integrated SWAP-WOFOST model to setup different boundary conditions to quantify capillary rise and leaching from different soil types. The manuscript concludes that the impact of capillary rise on crop growth and production is considerable and suggests that this impact should be accounted for in other crop growth simulation studies. In my view, the authors need to clarify a few details before the manuscript can be published.

My major concerns for the paper are: 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. 2. It seems that the second objective of the pa-

[Printer-friendly version](#)

[Discussion paper](#)



per with 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. 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. 4. Table 2: It is also not clear why 3 drought stress values were used for the first case study. 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 for?

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. 2. 125: Please mention that WOFOST is a crop growth simulation model. 3. 130: Please define crop assimilation before using the term. 4. Table 2: Please explain the term T_{pot} . Please do not use abbreviations like red. for reduction. 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.

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

Printer-friendly version

Discussion paper

