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Interactive Comment

Interactive comment on "Sensitivity and uncertainty in crop water footprint accounting: a case study for the Yellow River Basin" *by* L. Zhuo et al.

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The study is a valuable addition to the growing literature on Water Footprinting. Uncertainty and sensitivity analysis of models is a necessary step in all model development, and it is quite surprising that this is indeed the first quantitative assessment of water footprint uncertainties. Nevertheless, it is good that such an assessment has been made. However, the current study does not capture the whole uncertainty related with estimating and using Water Footprints in water management. I have a few suggestions on broadening the work and putting it more into context, the authors can consider, which comments to implement in this paper and which to leave for further studies. (In all cases, I would recommend mentioning the limitations of the current pioneering study more explicitly.)

1) What should be made more clear in the paper is that this study applies only to input parameter uncertainty/variability. In a more general uncertainty assessment there are three components of uncertainty: input parameters, model structure and scenarios. The current study focuses only on the first aspect, assuming that model and scenarios are certain and without error. The authors discuss this limitation, but only in the overall conclusions. I would suggest putting the scope of the study clearly in the title and the abstract. Perhaps a title like "Sensitivity to parameter uncertainty in crop water footprint accounting: ..." would be more specific.

2) Only four input parameters have been included in the uncertainty assessment (precipitation PR, reference evapotranspiration ET0, crop coefficient Kc and planting date). It would seem that the model contains much more input parameters. Looking at the equations, it would seem that Ky, Ym, Smax and p would also be relevant factors. It might be difficult to obtain standard deviations for these, as the original research has been done long time ago, but they should at least be included in the sensitivity analysis (by perturbing them by +-20%).

I'm also somewhat familiar with the crop water budgeting method by Allen et al. 1998 and the follow-up work which is applied in the "Checkbook method of irrigation scheduling" across the US. Looking at the implementation of the work here, it is unclear how rooting depth and available water holding capacity (AWC) have been implemented. Soybeans have much shallower rooting depth than wheat, which affects the drough stress and water uptake quite considerably. The authors apparently use an average AWC for the whole grid cell. Some more details on the approach and its limitations would be appreciated in the paper.

AWC affects also the starting level of the soil moisture S0. Increasing the water holding capacity and the starting level of soil moisture (through increased organic matter) can be a highly effective way of mitigating irrigation needs by storing off season rainfall. It is surprising that this is not discussed in the paper. This has also significance for the conclusions of the study, as delaying planting (as recommended by the authors) will usually result in a lower level of S0, since water is lost through evaporation before crop seeding/planting.

Also based on the report, it would seem that the soil moisture is simulated based on eq. (1). Eq. (1) contains several parameters, which are likely to be quite uncertain (or at least very difficult to estimate), such as capillary rise, runoff and deep percolation. In the current version of the paper, these are not sufficiently discussed and they should again be included at least in the sensitivity analysis part of the study.

3) A submodel for crop growth would definitely be needed. Now it is assumed that water is the only limiting factor for growth and water demands are not linked to expected yields. According to FAOSTAT, the average wheat yields in China have increased from 3.5 t/ha in 1995 to 4.3 t/ha in 2005 and to 5 t/ha in 2012. The increase in yields should be represented in the water requirements and vice versa, the irrigation benefits should not exceed other crop growth limitations.

Also now it is assumed that the growth time of crops is constant irrespective of planting date. This is a rough estimate, which reduces variability between years. A more common approach is to assume a constant growing degree days requirement for the crop. (GDD = sum(T-t)), where T is daily temperature and t is a threshold temperature.) The authors have all the required data available for the calculation and it would not be a significant effort to include this. At least the authors could check whether an inclusion of GDD would change the conclusion that later planting is beneficial to WF.

4) The economics of irrigation should be investigated in more detail. There is a cost of irrigating and an assumed benefit from increased yields (subject to other constraining factors, such as nitrogen availability). On a dry year, the costs of irrigating are higher than on a wet year. The world price for wheat has increased from the level of about

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100 US $ in the 2000 to about 300 US $ now (indexmundi.com). Currently the model assumes, that irrigation is applied to fully meet the requirements of the crop and on a wet year, less is applied. It might also be plausible, that on a good year, fertilizer inputs are increased and more is irrigated, as irrigation water is plentiful and yield potentials are good.

Also the efficiency of irrigation is not considered. With increased evapotranspiration rates, also the efficiency of irrigation decreases. It would be straightforward to add a parameter for irrigation efficiency.

5) The authors should be more careful in making conclusions about the reality based on the WF model. The aim of the current study has been to map the effect of input parameter uncertainty to model output. A separate study would be needed to map the effect to actual reality, and this study would have to include also the model and scenario uncertainties.

The finding about delayed crop planting dates underlines this problem. According to the model delayed planting would increase yields and decrease irrigation. However, the model currently does not take into account the loss of available water in the beginning of the growing period, the effect of delayed planting on growing days or the harvestable quality of the crop after delayed planting. If the authors wish to keep the recommendation of delayed planting in the paper, they should at least reference agronomic studies done on the region on the actual yield effects of delayed planting.

6) It is somewhat unclear, how spatial resolution has been used in the study. This could be made more clear in the text (i.e. if precipitation has been changed, has it been done similarly across the whole region or for each cell). If at all possible, presenting the Monte Carlo results in a map with the variability in yield and water footprint per grid cell would be a valuable visual addition to the study. Presenting basin level aggregated results hides much of the valuable variation between subregions. I would assume for example, that some areas would be much more vulnerable to drought than others.

7) Although this falls outside the scope of this study, I would like to point out that picking out only EF from equation (1) and using that as an indicator for the effects of agriculture on the water resources might be too narrow a view. Agriculture affects also run-off, deep percolation and capillary rise. The overall effect on water quality and availability may be either positive or negative, depending on the background conditions. In degraded landscapes, well designed agricultural practices may result in increased aquifer recharge through reduced runoff (i.e. increased infiltration) and reduced capillary rise (i.e. increased soil cover and increased subsurface moisture). In these circumstances, the beneficial effect may offset the losses through increased evapotranspiration. When interpreting Water Footprint results, it should be highlighted that the indicator focuses only on a part of the water cycle and that the results cannot be interpreted as negative without looking at the reference land use. This has become common practice in life cycle assessment (LCA) land use impact assesment. Perhaps some of the lessons learned in that field could be transferred also to Water Footprinting.

Some minor technical comments: - eq. 2-4 could be explained in text, they are conceptually simple - eq. 5-6 the factor 10 is missing an unit, therefore the overall units do not hold. Please add the unit or convert it into factor k in the equation and express the units and the magnitude in the text. - it is straightforward to test for normality of the distributions, please include the results for such a test. In the case the normality test fails, lognormal distributions could be considered. - the refereneces for crop moisture stress are now for Mekonnen and Hoekstra. Please refer to the original research. - in Fig 3. a constant scale for the y axis would be better. Now it would seem that rice and maize WF have similar sensitivities, while the former has much less sensitivity. - Fig 2. The secondary y axis does not start at zero, which overexaggerates the variability

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