**Response to Interactive comments on "Sensitivity and uncertainty in crop water footprint accounting: a case study for the Yellow River Basin"**

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Dear Dr. Tuomas Mattila,

We appreciate your very valuable and helpful comments and suggestions concerning our manuscript. We have studied all the comments carefully and responded as follows:

**Response to Dr. Tuomas Mattila:**

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.

Response #1: We agree your suggestion. A better adopted title would be "Sensitivity and uncertainty in crop water footprint accounting due to uncertainties in precipitation, reference evapotranspiration, crop coefficient and planting date: a case study for the Yellow River Basin". But it is too long. So we propose to keep the current title and emphasize the scope of the study clearly in the abstract and introduction of the revised paper.

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.

Response #2: We agree that the variability of parameters Ky, Ym, Smax and *p* could also affect the output. The current study is limited to testing the sensitivity and uncertainty in crop water footprint accounting due to uncertainties in the four variables (PR, $ET_0$, $K_c$ and planting date). The *p* value is defined by $ET_0$ and $K_c$ (Allen et al., 1998). Therefore, its sensitivity was tested indirectly while testing $ET_0$ and $K_c$.

The rooting depth varies with the crop type and increases over the growing period allowing the crop to get more water. The maximum rooting depth of the same crop type is longer when it is under rain-fed condition than when it is under irrigation (Allen et al., 1998).

We have taken an average value of AWC for the whole grid cell. The 5 arc minute spatial resolution is the highest resolution data we were able to get. In order to initiate the initial soil water level (S0), the model was run one year earlier with furrow land. This way, we have tried, at least, to get close to the actual soil water balance at planting. We acknowledge the referee's valuable comment regarding the use of organic matter to increase water holding capacity of the soil. However, this is out of the scope of the current study so have not tested its effect.

The model is built based on Eq. (1) and the components irrigation water (which equals to the irrigation requirement in the study), runoff and deep percolation were estimated by S, ET and PR. We will provide some more detail on this in the 'Method' section. We will also discuss the limitation in more detail in the revised paper.

This study has focused on PR, $ET_0$, $K_c$ and planting date, not seeing all input variables and parameters (Page151, lines 8-10). We will highlight this limitation in the revised paper.

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.

Response #3: A sub-model for crop growth and consideration of constant GDD for the crop growth would definitely improve the quality of the final modelling results. However, the model we used is a simple daily vertical soil water balance model as described in 3.1 (Page139, lines15-20). For this reason we will not be able to implement the suggestion for the current study.

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 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.

Response #4: The economy of irrigation is crucial for agriculture water management. It will surely be interesting and valuable to link the crop water footprint assessment to economics in irrigation water usage. However, it is beyond the scope of the current study. In our modeling, the irrigation water equals to the net irrigation requirement and is assumed to be met by sufficient irrigation water supply. This way we have implicitly considered the irrigation efficiency at field level but not the water loss through the canal and reservoir.

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.

Response #5: The suggestion on a separate study including model and scenario uncertainties in order to map the effect to actual reality is very valid and worth including in future studies.

With regards to the effects of delaying crop planting date on crop yield, we will discuss the limitation of the conclusion and include reference to existing published agronomic studies by Jin et al. (2012) and Sun et al. (2007) in North China, which support the conclusion for the increased maize yield (Jin et al., 2012; Sun et al., 2007) and decrease wheat yield (Sun et al., 2007) to late sowing date from local field experiments.

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.

Response #6: We acknowledged that uncertainties in crop water footprint estimation are scale dependent, and decline with growing extent of the considered study region. This study focused on the uncertainties in aggregated water footprint for the whole basin. We will highlight the limitation in the revised paper.

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.

Response #7: We appreciate the referee very much for such valuable ideas. Although it is out of the scope of this paper, the comment is very instructive. We agree that agriculture also affects runoff, deep percolation and capillary rise. With water footprint method, what we measure is the water consumed by a certain crop that cannot be reused. That is why we only focus on the ET in water footprint assessment.

**Some minor technical comments:**

- 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.

Response: The factor 10 is dimensionless and is applied to convert the unit mm to $m^3ha^{-1}$. We will re-write the sentence in the revised paper.

- 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.

Response: We have given detailed explanation on this comment in reply to Anonymous referee #1. Please refer to the "Response #1" in the reply to Anonymous referee #1.

We accept the other technical comments and we will address them in the revised paper.

**References:**

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