Referee comment to

Title: An improved method for calculating regional crop water footprint based on hydrological process analysis
Author(s): Xiao-Bo Luan et al.
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Summary and general comments

The paper presents a semi-distributed approach to model effective water resource requirements in crop production in terms of the volume of water used per unit crop production. The approach differentiates between green and blue water sources and puts emphasis on conveyance losses of irrigation water.

Modelling of the water cycle is based on SWAT, while conveyance losses between the water inlet of the irrigation scheme and the field are modelled depending on the location according to a new approach that, apparently, has not been published before.

The novel contribution to the field of science by this study is limited to the location-dependent modelling of conveyance losses, which can potentially have significant effect on crop water footprint calculations. Unfortunately, the derivation of the approach is neither explained in much detail nor is its validity tested against measured data.

Overall, the presentation of the theoretical background, methods and results is rather poor and, at least partly, hard to understand. The language is unprecise and redundant in major parts of the paper. It leaves room for interpretation (eg lines 64-66) and numerous sentences/paragraphs are unintelligible (e.g. lines 86-87,90-93, 104-105, 207-209). I am not a native English speaker but I feel the text needs revision with regards to pure language issues (grammar, mode of expression).

The paper does not provide a critical discussion of the approach and the results. In particular, uncertainties of inputs and results are hardly addressed. Major parts of the discussion section basically repeat the contents of the introduction. The conclusions section is basically a summary of the results and the few conclusions made are trivial. The title does not match the content of the manuscript (see comment on the term “water footprint” below).

Detailed comments on substantial shortcomings of the manuscript

The authors refer to the water resource requirements of crop production as “water footprint”, which is inappropriate two reasons. Firstly, indirect water uses, an important aspect of a footprint indicator, are not considered in the study. Secondly, the paper lacks a clear definition of the system (consumer or producer) that causes the footprint.

The paper presents water resource requirements for the production of three different crops (m^3 water use/t of crop production, referred to as “water footprint”) in subbasins of the Hetao Irrigation District (HID). Obviously, the “water footprint” is defined for a producer. It is not stated whether the footprint figures are calculated for (a) a single producer, i.e., the aggregate of “farms” growing a single crop type in the HID, or (b) many different producers, i.e., the aggregates of farms growing that crop within individual subbasins. However, this is important in order to understand the results correctly. In case (a) the volume of water used to produce x_i tonnes of crop in subbasin i needs to be related to the total crop production in HID (X). If r_i is the water resource requirement in subbasin i, the water footprint of the HID-wide crop production in subbasin i calculates as $F_i=x_i/X*r_i$. In contrast, the water footprint of subbasin-wide crop production (case (b)) in subbasin i is given as $F'_i=r_i$. Note that in case (b), the "water footprint" indicator is no longer geographically explicit, another important aspect of the water footprint, as the subbasins are the smallest geographical units presented.

The range of results shown in the maps implies that the water footprint is defined according to case (b). However, water resource requirements for crop production are intrinsic properties of the irrigation system in each subbasin and are independent of the actual allocation of crop production. Hence, the study is not a footprint analysis but, simply, an analysis of resource requirements (comparable to a potential analysis). However, the representativeness of the results is questionable due to methodological limitations. Subbasins are subdivided into hydrological response units (HRU) based on land use (supposedly land use=crop type) and soil type. Although it is not stated explicitly, one must assume that the results on HRU-level, based on the actual pattern of crop allocation and irrigation timing/quotas, are aggregated to subbasin-level (aggregation method not specified). This way, the results are only representative for potentially small parts of a subbasin, i.e., one or more HRUs within a subbasin under the given crop, as the conditions (soil type, canal losses, etc.) may be different in the remaining parts of the subbasin. The reader cannot judge the related uncertainties as the actual patterns of crop allocation and soil types are not shown.

The description of the methods to calculate the "water footprint" is difficult to understand. As the system boundaries are not defined precisely, the reader is forced to examine several possible system boundaries in order to judge whether the equations 6-9 are likely to be correct. For instance, it depends on the system boundary whether field discharge (Q_d) is actually consumption, i.e. it is a flow out of the system (to another basin or the sea), or returns to system itself. As the authors stress that the approach is regional-scale, a certain share in field discharge is likely a return flow, which would invalidate equation 7, which defines field discharge as water consumption. Equations 6-9 use a set of variables that are calculated for two different scenarios (s1=with irrigation, s2=without irrigation) but the notation is ambiguous as the scenario is not clearly indicated in the equations except for for ET (index s1 or s2). It might be considered obvious that canal losses (Q_c) and ET of field irrigation (Q_f) is only defined for the scenario with irrigation (s1). (Note, those variables can also be defined for s2, though with a value of zero.) However, capillary rise of groundwater (Q_g) and field discharge (Q_f) definitely can have non-zero values for s2. Hence, it must be indicated from which scenario the values are taken.

Q_g must not be added in eq 7. Although Q_g is per definition blue water, it simple changes soil moisture. The share of Q_g that is consumed is already included in Q_f+Q_d.

As I understand, canal losses in eq 7-8 are informed by the modelling approach represented by eq 10-15 but it remains unclear which of the variables mentioned in eq 10-15 are actually used and how. The notation of eq. 10-15 is confusing as I suspect most readers are familiar with a notation where n is the total number of elements and i is a running index. Here, it is used the other way around, which is not wrong but makes it more difficult to understand.

The section on calibration and validation of the model is wordy and interrupts the description of the modelling approach. For instance, the R^2 metric is widely used and there is no need to show the formula. If equations 2-4 are considered necessary, the notation should be corrected as the index i is missing in numerous terms.

Conclusions

Given the shortcomings addressed above, the quality of the manuscript is, in my opinion, not acceptable for publication, although the underlying material fits the scope of the journal and might be worth publishing. Due to missing definitions and precise description of the methods, I can hardly judge the validity of the work. I think the necessary revisions are too extensive to be done within a peer-review process. Apart from this, addressing all the issues where I see the need for revision in this reviewer comment would be an unreasonable effort. Therefore, my recommendation is to reject the paper.